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Some Memories of John Bolton from Caltech and Early Years at Parkes*

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Abstract

John Bolton had such an influence on radio astronomy that a major review of his contributions should be prepared by an historian of science. Here just some events from his last few years at the California Institute of Technology and from the first few years of the Parkes Telescope are recalled.

John Bolton had a tremendous influence on the science of radio astronomy. When you think back over his career—first to those pioneering days just after World War II that resulted in the first identifications of radio and optical objects beyond the solar system and produced the early 100-MHz sky surveys, and then to his establishing radio astronomy at Caltech and setting up the Owens Valley Radio Observatory and all that has flowed from that, then to Parkes, where he established and maintained the observatory over its golden years, providing tremendous support for research by others in fields like spectral-line, pulsar and polarisation studies, and organising numerous sky surveys and, with his colleagues, pursuing extensive studies of quasars—you can but be amazed at what he accomplished. And to this you must add the influence that he had on so many students and colleagues—off-hand, I can think of five or six of John's students who have become directors of radio observatories; one has received a Nobel Prize.

I would like to see a major review of John's contributions prepared by an historian of science. I think he deserves it. Such an undertaking is far beyond me. Here I just recall a few memories of John from his last few years at the California Institute of Technology and from the first few years of the Parkes Telescope.

In the rapid development of radio astronomy after the second world war, the US somehow got left behind. By the early 1950s, Australia was well ahead of the US in this field. In the mid-1950s, Caltech, already managing the Mt Wilson and Palomar Observatories and the Jet Propulsion Laboratory, decided to add radio astronomy to its fields of interest. Lee DuBridge, the president

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of Caltech, had been the head of the Massachusetts Institute of Technology's Radiation Laboratory during the war, when Taffy Bowen had worked there for some years. It was as a result of the friendship that Taffy had established with DuBridge that John Bolton went to Caltech early in 1955, to establish a radio astronomy section in the Astronomy Department, and to build their first radio astronomy observatory.

My association with John began two and a half years later, in September 1958, when I arrived at Caltech to spend a couple of years as a research fellow in radio astronomy. I had spent the previous five years working in solar radio astronomy with Paul Wild, and had hardly known John at Radiophysics. However, when I wrote and asked if he would suggest somewhere for me to stay on my arrival, the answer came back: 'Of course you will stay with us.' And so I did. It was Letty Bolton who helped me to find an apartment and buy the 'necessaries' to set it up. Indeed John and Letty were the centre of much of the social life of the radio astronomy group—their relaxed and friendly attitude set the tone, and the section was like a big family.

When I arrived, the Owens Valley interferometer was under construction at Big Pine. The building and the east–west track were complete, and the 90-footers were being erected. This instrument was designed mainly for accurate position finding of 'discrete sources' (Bolton 1956), and so its construction formed part of John's long pursuit of these sources, beginning with his pioneering discoveries at Dover Heights, and continuing through so many years at Parkes. Indeed, whenever his name is mentioned, we think of extragalactic sources.

It is interesting, then, that the first radio astronomy project that John started at Caltech was a hydrogen-line study. This used a 20-ft dish at Palomar. It was later extended to 32 ft and moved to the Owens Valley (Bolton *et al.* 1958*b*). John also co-authored a paper with Paul Wild that drew attention to the possibility of measuring magnetic fields in space by observing the Zeeman splitting of the 21-cm line (Bolton and Wild 1957). The second instrument constructed was a 25-MHz interferometer intended for scintillation studies. As far as I know, the only publication from that instrument was the report of a solar occultation of the Crab Nebula (Bolton *et al.* 1958*a*). One of the early uses of one of the 90-footers was for solar work. Convair Scientific Research Laboratory (San Diego) installed a sweep-frequency receiver covering the range 500–950 MHz, so bridging the gap between the low-frequency spectrographs pioneered by Paul Wild and the high-frequency extensions by Fred Haddock at the University of Michigan. Because of my background in solar radio work, John arranged for me to collaborate with the Convair and Michigan groups in analysing these records, which led, amongst other things, to my spending a very pleasant six weeks in Ann Arbor, Michigan.

In some ways it is quite surprising that the first large radio telescope constructed at Caltech was an interferometer. John went to Caltech as the protégé of Taffy Bowen, who strongly favoured a single big dish. In fact, Taffy had been involved in a proposal to build a big dish at Caltech well before John went there (Bowen 1981). The planning that led to the construction of the Owens Valley interferometer occurred before I arrived at Caltech and I never heard it discussed. The letter from Lee DuBridge offering John the job at Caltech said, 'We would certainly hope that we could proceed at once with the construction of a crossed dipole antenna array.... At the present time we do not see the funds in sight which will be required to construct a very large aerial of the steerable searchlight type...'.

The interferometer design must have been strongly influenced by John's earlier work at Dover Heights, which had shown that position and angular size measurements required more resolution than could be obtained from a single dish. But it would be interesting to know how the particular design evolved—two reasonably large (90-ft) dishes, able to be operated singly or connected as an interferometer while positioned at various stations along 1600-ft east–west and north–south rail tracks. This was a far cry from a 'crossed dipole antenna array', and the move to higher frequencies associated with this design had far-reaching results. [In a letter received since the symposium, Gordon Stanley writes, 'The interferometer was conceived as an act of faith, not logic. The Jervis Bay cylindrical parabolas became 90-ft paraboloids, as that was considered both the largest possible for an equatorial mount and within probable limits of a Navy budget.' For more on the cylindrical parabolas proposal, see Stanley on p. 507.]

A few days after I arrived in Pasadena, John announced that we would leave for the site at 5:30 the next morning. The early start was to avoid the heat on the 250-mile, 5-hour drive through the Mojave Desert. At Big Pine the first dish was up and the feed support was being installed. For the second 90-footer I saw the upper section of the tower (the polar-axis mount) welded to the lower 'tepee' section, the declination tube installed, the dish constructed on the ground, and finally lifted into place. All this was closely supervised by John at the theodolite. These must have been great days for him; after the years of planning, to see the dishes erected and then go into service must have been very rewarding. A lot of the wiring and fitting out of the inside of the tepees was done by the staff and students so that later, John was able to say of me that I went to Caltech as a theoretician but came back with some knowledge of practical things.

During the construction there was a plague of 'yellow jackets'—a type of large wasp. They got inside the tepees and under the welders' hoods. A pest exterminator came and sprayed the tepees, unfortunately with an oil-based insecticide, so that thereafter the insides of the tepees were as slippery as ice. Later, when yellow was chosen as the colour for the cover of the Owens Valley Radio Observatory publications, they were immediately dubbed 'yellow jackets'.

The Owens Valley dishes have polar-axis mounts. Now with a polar-axis mount it is easy to drive in equatorial coordinates, but very difficult to define limits that make full use of the potential sky coverage. To stop the dishes hitting the structure, they were limited to ±4 hours at northern, and ±1 hour at southern, declinations. But of course a clip lead will always short out a limit switch and it became common practice for keen observers to exceed the ±1-hour limit at low southern declinations. Supposedly, of course, you kept an eye open to see that the dish did not collide with the top of the jack screw, but I recall one occasion when a certain, now well-known, Indian astronomer severely dinted a dish panel while John Bolton was away. Much pressure was exerted on Big Al (Al Munger, the site manager) to straighten or replace the panel before John next appeared at the observatory.

As soon as the first dish was completed it was equipped with one of Gordon Stanley's state-of-the-art receivers. At that time, 'state of the art' meant a mixer receiver using a point-contact IN21 diode (a World War II invention), with a 'Stanley Steamer' local oscillator. At a frequency of 960 MHz, with both sidebands being accepted, the receiver temperature was 300 K, an exceptional performance for that era. Gordon Stanley attributes this to a special vacuum tube, used by Bell Laboratories in their cross-country telephone relays, which 'had an almost unbelievable figure of merit'. To stabilise the system for single-dish work, the antenna was switched against a sky horn or a resistor in liquid nitrogen. The switch and cabling added 90 K; spillover was not mentioned (Harris and Roberts 1960).

John gave Dan Harris, one of the graduate students, the job of searching for all the sources listed in the third Cambridge (3C) catalogue to help resolve the Sydney/Cambridge conflict about the source counts. Dan discovered some of the famous CTA sources, like CTA 21, in this project. He would set the telescope for a drift scan across some 3C source, and sometimes nod off and scan far beyond the 3C source, occasionally finding a new source. Because the lower-frequency surveys were regarded as unreliable, it was not immediately evident that these sources were of a new spectral class, undetected at metre wavelengths, but relatively strong at decimetre wavelengths. This was clearly established when Ken Kellermann combined his Caltech observations with those made at other frequencies at other observatories (Kellermann *et al.* 1962). Later spectral studies undertaken at Parkes by Bolton and his collaborators showed that such radio spectral peculiarities are a feature of quasars.

Before the dedication of the Caltech telescopes in November 1958, Taffy Bowen, according to John, extracted a promise from him that he would return to Australia and take charge of the Parkes Telescope when it was in its final stages of construction (Bolton 1994). Perhaps in preparation for this, John spent a couple of months back in Australia in early 1959. It was during his absence that one night we had the first rain after the construction of the telescopes. Yes, it sometimes rains even in the desert! Dan Harris was observing during the night and when I got up in the morning there was a note from Dan telling me that the world's most versatile radio telescope had been brought to a halt by the first rain—the cable trays all along the track weren't watertight!

On his way back to Caltech, John visited the Naval Research Laboratory in Washington, and heard that the emission from Jupiter at 3 GHz was stronger than that expected for black-body radiation. He immediately conjectured that, like the 'discrete sources' of radio emission, Jupiter might emit 'non-thermal' radiation that was stronger at the lower frequencies. He suggested that Gordon Stanley and I start a search for emission from Jupiter at 960 MHz. This led to the long series of observations by groups at Caltech and Parkes which, together with results from elsewhere, showed that Jupiter is surrounded by an intense radiation belt, and that the magnetic field of that planet is significantly non-dipolar.

It is interesting that the NRL result was reported at the Paris Symposium which I had attended en route to the USA (McClain and Sloanaker 1959; Drake 1983). I don't know whether, as a solar radio astronomer, I skipped this talk; if I did hear it, then its significance was lost on me. For the sake of historical accuracy I should also point out that the account given by Frank Drake (Drake 1983) can hardly be correct. He said that the Caltech Jupiter work started as a result of a visit to Green Bank by John Bolton during the later stages of the

Green Bank observations. However, the publications (Drake and Hvatum 1959; Roberts and Stanley 1959) give the dates of the Caltech observations as before those at Green Bank.

Since John had been responsible for the first identifications of radio sources with optical objects (Bolton *et al.* 1949), it is hardly surprising that accurate positioning of radio sources was one of the projects that engaged his attention with the new instrument—initially with the single dish and then with the interferometer. The positions measured at Owens Valley were plotted on the original Palomar Observatory Sky Survey plates, which were stored in the basement of the astronomy building at Caltech. Likely identifications were then followed up by the optical astronomers. Much of the position measuring with the interferometer fell to Tom Matthews and, later, to Dick Read. Tom worked closely with John on the identifications and continued this work after John left.

There were two notable outcomes of this project. The first was the identification of the source 3C 295 with a galaxy that Minkowski then showed to be at a redshift of $z = 0{\cdot}46$, a far greater redshift than that of any other galaxy then known, and a record that stood for many years. The second was the identification of 3C 48 (a source unresolved by the Manchester interferometer) with a star—but a star with a very unusual spectrum.

The 3C 48 result was reported at the end of 1960 (Matthews *et al.* 1961). John later reported (Bolton 1982) that in 1960 he and Jesse Greenstein had tried fitting the spectrum of 3C 48 with a large redshift and achieved a reasonable fit. However, two years after the first publication Jesse Greenstein presented a long paper at the December 1962 NASA Conference on the Physics of Non-Thermal Radio Sources, in which he managed to interpret the spectra of 3C 48 and three other later stellar candidates as the spectra of peculiar stars within our Galaxy. Strangely, this paper did not appear in the proceedings of that conference—less than two months after the conference Maarten Schmidt had shown that the spectrum of 3C 273 indicated a high redshift!

Other projects that John was responsible for initiating included:

- hydrogen-line absorption studies with Radhakrishnan, which Rad continued with Barry Clark and Bob Wilson, and took up again when he came to Parkes;
- a Galactic plane survey with Bob Wilson;
- source-size surveys and limited source mapping by Al Moffet;
- radio spectral studies of supernova remnants by Dan Harris.

Because John left for Australia only about 12 months after the interferometer began operating, many of these projects did not come to fruition while he was still at Caltech.

It seems to me that Caltech gave John the opportunity to develop and realise his potential in a way that had been denied him earlier. Anything that I know of him in the era before I went to Caltech is based on hearsay, but according to his own account (Bolton 1982), life at Dover Heights had been somewhat frustrating, involving disagreements about what line of research should be followed. At Caltech he was the boss of radio astronomy, and he decided what was to be observed. Not only that, he was part of the world's most prestigious astronomy department; he socialised with leading astronomers and physicists, and was respected by them; he was a respected voice in US National Committees. I think Caltech was good for him, and he was good for Caltech.

John returned to Australia at the end of 1960 to be involved in the final stages of the construction of the 210-footer, and to inaugurate the Parkes Radio Observatory. I followed him back from Caltech about three months later to find the Parkes dish in much the same state as I had found the Caltech dishes on my arrival there. Here the ribs were being constructed on the ground, then lifted and attached to the turret. John was again keeping a keen eye on everything, and as soon as the surface started to go on, he became heavily involved in the process of setting the surface to a parabolic shape. As at Caltech, you will find his mark on everything around the observatory. Anything that hadn't been settled in the original plans was settled by him—and numerous things that *had* been settled, but that he found unsatisfactory, he changed.

I want to pay tribute to the effort that John put into providing facilities at Parkes for programs that were not his own. In some of these programs he played no part except to provide the facilities; in others he played a major part but his name does not appear on the publications. The two most spectacular early results from Parkes—the 3C 273 occultations, which ushered in the era of quasars, and the polarisation studies—provide examples.

As I think most people know, the occultations of 3C 273 would never have been observed successfully without John's participation. It was Cyril Hazard who came along with the idea, but it was John's organising—extending, at one occultation, to grinding a piece off the telescope—that ensured the success of those observations.

The written documents show that programs proposed for Parkes included polarisation studies (Robertson 1992) but, in fact, no provision was made for rotating the feed. It was John Bolton, responding to our success at Caltech in measuring the polarisation of Jupiter's radiation, who gave priority to the design and installation of a feed rotator. And that led to some very successful observing programs, including the serendipitous discovery of Faraday rotation. I don't think that John's name appears on any of the polarisation publications, but they all owe a great deal to him. For example, it was he who found that adding pieces to convert the ground plane from square to circular drastically reduced the variation of receiver output as the feed was rotated.

Radio astronomy, and many radio astronomers, including myself, owe a lot to John Bolton. However, it would give an unbalanced picture to imply that relations with him were always easy. He had supreme confidence in himself and often dominated those around him—a characteristic shared, of course, by many successful people. When he was leaving Caltech he mentioned to me the name of the person he had suggested should succeed him, saying that he thought this person had the right touch of, I think ruthlessness was the word he used. I recall being rather taken aback at the time, but perhaps John did have a touch of that. Whether that characteristic is required for one to be a successful boss I'll leave you to consider. (As a matter of interest, Caltech did not appoint his nominee.)

In his science John Bolton worked to some extent by intuition and would become convinced of the truth of something, and state it to be a fact, when to my more conservative mind, the evidence did not warrant that conclusion.

At one point I demanded that my name be taken off the only paper I ever co-authored with him, for just this reason. However, a compromise was reached. Perhaps some people are more in touch with nature and can sense a truth before others are convinced.

To return to my opening remarks, John Bolton had a far-reaching influence on the science of radio astronomy. A comprehensive review of his work would be very welcome.

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