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# John Bolton's Variable-baseline Interferometer and the Structure of Radio Galaxies*

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*Abstract*

John Bolton was a pioneer in the study of extragalactic radio sources, first at the CSIRO Radiophysics Laboratory and then at the California Institute of Technology's Owens Valley interferometer. I became his Ph.D. student after he returned to the Parkes Observatory from California, and I helped in the construction of the Parkes interferometer. This consisted of a 60-ft dish connected to the existing 210-ft dish by a flexible cable trailing from the 60-footer. The interferometer was used to observe continuously at 467 and 1401 MHz while the 60-ft dish was pulled along a track. The wisdom of the design is illustrated by comparing observations of Pictor A made at Parkes and at Owens Valley.

To introduce the Parkes continuously variable baseline interferometer, and my Ph.D. work with John Bolton (Ekers 1969*a*), I will go back to the period of the discovery of radio galaxies. In 1949, John Bolton identified three discrete sources of radio emission with optical objects. This was at a time when radio astronomers were not taken particularly seriously by the astronomical community. Richard Woolley commented that 'Even if these objects do exist, they could be of no possible interest to astronomers.' However, John's identifications began the breakthrough that resulted in the recognition of radio astronomy as an important new branch of astronomy.

The seminal paper announcing the discovery of radio galaxies (Bolton *et al.* 1949) is entitled 'Positions of three discrete sources of *galactic* radio frequency radiation'. The first source discussed is Taurus A, which had already been identified with the Crab Nebula, a supernova remnant in our Galaxy (Bolton and Stanley 1949). The other two objects, M 87 (identified with Virgo A) and NGC 5128 (identified with Centaurus A) are now well-known galaxies. The Bolton *et al.* paper describes them quite accurately, but then goes on to comment that although some people think they are galaxies, they are both peculiar: NGC 5128 has a strange band across it and M 87 a jet poking out, and so they are presumably Galactic objects like the Crab Nebula! I discussed this point with John a few years ago. He said that he truly believed that these objects and their associated radio sources were extragalactic, but that to say this was unacceptable and therefore it could not be included in the paper—recall that at the time there

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was no suggestion that these 'radio stars' might be extragalactic, and certainly not that they might be some of the brightest objects in the radio sky! John's correspondence shows clearly that the transition from saying that these sources were Galactic to being able to say that they were *extra*galactic occurred very rapidly. At the time, I can well believe that it might have taken a John Bolton to make such a claim, rather than to say 'it can't be, because there are no radio sources that far away'.

These identifications of radio sources depended critically on the alignment between the radio and the optical positions. The Owens Valley Radio Observatory (OVRO) interferometer, which both Jesse Greenstein and Jim Roberts have already described (see the papers on pp. 555 and 561), was primarily designed to measure the radio positions more accurately in order to identify more of these 'radio galaxies'.

The next fantastic step, which Jesse Greenstein describes (see p. 559), must have had a huge impact. For decades the grand challenge at the great observatories of Mt Wilson and Mt Palomar had been to find more-distant galaxies. Then John Bolton arrived on the scene and suggested looking at an object found near the radio source number 295 in the Third Cambridge Catalogue, and, lo and behold, the redshift was three times bigger than anything they had ever achieved (Minkowski 1960)! It was a revolution, and radio astronomy had now to be taken very seriously. By the way, this became the most distant object known in the universe, a record for galaxies which John held for 15 years; he probably held the most-distant-object record for most of his professional life.

By 1961 a new complication had arisen. Many of the radio sources associated with these galaxies were of significant angular size and were double-lobed. Al Moffet, one of John's students at the California Institute of Technology, was given a project to measure the structure of radio sources with the OVRO interferometer. He showed that it was not just a few peculiar sources, e.g. Cygnus A and Centaurus A, that were double-lobed, but almost all of them (Maltby *et al.* 1963). When John came back to Australia with this new result fresh in his mind, he was interested in building an interferometer that could follow up on this work. This is where I come into the picture. Since I am the first of John's students to speak here, I will first make a few comments on his style of educating students.

First there was an initiation process. I failed the first test because I couldn't find the necessary things to make a cup of tea. Fortunately, the next test involved a tractor (John seemed to have a fixation on using tractors) and, as it happened, I came from a farm and knew about tractors. An ignition wire had come off the tractor and they couldn't start it. I walked around, put the wire back on—and it started. That got me my studentship with John Bolton instantly—my academic records were quite irrelevant. However, the next step was more serious and I was given my first job, which was to grade the north–south track for the interferometer he wanted to build.

I think it would be appropriate here to include an e-mail message from one of John's early Caltech students, Barry Clark, who has characterised this interaction between John and his students very well:

> The set of graduate students specialised in a lot of projects. I could drive the tractor, survey, and cut with the acetylene torch. Glen Berge could weld. Ken Kellermann could make solder joints that would stay connected. George Seielstad was taught to run the

> crane. Amongst us we could do anything mechanical. The next older generation—Allan Moffet, Bob Wilson and Dick Read—could design circuits.
>
> The summer of 1961 (I think), Ken Kellermann and I were sent to Owens Valley alone, with a statement that all the parts of an interferometer were there, and we should hook them up and start observing. I've never learned so much in such a short time in my life. That summer is the main reason I eventually became an interferometer instrumental specialist.

Barry Clark went on to build the NRAO interferometer, the first very long baseline interferometry (VLBI) system; he also designed most of the Very Large Array (VLA), and a good part of the Very Long Baseline Array (VLBA).

**Fig. 1.** The Parkes 210-ft/60-ft continuously variable baseline interferometer.

Now back to the Parkes interferometer. The 210-ft dish was taking most of John's time, but there was still a problem in getting on with the identifications. These needed accurate positions, and at that time John felt that the interferometer was still the only way in which you could get sufficient accuracy. The 60-ft dish was brought from Murraybank and the first rail track for it was to be built in the north–south direction. John's instruction was that I had to make it flat, and to follow some pegs already surveyed. That was my introduction to interferometry. We graded the track, and the interferometer was built with the help of many Parkes locals and staff members from the Radiophysics Laboratory in Sydney (Batchelor *et al.* 1969).

This interferometer provides an interesting transition between arrays and big dishes. As mentioned by Jim Roberts (see p. 561) there was a strong difference of opinion between the array builders [W. N. (Chris) Christiansen and B. Y. (Bernie) Mills], and the single-dish advocates [E. G. (Taffy) Bowen and Bolton]. The single-dish advocates talked about the flexibility of being able to make rapid changes so that you could use any frequency you wanted, and about the advantage of seeing the results of what you were doing straight away. On the other hand, arrays usually had a single fixed frequency and often involved very complicated calculations before you could see what was going on. They were further limited by the sidelobes, which resulted in false responses from other strong sources and occasional lobe ambiguities in the measured positions. The big dishes had low sidelobes and could operate at higher frequencies to avoid confusion but had insufficient resolution for accurate position work. The OVRO interferometer with its two big dishes was a compromise; it had the resolution of the arrays and some of their flexibility, and the low sidelobes of the single dish. John took this to the limit with the Parkes interferometer, where he satisfied all his principles by making this interferometer capable of observing continuously while moving the 60-ft dish along the track, thus also providing the real-time capability of the single dish (see Fig. 1).

[This technique has been reinvented recently by the optical–IR wavelength interferometrists (Vivekand *et al.* 1988, 1989), who needed an interferometer that could measure the visibility function more rapidly than by supersynthesis. They have called it 'hypersynthesis'.]

The 60-ft dish was pulled along in the manner of a punt and was connected to the 210-ft with a flexible trailing cable, using an ingenious pulley system to lay out the cable as the telescope moved along its rail track. In Fig. 2 you can see the interference fringes from the radio source Pictor A as the separation between the two elements of the interferometer changes, but most importantly, you can also see the amplitude of the fringes (fringe visibility) changing due to the structure in the radio source. We had two simultaneous frequencies, 467 and 1401 MHz, and the distance to the closest spacing was such that at the high frequency it was the same number of wavelengths as the longest spacing at the low frequency. This arrangement gave a range factor of six in spacings, with a continuous measurement of visibility over this range. For Pictor A we see four peaks in the co-sinusoidal modulation of the fringe visibility resulting from a very symmetrical classical double radio source—and this was all measured without ambiguity in a few minutes.

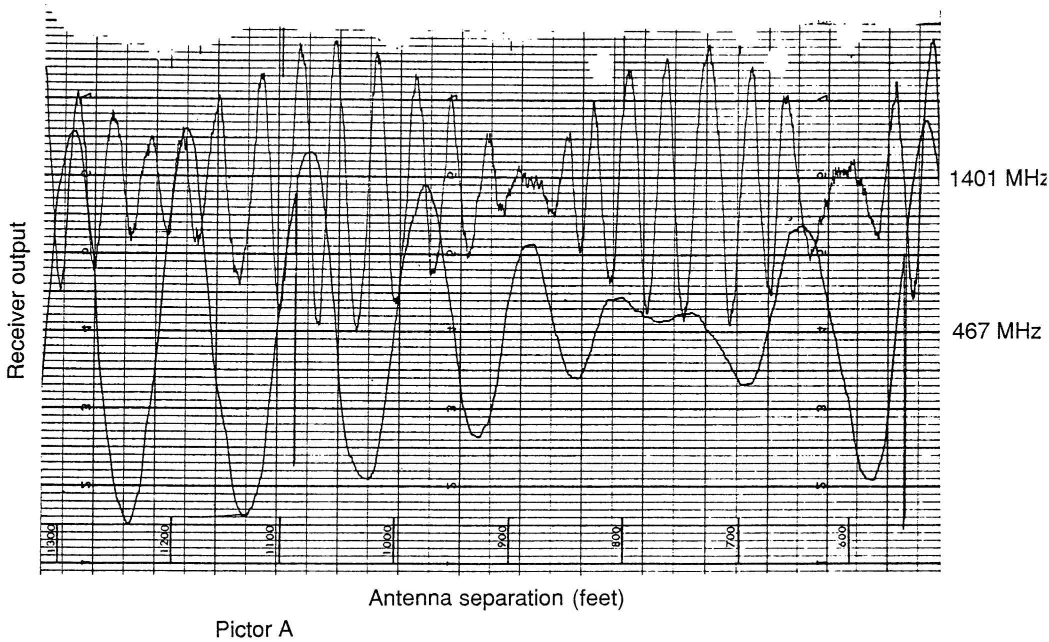


**Fig. 2.** Continuously variable baseline observations of the double source Pictor A. The upper trace is the 1401 MHz and the lower the 467 MHz receiver output. The observation was made on the east–west baseline. In this position angle the projected component separation is $4' \cdot 0$, but this separation increases with increasing resolution (Ekers 1969*b*).

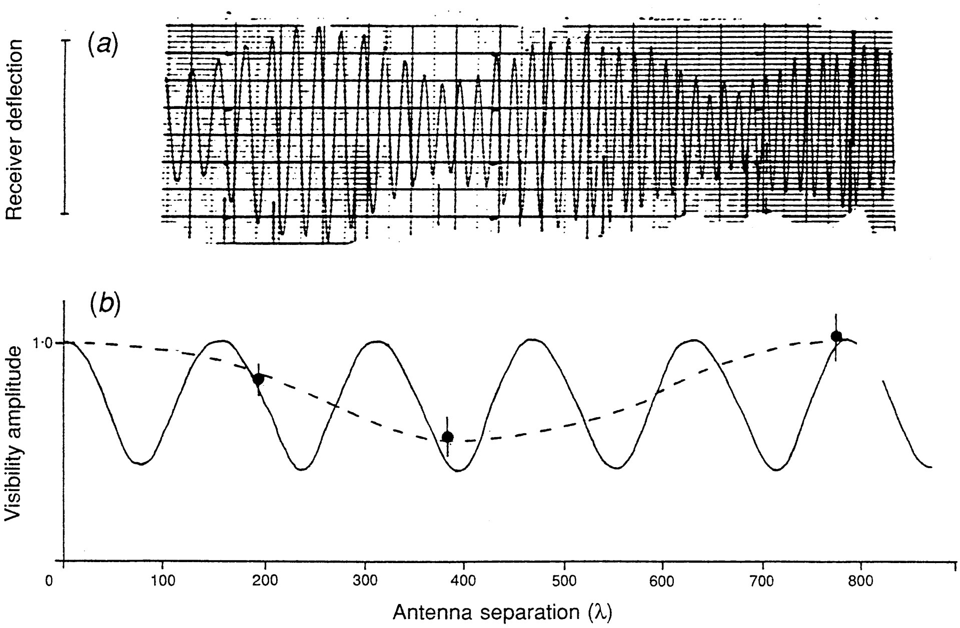


**Fig. 3.** (*a*) A 467 MHz continuously variable baseline interferometer observation of the pair of sources 3C 208 and 3C 208·1. (*b*) The fixed-baseline fringe visibility measurements from Moffet (1962). The broken curve is his interpretation and the continuous curve is the fringe visibility calculated from the known positions and amplitudes of the two sources.

Fig. 3 shows the radio source 3C 208 with the fringe amplitude as we measured it continuously. It also shows the great advantage of the continuously variable baseline compared with the observations of Moffet (1962). Al Moffet had to push his Owens Valley antenna along with a caterpillar tractor and sit it down to make each measurement. He had made only three measurements and obtained a quite incorrect solution. The continuously variable baseline was a wonderful way to fix the aliasing problems.

The interferometer spacing could be controlled remotely using a hydraulic motor on the 60-ft telescope to pull itself along its cable. One day Doug Cole was working up in the telescope when the wind started to blow. Normally the braking force of the hydraulic motor kept the telescope stationary, but on this occasion the wind blew so hard that the back-pressure threw the overload valve in the hydraulic motor so that, suddenly, there was no friction at all and Doug sailed the telescope down the track, over the stops and off the end of the rails! John normally got pretty upset when things like that happened but in this case, since he had designed the hydraulics, there was an element of culpability, and no-one got his infamous 'double whammy'. [See Brooks and Sinclair (1994) for their version as recipients of the 'double whammy'.]

The interferometer machinery and moving telescope really worked quite well. What didn't work well was the exposed trailing cable; it made the whole interferometer very phase-unstable and it was never useful for position measurements, for which John had originally intended it. In the meantime, by using special calibration procedures, the Parkes telescope was able to do that anyway (Shimmins *et al.* 1966), and the interferometer had a limited lifetime. However, it was also used for the famous Radhakrishnan HI absorption work (see Radhakrishnan 1994).

I will include only one scientific result from our interferometer observations. At the time, everybody *knew* that these double-lobed radio sources resulted from explosions in galaxies, which threw out two lobes that would expand, get weaker and fade away as the source aged. Our observations of size versus luminosity showed absolutely no such effect. One of John Bolton's traits was to question any idea, and he always insisted that there was no evidence that the lobes expanded, and no evidence that the big sources were older than the small ones. Many decades later, this has become the new conventional wisdom; the lobes are continually powered by jets from the nucleus and have not expanded to their current state from a simple initial explosion.

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