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Recollections of John G. Bolton at Dover Heights and Caltech*

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Abstract

Personal recollections are presented by the author of the commencement of galactic and extragalactic radio astronomy at Dover Heights and the design and construction of the California Institute of Technology's Owens Valley Interferometer.

1. The Years at Dover Heights

For a number of reasons, I cannot recall exactly when John and I first met. The years 1945 and 1946 had been difficult for me—1945 had been the final year of my engineering studies and in early 1946 my mother died after a prolonged illness. So, for that time there are long gaps in my memory. For some of the critical events however, Don Yabsley has kindly supplied me with dates from the CSIRO Radiophysics Laboratory (RP) records. I do, however, recall observing, early in 1945, the Sun at North Head as a pleasant assignment. It was the time of the Pawsey, Payne-Scott and McCready (1946) press release on their *Nature* paper on 'Radio-frequency energy from the Sun'.

By John Bolton's own account, he had taken his discharge from the Royal Navy in Sydney after serving on the aircraft carrier *Unicorn* in the Pacific. Recent information records the date of his arrival at RP as September 1946. Bruce Slee had been assigned to him as an assistant and was to join him in late October. I do not recall Bruce being at Dover Heights before late 1947.

As John has related the events which followed, he was instructed to continue the patrol observations of the Sun at 1·5 m. However, on the occasion of a visit by J. L. (Joe) Pawsey to the Dover Heights field station, John had the antenna pointing elsewhere and was ordered back to the RP Lab (in Chippendale, in the grounds of the University of Sydney). At that time, I barely knew him and have to rely on his account of his return.

In early 1945 I had been assigned to Group D (receivers), leaving Garth Dewsnap's modulator group. L. L. (Lindsay) McCready was my supervisor. Under his direction, I developed a 200-MHz system for C. W. Allen's solar work and, with Lindsay, installed it at Mount Stromlo. Later, the same system

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was used by Allen and Colin Gum for a Galactic survey. The development of instruments for a proposed expedition to Brazil (to observe an eclipse of the Sun occurring in May 1947) was my last project for Lindsay and Ruby Payne-Scott. At the same time, there were several other projects which kept me occupied.

In view of the letter to *Nature* by Hey *et al.* (1946) it would be surprising had attempts to detect Cygnus not taken place. Although it was not the only explanation of what Hey *et al.* saw, a small-diameter source outside the ionosphere rated high on the list. So, within RP committees, there had to be speculation. The first and more important attempt that I witnessed, to detect Cygnus as a radio source, took place most probably in mid-October 1946, at 75 MHz, using antennas intended for the Brazil expedition. Since John was working in the main receiving room, the equipment was installed in the generator hut, with some minor assistance from me, and the observations made from there by a group from Chippendale. This went on for about three days, but I saw no records.

I did not see John's attempt to detect Cygnus, although he later credited David F. Martyn with the idea, which seems to be missing from other accounts. Of the two receivers, the one at the generator hut was the better design, using a frequency close to that used by Hey *et al.* Possibly the fact that Cygnus was difficult to observe from Sydney may have inhibited any enthusiasm for perservering with the observations, and the attempt seems to have been hurriedly conceived.

Both John's attempt and the October 1946 one were more unlucky than unskilful as Cygnus was rising in the afternoon. Later experience showed that even in times when the Sun was quiet, which it was not at this time, records gradually improved into the evening hours; the best records occurred shortly before dawn, the worst were shortly before, and sometimes just after, sunset.

In the above, I have assumed that John was looking for Cygnus. Knowing him, and given the circumstantial evidence presented by Hey *et al.* in their letter which had just appeared, I think this is very likely to be true. He must have been back at the Lab no later than mid-November 1946. There is very little room for changing the times of the occurrence of the above three events, since there was a recorded observation by John and myself at Dover Heights on 8 March 1947.

Perhaps Joe Pawsey's concern was not John's attempt to detect Cygnus. Indeed, it would be difficult to see how such an attempt interfered with solar observations, as the Dover array moved only in azimuth. With Cygnus rising in the afternoon, the solar observations would be over unless John was using another antenna. Rather, Joe may have been worried that the Brazil equipment might not be completed on time.

In the two months following John's return to the Lab, he and I became better acquainted. Although we were in the same room, he was assigned different tasks and had little to do with the Brazil work. John and Paul Wild began a translation of Waldemeier's book on the Sun but they did not go beyond the first chapter.

At lunchtime in the Group D Lab, John and I often played cards as partners. When the weather permitted it was cricket on the St Paul's College Oval. It was a very pleasant time for me and I regretted that it had to change when Lindsay reported that the Brazil expedition had been cancelled. In the immediate postwar years, logistic problems in shipping and ventures to remote sites were very risky.

In my very early years, Lindsay and several other research officers in the group had been very patient while I was learning techniques; these ROs were Garth Dewsnap of the modulator group, Ted McCarthy, and a young man with whom I had carried out the North Head measurements. They all left at the end of the war.

Before Pawsey's announcement that the Brazil expedition had been cancelled, John and I must have restarted solar observations at Dover Heights. There was more than a month's work in setting up equipment before the March sunspot of 1947. Our initial attempt to detect Cygnus was made in May. We fully appreciated now the difficulties of trying to observe weak sources in the daytime so, with the Cygnus area rising in the middle of the night, this seemed timely. Cygnus appeared on the first record but it took some days until we could be perfectly sure, as the system was erratic. At that time we divided our efforts, although some individual observing shifts were as long as 48 hours. Ultimately, the result was an apparently meagre five sources. The first one, which appeared on my watch, was Taurus A.

By mid-July, although the record analysis had not proceeded very far, we decided to try for a more accurate Cygnus observation near its rising and setting times. The nearest possible location for such an observation was the western headland of Pittwater, looking out to sea beyond Lion Island to the north–east and across the widest part of the Hawkesbury River to the north–west.

Joe and Lindsay arrived in the late afternoon carrying a picnic supper; at that time they had not seen a record. I struggled that evening with a balky generator which refused to work. Owing to the severe winter cold, Joe came down with influenza several days later and did not return to the Lab for a couple of weeks. Even when the generator did work on succeeding nights, the experiment was a failure. When record analysis became more advanced and we were still not making identifications, we considered the possibility of finding a site with a clear northerly view over the ocean or, failing that, two sites, preferably close together, with clear westerly and easterly views over the ocean. Three sites were identified in New Zealand. The preferred site, on the northern-most cape of the North Island, was not used as I was troubled by the poor roads in that area. Alternatively, we used two sites: Pakiri Hill at Leigh, and Piha, 30 km west of Auckland. (For an account of the NZ expedition see Orchiston, this issue p. 541.)

One comment I have on the NZ expedition is that the dates were such that only one source both rose and set at night—this was Cygnus A, the only source not identified. Daytime records were notoriously unreliable; for example, the Sun at Dover Heights could rise minutes late at our lowest observing frequencies.

Despite the foul weather at Leigh, all the NZ records were of a much higher quality than those at Dover Heights and this had a lot to do with the identifications. Also, in hindsight, custom had corrupted our thinking. Had we constructed the simplest of east–west interferometers, we could have detected Cygnus A much more easily at Dover Heights. Martin Ryle at Cambridge had realised this. It is probable that his measurements were prompted by the letter by Hey *et al.* in August 1946, as were our own. Bernard Mills also correctly saw the future in the multiple-element interferometers at a much later date; he had returned to Radiophysics in 1948 after a long illness. He declined an invitation to join the Dover Heights group because he preferred to start his own experiments.

In this very early period many questions were raised that remain unanswered. Some of them can be answered from the RP archives, although John could have provided vital clues. These questions were not easy to raise through correspondence and I looked forward to a personal meeting and interchange of ideas. Between late March and early June 1993, on a visit to Australia, I made a point of visiting John, having been unable to do so on my last trip in 1985. It had been 20 years since we met at the IAU meetings of 1973, in Sydney. He wanted to talk. In the past, our discussions would often become animated, but now that the chance had come, I realised that it was too late. Although his mind was still alert and his memory good, it all seemed unimportant when I saw how his physical condition had deteriorated. On that day in Buderim, in May 1993, we remembered only that those early years were the happiest and most carefree days of our lives.

There were no profound insights into the past that day, but it was as though time had stood still. He received me with great cordiality as he always did: never the cold pragmatist, his heart often directed his relationships and decisions, at times to his disadvantage. He would go out of his way to correct a misunderstanding with a subordinate and make an offended employee his friend. On the other hand, he could be unrelenting, even prejudicial in opposing a researcher whose work or abilities he did not accept. This prejudice was persistent throughout his life.

In his approach to research, John saw forests and painted the trees with a broad brush. There were times when he could take my break away in assuming that something, for which I dared not even hope, was a fact. I doubted the first identification of M 87, whereas John never hesitated to accept it as being correct. He was emotional and sensitive, particularly to the problems of others. He showed much kindness to my young family who were adapting to life in a new country. He was human; he was a good friend.

The most memorable moment of my association with John occurred when we first saw interference fringes from Cygnus A. In a world now accustomed to inexplicable results from radio observations, it is difficult to comprehend the emotional impact of an observation which took us from the partially explicable solar system and Galactic radio emission phenomena, into the realms of phenomena with inexplicably high energy outputs, no matter where they were located. Within a few short months, with the identification of NGC 5128 and M 87, the realm of our observing world was the universe! Neither of us ever approached such an emotional high again in our work. (For the original papers see Bolton and Stanley 1948*a*, 1948*b*.)

One question to ask is why did the Dover Heights field station lose its significance? With the publication of the first source surveys by Stanley and Slee (1950), and Ryle *et al.* (1950), some doubts arose about the future of the sea interferometer. By 1952, when Mills published his first survey using a two-element interferometer (Mills 1952), the inescapable conclusion was that Dover Heights had very little time left. By then there were well over 100 known sources but very few identifications. Compelling arguments were that multiple-element interferometers, although more expensive, could measure positions with greater precision and were more flexible tools for measuring source diameters.

Superficially, despite the apparent advantages of the sea interferometer, it also faced a number of problems. The calibration errors were mainly due to external effects such as tides and atmosphere and, at frequencies around 100 MHz, with only one antenna and one receiver, the source was often seen against a spatially changing background. Another limitation was that fringes could be viewed only for a limited time as the measurement period was restricted by the antenna elevation beamwidth and receiver bandwidth.

Rising times could be measured at Dover Heights with reasonable accuracy, although atmospheric refraction is a maximum at the horizon. Accurate position measurements require both the rising time and its relationship to lobe period. The cumulative errors produced unsatisfactory answers. To reduce these errors, an ideal site should have either clear east–west or north–south views over the ocean. The NZ expedition of 1948 had used two sites to watch a source both rising and setting; this method is obviously clumsy and expensive and has no merit except when only a few sources are studied.

There is no need to mention the other options under discussion at RP at that time as there is ample documentation in the RP archives. It is sufficient that, for conventional interferometers or arrays, most of those problems did not exist or could be countered with such techniques as phase switching and phase tracking. Those types of instruments were largely dominated at RP by the innovative ideas of Bernie Mills, for cosmic observations, and W. N. (Chris) Christiansen, for the Sun. On the other hand, E. G. (Taffy) Bowen (chief of the RP Lab) was advocating a large fully steerable paraboloid. It would be less accurate for position measurements but in combination with interferometers it would be a very powerful instrument. The combination would reduce confusion problems considerably.

By early 1950, our first major telescopes at Dover Heights were nearing the end of their useful lives. We had built two large telescopes, a fully steerable nine-element yagi array and a 16-ft paraboloid for spectral work. I felt that we should try to move away from this configuration and enter the field of two-element interferometry. The idea was politically naive as I offered nothing very original in the way of technological ideas. However, John went along with the idea as an alternative to a larger telescope at Dover.

We concocted a scheme whereby two cylindrical parabolas were mounted on tracks. These were to have line feeds capable of multiple frequency operation. It was my misconception that the antennas could be built cheaply from wood! The idea was never developed, nor did it have the enthusiastic support of the rest of the Dover Heights group. In mid-1950, John and Kevin Westfold went to England for six months. John was still committed to his large telescope at Dover Heights.

Paul Wild invited me to join him on a trip down the south coast of New South Wales in September 1950, as he was looking for a new site for a solar telescope. Since this would be an opportunity for me to find a site suitable for the interferometer which we had in mind, I agreed. We borrowed an old army ambulance from the military and Paul, John Murray, Steve Smerd, and I as the only driver, set off on our missions. The trip is an epic story in itself and full of the unexpected problems peculiar to neglected army vehicles. Following a major breakdown, Paul and Steve left us at Berry (near Nowra, on the south coast of NSW) whilst John Murray and I travelled down the coast close to the Victorian border. I chose a site at Jervis Bay and although it was never used, the idea of the interferometer was the genesis of the one built at Owens Valley in California.

Subjective personal relationships play a great part in determining the course of science. In 1951, Pawsey was faced with a decision which would have taxed the wisdom of Solomon. Dover Heights, which had led the way from solar observations to the cosmos, was winding down. No matter what the future held, the Lab could no longer afford the apparent *laissez faire* policy of having an almost unlimited number of observational groups. Meaningful instruments were becoming much more expensive, and Pawsey procrastinated.

At that time, nothing in the scientific literature showed how good the Jervis Bay interferometer could have been. In the intervening years up to 1953, when Bernie Mills developed his ideas for the Mills Cross, it became even less likely that such a system could be supported at Radiophysics. When John returned from England, he found decreasing support for his idea of a larger telescope at Dover Heights. Finally, we were forced to compromise with an enlarged version of the nine-element yagi array, using twelve elements, nine of which came from the original array. The mount was from an existing radar system which could be turned only in azimuth. The array's cost was very small and needed no special budgetary allocation. Meanwhile, I felt that this was only delaying the inevitable.

Despite a meagre increase of 30% in collecting area, the array did produce very good results and two important papers: Bolton, Stanley and Slee (1954) and Bolton, Westfold, Stanley and Slee (1954). Surprisingly, the array produced a catalogue of 104 sources, mainly as a result of improvements in the electronics and azimuth beamwidth. By this time the design for the Mills Cross had been completed which presaged the end of the Dover Heights field station.

John's last idea at Dover Heights was the 80-ft paraboloid for which we all willingly dug the hole: Kevin Westfold, Bruce Slee, Dick McGee, John and I (see Fig. 3 in the paper by Westfold, p. 535). It produced several papers, including McGee *et al.* (1955) on the Galactic centre at 400 MHz and Stanley and Price (1956) on an attempt to detect Galactic deuterium. I was also able to try out some electronic ideas which later bore fruit for the Owens Valley interferometer. Following this, the only hope remaining was the Jervis Bay interferometer, and its future was very doubtful.

By his own account, John had been at odds with Joe Pawsey since the first few weeks of his appointment. It could have been that John found it difficult to get on with anyone to whom he was accountable; he could be arrogant sometimes, or in the depths of depression. To get to Dover Heights, he would usually go directly from his home in Bellevue Hill whereas I, coming from across the harbour on Sydney's north shore, would go into the Lab and then to Dover Heights. So, one morning in 1953, it was a surprise when John walked in, very early, to my office. At the time, Joe Pawsey was probably ready to go ahead with the Mills Cross. John talked to me for a few minutes, saying that he was going to issue Joe with an ultimatum, but not giving me any indication that he had a position to fall back on or what it was he wanted exactly. Presumably, it was financial support. He was in Joe's office for a few minutes, then both of them went in to Bowen's. John returned within 30 minutes saying 'I'm out of radio astronomy!' and left the building.

Bolton then spent over a year in the Rain and Cloud Physics group in the Lab working on rainmaking experiments (see Milne on p. 549) and, although he never spoke about it when we met outside the Lab, he seemed to adapt as well as he did to any work. Even to the end, he never spoke of what was said in Bowen's or Pawsey's office that day.

Based on what was then known of radio astronomy, there is reason to support Joe Pawsey's decision; however, hindsight proves him wrong. He and John had fundamentally different personalities. John's philosophy espoused the belief that 'a man's grasp should exceed his reach', whilst Joe's was caution. Not an intuitive man, he regarded schemes involving intuitive use of imagination as grandiose. To him, John's dreams must have been presumptuous indeed. John Bolton was offered a two-year appointment by the California Institute of Technology as a senior research fellow in physics and astronomy, to introduce radio astronomy in California. He arrived in Pasadena in January 1955 and was promoted to professor of radio astronomy after two years.

Back at Dover Heights, the group continued to work on the 80-ft paraboloid and for about a year I worked there with Robert Price from the Massachusetts Institute of Technology; we have continued a close friendship ever since. I was building a house on Sydney's Middle Harbour and hoped to inhabit it by November 1954 when John asked me to come to California to work with him at Caltech, to introduce radio astronomy there. Although it was a wrenching decision, my family and I arrived in Pasadena on 24 June 1955.

2. Starting Radio Astronomy at Caltech

John had already selected a site near Ojai, California, and true to form he wrote to Robert F. Bacher, the Division Chairman at Caltech, describing the site in glowing terms as the perfect place for a 250-ft radio telescope. Bacher was a great administrator and had put together the team at Los Alamos that solved the most intractable problem in the atomic bomb, the implosion mechanism. He was suspicious however of schemes which seemed to him too costly, and conscious of what the Office of Naval Research was capable of funding. He had laid down an edict that the new observatory was to be within 100 miles of Pasadena so as to limit housekeeping and accommodation costs. It was unlikely therefore that he would approve a 250-ft telescope before his new research project proved its worth.

Funding of the physical sciences was in a transition period at that time and the functions now performed by the National Science Foundation were assigned to the Office of Naval Research. Caltech had a strong ally in the ONR. Ironically, the US Navy had classified the Ojai site and the search had resumed for an alternative site by the time I arrived. John and I searched from Pasadena to the Mexican border and into the nearby deserts, following Bacher's edict. It was a fascinating introduction to a strange country where the police sirens wailed throughout the night, helicopters could be heard hovering and waiters were polite even in the smallest eating places.

In August 1955, John went to the IAU meeting in Dublin and I continued the frustrating search. After reaching the eastern limit of Bacher's zone at Inyokern, the hottest point of the Mojave Desert, I abandoned the edict and with a colleague who knew the area well, Temple Larrabee, drove to the Owens Valley. Here stands the magnificent Sierra Nevada Mountains to the west with the Inyo Mountains to the east (about five miles apart) and continuing for about 100 miles, rising in parts to 14,000 ft. There was no need to test, we had found the site!

In the interim, we constructed a pilot instrument at Mount Palomar. It was a 32-ft paraboloid which I designed along the principles Keith R. McAlister had used at RP, and installed a 1420-MHz receiver.

Within the radio astronomy group at Caltech, three men were assigned to me to help construct the first instrument. Two of them were as exotic and unusual as the new environment. Temple Larrabee, an engineer with multiple talents, was an extremely imposing figure with a huge frame, a mass of black, curly hair and dark eyes. He had flair and at the same time was a very simple man. He made many contributions which enabled us to complete the 32-ft telescope within six months. Ronald Hargrove was Temple's assistant—intelligent and likeable but who had never outgrown his late teens and was unable to cope with adult life. He built the telescope from aluminium angle with an aircraft rivet gun!

The third assistant was John Harriman, who had recently arrived with his family from Massachusetts, and whom we were very fortunate to have found. He was the best electronic technician I have ever worked with, anywhere. A quiet, intensely proud man, he had a great talent, used it supremely well and was content with it. He was valued also as a friend along with his Boston Irish family and remained at Caltech until his retirement. Sadly, he did not live long enough afterwards to enjoy what he loved most, his home and family.

Initially at the Owens Valley Radio Observatory, we had a staff of two: Rachel Gates was housekeeper and cook and Al Munger did everything from driving heavy equipment to general supervising. He became the superintendent soon afterwards. Al was even larger than Temple; a huge mountain man, he was gentle and insecure, always wanting to please. His family had come to the valley 100 years previously and his grandmother had described to her family, from memory, the effect of the huge 1872 earthquake on Big Pine and surrounds. Rachel was a living legend at the observatory. She was a Texan and part Cherokee, one of the Iroquoian Indian tribes that moved to Oklahoma. She was very proud of her heritage and was a woman of the most remarkable intuition and shrewdness who could outwit any man and had a great sense of humour. Her husband was also part Cherokee. They were childless but found great joy in parenting the students. Woe betide anyone who used bad language in her presence! My children considered her part of the family and she accepted the role, spoiling them with gifts and treats on their visits and at Christmas. Our lives were enriched by these great Americans and we miss and remember them all. As the observatory grew, so did the staff but the simple, homely atmosphere dictated by the original people prevailed.

Meanwhile, with his contacts at ONR, Bacher raised the money for two 90-ft paraboloids as well as private funding for the buildings. Bruce Rule, the chief engineer at Caltech at the time, handled the conceptual design for the telescopes. By late 1956, the project was well under way. The first buildings were started and were completed in early 1957. By April of that year, John was practically a permanent resident at the observatory. The first graduate students arrived for the summer vacation; they dug ditches and at a later date wired the baseline of the interferometer. By mid-1957, construction of the telescopes was under way.

In the summer of 1958, I went to meetings in Europe and to the IAU meeting in Moscow. In London on my way home I received a letter from John saying that the crane and had collapsed, dropping the declination tube of one telescope, and that there would be a delay. In January 1959, John departed for a three-month stay in Australia and England, leaving me in charge. As the interferometer would be delayed, I devised a diode Dicke switch, probably the first of its kind working beyond 900 MHz, and started single-antenna observations.

John made a final commitment to return to Australia during that visit in 1959, although he did not tell me so until August 1960. (In fact a memo from Bob Bacher, dated August 22, 1960, indicates that he had not been told either.) When he returned to the Owens Valley, John began an intensive observing program starting with the elegant study of NGC 5128. J. A. (Jim) Roberts, a visiting research fellow from the Radiophysics Laboratory, and I observed Jupiter (see p. 561 of this issue). By this time the first of our students had passed their orals and were ready to begin their thesis work.

By July 1959 the interferometer was at last operating. During the single-dish measurements, I had decided that we could safely push the operating frequency to at least 900 MHz, thereby reducing confusion in position measurements. Through the summer of 1959 to August 1960, John observed constantly, using the one available baseline, east–west, making accurate RA measurements of a large number of sources. The north–south baseline did not operate until after he left. I had assigned R. B. Read the problem of measuring declination and designing the phase-lock system.

It is important to realise that the instrument was not designed for synthesis. We still thought in terms of the sea interferometer and source-diameter measurements. Ryle's synthesis papers were only published in 1959 to 1960. Our primary concern was position and identifications. The essential spacings for synthesis were missing beyond the 800-ft separation of the two paraboloids.

John returned from the Owens Valley to Pasadena and flew to the London URSI meetings in late August 1960. Before leaving, he told me of his decision to return to Australia. I argued that it was foolish as we were in a strong position. Funding was almost certainly available for a larger instrument, if he wished. However, he was irrevocably committed to the Australian promise and left Pasadena in December 1960. Bob Bacher gave him a generous tribute at a farewell ceremony in the Astronomy Library at Caltech. I cannot give the reasons easily for his departure; they were not as simple as they appeared. He left with only one of the Ph.D. students, Dan Harris, close to finishing his thesis although he set the direction for at least four others. We were placed in a precarious position with ONR which made the continuation of the existing funding level problematical.

Fortunately, during the following year, our research produced a number of outstanding results. In particular was the work of A. T. Moffett on the brightness distribution, which combined the then published ideas of Ryle with model-fitting techniques dating back to Michelson to overcome the existing limitations of the interferometer.

In 1961, I was appointed the first director of the Owens Valley Radio Observatory and remained such until 1975.

Joe Pawsey came through Pasadena for the last time in 1962, on his way to the National Radio Astronomy Observatory to succeed Otto Struve as director. He stayed the night with my family, a perplexed and noticeably ill man, still very gentle in his manner. He asked me why things had happened that way, to which I had no answers.

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