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THE BEGINNINGS OF AUSTRALIAN RADIO ASTRONOMY

Woodruff T. Sullivan, III
Department of Astronomy, Box 351580, University of Washington,
Seattle, WA 98195, USA.
E-mail: woody@astro.washington.edu

Abstract: The early stages of Australian radio astronomy, especially the first decade after World War II, are described in detail. These include the transition of the CSIRO Radiophysics Laboratory, under the leadership of Joseph Pawsey and Taffy Bowen, from a wartime laboratory in 1945 to, by 1950, the largest and one of the two most important radio astronomy groups in the world (with the Cavendish Laboratory at Cambridge University). The initial solar investigations are described, including discovery of the hot corona and development of the sea-cliff interferometer. During this same period painstaking 'radio star' observations by John Bolton and colleagues led to the first suggested optical identifications of Taurus-A (the Crab Nebula), Centaurus-A (NGC 5128), and Virgo-A (M87).

The factors that led to the extraordinary early success of the Radiophysics Laboratory are analyzed in detail, followed by discussion of how the situation changed significantly in the second decade of 1955-1965. Finally, the development of major Australian instruments, from the Parkes Radio Telescope (1961) to the Australia Telescope (1988), is briefly presented.

Keywords: history of radio astronomy, Australia, CSIRO Division of Radiophysics, Sun, radio sources

1 INTRODUCTION

Australian radio astronomy has been at the forefront since its foundation during World War II, with imaginative scientists and engineers, innovative equipment, and strong sponsorship. Soon after War's end a multifaceted program, by far the largest of its kind in the world, was well established at the Radiophysics Laboratory (RP) in Sydney and continually producing pioneering results. The Australians developed fundamental methods of interferometry, discovered the Sun's hot corona, pinpointed the location of solar bursts, and discovered numerous discrete radio sources. By the late 1950s they had produced far more papers in radio astronomy than any other group in the world, and were acknowledged leaders in research on the Sun, Galactic structure, and radio sources. But as new telescope projects became ever more costly, tensions developed over which ones could be funded, resulting in many of the key researchers leaving RP in the 1960s. RP's subsequent work focused on the Parkes Radio Telescope (1961) and Culgoora Radioheliograph (1967), while Sydney University sponsored the Molonglo Cross (1965) and the Fleurs Synthesis Telescope (1973). In subsequent years, these instruments were continually improved, but to remain competitive with the rest of the world eventually a yet larger, national centerpiece was needed—this became known as the Australia Telescope (1988).

How was it that a small, isolated country such as Australia succeeded so impressively in such an arcane field as radio astronomy in the mid-twentieth century? The answers go back to World War II and Australia's relationship with the mother country, as well as the Australian government's policies toward its scientific laboratories. First, a strong community of radio physicists developed in Australia in the 1930s, based on intimate ties with the ionospheric community in England. Second, Britain shared the secret of radar with its Dominions as the War began, nurturing intense radar research, development, and manufacture in Australia. Third, the team of scientists and engineers that grew out of that effort, primarily at RP, remained intact at War's end, and soon put their new skills to use in developing peacetime research ventures. And finally, for two decades dynamic and skillful leadership was provided by E.G. 'Taffy' Bowen and Joseph L. Pawsey—two men whose styles of science and complementary personalities produced a favorable mix for exploring and exploiting the most profitable avenues into the radio-sky.

It is impossible to relate the entire story in the alloted space—major published sources for various aspects of the history of Australian radio astronomy are by Bhathal (1996), Collis (2002: Chapter 13), Goddard and Milne (1994), Haynes et al. (1996: Chapter 8), Orchiston and Slee (2005), Orchiston, Sullivan and Chapman (2006), Robertson (1992), and Sullivan (1988). More general histories, of which major portions cover Australia, are by Edge and Mulkay (1976), Hey (1973), and Sullivan (1982, 1984a, 1984b). This paper is based on and is substantially similar to Sullivan (1988), although Section 6 is wholly new. I treat the first decade after World War II in some detail, and only briefly cover the ensuing years, for it was during this period that the technical, scientific and cultural foundations were laid for many decades of success.

2 THE RADIOPHYSICS LABORATORY DURING THE POSTWAR DECADE

2.1 Transition to Peacetime

The Radiophysics Laboratory (RP) had been established in 1939 in the grounds of Sydney University as a secret branch of the Council of Scientific and Industrial Research (CSIR). Its staff was largely drawn from the strong radio ionospheric community that had been built up during the 1930s by J.P.V. Madsen at Sydney University, T.H. Laby at Melbourne University, and the Sydney research laboratory of Amalgamated Wireless (Australasia), Ltd. (AWA) (Gillmor, 1991). During World War II, RP both designed wholly new radar systems and adapted British radars to Australian needs, and by War's end the staff numbered over three hundred, of whom sixty were professionals and fourteen bore names that would later become familiar in radio astronomy.

As the War closed, various memoranda began to circulate on potential peacetime roles for the Laboratory, culminating in an agenda paper put together by Taffy Bowen (1911–1991) (Figure 1) for a meeting of the CSIR Council in July 1945 (Bowen, 1945a). Bowen, who would soon take over as Divisional Chief, had been working on radar for over a decade (Bowen, 1987). He had been trained in physics at the University of Wales and studied atmospheric physics for his Ph.D. under E.V. Appleton at the University of London in 1933. Two years later, Robert Watson-Watt coopted him into the initial team of four that developed the first operational military radar systems, which soon became vital in the defense of Britain against the Luftwaffe. He led the development of 200 MHz airborne radars, for which he flew thousands of hours (often with Robert Hanbury Brown, who would himself later become a central figure in the development of radio astronomy). In 1940 Bowen was a member of the famous Tizard Mission that delivered radar secrets to the United States, including the cavity magnetron, the first source of power sufficient to make microwave radar a feasible proposition. He remained in the USA for three years, eventually developing airborne radar systems at the MIT Radiation Laboratory. In early 1944 he went to RP as its Deputy Chief, and, although still officially on loan from the British, soon took a liking to RP and to Sydney and spent the remainder of his career (until 1971) there as Chief.

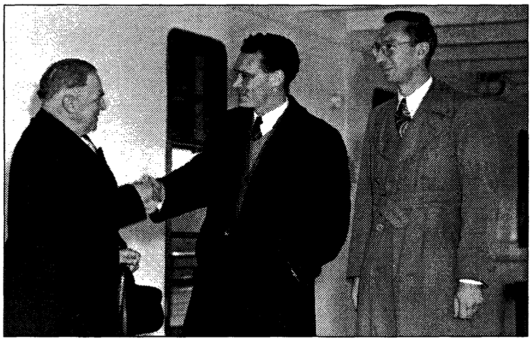

Figure 1: E.G. Bowen (center) and J.L. Pawsey (right) greet E.V. Appleton on board ship after his arrival in Sydney in 1952 for the URSI General Assembly (courtesy: ATNF Historic Photographic Archive).

Bowen's proposals for RP's peacetime role were warmly received and quickly endorsed by his CSIR bosses A.C.D. Rivett and Frederick G.W. White, a New Zealander who preceded Bowen as RP Chief for three years. Rivett felt strongly that each CSIR division should achieve a roughly even balance between free-running basic research and applied research, a vital element in the culture of CSIRO. RP's proposed program thus emphasized new scientific possibilities as well as areas where Australian commerce and industry would more immediately benefit. Bowen and his staff were clearly as excited about the potential of radar techniques in peace time as they were weary of applying them to warfare. It mattered not that RP's original *raison d'être* had disappeared—fresh directions could now be charted. The agenda paper enthused that the new radar techniques were "... perhaps as far-reaching in themselves as the development of aircraft [during World War I] or the introduction of gunpowder in a previous era." It laid out a long shopping list of possible projects in radio propagation, vacuum research (directed toward generating power at millimeter wavelengths), radar aids to navigation and surveying, and radar study of weather. These topics, along with producing the *Textbook of Radar*, which incorporated RP's knowledge and was edited by Bowen (1947b), were to form the initial postwar program.

At this time, RP was CSIR's glamour Division, arguably containing within its walls the densest concentration of technical talent on the continent, and CSIR was eager to keep this 'winner' intact. As Frank Kerr, one of RP's early staff members, recalled:

> [Basic radio research] ... was thought of as a good subject for the Lab to get into, partly in order to keep the Lab in being because it was a collection of good people, well trained in the arts of radio. Especially at that time there was a feeling that it had been a great national value to have had the Lab, and so it was possible to sell the idea to the authorities that the group should be kept in existence as a 'national asset'. (Kerr, 1971: 7T).[1]

Keeping the best of the research staff at RP was also immeasurably aided by the fact that research in physics and engineering at Australian universities after the War was minimal; Government funds for university scientific research were thirty times less than for CSIR. In contrast to the situations in England and the United States, the young cadre of RP researchers saw their wartime Laboratory as the best place to continue their peacetime careers. Despite a considerable reduction of staff at War's end, most who left were not oriented toward research. Few researchers had come from the universities (except as recent graduates) and fewer returned. Academia's role in Australian physics research would not strengthen until the 1950s when students were first able to do their postgraduate work at home (Home, 1982-83).

RP's assets included not only its scientists and engineers, but also its significant support staff of technicians, a camaraderie molded during the War, ample laboratory space and workshops, and bulging stores with the latest radio electronics. This last was considerably augmented shortly after the War's end by an extraordinary bonanza. A large amount of American and British equipment (including whole aircraft!) was being discarded by loading it on the decks of aircraft carriers, taking it a few miles offshore, and bulldozing it into the sea. Bowen got wind of this, however, and for two or three weeks was allowed to take RP trucks down to the Sydney docks and load them up with radar and communications gear, often in unopened original crates. For several years thereafter RP researchers drew on this surfeit.

While RP's continued existence was assured, its direction changed from developing military hardware to a mixture of fundamental research and applications of radio physics and radar to civilian life. This policy was one of the key ingredients of RP's postwar success; in his 1945 strategy document Bowen stated that peacetime military work by CSIR "... stifles research and seldom produces effective assistance to the Armed Forces." In 1949, CSIR became CSIRO, the Commonwealth Scientific and Industrial Research Organization, and White became its number-two man

—over the next two decades he was a major force in fostering the growth of his old division (see Minnett and Robertson, 1996).

2.2 Overall Research Program

Major programs at RP waxed and waned over the years 1946-1953. The plots of Figure 2 show the bare trends,[2] but one of the leading researchers, namely Paul Wild (1965), more elegantly likened these years to the Biblical parable:

> A sower went out to sow his seed, and as he sowed some fell by the wayside and it was trodden down and the fowls of the air devoured it. And some fell upon a rock, and as soon as it sprung up it withered away ... And other fell on good ground, and sprang up and bare fruit an hundredfold. (i.e. Luke 8: 5-8).

Vacuum physics work died away within two years and work on radar applications steadily lessened over the first five years. The two research programs that grew were radio astronomy (although note that this term was not used until the 1949 report) and rain and cloud physics. Between 1946 and 1949 these increased their share of the professional staff from 6% to 63%. Because the total staff grew by only one-quarter over this same period, there were clearly many reassignments of personnel. In terms of papers published in the scientific literature, radio astronomy and rain and cloud physics also dominated, accounting for 71% of the papers by 1949 and 65% over the eight-year span. The radio astronomy staff (compared to cloud physics) produced more than double the number of papers per person.

Rain and cloud physics, in which Bowen himself specialized and which he personally oversaw, attempted to understand the way clouds and rain behave. Microwave radar measurement of clouds and rain, often from aircraft, became a central technique. Buoyed by one of the first successes in seeding clouds, the RP group hoped that rainmaking for the dry Australian climate would ultimately become a reliable and economic proposition. Although this never happened, Bowen's group became one of the international leaders in the field (Home, 2005). This effort, as well as the development of radar systems for commercial aviation, such as a distance-measuring equipment allowing airliners to locate themselves relative to beacons, were important as practical areas balancing off fundamental research in astronomy, fast becoming RP's most visible sector (Bowen, 1984: 105-109; Minnett, 1978: 66-68T). But even radio astronomy was sometimes shoehorned into the role of practicality, as in the 1949 *Annual Report*:

> Radio astronomy has already made important contributions to our knowledge and, like any fundamental branch of science, is likely to lead to practical applications which could not otherwise have been foreseen. For example, attempts to explain how certain types of radio waves arise in the Sun are already leading to new techniques for the generation and amplification of radio waves.

Other projects included a mathematical physics section and (after the late 1940s) a group developing an early electronic computer (CSIRAC). And there were always a few ionospheric radio projects going on. The one example of a major effort that failed was vacuum physics, when costs of a desired particle accelerator became too great.

2.3 Growth of Research on Extraterrestrial Radio Noise

Buried in the twenty-four pages describing RP's post-war plan is a fraction of a page under "Radio Propagation" called "Study of extra-thunderstorm sources of noise (thermal and cosmic)":

> Little is known of this noise and a comparatively simple series of observations on radar and short wavelengths might lead to the discovery of new phenomena or to the introduction of new techniques. For example, it is practicable to measure the sensitivity of a radar receiver by the change in output observed when the aerial is pointed in turn at the sky and at a body at ambient temperature. The aerial receives correspondingly different amounts of radiant energy (very far infrared) in the two cases. Similarly, the absorption of transmitted energy in a cloud can be estimated in terms of the energy radiated to the receiver by the cloud. None of these techniques is at present in use. (Bowen, 1945a).

It was this enigmatic paragraph, with its heading designed primarily to indicate that it was *not* talking about thunderstorm noise (atmospherics), that would develop into RP's radio astronomy program! It surprisingly did not explicitly mention *solar* noise, but instead proposed an exploratory program of "very far infrared" radiometry wherein antennas would be pointed to different parts of the sky.

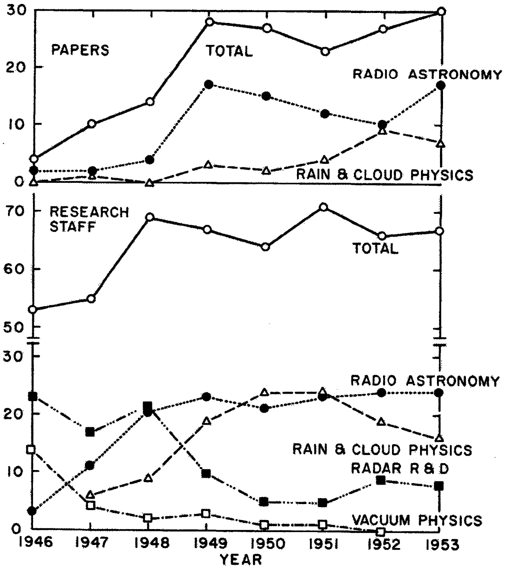

Figure 2: The growth and decline of different research areas at RP over the period 1946-1953, as gauged by the number of published papers per year and the number of research staff. Staff levels not plotted include those for ionosphere (which fell in a similar manner to vacuum physics) and mathematical physics and electronic computing (which rose to a level of about 10 by 1951-1953).

But when reports of anomalies arrived from radar stations (Section 3.1), Pawsey and his colleagues jumped on solar observations in October 1945 and never turned back. Joseph L. Pawsey (1908–1962; Figure 1) by this time had become the linchpin and recognized leader of RP's fundamental investigations through his Propagation Research Group. He had studied physics at Melbourne and obtained his Ph.D. in 1934 under J.A. Ratcliffe at Cambridge, with a dissertation on radio waves reflected off the abnormal E layer of the ionosphere. For five years he then developed equipment needed to make television a viable reality at the famous BBC station at Alexandra Palace. Pawsey's main contributions, which involved no fewer than twenty-nine patents, were in designing the transmission lines and antennas necessary for television's broad bandwidth. After the outbreak of WWII he hastened home and joined the RP staff early in 1940. He became the local 'wizard' on antennas and transmission lines, but by War's end he had also gained new skills working on receivers, operational aspects of radar systems, and atmospheric propagation. Just as importantly, the intense wartime environment had cultivated and honed his abilities to lead scientific research teams. For further biographical details of Pawsey, see Christiansen and Mills (1964) and Lovell (1964).

Work on extraterrestrial noise steadily grew, with Bowen as Chief and Pawsey as his right-hand man (in charge of most research activities) willing to shift resources toward those persons showing superior results or great promise. This flexibility was the CSIR style, largely molded by Rivett, who believed that research programs should be based on people, not topics—getting the right people and then letting them loose. As Bowen (1978: 42T) recalled:

> We tried many things, but the criterion for going on with any program was, of course, success. And the things that Pawsey was trying on the Sun and Bolton on point sources were so outstandingly successful that that's the way we went ... With our first-rate staff as a handout from the War, we had the freedom and the encouragement to find new projects.

Or as Pawsey (1961: 182) put it:

> [Scientific directors must] ... very quickly make decisions and supply facilities for the really promising developments. In all too many cases elsewhere the energies of scientists are taken up in advertising the potentialities of their prospective investigations in order to obtain any support at all.

As the years passed, work on solar and cosmic noise grew in importance at RP and a circle of group leaders emerged (most are pictured in Figure 3). Besides his overall supervision, Pawsey led a large group studying numerous aspects of the radio Sun (Section 3). In 1947 John G. Bolton began his pioneering work on discrete radio sources (Section 4) and soon had an active group around him. J. Paul Wild arrived in 1947 and, after a year languishing in the instrument test room, moved into research on solar radio bursts with a swept-frequency receiver. Bernard Y. Mills worked on a variety of projects before permanently switching in 1948 to radio astronomy; he briefly researched the Sun and then began his own program on discrete sources (see the paper by B. Slee in a forthcoming issue of this journal). W.N. 'Chris' Christiansen arrived at RP in 1948 from AWA and immediately plunged into his own solar research program. He was unique among this group in that, despite his career as an antenna engineer, he had long wanted to be an astronomer (Christiansen 1976: 1, 6T). Jack Piddington and Harry C. Minnett began a program of microwave research in 1948 (for example, measuring the brightness temperatures of the Moon), and Frank J. Kerr and C. Alexander Shain started on lunar radar in 1947 in order to study the ionosphere. Finally, Stefan F. Smerd and Kevin C. Westfold complemented all of the observational work by working on the theory of solar radio emission.

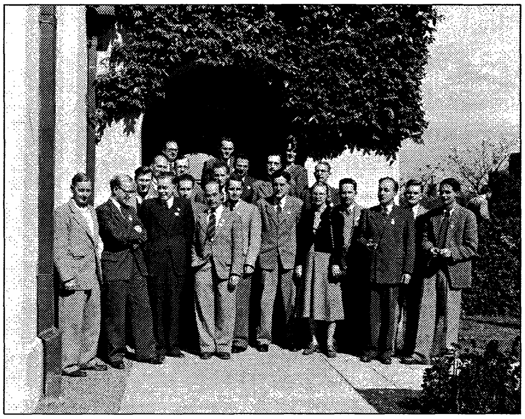

Figure 3: Radio astronomers at Sydney University for the 1952 URSI General Assembly. Left to right, ground level: W.N. Christiansen, F.G. Smith (England), J.P. Wild, B.Y. Mills, J.-L. Steinberg (France), S.F. Smerd, C.A. Shain, R. Hanbury Brown (England), R. Payne-Scott, A.G. Little, M. Laffineur (France), O.B. Slee, J.G. Bolton. First step: C.S. Higgins, J.P. Hagen (USA), J.V. Hindman, H.I. Ewen (USA), F.J. Kerr, C.A. Muller (Netherlands). Second step: J.H. Piddington, E.R. Hill, L.W. Davies (courtesy: ATNF Historic Photographic Archive).

Among all these successes the RP archives also give evidence of at least one important (in retrospect) missed opportunity, that of discovering the 21 cm spectral line arising from interstellar hydrogen. The line had been predicted in 1944 in Holland, and its 1951 discovery at Harvard and Leiden Universities was to be one of the major turning points in early radio astronomy. Pawsey first got wind of the idea in early 1948 while on a tour of the United States. His report home triggered two years of intermittent activity at RP. Wild published a full theoretical analysis, Bolton and Westfold translated a Russian paper and were eager to search for the line, and Mills also gave the hunt serious consideration. But despite all this activity, in the end the decision every time of Bowen, Pawsey and their staff was to not pursue the line. For example, Mills was looking for an independent line of research on cosmic noise in 1949. He and Pawsey discussed two main avenues:

> One was a search for the hydrogen line. Pawsey was very interested in it at the time. And the other was trying to locate very precisely the positions of radio sources. And it was a difficult decision to make. I eventually chose the precise positioning because I was more familiar with some of the techniques, and it looked as if it was something

> that would lead to an immediate result, whereas the other was extremely speculative. (Mills, 1976: 6-7T).

Mills went on to make vital contributions to knowledge of discrete sources, so the decision in his case not to pursue the hydrogen line can hardly be called a managerial mistake. Nevertheless, given its resources and technical expertise, the fact remains that RP surely would have soon succeeded in detecting the interstellar 21 cm line if it had ever made a serious effort. As it turned out, RP *did* make first-rate contributions to 21 cm hydrogen observations in the early 1950s (starting with Christiansen and Hindman, 1952), but only after others had taken the initiative.

3 EARLY SOLAR WORK

3.1 Wartime Efforts

As radar receivers during the War became more sensitive and moved to higher frequencies, concepts of receiver noise, background noise, and antenna temperature gained currency:

> Receivers were getting more and more sensitive and we were concerned with the whole thermodynamic theory of their noise level and its relationship, through the antenna, to space—if the antenna were in an enclosure at three hundred degrees, what would be the noise level? This was different from the purely circuit approach that had been worked up by Nyquist and others ... And it obviously occurred to Ruby Payne-Scott and Joe Pawsey that radiation from objects might possibly be seen. I remember that Ruby had a small paraboloid poking out a window at certain objects in the sky to see how the noise level varied. (Minnett, 1978: 10T).

Ruby V. Payne-Scott (1912–1981; Figure 3), the only woman to make a substantial contribution to radio astronomy during the postwar years, was able to do so only because she kept her marriage secret from 1944 to 1950, when CSIRO changed its policy forbidding married women to join the permanent staff. But the following year she resigned from RP in order to raise a family, and never again participated in research. She trained before the War as a physicist at Sydney University, worked on cancer radiology, spent two years at AWA, and from 1941 on at RP mainly worked on display systems and calibration of receivers. She soon became known around RP for her considerable intellectual and technical prowess, forthright personality, and 'bushwalking' avocation.

It was in March and April 1944 that Pawsey and Payne-Scott (1944) first looked at the microwave sky. In their subsequent RP report they discussed various contributions to the noise power measured by a receiver-antenna combination and cited Karl Jansky's and Grote Reber's (1940) work in the US on cosmic static. But their operating wavelength of 10 cm was far shorter than that of earlier reported work on noise from either terrestrial or extraterrestrial sources. They used a 20 × 30 cm horn connected to a receiver with a system temperature of ~3500 K, one person pointing the horn around the room or out the window in various weather conditions, the other taking readings from a meter. Changes of 20 to 300 K in antenna temperature were noted, and Pawsey and Payne-Scott were particularly struck by the apparently low absolute temperature of the sky, less than 140 K. Moreover, they noted a "most unusual" consequence of this: inserting attenuation between the horn and receiver actually *increased* the output!

They also tried to detect microwaves from the Milky Way with the same receiver and a 4 ft dish pointing first in the vicinity of Centaurus and then away. There was no detectable difference, that is, less than ¼% (<10 K), "... very much less than that observed by Jansky and Reber." (ibid.). Appealing to Eddington's work in the 1920s (about which they undoubtedly learned from a citation by Reber), they ascribed the low signal to a very low temperature for the material in space.

These Milky Way results were accompanied by a single sentence stating that they did not try for any solar radiation. It would seem that they were then unaware of either J.S. Hey's British or G.C. Southworth's American secret reports on the Sun, but given that they mentioned the Sun at all, why did they not try for it? If they had, they probably would have easily detected a change in power output.[3]

These kinds of ideas were thus in the air around RP and therefore, as already discussed, merited a short paragraph in Bowen's proposed postwar program. But the archival evidence indicates that what really galvanized Bowen and Pawsey into jumping onto solar noise was not this preliminary experiment, nor reports from overseas, nor *ab initio* calculations, but the 'Norfolk Island effect'—solar radio bursts observed by New Zealand military radar stations from as early as March 1945 (Orchiston, 2005a). When Bowen learned of this in July 1945, he was entranced:

> These results are remarkable in that while one would expect to receive solar noise radiation on S. or X. band equipment [10 or 3 cm wavelength], a C.O.L. antenna and receiver at 200 Mc/s is quite unlikely to do so. I have heard rumours of the same thing happening in England, but as far as I am aware, the subject has never been followed up. We are therefore going to attempt to repeat the observations here in Sydney to see if we can track down the anomaly. (Bowen, 1945b).

This letter testifies that in August 1945 Bowen and Pawsey knew about thermal, microwave radiation from the Sun, presumably from Southworth's restricted report or his early 1945 paper, but were unaware of Hey's low-frequency solar bursts, either from his 1942 report or its later June 1945 version. Instead, their first investigations were triggered by the New Zealand work, which itself was never published as more than a laboratory report and a short paper in an obscure journal (Alexander, 1945, 1946; Orchiston, 2005a; Orchiston and Slee, 2002). Furthermore, the thrust of these investigations was toward monitoring the Sun for non-thermal bursts of radio waves, unlike what was stated in Bowen's proposed program.

3.2 Solar Bursts and the Sea-cliff Interferometer

Pawsey swung into action and mounted an observing program on a frequency of 200 MHz using existing Air Force radar installations along the coastline near Sydney. Working with him on this were Payne-Scott and Lindsay L. McCready (1910–1976), a receiver expert, pre-war AWA engineer, and RP veteran who at the time was Pawsey's deputy and eventually head of all engineering services at RP. The first observations were on 3 October from Collaroy, fifteen miles north of Sydney (Figure 4) and one-half mile inland. The antenna was an array of 32 half-wave dipoles (Figure 5), and observations were carried out by Air Force as well as RP personnel. After only a week or two of data, Pawsey (1945) noticed that the general level of "this noise effect" was highly variable and seemed to correlate with the number of visible sunspots. For the latter information he had made contact with Clabon W. 'Cla' Allen (1904–1987), a longtime solar astronomer at the Commonwealth Solar Observatory, Mt. Stromlo (near Canberra). After three weeks of monitoring, Pawsey, Payne-Scott and McCready (1946) sent a letter to *Nature* pointing out the close correspondence between the total area of the Sun covered by sunspots and the daily noise power from the Sun. Because the antenna's elevation angle could not be changed, observations were only possible at dawn or dusk and various corrections had to be made for ground and sea reflections, but it was nevertheless clear that the daily values of solar noise varied by as much as a factor of thirty over the three weeks. They also pointed out that, for a thermally emitting disk the size of the optical Sun, their detected levels implied 'equivalent temperatures' ranging from 0.5 to 15 × 10^6 K, much higher than the Sun's 'actual temperature' of 6000 K. Such incredible signals, they reasoned, could not come from atomic or molecular processes, but more likely from "... gross electrical disturbances analogous to our thunderstorms."

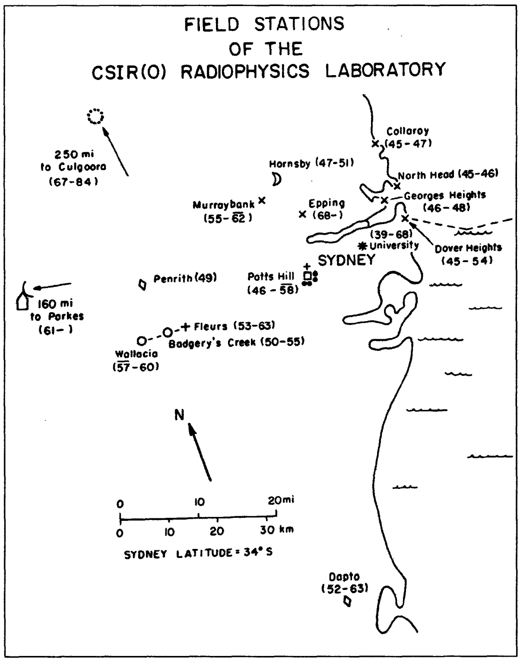

Figure 4: Sites of chief RP field stations and headquarters at Sydney University and at Epping. Each station has the years of operation indicated (years with a bar overhead are uncertain).

With such a promising start, Bowen and Pawsey decided to increase their efforts on solar noise, and continued monitoring for another ten months. Gradually Air Force personnel and equipment were phased out as RP took over. Pawsey's group made measurements at a variety of frequencies (but mostly at meter wavelengths), and used antennas (e.g. see Figure 6) located at four different coastal radar sites around Sydney: Collaroy, North Head, Georges Heights, and Dover Heights (Orchiston and Slee, 2005) (see Figure 4). And while these data rolled in, they also educated themselves about the solar atmosphere and began thinking about how it might emit radio waves. This led to Payne-Scott's discovery of incorrect calculations by Southworth, and to some changing interpretations. For instance, Bowen wrote to Appleton in January 1946 in order to comment on the latter's letter in *Nature* on radio noise and sunspots. Bowen (1946a) pointed out that RP had now obtained the "... first direct experimental verification of this effect ...", and that, unlike in the upcoming *Nature* letter from RP, he now felt that the solar noise was not "electromagnetic", but of thermal origin, either "... from the depths of the sun or in some way from the corona."

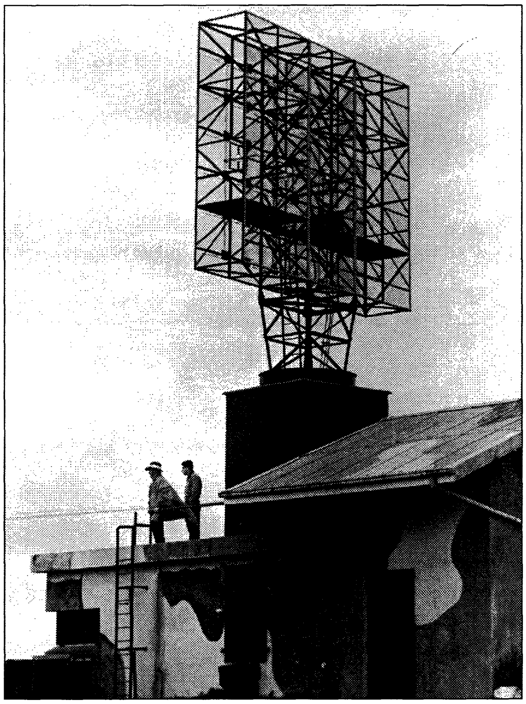

Figure 5: A 200 MHz wartime radar antenna for shore defense, consisting of a 36-dipole array rotatable only in azimuth, at Dover Heights (1941). This and a similar antenna at Collaroy were used by Pawsey's group in 1945-1946 as sea-cliff interferometers for observations of the rising Sun (courtesy: ATNF Historic Photographic Archive).

The climax of this initial period came in early February 1946 when by good fortune the largest sunspot group ever seen (until then) chose to make its appearance. When Allen phoned with news of the giant group (covering about 1% of the Sun's visible disk), the RP group intensified their monitoring and realized that they now had the opportunity to take advantage of a property of their antenna system that had previously been more bother than help. A single antenna situated on the edge of a cliff or a hilltop, looking near the horizon over a relatively smooth terrain or over the sea, in fact acts as an interferometer and can achieve far better angular resolution than would otherwise be possible. The interference in this case is between that portion of a wave-front directly impinging on the antenna and that portion reflected from the sea, which must travel an additional length equal to twice the cliff height times the sine of the source's elevation angle. In classical optics this arrangement is known as 'Lloyd's mirror', and the fringes obtained are equivalent to those with a conventional interferometer consisting of the antenna and an imaginary mirror image located under the base of the cliff. With the antennas at Dover Heights and Collaroy located 85 and 120 m above the sea, the respective fringe lobes at 1.5m wavelength were spaced by 30′ and 21′. In principle, then, one could locate objects with an accuracy of ~10′, far better than the ~6° beam of the antenna considered by itself.

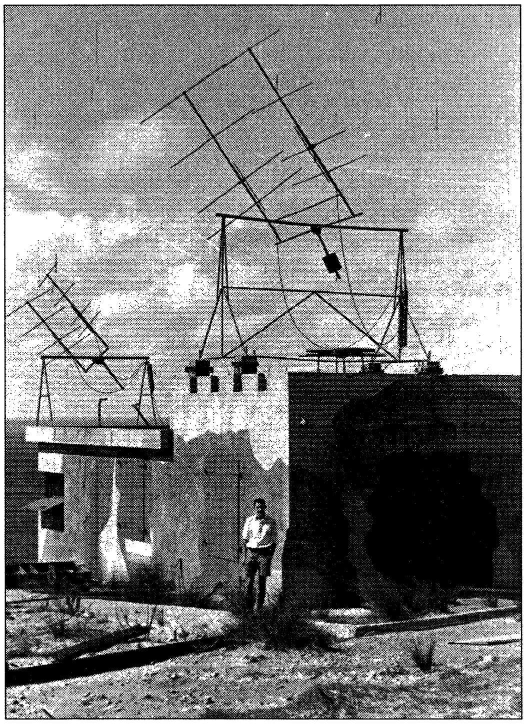

Figure 6: WWII blockhouse with 100 MHz (left) and 60 MHz (right) twin Yagi antennas at Dover Heights (1947). The same pairs, with the Yagi elements parallel and pointing toward the horizon, were used for the first studies and surveys of discrete sources by John Bolton (pictured) and his group (courtesy: ATNF Historic Photographic Archive).

This phenomenon was nothing new to those who had been developing radar systems, for during the War radar beams often pointed near the horizon, as with search radars on a ship or a coastline. The reflected signal from a distant aircraft was well known to oscillate as it passed through the fringes or lobes of such a radar. This effect was both a blessing and a curse to the radar systems designer, for it could be used to gather precise information on a target's height, but on the other hand it meant that low-flying aircraft could sneak in 'under' a radar, since the first lobe was *not* at the horizon, but above it by a considerable amount, especially if the antenna was not high above its surroundings.

So Pawsey and his colleagues used this sea-cliff interferometer[4] to advantage as the bespotted Sun rose over the ocean. The general level of solar emission was far above normal for several days and often interspersed with bursts. As before, they found that the solar signal appeared at sunrise and gradually faded as the Sun rose above the antenna beam, but now superimposed were striking oscillations, the interferometric fringes (Figure 7). And the exciting thing was that the very presence of these oscillations implied that the source of the solar signal was a good bit smaller than the spacing of the fringes (20-30′) and the 30′ size of the optical Sun. Exactly how much smaller the emitting region was, as well as its location, could be worked out through details of the oscillations' amplitudes and phases. This led to Figure 8, where they inferred that the emitting region on any given day had a width of 8–13′ and coincided with the giant sunspot group being carried along by the Sun's rotation. Even though the fringes of the sea-cliff interferometer were oriented parallel to the horizon and thus could give no information about the azimuth of the emitting region, it seemed eminently reasonable that the sunspot group itself originated the enhanced radiation, directly confirming what Hey, Appleton and Alexander had earlier only surmised.

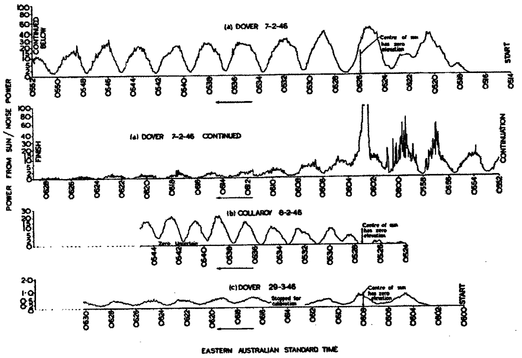

Figure 7: Sea-cliff interference patterns obtained at 200 MHz in February and March 1946. When the Sun rose, the fringes suddenly appeared (note that 'radio sunrise' came earlier than optical sunrise) and then gradually faded away an hour later as the Sun moved above the antenna beam. Note the greater ratio of fringe maximum to minimum for the top observations, indicating that the radiation originated from a smaller region of the Sun. The very fast variations and intense signals recorded at 0600 on 7 February indicate a solar outburst. Note the closer spacing of the fringes for the Collaroy observations, taken from a higher elevation above the sea (after McCready, Pawsey and Payne-Scott, 1947).

In a paper submitted to the *Proceedings of the Royal Society* in July 1946,[5] McCready, Pawsey and Payne-Scott (1947) reported the above results and much more. They expanded on their first results in *Nature* and now characterized the solar radiation as consisting of two components: (1) a slowly changing type that could vary by a factor of 200 in intensity over many days, and (2) intense bursts, lasting from less than a second up to a minute, that could be tens of times more powerful than the general level on a given day. These results were so unexpected that they worried at length that the ionosphere might somehow induce the bursts, but various arguments, principally the fact that separate sites observed the bursts at the same time (to within a second), convinced them that this indeed was an extraterrestrial "and presumably solar" phenomenon. As in their previous letter to *Nature*, they pointed out that the equivalent brightness temperatures for these bursts were extraordinarily high, as much as 3×10^9 K.

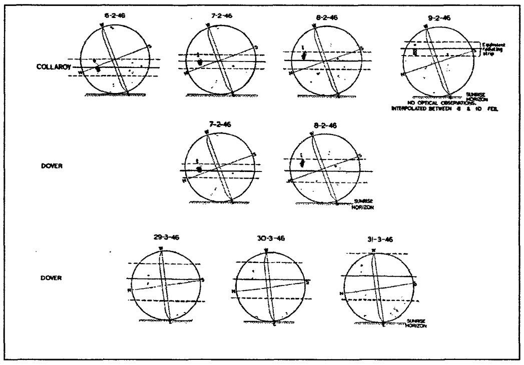

Figure 8: Sketches of optical sunspots visible on the days corresponding to Figure 7. The top two rows are dominated by the great sunspot group, while the March observations show much less activity. 'N-S' indicates the rotation axis of the Sun. The three horizontal lines on each sketch indicate the center and estimated width of the 'equivalent radiating strip' causing the radio fringes (after McCready, Pawsey and Payne-Scott, 1947).

This seminal paper also explained many basics of the sea-cliff interferometer, considering effects such as refraction (the worst uncertainty), the Earth's curvature, tides, and imperfect reflection from a choppy sea. As mentioned above, many of these effects had already been worked out during the War; for instance, in a 1943 RP report by J.C. Jaeger. But Pawsey's group also introduced a vital *new* principle, namely that their interferometer was sensitive to a single Fourier component (in spatial frequency) of the brightness distribution across the Sun, and that in principle a complete Fourier synthesis could be achieved if one had enough observations with interferometers of different effective baselines:

> Since an indefinite number of distributions have identical Fourier components at one [spatial] frequency, measurement of the phase and amplitude of the variation of intensity at one place at dawn cannot in general be used to determine the distribution over the sun without further information. It is possible in principle to determine the actual form of the distribution in a complex case by Fourier synthesis using information derived from a large number of components. In the interference method suggested here ... different Fourier components may be obtained by varying the cliff height *h* or the wave-length λ. Variation of λ is inadvisable, as over the necessary wide range the distribution of radiation may be a function of λ. Variation of *h* would be feasible but clumsy. A different interference method may be more practicable. (McCready, Pawsey and Payne Scott, 1947: 367-368).

Much of the subsequent technical development of radio astronomy was to be concerned with this method of making high-resolution cuts across sources, and eventually complete maps. By the early 1950s their suggested type of Fourier synthesis was indeed central to much of radio astronomy. But the last two sentences of the above quotation were prophetic, for it was not sea-cliff interferometry, but the more tractable and flexible conventional interferometry with separate, movable antennas, that made such mapping a reality.

3.3 The Million-degree Corona

Sometime toward the middle of 1946, Pawsey extracted another jewel from his wealth of data. He noticed that his large set of daily values of the 200 MHz solar flux density had a peculiar distribution (Figure 9), with a sharp lower limit corresponding to an equivalent brightness temperature for the solar disk of about 1×10^6 K. This was drawn from the same data presented earlier, but looked at in a new way: first, with a histogram of values (~150 values over seven months) rather than a plot against time, and second, using single-day values rather than three-day averages. Pawsey had earlier argued that three-day averages were necessary because the solar bursts frequently vitiated daily observations, but now he saw that this averaging had also tended to mask the marked lower limit of intensity, since about two-thirds of all days exhibited enhanced levels.

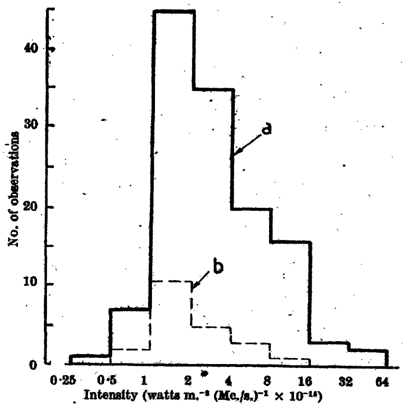

Figure 9: Histograms of daily 200 MHz solar intensity: (a) October 1945 to March 1946, observed by Air Force personnel, and (b) March-May 1946, RP staff. Note the marked lower limit, corresponding to an effective (brightness) temperature for the solar disk of ~1 x10^6 K (after Pawsey, 1946).

At this same time David F. Martyn (1906–1970), the leading ionospheric theorist in Australia (then at the Commonwealth Solar Observatory, Mt. Stromlo) and a key figure in wartime RP (Section 3.4), became very interested in this hot new field and introduced a theory that could explain a million-degree base level for the solar radiation. He learned, undoubtedly from discussions with Allen and Richard v.d.R. Woolley, the Observatory's Director, that recent studies of ionization states and spectral-line widths strongly suggested that the solar corona had a temperature of about 1×10^6 K. *Why* the corona was so hot was not at all understood, but the evidence was there. Martyn realized he could apply standard techniques in ionospheric theory to calculate the expected radio emission from the Sun. Once he had adopted likely values for the electron densities in the corona, he found that the corona was opaque at Pawsey's kind of frequencies. The observed radio waves were therefore emanating not at all from the 6000 K optical surface (photosphere) of the Sun, but from well above the photosphere out in the million-degree corona. When the Sun was quiet, this coronal thermal emission constituted the entire solar signal; when active, the coronal emission was dwarfed. Furthermore, at shorter wavelengths, the observed emission would come from deeper in the corona, and eventually even from the chromosphere. This powerful idea thus explained why the measured brightness temperature of the quiet Sun always seemed greater than 6000 K and sharply increased at longer wavelengths. It also meant that one could now study the corona without the inconvenience of having to chase down a total eclipse or resort to a coronagraph. As it turned out, in the Soviet Union a few months earlier Vitaly L. Ginzburg had indedependently made similar calculations while considering the possibility of reflecting radar off the Sun; and the basic ideas were again independently presented, yet a third time, in the Russian literature in a late 1946 paper by Iosif S. Shklovsky. But Martyn had access to better confirming data and was positioned more in the mainstream of postwar radio astronomy. His paper, in *Nature* for 2 November 1946, had far more influence.

The above description of Pawsey's and Martyn's work seems fairly well established, but there is controversy over whether Martyn first predicted the million-degree corona and then suggested that Pawsey seek it in his data, or whether Pawsey first found it empirically and so instigated Martyn's working on the problem. The archival evidence unfortunately does not speak with certainty. It does show that Pawsey and Martyn were planning a joint publication on this subject in July and August of 1946, but that Martyn then backed out since he and Woolley had decided to do their own theoretical study. Pawsey then persuaded Martyn to change his mind, but in the end Martyn (1946) sent off his own note to *Nature* early in September, apparently without Pawsey's knowledge. Pawsey got wind of this, however, and within a week convinced Martyn to agree that Pawsey (1946b) should send in his own short note and suggest to *Nature*'s editor that it follow Martyn's.[6] The collaboration had clearly gone sour, resulting in two adjacent notes: Martyn's did not mention Pawsey's base-level data at all (citing only Reber's and Southworth's measures of the solar intensity), while Pawsey's acknowledged his indebtedness to Martyn for "... pointing out to me the probable existence of high-level thermal radiation."

Within a few months, however, arguments developed over who had priority over 'the million-degree corona'. In January Bowen (1947a) wrote to Martyn because of "... your insistence on the importance of the written as against the spoken word." Bowen cited a year-old press release, which referred to the RP work as indicating that the usual 'apparent temperature' of the Sun was a million degrees, as clearly antedating Martyn's note in *Nature*, and said that RP knew about million-degree radiation from the Sun long before Martyn came along. Further direct evidence lies in 1948 letters commenting on a draft of a radio astronomy review then being written by Appleton for the International Union of Radio Science (URSI). Martyn (1948) wrote:

> There is a natural tendency now to look on my theory as one designed to explain the observed facts, which followed rapidly upon its heels. In point of fact it was developed (see the internal evidence in Pawsey's *Nature* letter) before the facts were known. It is a theory of prediction rather than explanation, and perhaps has correspondingly greater weight because of that.

And Pawsey (1948d) separately wrote:

> The actual sequence of events ... was as follows: (a) observation of considerably high and very variable effective temperatures, 10^6-10^8 degrees on 200Mc/s – J.L.P. and colleagues. (b) Suggestion of high-temperature coronal thermal emission – D.F.M. and colleagues. (c) Successful search for 10^6 degree base level on 200 Mc/s – J.L.P. (d) Detailed theory – D.F.M.

Pawsey (1948c) also wrote from overseas to Bowen about this time:

> I think Martyn might get a mention in [Appleton's] section on the discovery of thermal radiation. I am all for a quiet life and the theory was a vital part of the discovery.

From this evidence it would appear most likely that Pawsey's own recounting of events best tells the story, although it should be noted that he was at times self-effacing. It is clear that Martyn's withdrawal from collaboration and lack of any mention of Pawsey's base-level work upset the RP staff. But this notwithstanding, it seems that Martyn was indeed the one who brought in the previous astronomical evidence of a million-degree corona and who pointed out that the million-degree 'effective' or 'apparent' temperatures cited by the RP group could actually represent *thermal* emission from the solar atmosphere. Pawsey and his colleagues had calculated these temperatures, but thought of them only in a formal sense. In fact, to them these incredibly high values were at first prima facie evidence of *non*-thermal phenomena.

3.4 Mt. Stromlo

It is remarkable that the Commonwealth Solar Observatory at Mt. Stromlo worked so closely with RP right from the start—such active collaboration between astronomers and active radio investigators occurred no where else in the world in the first few years after the War.[7] Since 1930, however, the Observatory had been doing a small amount of RRB-funded ionospheric research (in particular by Arthur J. Higgs, who after the War became RP's Technical Secretary). Moreover, during the War, Cla Allen had worked on the effects of sudden solar disturbances on ionospheric conditions and optimum communications frequencies. As we have seen, Allen from as early as October 1945 was feeding optical solar data to RP and indeed over the years his ties with RP remained strong (Smerd, 1978: 92A). Since the Commonwealth Solar Observatory was already in the solar-monitoring business at optical wavelengths and RP did not want to maintain a strict daily patrol, the idea soon developed of RP installing a radio system at Mt. Stromlo. From April 1946 onward Allen oversaw regular 200 MHz solar monitoring with a steerable array of four Yagi antennas (similar to the 2-Yagi antennas shown in Figure 6). In early 1949 he used the same array to make a complete map of the Galactic background radiation (Allen and Gum, 1950).

In addition to Allen, Martyn worked on extraterrestrial noise as a sideline to his ionospheric research.

Besides his important work on the million-degree corona, he also pointed out in his 1946 *Nature* note that at wavelengths of 60 cm or less the quiet Sun should appear brighter at its edges than in the center. This prediction of 'limb brightening' turned out to be qualitatively correct, although it took more than five years before observations of sufficient detail seemed to settle the question.

This radio activity could not have flourished without the encouragement of the Observatory's Director and Commonwealth Astronomer, Richard Woolley (1906–1986). Woolley was a stellar and dynamical theorist, an Englishman who had come to Australia to take over the Commonwealth Solar Observatory in 1939, and who would return to Britain in 1955 as Astronomer Royal. He had a personal interest in the radio work; for instance, he authored an early paper on the theory of Galactic noise and others on solar models incorporating radio data. In late 1946 Woolley suggested that Bolton should check for radio emission from the nebulosity near the bright star Fomalhaut, and in 1947, after Martyn had speculated that the Cygnus source (Section 4) might be a distant comet, Woolley searched for such an object. Moreover, relations between Woolley and RP were cordial enough that Bowen first checked with Woolley before sending off the first RP paper on the Cygnus source. Woolley also was elected in 1948 as the first Chairman of the International Astronomical Union's new Commission 40 on Radio Astronomy, and shortly thereafter became Vice-Chairman of Australia's national URSI organization.

Yet despite these fruitful exchanges of ideas, data, and know-how between the astronomers and the radio physicists, tension also existed between Woolley and Martyn on the one hand and Bowen and Pawsey on the other. Much of this stemmed from Martyn and his status as an 'exiled' RP staff member, seconded to the Observatory from Sydney. Martyn had been removed as RP Chief late in 1941 after two years of continual problems—despite his scientific excellence, he did not have the managerial skills or temperament needed to run a large organization developing new technology under the threat of Japanese attack. By 1941 his relations with the military, with industry, and with his own staff were abysmal. On top of this, in early 1941 he was viewed as a security risk because of his liaison with a German woman who had recently emigrated to Australia (Schedvin, 1987: 253-259). With this background, one can understand that his postwar relations with RP often went less than smoothly.

Woolley, too, appears to have developed an ambiguous relationship with RP in particular and with radio astronomy in general. For instance, in a major address on the solar corona, he mentioned Allen's and Martyn's work, but none of RP's results (Woolley 1947). In another talk the same year on "Opportunities for astrophysical work in Australia", radio was not mentioned once, although this may have resulted from his definition of *astrophysical* (Woolley, 1946b). Several interviewees from RP and from Mt. Stromlo have testified to Woolley's lack of support for radio astronomy. Even as late as 1954, when asked about radio astronomy after a popular talk, Woolley apparently replied that in a gathering of 'real' astronomers it was not considered decent to mention radio astronomy (see Bok, 1971; Bowen, 1973, 1984; de Vaucouleurs, 1976; Kerr, 1971, 1987; Mills, 1954; Stanley, 1974; Wild, 1987). On the other hand Woolley was part of a proposal for an independent department of radio astronomy at Mt. Stromlo. The matter culminated in 1951-1952, after the departure of Allen to take up a professorship in England. Professor Mark L. Oliphant (head of physical sciences at the new Australian National University in Canberra) and Woolley made a major thrust to acquire a large radio telescope, but were beaten down by White at CSIRO headquarters and by Bowen and Pawsey (Robertson, 1992: 107-113).

4 RADIO STARS

In August 1946 Bowen, then visiting England, excitedly sent Pawsey a reprint of the recent letter in *Nature* by Stanley Hey, John Parsons and James Phillips (1946) of the Army Operational Research Group. While mapping the general distribution of Galactic noise, they had accidentally discovered that the noise from one particular spot in the constellation of Cygnus fluctuated in intensity on a time scale of minutes. Although they could measure with their beam only that the fluctuating region was less than two degrees in size, they argued that such rapid changes must originate in a small number of discrete sources, perhaps only one. These sources were taken to be stars, by analogy with the Sun and its radio bursts. Pawsey jumped on this. As he wrote (within a few days of receiving Bowen's letter):

> ... we immediately made some confirmatory measurements on 60 and 75 Mc/s, obtaining similar fluctuations, of the same form as the "bursts" observed in solar noise. We have no hint of the source of this surprising phenomenon. (Pawsey, 1946a).

This early success, however, was apparently followed by a period of conflicting observations, during which the reality of the Cygnus fluctuations came into question. In the end Pawsey's group gave up, no longer knowing what to make of Hey's claim (Bolton, 1976: 3-4T; Stanley, 1974: 4-5T).

Cygnus investigations thus lay dormant for several months until resumed by John Bolton (1922–1993), who had joined the RP staff as its second postwar recruit in September 1946 (see Kellermann, 1996). Bolton (Figure 3) was a Yorkshireman who had studied undergraduate physics at Cambridge before joining the Royal Navy, where he first developed radar and then served as a radar officer before demobilization in Sydney Harbour. Assigned to the solar noise problem at Dover Heights, Bolton built two 60 MHz Yagi aerials to follow up on Martyn's earlier detection of circular polarization, and was soon joined by technician O. Bruce Slee, a former Air Force radar mechanic who also had just started at RP.[8] But the Sun was not co-operating with much activity, and so Bolton decided to check for radio emission at the positions of various well-known astronomical objects, as listed for instance in the venerable *Norton's Star Atlas*. His inattention to solar monitoring, however, got him in trouble:

> After a week or two our efforts were cut short by an unheralded visit from Pawsey, who noted that

> the aerials were not looking at the sun. Suffice it to say that he was not amused and we were both ordered back to the Lab for reassignment. (Bolton, 1982: 349-350).

Notwithstanding this setback, a few months later Bolton managed to resume at Dover Heights, where he was joined by electrical engineer Gordon J. Stanley (1921–2001), a New Zealander who had come to RP upon leaving the infantry three years before (Kellermann et al., 2005). This time the goal was to follow up recent studies by Payne-Scott and Donald E. Yabsley on simultaneous solar burst observations at widely spaced frequencies. On 8 March 1947 the Sun obliged with a remarkable burst exhibiting delays of a few minutes between signals arriving first at 200 MHz, then 100 MHz, and finally 60 MHz. The behavior of this and earlier bursts was taken to arise from emission at various critical frequencies as successively higher coronal layers were excited; with a model of electron densities in the corona, it was even possible to infer a speed of ~600 km/s for the ejected material (Payne-Scott, Yabsley and Bolton, 1947). Here indeed was a dramatic confirmation of Martyn's model of different coronal levels effectively emitting different radio frequencies.

Bolton again grew tired of solar monitoring, however, and together with Stanley returned to the Cygnus phenomenon in June 1947 (see Bolton, 1982). The antenna was nothing more than a pair of 100 MHz Yagis (shown in Figure 6) connected to a converted radar receiver and operated as a sea-cliff interferometer. This allowed a three-week reconnaissance of the southern sky, during which they at last reliably found the Cygnus source, as well as hints of two weaker ones (Bolton, 1947). They spent several months checking out the Cygnus source, and by the end of the year submitted papers to *Nature* and the very first issue of the *Australian Journal of Scientific Research* (Bolton and Stanley 1948a, 1948b). Cygnus usually gave a workably strong set of fringes as it rose (Figure 10), and this directly implied that the radiation emanated from a very small, single region of the sky. But the source never rose more than 15° above the northern Sydney horizon and observations were continually harassed by the strong intensity fluctuations that had led to Hey's discovery in the first place. By analogy to the Sun, Bolton and Stanley's analysis split the signal into a constant component (which they estimated as 6000 Jy) and a variable component that added (never subtracted) amounts that fluctuated over times of 0.1-1 minutes. Through auxiliary observations made with other Yagis they found a maximum in the spectrum of the constant component at ~100 MHz, whereas the variable component's intensity increased sharply as frequency was lowered.

The heart of their study was concerned with the size and position of the source. Size came from the solar technique worked out before, namely from measuring the ratio of fringe maximum to minimum. As the 'equivalent radiating strip' became broader, the fringes would wash out in a predictable manner. But Cygnus gave difficulties with (1) subtracting off a considerable baseline slope caused by strong Galactic noise in the vicinity, (2) isolating the constant component from the variable, and (3) determining a proper upper limit for the fringe minimum, for it appeared that the best records in fact showed minima that were not distinguishable from zero (Figure 10). They estimated that the maximum-to-minimum ratio was at least 50, implying that the source size was <8′ (about one-eighth of the lobe separation). Hey's group had inferred that the Cygnus fluctuations must arise from a discrete source or collection of sources, perhaps scattered over two degrees of sky, but here was strong evidence for a single, small source. In fact they thought the source even smaller than their published limit:

> Careful examination of the records suggests a much smaller source size than stated above [8′]. Further experiments using improved receiver stability and greater aerial height will probably substantiate the authors' belief that the source is effectively a "point." (Bolton and Stanley, 1948b: 64).

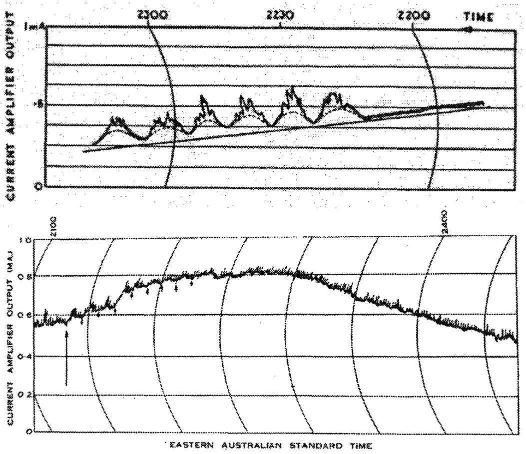

Figure 10: Sea-cliff interference patterns obtained at 100 MHz at Dover Heights for Cygnus-A in June 1947 (top) and for Taurus-A in November 1947 (bottom, discovery recording). Note the ionospheric scintillations superimposed on the Cygnus fringes and the sloping baselines from Galactic background radiation. For the Taurus-A record, the rising point and probable minima of the weak fringes are shown by arrows. Vertical lines on this record are due to interference from a timing mechanism (after Bolton and Stanley 1948b; 1949).

With a source size in hand, they moved on to the even trickier task of a position. This involved analyzing the timing and spacing of fringes in terms of sky geometry and radio wave propagation. One had to find the sidereal time when the source was highest in the sky (culmination) and the length of the arc travelled by the source between rising and culmination. But Bolton and Stanley were forced to tie together observations at three different cliffs around Sydney[9] in order to secure reliable data; furthermore, the necessary corrections for refraction were large and, as it turned out, uncertain. In the end, the derived position was 19h 58m 47s ± 10s, +41° 47′ ± 7′. With this first well-defined position for the enigmatic Cygnus source (Hey's group had been able to give its position only to within 5°), their next step was of course to consult the optical catalogs and photographs. But this was disappointing:

> Reference to star catalogues, in particular the Henry Draper Catalogue, shows that the source is in a region of the galaxy distinguished by the absence of bright stars and objects such as

> nebulae, double and variable stars, i.e., the radio noise received from this region is out of all proportion to the optical radiation ... The determined position lies in a less crowded area of the Milky Way and the only obvious stellar objects close to the stated limits of accuracy are two seventh magnitude stars. (Bolton and Stanley, 1948b: 68).

They did, however, request that Woolley take a special photograph of that portion of the sky, and this appeared as a plate in their paper, along with a tracing-paper overlay indicating their source position and error box. It certainly appeared a nondescript patch of sky (but we should note that this initial position for Cygnus-A was a full degree north of the correct position and so there was no chance of finding an optical counterpart, and even positions obtained years later to accuracies of a few arc minutes at first did not disclose an optical identification).

Given that there was no optical counterpart, could one nevertheless put any constraints on the distance to the object? Since they had been observing the source for three months, the changing position of the orbiting Earth might have caused an apparent shift in position if the object were nearby. But they had detected no shift greater than $2.5'$ (corresponding to their 10sec accuracy in timing the sudden appearance of the source at rising), and this meant the source was at least ten times the 50 light-hour distance to Pluto, that is, well outside the Solar System. But *how* far outside? Bolton and Stanley could only suggest that the farthest imaginable would be if somehow the radio object were a star with total power output similar to that of the Sun, but all channeled into the radio spectrum. That distance worked out to 3,000 light years. But no matter what the distance, the cause of the radio radiation was not at all understood. They could only say it had to be a non-thermal mechanism, for the measured effective (brightness) temperature was $>4 \times 10^6$ K.

Just as Bolton and Stanley were writing up these results, they received an interesting communication from Pawsey (1947), who was then on the first leg of an around-the-world tour. He had visited Mt. Wilson Observatory in Pasadena and there found Rudolph Minkowski and Seth B. Nicholson "intensely interested" in the Cygnus results and willing to undertake observations directed toward finding an optical counterpart. Pawsey then described optical objects that Minkowski had showed him near the Cygnus position, mentioning that in the process they had had to convert Bolton's derived position to account for "... the change of axes due to 'precession of the equinoxes'."[10] Pawsey's letter (ibid.) ended with a raft of suggestions from Minkowski for possible places to look for radio noise:

> The Magellanic Clouds [are] the nearest external galaxies, abnormal with much dust and blue stars ... If we are interested in interstellar dust, etc. the "Crab Nebula", NGC 1952, is a good sample. If white dwarfs are of interest, the companion of Sirius is a convenient sample. The Orion region is a region of emission nebulae. [But] I do not think these ideas get us very far. I should recommend the method of empirical searching; our tools are not too fine to prevent this.

With the Cygnus case temporarily closed, Bolton and Stanley, assisted by Slee, indeed set out in November 1947 to search the sky in Pawsey's 'empirical' fashion. Stanley and Slee had made significant improvements to their receiver's short-term stability, in particular through constructing power supplies able to provide voltages stable to a part in a few thousand. Even weak fringes could now be reliably detected. They methodically took records at different points along the eastern horizon, and were delighted when fringes for several sources appeared over the next few months. As it became clear that the sky had a lot more to offer than just the Cygnus source, Bolton introduced a nomenclature still used today: in the tradition of Bayer's notation for stars, the strongest source in a constellation would be called A, the next B, etc. And so their second source became Taurus-A, one-sixth as strong as Cygnus-A, followed by Coma Berenices-A at a similar level. The uncertainties of this work can be appreciated by noting that Taurus-A appeared nicely on one November night (Figure 10), but it took another three months for confirmation of its existence and measurement of a position good enough to assign a constellation.

By February 1948 Bolton, Stanley and Slee had surveyed about half of the southern sky (man-made interference made daytime observations nearly worthless), and had good cases for six new discrete sources. Bolton (1948) sent a short note to *Nature* announcing that a new class of astronomical object existed: Cygnus-A was not unique, either in its existence or in its lack of association with "... outstanding stellar objects". Upper limits on the new sources' sizes were no better than $15\text{-}60'$, but Bolton was becoming convinced that all these discrete sources were truly stellar, "... distinct 'radio-types' for which a place might have to be found in the sequence of stellar evolution." (Bolton, 1948: 141). Since even the most powerful solar-style bursts would not do the trick, he appealed to either pre-Main Sequence, collapsing, cool objects or to old, hot objects related to planetary nebulae.[11] He felt, too, that a large portion of the general Galactic noise probably originated from the aggregate effect of solar-burst type emissions.

After this survey Bolton chose to improve his source positions, in particular to eliminate systematic errors, by observing source *setting* as well as rising. High westward- and northward-facing cliffs were needed and so Bolton and Stanley headed off to New Zealand in the southern winter of 1948 (Orchiston, 1994). As Bolton (1976: 8T) recalled:

> [Just before the New Zealand trip] ... I remember Taffy Bowen asking me what I really thought of the positions of my sources, and I said, "Well, they're the best I can do at the moment, but I'd like to be the first to correct them." And indeed the corrections were absolutely massive when they came in.

The 300 m sea cliffs at Pakiri Hill and Piha (see Figure 11) led to superior observations which put an even tighter limit on Cygnus-A's size ($<1.5'$), and, together with simultaneous observations by Slee in Sydney, provided strong evidence that most of the intensity fluctuations originated in the Earth's atmosphere, not in the source itself. Many new sources also turned up and it became apparent that incorrect refraction corrections and other problems had thrown most previous positions 5° to 10° off. Some even changed names as when Coma Berenices-A migrated into Virgo! But Cygnus-A was still vexing, as neither its new position (shifted about 1° south from earlier) nor its old one agreed with that measured by Martin Ryle at Cambridge (privately communicated in June 1948). For a while it seemed that the source might actually be moving, but after six months of sorting out, both Hemispheres admitted earlier errors and came to agree on a common position.

The beautiful outcome of the new positions of $\sim 10'$ accuracy was that for the first time optical counterparts could be tentatively suggested (Bolton, Stanley and Slee, 1949). And these were no ordinary objects. Taurus-A was associated with the Crab Nebula, the expanding shell of a supernova known to have exploded 900 years before (Bolton and Stanley, 1949); Centaurus-A was found to coincide well with one of the brightest and strangest nebulosities in the sky, so peculiar that astronomers were not even sure whether or not it was part of our Galaxy; and Virgo-A's position correlated with that of a bright elliptical galaxy six *million* light years away. These associations quickened interest in the study of discrete sources and caused several optical astronomers, among them Minkowski, to take serious note.

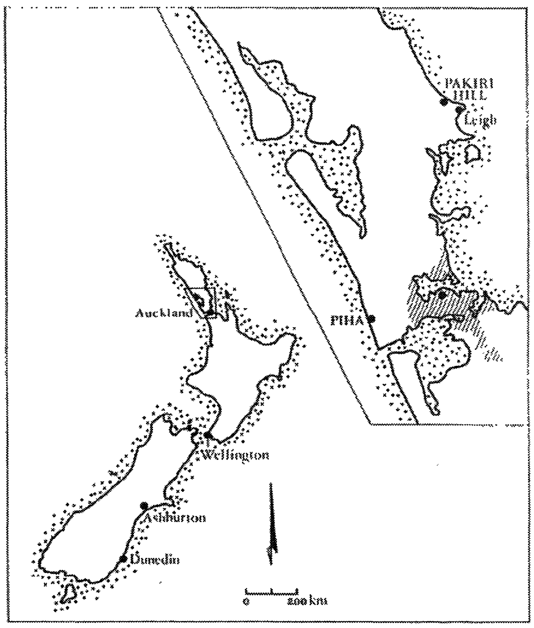

Figure 11: Locations of Pakiri Hill and Piha, near the city of Auckland (hatching), New Zealand (after Orchiston, 1994).

5 OVERVIEW OF RP'S FIRST DECADE

5.1 The Isolation Factor

Almost any analysis of things Australian must consider the geographical isolation of Australia from the other centers of Western culture. Geoffrey Blainey (1968) speaks of "... the tyranny of distance...", that is, the overwhelming importance of distance, isolation, and transport in molding the general history of Australia. In the sciences also, isolation has played a major role. Although the vestiges of a subservient colonial relationship between British and Australian radio science created an asymmetry in status and power that was independent of the distance from London, many problems of Australian science during the postwar years were notably exacerbated by the antipodal separation.

The early RP years are rife with examples of things that would have gone differently if RP had not been located 10,000 miles from its sister institutions, but instead 100 miles, or even 1,000. The best airline connections to Europe required a gruelling three days (or a civilized week) and more common passage by ship took about four weeks; moreover, the cost of a ship's berth amounted to one or two months' pay for an RP staff member. The inability to have frequent contact with colleagues from other institutions, the long interval before learning about research conducted elsewhere, the delays in publishing Australian results in the prestigious overseas journals, and the lack of foreign readership of Australian journals—these circumstances constantly bedeviled the RP staff (and Australian science in general). One counterforce was the maintenance of Australian Scientific Research Liaison Offices in London, Washington, and Ottawa. These had been originally set up during the War to coordinate radar research, and served as scientific embassies to increase the flow of information to and fro. But a far better solution was to send an RP researcher on an extended 'jaunt' through North America and Europe. In the six years after the War, the primary overseas stays or trips of importance for the development of Australian radio astronomy were undertaken by Bowen (in 1946), Ronald N. Bracewell (1946-1949), Pawsey (1947-1948), Westfold (1949-1951), Bolton (1950) (see Bolton, 1982: 353) and Kerr (1950-1951). The RP correspondence files relating to these trips are particularly good sources for understanding the influence of the isolation factor on RP's work. What emerges is that such trips served five primary purposes: (1) intelligence (in the military and political sense of the term), (2) education, (3) publicity, (4) establishment of personal contacts, and (5) fund-raising for large antennas, which first paid off with American grants in the mid-1950s towards what became the Parkes Radio Telescope.

The first purpose of the overseas trips was simply to find out what was going on. The RP visitor to an overseas laboratory typically sent back a detailed report of recent and ongoing research, and this report (as evidenced by multitudinous initials on the original documents) was widely circulated back home. Pawsey and Bowen, in particular, were masters at picking up what was being done elsewhere and analyzing its effects on RP's current research program and future plans. To give but two examples: in August 1946 Bowen (1946b) cabled back that British work on the Sun was ahead of RP's at observing frequencies less than 200 MHz and that therefore RP should concentrate on higher frequencies. And in April 1950 Bolton sent word home that the solar work by Hey's group lagged Wild's by at least eighteen months.

A second purpose of the overseas trips was education. Sometimes this was in the formal sense, as when Bracewell took a Ph.D. at Cambridge and Westfold one at Oxford, and Kerr a Master's degree at Harvard; but more often it was simply the wealth of knowledge to be garnered from overseas contacts. The background of the RP staff was of course far weaker in astronomy than in radio physics, and thus it was the visits to observatories that were particularly valuable. As Kerr (1971: 19T) recalled:

> Bolton and also Pawsey did some touring at that time and learned something of what generally were the interesting problems in astronomy, acquiring some of the attitude of astronomers toward astronomy, instead of just the electrical engineers' and physicists' attitudes.

On the other hand, Mills (1976: 20T) points out that the paucity of astronomers in Australia may have helped more than it hindered:

> Our isolation did help us develop with an independent outlook. We had no famous [astronomer] names to tell us what we should believe, and to some extent we just went ahead following our noses.

Overseas voyagers also served to spread the word about RP research—Pawsey called them "... ambassadors for Australian science". The RP archives are full of instances where Bowen and Pawsey sent reprints, complained about Australian work being neglected in reviews overseas, and urged people to subscribe to the *Australian Journal of Scientific Research*, started by CSIR in 1948 and a further sign of the growing independence of Australian science from British hegemony. RP sent thirty full papers to this journal in its first four years, but only eight to British journals (plus eight letters to *Nature*).[12] Although this corpus probably lent more stature to the journal than did any other single field, it took years to foster a world readership, even among radio astronomers. For example, Jodrell Bank did not subscribe until 1950; before then, the only copies they could locate were in London.

Direct word-of-mouth, when possible, was of course also important. After attending a 1948 URSI meeting in Stockholm, Pawsey (1948b) wrote back, "Martyn and I, to put the matter rather bluntly, attempted to put Australia on the map, and I think were fairly successful." And Bracewell (1980: 131A) recalls that while he was at the Cavendish as a postgraduate student, Ryle's group thought Sydney work way behind, but when he returned to RP he found that they thought the same of the Cambridge work—each side was simply acting on dated information. Preprints were not common in those days, and in any case were sent by sea mail (as were journals, even *Nature*), taking two to three months for the passage.[13] Bracewell (1948) also remembers wanting to act as a link between the two groups: "Being young and idealistic, I felt that I should try to close the gap, that a freer flow of information was a blow struck against entropy, as well as my duty." As he wrote to Bowen in early 1948:

> Publication [of Australian work] is slow and the diffusion of advance news by word of mouth does not occur. It results that ideas of priority are fixed before Australian work filters through. This is the case with solar noise. The attitude in the Cavendish Lab. is that nothing much of value is done elsewhere ... [Since] I am in an effective position for informal dissemination of news from Radiophysics, I recommend for your consideration the transmission of this news. (ibid.).

Despite his request, it appears that Bracewell himself remained little better informed than others in Cambridge. Upon his return to Australia in late 1949, he wrote back to Ryle:

> There is a lot of good work going on, and the people are very keen. Very little pre-publication news seemed to filter through to me in the Cavendish from Australia ... Do not hesitate to let me know if ... I can make enquiries which you may think can be better done informally through me. There is a lot of interest in your work – I have had to ward off quite a barrage of minor queries about your set-up since arriving. (Bracewell, 1950).

This induced Bracewell over succeeding years to send three short papers to *Observatory* for the purpose of advertising RP's work.

One of the grandest opportunities for interchange and to advertise RP's work was the URSI General Assembly that met in Sydney in August 1952. This was a feather in the hat not only for Australian radio research, but for all of Australian science, as it marked the first time that *any* international scientific union had met outside Europe or North America. In 1948 URSI had created a new Commission V on Extraterrestrial Radio Noise with Martyn as its first President and Pawsey as Secretary. Martyn in particular engineered the General Assembly coming to Sydney and masterminded the organization and funding. Sir Edward Appleton (Figure 1) was the patriarch among the fifty foreigners in attendance, of whom about a third were active in radio astronomy (Figure 3). At last the RP staff could associate faces with names like Jean-Louis Steinberg from France, Robert Hanbury Brown from Jodrell Bank, F. Graham Smith from Cambridge, C. Alexander Muller from Holland, and H. I. 'Doc' Ewen from the United States. RP of course put on its best show for the guests with a detailed, glossy *Research Activities* booklet and tours of several of the field stations. The home team was greatly stimulated, and the visitors went away impressed.

5.2 The Field Stations

The RP radio astronomy work took place at individual field stations, some as far as 30-50 miles from home base on the grounds of Sydney University (Figure 4). These sites provided sufficient land and isolation for observations free from man-made electrical interference. But why not just one or two sites well removed from Sydney? Many small sites also provided freedom from a second type of 'manmade interference': the RP staff simply preferred to spend most of their time alone at the field stations, not in a central laboratory, and management too found this a productive style of operation. By the late 1940s RP's research in radio astronomy was divided into many teams of two or three: leaders and sites about 1948-1950 were Piddington and Minnett (University grounds), Kerr and Shain (Hornsby), Bolton (Dover Heights), Wild (Penrith), Mills (Badgerys Creek), Payne-Scott and Christiansen (Potts Hill), and Lehany (Georges Heights). Orchiston and Slee (2005) discuss in detail these field stations and their major research programmes over their lifetimes. Christiansen (1984: 113-114) has evoked the atmosphere of these stations:

> Each morning people set off in open trucks to the field stations where their equipment, mainly salvaged and modified from radar installations, had been installed in ex-army and navy huts ... The atmosphere was completely informal and egalitarian, with dirty jobs shared by all. Thermionic valves were in frequent need of replacement and old and well-used coaxial connectors were a constant source of trouble ... During this period there was no place for observers who were incapable of repairing and maintaining the equipment. One constantly expected trouble.

Although groups had little day-to-day contact, Pawsey's skill as roving monitor and coordinator gave cohesion to RP's radio noise work. This was achieved first through meetings every two to four weeks of his 'Propagation' Committee (which changed to 'Radio Astronomy' in 1949). These meetings provided a forum for progress reports, discussion of astronomical results and technical problems, floating of new ideas, coordination of experiments, and arguments about priorities. They also served to counter the danger that isolated groups would develop too narrow scientific or organizational perspectives. Several interviewees commented on the value of these sessions; for instance, Christiansen (1976: 19-20T) reported:

> Despite the fact that we were independent groups, we used to have these sessions, sort of what Americans call 'bull sessions', thinking of every conceivable sort of aerial ... A really good one would last all day. Joe Pawsey was one to stimulate that.

Pawsey's second device for holding the radio noise research together was to frequently visit the field stations to see for himself what was happening and to give advice. As Wild (1972: 5) recollects:

> On some days he would arrive unexpectedly at one's field station, usually at lunch time (accompanyied by a type of sticky cake known as the lamington, which he found irresistible), or else infuriatingly near knock-off time. During all such visits one had to watch him like a hawk because he was a compulsive knob-twiddler. Some experimenters even claimed to have built into their equipment prominent functionless knobs as decoys, especially for Pawsey's benefit ... [But] when one ran into problems, half-an-hour's discussion with Joe tended to be both soothing and rewarding.

These visits, however, sometimes led to Pawsey seeing things he did not like:

> Pawsey was in direct linkage with the little isolateed groups. He'd try and make sure they didn't clash. And he stopped us working at times when Jack [Piddington] had had some idea and we'd started in a new direction ... For instance, one day he found me [working on a radio analogue to a Fabry-Perot interferometer] and I was stopped. He said there are other people already there, and they've got a prior claim. (Minnett, 1978: 29-30T).

But although Pawsey usually assigned exclusive turf to each small group, he sometimes encouraged two groups to plow ahead on the same problem if he felt their approaches differed enough. For example, Mills and Bolton for many years both observed discrete sources, albeit with different types of interferometers:

> Bolton's group and mine each felt rather strongly that our own technique was the best. Although we saw each other sometimes, Bolton lived out at Dover Heights and didn't come into the Lab very often and I spent most of my time out at Badgerys Creek. So we didn't actually have very much contact, and there were quite a few arguments about interpretation of the results. (Mills, 1976: 26-27T).

5.3 Management of Radio Noise Research

Bowen turned over scientific leadership for radio noise investigations to Pawsey, who was the Division's number-two man from the start (although the office of Assistant Chief was not created until 1951). Pawsey thus had a free hand in running the radio noise side of things while Bowen took on the general administrative burden and concentrated on the rest of RP's program, taking a particular interest in the rain and cloud physics research to which he himself made several contributions. Bowen, however, minimized the number of his collaborations and so through 1951 published only seven papers. His career had seen more than its share of scientific directors (such as Appleton, Watson-Watt, and Martyn) who claimed credit for too much of what happened in their laboratory:

> When I became Chief, I was going to be quite certain of one thing ... I was not going to jump in and claim credit when somebody else did the work ... My previous experience of some pretty hard cases was that the best way to get first class work out of people was to give them the credit. (Bowen, 1973: 27-28T).

Along this line, Bolton (1978: 118T) recalled:

> Bowen was on our side in terms of letting people have their head—giving you a pat on the back when you did well and commiserating with you when something failed.

Bowen's philosophy also was expressed in a 1948 letter to Pawsey after the latter had been overseas for eight months:

> It is true that those of us who have had a fair amount of experience can give a lot of help in choosing problems for the younger people, keeping their sights on the target and helping them snatch the odd pearl out of the tangled mass, but I am quite sure that what we are suffering from in the Lab. is not that there is too little of this help but too much. With few exceptions our youngsters have not learnt to stand on their own feet and go for a line of their own ... The boys in the Radio Astronomy Group are feeling your absence quite keenly, but I am taking the view that their present gropings are part of their education. (Bowen, 1948).

Bowen and Pawsey's leadership styles very much fitted in with Rivett's philosophy discussed earlier: get the best people possible, give them the needed resources, and then let them run free. But there were bounds to this freedom, as we have seen, leading to a creative tension between tight control of the Laboratory's work, as it had necessarily operated during the War, and the kind of individual freedom one might find in a university department. This delicate balance is well illustrated by the juxtaposition of allowing workers to be scattered all over the countryside, while still keeping close tabs on what they did. Other strong limits existed. For example, most scientific correspondence was routed through either Pawsey or Bowen. More significantly, RP maintained a system of rigid internal reviews of all proposed publications, involving one or more of Bowen, Pawsey, and Arthur Higgs (Technical Secretary). The RP archives are replete with internal memoranda shuttling drafts back and forth between authors and management (and sometimes anonymous third-party RP referees), often to the frustration of the authors. But once a paper surmounted this first hurdle, a journal's referees usually seemed easy by comparison. The extent of Pawsey's influence on the radio noise papers can be gauged by the fact that half of them from the 1946-1951 era specifically acknowledge his assistance with either preparation of the paper itself or the project in general. Yet he, like Bowen, published only seven papers through 1951.[14]

Bowen and Pawsey agreed on the basic policies needed to run RP, but their differing temperaments led to differing contributions to RP's success in radio astronomy:

> [Pawsey's scientific style] ... set the tone completely ... but he was a very, very unworldly fellow ... Bowen was the man who got the money, the tough businessman, while Joe was the rather academic scientist. And it was an excellent combination. (Christiansen, 1976: 22T).

Bowen knew how to deal with the CSIRO hierarchy, how to pull off the necessary balance of applied and fundamental research, how to use his connections to source funding, and how to manage RP as a whole. On the other hand, interview testimony of numerous RP staff members indicates that Pawsey by nature was not suited for such things. For instance, he abhorred (and avoided) making managerial decisions that he knew would cause upset.

Pawsey played a vital role, however, as scientific father figure and mentor. He was about ten years older than most of the radio noise researchers, who averaged only about thirty years of age, and he quickly gained their respect and confidence. The words of his protégés speak for themselves:

> He had the ability to develop the latent powers in other people. All of the people who came out of that group—Christiansen, Mills, Wild, and so on—I also count myself in it—were made independent and skillful in their subject, experienced and self-reliant, quite largely because of Pawsey's way of drawing people out. He was not the kind of research leader who'd insist on claiming everything himself. But he fed in the ideas that other people developed—he was a teacher as much as anything. In the written record you don't find his name on many papers, but he was the inspiration behind an awful lot. (Kerr, 1971: 41-42T).
>
> There were, and are, few scientific groups of comparable size where the head of the group had such a detailed knowledge of the work of each member and where every paper was criticised in detail by him. Yet this ... did not lead to any authoritarian regime. Pawsey's criticisms were usually accepted not only because they were sound but because they were so clearly and intelligibly expressed that acceptance was inevitable. (Christiansen and Mills, 1964: 139).

Pawsey's style of science grew out of his training under Ratcliffe in the Cavendish Laboratory of Ernest Rutherford. He inherently loved the simple, inexpensive experiment and distrusted anything coming from complex setups. He also had an innate distrust of theory and mathematics (Westfold, 1978: 97B), complemented by a faith in experimentation. As he himself wrote in 1948 (regarding the possibility of solar bursts at frequencies less than a few hundred hertz):

> My present guess is that the theory is wrong in general, and consequently I do not advise any time-consuming observations which are based on the theory. On the contrary, the observation of low-frequency noise is a fundamental scientific observation which is of value independent of the theory. Positive or negative results are of use. Hence this investigation is in order, and it is up to the experimenters to decide how far they go. (Pawsey, 1948a).

The experimental style that Pawsey inculcated was particularly striking to H.I. 'Doc' Ewen (1979: 42T), accustomed to much larger American budgets, when he visited Sydney for the 1952 URSI meeting:

> Their equipment was shoestring stuff, but there were a lot of cute tricks ... They didn't waste much time with hardware where it wasn't all that important, [or with] trying to make it look pretty. But wherever a part was critical to the operation of a device, they spent a lot of time thinking about it.

And from the other perspective, Christiansen (1976: 31-32T) recalls how Ewen reacted upon seeing his 21 cm hydrogen line receiver:

> Ewen came out and said he had to see how these damn Australians did in three weeks what took other people eighteen months to do. And when he saw our gear, lying all over the room and on the floor, he just about passed out.

Pawsey's scientific style was distinctive and exemplary:

> He had an enormous enthusiasm. It was always a delightful experience to bring to Pawsey some new idea or some interesting new observation. His immediate reaction would be one of intense interest, followed by suspicion as he looked for some mistake or misinterpretation, or what he called 'the inherent cussedness of nature'. Finally, if convinced that all was well, his face would shine with boyish pleasure ... He never forced his opinions on a younger colleague; if the matter was open to doubt he was willing to leave it to experiment. He was, in fact, the arch-empiricist. "Suck it and see" was one of his favourite expressions ... He did not in general accept theoretical predictions as a guide to experiment; he preferred to investigate the questions that arose from previous experiments. "Following his nose" was how he described this process. (Christiansen and Mills, 1964: 139).
>
> Pawsey had a childlike simplicity about him, a childlike curiosity. He was not a sophisticated man in the least. I find this is a talent that a lot of people who are truly great have in common—retaining a feeling that science is not a business, that it's a game ... If Joe had been a businessman, you would have called him a sucker, but [for science] I think that's actually an important characteristic. (Stanley, 1974: 25-26T).

> I think you could say Pawsey was a very simple soul ... But he could floor a speaker: there'd be a fellow turning up a great piece of astrophysics and Joe would get up at the end and say quite innocently, and it *was* innocent, "I can't reconcile this with Ohm's Law." It would absolutely torpedo the speaker. (Christiansen, 1976: 21T).
>
> Pawsey did not have much of a mathematical background – he once asked me what [statistical] 'variance' meant – but he thought in physical terms ... He once proposed what he called the Sausage Theorem: "If the error bars on a set of visibility measurements fit inside a certain sausage, then the calculated source distribution [from the Fourier transform of the visibilities] runs down the middle of another sausage." Pawsey very reasonably wanted to know how fat this other sausage was and my job was to find out. It is a very good question. (Bracewell, 1984: 171).

Finally, a dissenting view has been given by Francis F. Gardner (1986), an RP ionospheric colleague of Pawsey's during these years:

> The impression of Joe as a naive, unworldly type is misleading. To some extent this was a pose, which contributed to his ability to 'draw people out' ... Nor was he opposed to theory ... In discussions he was able to grasp immediately what was said to him, even if poorly expressed, and he also was able to concentrate one's attention on the problem under consideration. Occasionally he would suggest solutions to some degree with tongue-in-cheek. His suggestions might not be appropriate, but enabled others to see the solutions.

5.4 Why was RP So Successful?

When an institution is created for one specific mission and then, because of changed circumstances, tries to adapt to a different role, the results are often less than satisfactory. RP's shift from war to peace, however, was a striking counter to this. Through skillful leadership, scientific expertise, and good fortune (for instance, how might the fledgling solar noise efforts have gone if the 'sunspot group of the century' had not shown up in February 1946?), RP put Australia at the forefront of radio astronomy over the postwar decade. By the early 1950s, RP was also clearly CSIRO's scientific leader (Schedvin, 1987: 360). In fact, in no other natural science did such international stature come to Australia during these years—perhaps the closest was the immunology research led by F. MacFarlane Burnet at Melbourne's Walter and Eliza Hall Institute of Medical Research, or the neurophysiology led by J.C. Eccles (see Courtice, 1988). Many of the factors important in this achievement have already been discussed, but others deserve mention.

One was the sheer size of the radio noise group, far larger than other institutions in the field—with so many projects going on simultaneously, one is much more likely to have at least one winner at any given time. The radio physicists were also supported by invaluable assistance from the large staff of technicians for electrical and mechanical work. A mild climate also conferred distinct advantages for research involveing outdoor construction and experimentation (Bolton, 1978: 107T). We can dismiss, however, one possible factor for the Australians' success, namely that they had the southern sky to themselves and therefore had no competition and only needed to mimic northern observers. Although for over a century Australia had been a fertile outpost for research precisely because of its unique flora and fauna and non-European skies, the evidence of this chapter shows that for radio astronomy this notion is patently untenable. After all, the same Sun is shared between north and south. In fact, Bolton took the view that any new setup should work the reachable northern sky first so as to beat the northerners—the southern regions would always be there later (Kerr, 1987; cf. Piddington, 1959). Witness the trouble Bolton made for himself by observing the notably northern source Cygnus-A as it barely scraped his horizon, although overhead in England.

5.5 Transforming Radio Physicists into Radio Astronomers

As work on radio noise developed in the Radiophysics Laboratory over the years, there was a gradual integration of the research into astronomy proper and the transformation of radio physicists into radio astronomers. Even from the beginning, Pawsey recognized that this new radio technique was fundamentally altering *astronomical* knowledge. As he stated during a talk to an August 1946 meeting in Adelaide:

> This [solar noise] work is a new branch of astronomy ... New observational tools [in astronomy and astrophysics] have an unusual importance. The last outstanding development in solar instruments was probably the spectroheliograph (developed at the turn of the century). Consequently it is reasonable to expect that the discovery of this radiation will come to be recognised as one of the fundamental advances in astrophysics. (Pawsey, 1946c).

Yet although the RP staff realized that they were essentially doing astronomy, albeit of a wholly different type and not well understood by astronomers, their astronomical education proceeded in a checkered manner. Whereas Bolton (1978: 30, 36-37T) chose to plow methodically through volume upon volume of the *Astrophysical Journal* during long observing nights, most just picked up what they deemed necessary as they went along. Books such as George Gamow's *The Birth and Death of the Sun* were read and Bolton undertook a partial translation of Max Waldmeier's 1941 treatise on the Sun. The exposure to Mt. Stromlo, including occasional joint colloquia, was also important. But RP had nary an astronomer on its staff and its orientation during the first postwar years was as often toward the techniques as the astronomy:

> We were simply radio people trying to provide another tool for detecting what these astronomers said was likely to be there ... We didn't consider ourselves to be astronomers—our primary interest was in the equipment. In fact we'd just left a wartime situation and we knew that our success in radar stemmed from having people who were very well trained in the techniques. (Hindman, 1978: 98B).

By the early 1950s, however, overseas trips, increasing contacts with astronomers, and a gradual accumulation of astronomical knowledge had caused a clearer picture to emerge of how the radio work fitted into astronomy as a whole (cf. Jarrell, 2005).

5.6 The 1950s as a Watershed

The 1950s represent a watershed from several perspectives. For the first time research on solar noise was overtaken in quantity by that on 'cosmic' (non-solar) noise—the percentage of solar papers dropped from ~70% before 1951 to ~40% during 1952-1954. At this time also, Pawsey and Bracewell (1955) wrote a masterful monograph, *Radio Astronomy* (mostly written in 1952). It formed a capstone to the first stage of the field's development and was to remain the definitive textbook for a decade. And of course the 1952 URSI meeting also happened at this juncture.

A key change during the early 1950s was the shift from a large number of relatively small experiments to a smaller number of projects on a large scale. This was the start of the transition from 'Little Science' to 'Big Science'—or, as Wild (1965) has pungently described it, moving from trailers "... with a characteristic smell ..." to air-conditioned buildings. Progress in the science now demanded huge antennas and arrays, and many of these were beyond the capacity of RP to produce in-house. For example, in 1951 *outside* bids for antennas were sought for the first time for 50 ft and 80ft dishes ("Tentative specifications ...", 1951). In 1953 RP funded its last major antenna from its own resources: the 1500 ft Mills Cross array at Fleurs for £2,500 (Mills, 1953). More costly ventures did not come easily, however, for the Government and CSIRO were not willing to support large capital projects (Bolton, 1978: 56T; Bowen, 1978: 44T). Eventually, however, the first one emerged in the form of a 'Giant Radio Telescope' whose cost and planning dominated the second half of the 1950s (see below).

6 THE SECOND DECADE AND BEYOND

This section gives a very brief overview of the period beyond 1955, focusing on the major instruments that were built and the personnel changes that led to an entirely different Radiophysics Division.

6.1 A Giant Radio Telescope and a Solar Ring

Nascent thoughts about a 'Giant Radio Telescope' (GRT) and its funding began as early as 1948. Bowen at that time tried to convince the Royal Australian Air Force to build a huge radar antenna that could do radio astronomy on a part-time basis. Several designs were studied over the next few years, some as large as 500 ft in dimension, but the funding never materialized. By 1951 the search for funds shifted to non-military sources and eventually the key money came from American foundations, starting in 1954 when the Carnegie Foundation made a major grant for a giant dish. But such a facility would represent a wholly different philosophy from that of RP's small field stations and their concomitant research groups, since it would command such a high fraction of RP's resources that it necessarily had to be all things to all people. In 1961 RP consummated the transition to 'Big Science' with the commissioning of a 210 ft parabolic antenna at Parkes, 260 km west of Sydney. For complete details of the fund-raising, design, construction, and research programme of the Parkes Radio Telescope, see Robertson (1992). Today, after forty-four years and many upgrades, 'The Dish' remains amazingly productive; it is still the largest stand-alone radio telescope in the Southern Hemisphere, and the only one ever to be the star of a feature film (in 2000).

The other major RP instrument of this period was the pet project of Wild, who later became Division Chief and then Chief Executive of all of CSIRO. In the early 1960s the US Ford Foundation funded the Culgoora Radioheliograph, a 3-km-diameter circle of 96 low-frequency dishes that could produce a detailed, second-by-second 'movie' of the changeable Sun, ultimately at three different frequencies. Over the period 1967-1984 it was the premiere solar radio telescope in the world, and it was only closed down in order to make way for the Australia Telescope Compact Array (see Section 6.3, below).

6.2 Dissension and Exodus

The decision to build the Parkes dish had far more than scientific consequences, for it created dissension among the maturing RP group leaders (most of whom were then in their early 40s). Pawsey had had great success in scientifically rearing his junior colleagues, but RP was not like a university department with its steady stream of students—the RP 'students' had no where to go in the first decade, and there were no positions for new ones. Already, in 1951, Pawsey was saying that the outstanding defect in the radio astronomy group was its lack of the 'research student' type with which he worked so well. Through the 1950s the various group leaders became strong-willed, confident individuals, arguing their own particular visions of how radio astronomy at RP should be done. Major disputes centered on two questions: (a) Should the focus continue on small technique-oriented groups or shift to a single major facility?, and (b) Which types of antennas would pay off best? Regarding the latter, the cost of major projects was now such that only roughly every decade or so could one be afforded.

The first major figure to leave Pawsey's group was Bolton, who in 1953 switched to cloud physics (after denial of funding for a new type of interferometer) and then in 1955 (assisted by Stanley) founded a new radio observatory at the California Institute of Technology (Kellermann et al., 2005; Stanley, 1994: 511-513). In 1960, however, Bolton returned (without Stanley) to become Director of the new Parkes Radio Telescope. Also about this time, Bowen and Pawsey began to work less well as a team and developed significant differences, in particular over the choice of a big dish and how to run it. This led to Pawsey accepting a position to direct the fledgling National Radio Astronomy Observatory in Green Bank, West Virginia, USA, but he died of a brain tumor in 1962 (at age 54) before he could assume his duties. Others who permanently departed in the second decade were Stanley (to Caltech), Bracewell (to Stanford), and Kerr (to the University of Maryland).

Christiansen was also lured away about this time to the University of Sydney (he called the Parkes dish "... the last of the windjammers."), where he continued to develop aperture synthesis techniques at the Fleurs field station (Orchiston and Slee, 2005), culminating in the Fleurs Synthesis Telescope, which made continuum maps over the period 1973-1988. Likewise, Mills, after his proposal to RP for a giant (Mills) Cross antenna was passed by (in favor of the Culgoora Radioheliograph), also moved in 1960 to the University of Sydney and built his cross at Molonglo (once again with US funding, this time from the Government's National Science Foundation). It was completed in 1967, and in its first decade catalogued more than 10,000 radio sources at 408 MHz; in 1981 it was transformed into the Molonglo Synthesis Tele-scope (MOST).

6.3 The Australia Telescope

During the 1970s, Australian radio astronomy remained strong, but did not keep up with the major facilities being constructed overseas (e.g., the Westerbork Synthesis Radio Telescope (12 dishes) in the Netherlands, a 100 meter fully-steerable dish in Germany, and the Very Large Array, a 27-dish synthesis array in the US). On the other hand, the pace and scale of worldwide astronomy meant that Australian astronomers (of all stripes, not just radio) wanting any new major facilities needed to act in unison in order to secure funding from the Government. The radio astronomy community had already been supportive of various major optical projects with its expertise and personnel. For example, Bowen and Minnett had contributed to the design of the Anglo-Australian (optical) Telescope at Siding Spring, and Robert Hanbury Brown (a transplant from Jodrell Bank in England) established a specialized optical observatory at Narrabri for measuring stellar diameters (employing his intensity interferometer principle).

Thus in the years around 1980 a (mostly) united front of astronomers sought and eventually secured major funding from the Government for a synthesis array of dishes (final cost was about A$50 million). This eventually became known as the Australia Telescope (AT) when it was accepted as an official Australian Bicentennial Project, which dictated that it had to officially open in 1988—although the first synthesized map (using just three antennas) was not produced until the following year. The AT was centered on a set of six 22-m dishes located at the site of the Culgoora Radioheliograph, near Narrabri, New South Wales. With precision surfaces that could operate at wavelengths as short as 3 mm, it did much to restore Australia's prestige in radio astronomy. A new CSIRO Division, the Australia Telescope National Facility, was set up to operate this major new resource. The inaugural Director was Ronald Ekers, who had trained under Bolton in the 1960s but now came home after two decades overseas.

Australia would never again be as dominant in radio astronomy as it had been in the decade after World War II when the field was brand new, but with the Australia Telescope it was now again fully competitive.

7 NOTES

1. Citations of the form '1971: 7T' refer to page 7 of the transcript of my 1971 interview. The form '1971: 32A' refers to side A of Tape 32 of my (untranscribed) 1971 interview, and 'B' to side B.
2. The data of Figure 2 come primarily from annual reports and lists of publications issued by the Division of Radiophysics (File D2, RPS).
3. If we assume a brightness temperature of the Sun (at solar minimum at 10 cm wavelength) of 35,000 K, then Pawsey and Payne-Scott would have detected an antenna temperature with their 4-ft dish of ~150-200 K, well above their sensitivity to relative changes of ~20-30 K. This type of dish and microwave receiver was in fact very similar to that employed by George Southworth in 1942-1943. Minnett (1986) has speculated, from his memory of the room used, that in March the Sun was not easily observable from any window. Although there is no written record of anyone at RP trying for the Sun before the end of the War, Frank Kerr (1971: 7T, 1976: 53T) recalled a brief attempt he made at a wavelength of 1.5 m with a small antenna. He also recalled that the first RP solar observations were motivated by overseas reported detections of the Sun, and not by the 'Norfolk Island Effect' as I have concluded. Piddington and Martyn also made a brief attempt to observe the Sun in 1939 (Piddington, 1978: 1-4T).
4. I use the term *sea-cliff interferometer*, although at the time the arrangement was called either a *sea interferometer* or a *cliff interferometer*.
5. The introduction to this paper (McCready, Pawsey, and Payne-Scott, 1947) provides an especially good example of how historical information is usually lost in the formalism of a scientific paper. In this case it appears to have happened because of referee's comments rather than in the initial writing. The submitted manuscript was Report No. RPR 24 (for some reason with a different author order: Pawsey, Payne-Scott and McCready), dated 16 June 1946, and contained historically-interesting material about what the Sydney group knew from overseas reports and when they knew it. The finally-published version, however, was modified in several places to merely recite who published what and what they said. In particular, the phrase "In a prior letter, not available here until our initial work was completed, Appleton (1945) ..." was changed to simply "Appleton (1945) ...".
6. Correspondence between Pawsey, Martyn, Woolley and Bowen, July to September 1946, is all contained in the in RP file B51/14.
7. The only minor exception was neighboring New Zealand. Both Alexander's group (which Unwin subsequently inherited) and the Burbidge-Kreilsheimer-Maxwell group at the University of Auckland—where Maxwell was doing M.Sc. research on solar radio emission—worked closely with Ivan Thomsen, the Director of Carter Observatory. At the time, Carter Observatory specialized in optical solar work, and Thomsen (1948) eventually published a paper in *Nature* on the correlation between solar radio emission and optical features.
8. Although Slee did not join RP until November 1946, he had made an independent discovery of the radio Sun while operating a radar set near Darwin in late 1945-early 1946, a discovery which he duly reported to RP (Briton, 1946 Slee, 1946; Sullivan, 1988: 342). Orchiston and Slee (2002) recently reported in detail on these observations, and placed them in the public domain. Slee (1994) has published his memories of the years 1946-1954 at Dover Heights, and Orchiston (2004, 2005b) gives detailed overviews of Slee's career.

9. Because Dover Heights was not suitable to follow Cygnus-A's entire track low across the northern sky, Bolton and Stanley used two other sites in the vicinity of Collaroy, namely Long Reef and West Head (Figure 4 shows Collaroy).

10. The phenomenon of precession of co-ordinates, covered at the start of any basic astronomy text, had apparently not been previously known to Pawsey. It also almost slipped the attention of Bolton and Stanley (1948b) when they constructed their photographic overlay (see Stanley, 1974: 7-7T).

11. It was only fitting that an ailing Australian star should radiate its Swan song in the form of Cygnus-type radio noise.

12. One of the factors in the founding of the Australian journal was the well-founded suspicion that letters and research papers sent to British journals in many cases were not being treated fairly, either through premature dissemination of their contents or through delays in publication (Kerr, 1987: 8).

13. A check of accession dates for *Nature* in the Sydney University Library (which was used by RP) revealed that each weekly issue was received fully 5 to 11 weeks after its date of issue. This situation continued until 1954, when the delay became only one week, presumably because of airmail delivery. I thank J. Threlfall for this information.

14. RP researcher Donald Yabsley (1986) has pointed out a typical example of Pawsey's keeping his name off publications. With regard to the paper by Payne-Scott, Yabsley and Bolton (1947), Pawsey contributed much to the project and to the paper, and originally he was intending to be a co-author. But in the end he withdrew his name because he felt that three authors were quite enough for a letter to *Nature*.

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9 REFERENCES

The following abbreviations are is used:

RP = CSIRO Division of Radiophysics
RPA = CSIRO Division of Radiophysics Archives

Alexander, E., 1945. Report on the investigation of the "Norfolk Island effect". R.D. 1/518, Radio Development Lab., D.S.I.R., Wellington, New Zealand.

Alexander, F.E.S., 1946. The Sun's radio energy. *Radio & Electronics*, 1, 16-17, 20.

Allen, C.W. and Gum, C.S., 1950. Survey of Galactic radio-noise at 200 Mc/s. *Australian Journal of Scientific Research*, A3, 224-233.

Bhathal, R., 1996. *Australian Astronomers: Achievements at the Frontiers of Astronomy*. Canberra, National Library of Australia.

Blainey, G., 1968. *The Tyranny of Distance: How Distance Shaped Australia's History*. Melbourne, Macmillan.

Bok, B., 1971. Taped interview with W. Sullivan.

Bolton, J.G., 1947. Summary of Cygnus Results – 4th-25th June, 1947. Undated 1-page report, in File A1/3/1a (RPA).

Bolton, J.G., 1948. Discrete sources of Galactic radio frequency noise. *Nature*, 162, 141-142.

Bolton, J.G., 1950. Letter to E.G. Bowen, dated 14 April. In File F1/4/BOL/1 (RPA).

Bolton, J.G., 1982. Radio astronomy at Dover Heights. *Proceedings of the Astronomical Society of Australia*, 4, 349-358.

Bolton, J.G. and Stanley, G.J., 1948a. Variable source of radio frequency radiation in the constellation of Cygnus. *Nature*, 161, 312-313.

Bolton, J.G. and Stanley, G.J., 1948b. Observations on the variable source of cosmic radio frequency radiation in the constellation of Cygnus. *Australian Journal of Scientific Research*, A1, 58-69.

Bolton, J.G. and Stanley, G.J., 1949. The position and probable identification of the source of Galactic radio-frequency radiation Taurus-A. *Australian Journal of Scientific Research*, A2, 139-148.

Bolton, J.G., Stanley, G.J. and Slee, O.B., 1949. Positions of three discrete sources of Galactic radio-frequency radiation. *Nature*, 164, 101-102.

Bowen, E.G., 1945a. *Future Programme of the Division of Radiophysics*. Report dated 2 July, in File D1/1 (Copy in RPA).

Bowen, E.G., 1945b. Letter to F.G.W. White, dated 1 August. In File A1/3/1a (RPA).

Bowen, E.G., 1946a. Letter to Sir Edward Appleton, dated 23 January. In File A1/3/1a (RPA).

Bowen, E.G., 1946b. Letter to J.L. Pawsey, dated 23 August. In File A1/3/1a (RPA).

Bowen, E.G., 1947a. Letter to D.F. Martyn, dated 28 January. In File A1/3/1a (RPA).

Bowen, E.G. (ed.), 1947b. *A Textbook of Radar*. Sydney, Angus and Robertson.

Bowen, E.G., 1948. Letter to J.L. Pawsey, dated 17 June. (#4). In File F1/4/PAW/1 (RPA).

Bowen, E.G., 1973. Taped interview with W. Sullivan.

Bowen, E.G., 1984. Taped interview with W. Sullivan.

Bowen, E.G., 1987. *Radar Days*. Bristol, Adam Hilger.

Bracewell, R.N., 1948. Letter to E.G. Bowen, dated 19 January. In File F1/4/BRA (RPA).

Bracewell, R.N., 1950. Letter to M. Ryle, dated 6 January. In Box 1/1, RYL.

Bracewell, R.N., 1984. Early work on imaging theory in radio astronomy. In Sullivan, 1984a, 167-190.

Briton, J.N., 1946. Letter to O.B. Slee, dated 18 March. In File A1/3/1a (RPA).

Christiansen, W.N., 1984. The first decade of solar radio astronomy in Australia. In Sullivan, 1984a, 113-131.

Christiansen, W.N. and Hindman, J.V., 1952. A preliminary survey of 1420 Mc/s line emission from Galactic hydrogen. *Australian Journal of Scientific Research*, A5, 437-455.

Christiansen, W.N., and Mills, B.Y., 1964. Biographical memoir of Joseph Lade Pawsey. *Australian Physicist*, 1, 137-141.

Collis, B., 2002. *Fields of Discovery: Australia's CSIRO*. Crows Nest, Allen & Unwin.

Courtice, F.C., 1988. Research in the medical sciences: the road to national independence. In Home, 277-307.

de Vaucouleurs, G., 1976. Taped interview with W. Sullivan.

Edge, D.O. and Mulkay, M.J., 1976. *Astronomy Transformed: The Emergence of Radio Astronomy in Britain*. New York, John Wiley.

Gardner, F.F., 1986. Letter to W. Sullivan, dated 12 November.

Gillmor, C.S., 1991. Ionospheric and radio physics in Australian science since the early days. In Home, R.W. and Kohlstedt, S.G. (eds.). *International Science and National Scientific Identity*. Dordrecht, Kluwer. Pp. 181-204.

Goddard, D.E. and Haynes, R.F. (eds.), 1994. Pioneering a New Astronomy: Papers in Memory of John G. Bolton. *Australian Journal of Physics*, 47(5), 495-680.

Goddard, D.E. and Milne, D.K. (eds.), 1994. *Parkes: Thirty Years of Radio Astronomy*. Melbourne, CSIRO Publishing.

Haynes, R., Haynes, R., Malin, D. and McGee, R., 1996. *Explorers of the Southern Sky: A History of Australian Astronomy*. Cambridge, Cambridge University Press.

Hey, J.S., 1973. *The Evolution of Radio Astronomy*. London, Elek Science.

Hey, J.S., Parsons, S.J. and Phillips, J.W., 1946. Fluctuations in cosmic radiation at radio-frequencies. *Nature*, 158, 234.

Home, R.W. (ed.), 1988. *Australian Science in the Making*. Cambridge, Cambridge University Press.

Home, R., 11982-83. Origins of the Australian physics community. *Historical Studies*, 29, 383-400.

Home, R., 2005. Rainmaking in CSIRO: the science and politics of climate modification. In Sherratt, T. et al. (eds.). *A Change in the Weather: Climate and Culture in Australia*. Canberra, National Museum of Australia Press. Pp. 66-79.

Jaeger, J.C., 1943. *Theory of the Vertical Field Patterns for R.D.F. Stations*. RP Report, No. RP174 (copy in RPA).

Jarrell, R., 2005. "Radio astronomy, whatever that may be." The marginalization of early radio astronomy. In Orchiston, W. (ed.). *The New Astronomy: Opening the Electromagnetic Window and Expanding our View of Planet Earth*. Dordrecht, Springer. Pp. 191-202.

Kellermann, K.I., 1996. John Gatenby Bolton (1922–1993). *Publications of the Astronomical Society of the Pacific*, 108, 729-737.

Kellermann, K.I., Orchiston, W., and Slee, B., 2005. Gordon James Stanley and the early development of radio astronomy in Australia and the United States. *Publications of the Astronomical Society of Australia*, 22, 1-11.

Kerr, F.J., 1971. Taped interview with W. Sullivan.

Kerr, F.J., 1976. Taped interview with W. Sullivan.

Kerr, F.J., 1987. Letter to W. Sullivan, dated 6 April.

Lovell, A.C.B., 1964. Joseph Lade Pawsey 1908–1962. *Biographical Memoirs of Fellows of the Royal Society*, 10, 229-243.

Martyn, D.F., 1946. Temperature radiation from the quiet sun in the radio spectrum. *Nature*, 158, 632-633.

Martyn, D.F., 1948. Letter to Sir Edward Appleton, dated 27 October. In Appleton Papers, Edinburgh University.

McCready, L.L., Pawsey, J.L., and Payne-Scott, R., 1947. Solar radiation at radio frequencies and its relation to sunspots. *Proceedings of the Royal* Society, A190, 357-375.

Mills, B.Y., 1953. Large aerial for meter wavelengths. Report, in File A1/3/10 (RPA).

Mills, B.Y., 1954. Letter to J.L. Pawsey, dated 27 August. In File F1/4/PAW/3 (RPA).

Minnett, H.C., 1986. Letter to W. Sullivan, dated 22 October.

Minnett, H.C., and Robertson, Sir Rutherford, 1996. Frederick William George White 1905–1994. *Historical Records of Australian Science*, 11, 239-258.

Orchiston, W., 1994. John Bolton, discrete sources, and the New Zealand field-trip of 1948. *Australian Journal of Physics*, 47, 541-547.

Orchiston, W., 2004. From the solar corona to clusters of galaxies: The radio astronomy of Bruce Slee. *Publications of the Astronomical Society of Australia*, 21, 23-71.

Orchiston, W., 2005a. Dr. Elizabeth Alexander: first female radio astronomer. In Orchiston, W. (ed.). *The New Astronomy: Opening the Electromagnetic Window and Expanding our View of Planet Earth.* Dordrecht, Springer. Pp. 71-92.

Orchiston, W., 2005b. Sixty years in radio astronomy: a tribute to Bruce Slee. *Journal of Astronomical History and Heritage*, 8, 3-10.

Orchiston, W., and Slee, B., 2002. The Australasian discovery of solar radio emission. *AAO Newsletter*, 101, 25-27.

Orchiston, W. and Slee, B. 2005. The Radiophysics field stations and the early development of radio astronomy. In Orchiston, W. (ed.). *The New Astronomy: Opening the Electromagnetic Window and Expanding our View of Planet Earth.* Dordrecht, Springer. Pp. 119-168.

Orchiston, W., Sullivan, W.T. and Chapman, J., 2006. *The Foundations of Australian Radio Astronomy*. New York, Springer.

Pawsey, J.L., 1945. Letter to C.W. Allen, dated 15 October. In File A1/3/1a (RPA).

Pawsey, J.L., 1946a. Letter to R. Woolley, dated 11 September. In File B51/14 (RPA).

Pawsey, J.L., 1946b. Observation of million degree thermal radiation from the sun at a wavelength of 1.5 metres. *Nature*, 158, 633-634.

Pawsey, J.L., 1946c. Solar radiation at radio frequencies and its relationship to sunspots: interpretation of results. Paper presented on 19 August at the ANZAAS Meeting. Copy in FileA1/3/1a (RPA).

Pawsey, J.L., 1947. Letter to J. Bolton and G. Stanley, dated 11 November. In File A1/3/1a (RPA).

Pawsey, J.L., 1948a. Letter to E.G. Bowen, dated 13 February. In FileA1/3/1a (RPA).

Pawsey, J.L., 1948b. Letter to E.G. Bowen, dated 12 August. In File F1/4/PAW/1 (RPA),

Pawsey, J.L., 1948c. Letter to E.G. Bowen, dated 15 August. In File F1/4/PAW/1 (RPA).

Pawsey, J.L., 1948d. Letter to Sir Edward Appleton, dated 8 September. In File F1/4/PAW/1 (RPA).

Pawsey, J.L., 1951. Letter to M. Oliphant, dated10 September. In File A1/3/1n (RPA).

Pawsey, J.L., 1961. Australian radio astronomy. *Australian Scientist*, 1, 181-186.

Pawsey, J.L. and Bracewell, R.N., 1955. *Radio Astronomy*. Oxford, Oxford University Press.

Pawsey, J.L. and Payne-Scott, R., 1944. *Measurements of the Noise Level Picked up by an S-band Aerial*. Report, No. R.P. 209 (dated 11 April. Confidential). (Copy in RPA).

Pawsey, J.L., Payne-Scott, R. and McCready, L.L., 1946. Radio-frequency energy from the sun. *Nature*, 157, 158-159.

Payne-Scott, R., Yabsley, D.E. and Bolton, J.G., 1947. Relative times of arrival of bursts of solar noise on different radio frequencies. *Nature*, 160, 256-257.

Piddington, J.H., 1959. Proposed early investigations using 210 ft radio telescope. Planning document, dated 16 September. In File A1/3/11/17 (RPA).

Piddington, J.H., 1978. Taped interview with W. Sullivan.

Reber, G., 1940. Cosmic static. *Proceedings of the Institute of Radio Engineers*, 28, 68-70.

Robertson, P., 1992. *Beyond Southern Skies: Radio Astronomy and the Parkes Telescope*. Cambridge, Cambridge University Press.

Schedvin, C.B., 1987. *Shaping Science and Industry: A History of Australia's Council for Scientific and Industrial Research, 1926-49*. Sydney, Allen and Unwin.

Slee, O.B., 1946. Letter to Radiophysics Division, CSIRO, dated 4 March. In File A1/3/1a (RPA).

Slee, O.B., 1994. Some memories of the Dover Heights field station, 1946-1954. *Australian Journal of Physics*, 47, 517-534.

Stanley, G.J., 1974. Taped interview with W. Sullivan.

Stanley, G.J., 1994. Recollections of John G. Bolton at Dover Heights and Caltech. *Australian Journal of Physics*, 47, 507-516.

Sullivan, W.T., 1982. *Classics in Radio Astronomy*. Dordrecht, Reidel.

Sullivan , W.T. (ed.), 1984a. *The Early Years of Radio Astronomy: Reflections Fifty Years after Jansky*. Cambridge, Cambridge University Press.

Sullivan, W.T., 1984b. Early radio astronomy. In Gingerich, O. (ed.). *Astrophysics and Twentieth-Century Astronomy to 1950*. Cambridge, Cambridge University Press. Pp. 190-198.

Sullivan, W.T., 1988. Early years of Australian radio astronomy. In Home, 308-344.

Sullivan, W.T., 1990. The entry of radio astronomy into cosmology: radio stars and Martin Ryle's 2C survey. In Bertotti, R. et al. (eds.) *Modern Cosmology in Retrospect*. Cambridge, Cambridge University Press. Pp. 309-330.

Tentative specifications for 80-foot and 50-foot radio astronomy aerials. Document dated 8 August 1951, in File A1/3/1h (RPA).

Thomsen, I.L., 1948. Solar radio emissions and sunspots. *Nature*, 161, 134-136.

Wild, J.P., 1965. Origin and growth of radio astronomy in CSIRO. Lecture delivered to CSIRO Division of Plant Industry. Copy in File D/12/1/5 (RPA).

Wild, J.P., 1972. The beginnings of radio astronomy in Australia. *Records of the Australian Academy of Science*, 2(3), 52-61.

Wild, J.P., 1987. The beginnings of radio astronomy in Australia. *Proceedings of the Astronomical Society of Australia*, 7, 95-102.

Woolley, R., 1947. Opportunities for astronomical work in Australia. [Section A Presidential Address.] In Ross, A.G. (ed.). *Report of the 26th Meeting of ANZAAS, Perth 1947*. Canberra, Government Printer. Pp. 64-80.

Yabsley, D., 1986. Letter to W. Sullivan, dated 10 October.

Woodruff Sullivan holds a Bachelor of Science in Physics (Massachusetts Institute of Technology, 1966) and a Ph.D. in Astronomy (University of Maryland, 1971). After a two-year postdoctoral position at the University of Groningen in Holland, he has ever since been on the faculty of the University of Washington in Seattle, USA. He teaches and does research in astronomy, astrobiology, and history of science. Sundials are a particular passion and he has designed many public sundials in the greater Seattle area and further afield.