THE DEVELOPMENT OF MICHELSON AND INTENSITY LONG BASELINE INTERFEROMETRY

R. Hanbury Brown University of Sydney

In 1949 at Jodrell Bank, we were working on radio sources with the 218-foot paraboloid which Bernard Lovell described and which was a very purposes. It was just there and Lovell suggested to me that we might use it for cosmic static, so that was what was done, simply because the instrument was there. Victor Hughes started before me and I came to help him.

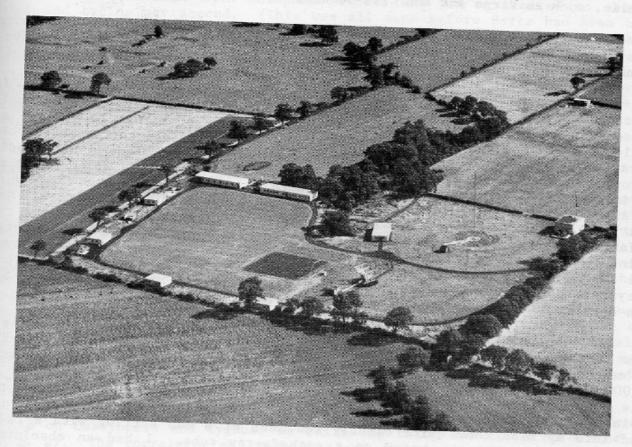


Fig. 1. Aerial view of Jodrell Bank in 1949. The 218-ft. paraboloid can be seen on the right.

At that time not much was known about the radio sources. I will deal first of all with the problem of finding out what the radio sources were. This was a great puzzle. What are these things you see in the sky? In 1949 three of them had been identified; I think largely due to work in Australia. They were the Crab Nebula, M87, and NGC 5128 in Centaurus. Nothing had been identified from Cygnus and nothing had been identified from Cassiopeia, the two strongest radio sources in the sky.

Bernard Mills had suggested to Minkowski that Cygnus might perhaps be identified with a faint galaxy which they found on the pictures of the sky. Minkowski said no, it was a perfectly ordinary galaxy; later on they found that he was wrong, it wasn't perfectly ordinary.

I might point out that discoveries have to be made not only of something that is interesting but also they have to be made at the right time. So, we were guided in what we did instrumentally by the current theory. The current theory was that the radio sources were radio stars; in the Cambridge theology, at least, these things were stars. If you read the literature, they were believed to have a space density greater than one per cubic parsec. We thought we were dealing with stars and so we had to design an instrument which could measure stars, although the only sources that had been identified were nebulae, such as Virgo and the Crab Nebula.

The problem was therefore to measure something which had the angular size of a star. And that was a problem that I faced. I am, by the way, a professional radar engineer.

The angular diameter of a star is extremely small and is measured in fractions of a second of arc. Michelson measured the angular size of Betelgeuse to be 0.047 seconds of arc. So that was our problem. To measure an angle of a few thousandths of a second of arc with a radio interferometer. In a radio interferometer you compare the amplitude and the phase of the radiation at two space points. If we work out how far apart these points have to be in order to measure a star at a wavelength of one meter in order to measure an angle of 0.047 seconds of arc, you need a baseline of 4000 kilometers. Now you realize that was difficult! So the technical problem was to preserve the relative phase and amplitude of the signal received from two widely separated points, and I could not see how to do it. There were no masers in those days. All we had were quartz crystals; there were plenty of quartz crystals, but they were not stable enough. It's very much easier now. I could go into all the numbers but I won't because you probably know them anyhow.

But the point was that we couldn't see how to do it. One night while I was thinking about this thing, I thought to myself, "Well, if the radiation from the sky is picked up at two points on the Earth, which could be, say, 4,000 kilometers apart, the similarity which Michelson would have looked at was the relation between the phase and amplitude of the wave at those two points. Is there anything else, any other parameter that we could look at?" And into my mind came quite clearly the idea of a man sitting with a radio receiver looking at the noise on a cathode-ray tube. I had an absolutely clear vision of the noise on two cathode-ray tubes, one at each end of the baseline, and I thought to myself, "Ah, those two noises are the same or ought to be!"

Next morning we worked this out; I worked it out as a matter of fact! The answer is of course that if the predominant noise is from the source, and the source is unresolved, then they are the same. In other words, we are dealing with a plane wave. You can see how that is so, if a radio station is transmitting a modulated wave or a radio program, then the modulation would look the same at two spaced points. The modulation at these two points will be in phase although the radio frequency phases bear no relation to each other. So I realized that this was the way in which we could compare the

signals received at two points which could be far apart; we could record them on tape recorders, driven by quartz crystals, and in this way we could have adequate stability to make an interferometer which would span the Atlantic. I wrote a proposal for an instrument which in principle would span the Atlantic. I thought of going west from England rather than east, which reflects my ideological bias. I never thought of an instrument which would go to Moscow! I just didn't! Don't read anything into that!

Anyhow, the answer was to build the thing; how do you do that? Well, what you do is you demodulate the signals at the two spaced antennas and then compare them. That's all there is to it! You have an antenna and you have a receiver and a detector; from this detector you take out the low frequency envelope of the wave in a low frequency band which you can record with tape recorders or transmit with telephone lines; say you're limited to about a thousand cycles per second. (This was all done before Hertz had been heard of.) You have a tape recorder here and another tape there and a little man on a horse with a forked stick who brings the two tapes together. That's all; absolutely straightforward technically and easy for a radio engineer who had worked on the sophisticated radar which we had by the end of the war. Most of the technical problems of radio astronomy were actually nothing to people who had gone through development of modern radar as I had. Technically, they weren't difficult; it was simply that you had to do the actual work!

Now, the snag with this type of interferometer is that you have a low signal-to-noise ratio because in the process, the signal-to-noise ratio gets squared. I was worried about the actual calculation of the signal noise ratio, but I couldn't work it out myself, so I got a friend of mine to introduce me to someone who could; someone who had been working on low noise development at MIT during the war at Radiation Lab. His name was Richard Quentin Twiss; I'm sorry he is not here. He worked out the signal-to-noise ratio for me in quantitative matter. In fact he worked out the whole theory on about 17 pages of paper during the night in purple ink, and he came the next day and said, "This idea is no good, it doesn't work!" And in doing so he was agreeing with 98% of the physicists I've met who also came to the same conclusion — that it doesn't work. But in fact he had put the integral of $\cos^2\theta$ equal to zero in the process of the mathematics, and it doesn't equal zero! I pointed that out to him in the course of the next day and he decided that it did work!

And so we calculated that it would give us a reasonable signal/noise ratio on Cassiopeia and Cygnus; we built the thing and tried it on the sun (Fig. 2). The correlator and receivers and all that stuff were built by my students, Roger Jennison and Das Gupta (Fig. 3). To measure the sun, we split the aerial into two halves and we measured the angular size of the sun; it didn't work at all, and this was discouraging. The reason why it didn't work was that the two halves of the array had been connected the wrong way in the middle. We found that out in a few days and then we were very relieved because it did work.

I can tell you about as many stories about Roger Jennison as you can tell about Reber, and I finally lost him as a colleague because he developed an interest in relativity and he wanted to do experiments on rotating frames of reference. He went away to do that and since then has been at Canterbury and

has written a lot of papers on relativity. The other gentleman went back to India.

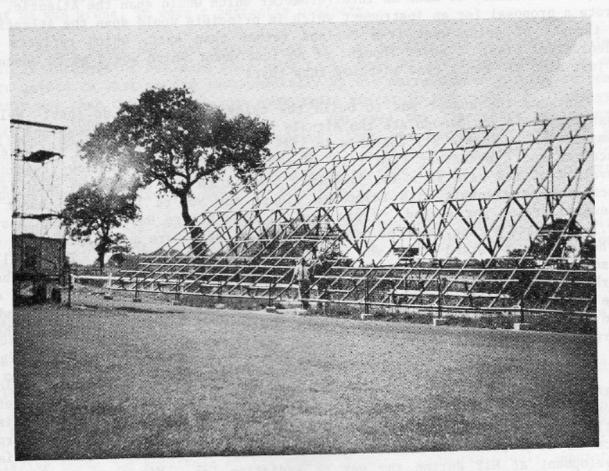


Fig. 2 The antenna of the first Radio Intensity Interferometer at Jodrell Bank (1950).

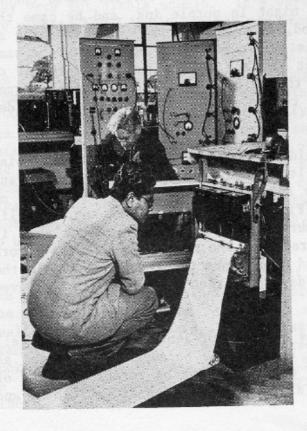


Fig. 3. R. C. Jennison and M. K. Das Gupta with the Radio Intensity Interferometer at Jodrell Bank (1950).

So what you do is receive the source on one antenna and then you receive the source on the other antenna and correlate the fluctuations in the intensities of the two signals in a bandwidth of one thousand Hertz (I'm starting to use Hertz) or a thousand cycles! Figure 4 shows the transit of Cas A through the aerial beam - the square signals are calibration signals. We built this thing and it worked on the sun, and so we built a mobile unit; we put one of those antennas on a track, we negotiated with a number of rural personages who ran farms in the neighborhood and persuaded them to allow our truck to go into their farmyards, and this way we went, farm by farm, across Cheshire.

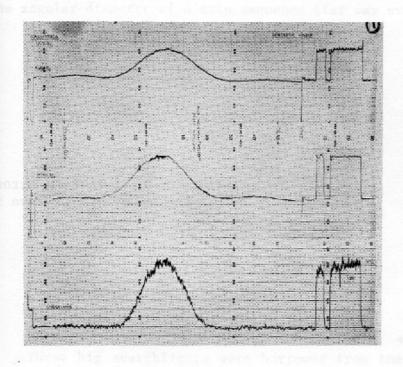


Fig. 4. A record of the transit of Cassiopeia A observed at Jodrell Bank (July 1952) with the first Radio Intensity Interferometer using a baseline of 900 ft. The upper and middle records give the total power received at each end of the baseline, the lower record shows the correlation.

I can't remember when all this happened, but I think it was 1950. We put the antenna on the truck finally in 1952; things happened very slowly, and we measured correlation as a function of the aerial spacing at a wavelength of about 1.8 m, or something like that. Figure 5 shows the points measured by Mills in Sydney and by Graham Smith in Cambridge, as well as our own. The interesting point is that our measurements at Jodrell Bank showed a second bump in the graph of correlation versus baseline, which showed that Cygnus was a double source.

Figure 6 is important for two reasons. First of all, it shows the double model of Cygnus which is derived from the measurements. Secondly, it was while trying to sort out the phase of the secondary maximum that Jennison thought of the idea of phase closure. This idea is now used in many instruments, such as MERLIN. That is where it started and it is a good idea, although there was little interest at the time; it was published and left in the literature. The interferometer showed that Cygnus had an angular size of something like 2 by ½ minute of arc, and Cassiopeia was circular with an angular size about $3\frac{1}{2}$ minutes of arc. In other words, they weren't stars. And therefore the whole development of this instrument was unnecessary.

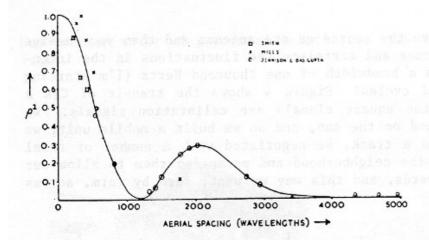
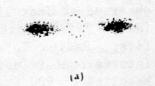


Fig. 5. The correlation from Cygnus A measured with the first Radio Intensity Interferometer in 1952. The measurements were made at 125 MHz. The ordinate shows the correlation (ρ^2) and the abscissa shows the baseline length in wavelengths.



 $\frac{\text{Fig. 6}}{\text{Cygnus}}$. The distribution of intensity across $\frac{\text{Cygnus}}{\text{Cygnus}}$ A derived from the measurements shown in Fig. 5.

Quite true! If we had never developed the intensity interferometer, we could have done the whole job in half the time and we would have done it first. We all three, Mills, Smith and us, published together in the same issue of Nature, but we could have done the thing a couple of years before this if we hadn't messed around with the intensity interferometer. To develop an ordinary "Michelson" radio interferometer is something I could have done with my eyes shut; to a professional radio engineer, there were no problems at all!

So, that was the story. But all the effort was not wasted. First of all, it made good measurements. Secondly, Jennison invented this irrelevant thing about phase closure. And the other thing was that while Richard Twiss and I were watching the thing working — Richard Twiss is a great watcher of people's work — we saw that on certain occasions the radio "stars" scintillated like mad. They scintillate at meter wavelengths — people don't see that sort of thing nowadays. And we noticed that when we integrated the correlation, we got exactly the same result as when they were not scintillating. In other words, the instrument worked perfectly even when the sources were

scintillating, and Richard Twiss and I said to each other, "OK, that's the answer to the problem of measuring optical stars through the atmosphere."

So this new intensity interferometer was a dead duck from the point of view of radio astronomy. Unnecessary development, but of course what we did with that thing was apply it to optics, right? And when you go into optics you have photons, and you have visitors with long hair talking about quantum theory and God knows what! It's a different world. In radio you have Maxwell and everything goes up and down like proper waves! I had to relearn it all; anyhow we worked it all out in the end. Figure 7 shows a picture of an optical interferometer which measured the angular diameter of Sirius at Jodrell Bank in 1956; this is the first time in the history of astronomy that the angular diameter of a main sequence star was ever measured.

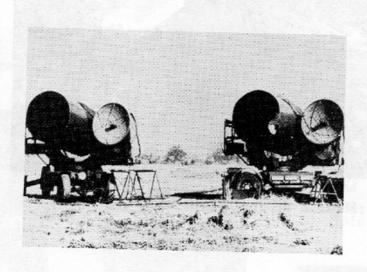


Fig. 7. Pilot model of an Optical Intensity Interferometer at Jodrell Bank in 1956. The interferometer was used to make the first measurement of the angular diameter of a main sequence star (Sirius). The phototubes were mounted at the focus of searchlight mirrors.

These big searchlights were borrowed from the army! I purloined two of the biggest searchlights you can get. I often think the next generation is going to have a problem if they are going to have to purloin an MX missile!

In Figures 8 and 9 we are back at Jodrell Bank in 1949. We have this serendipitous structure, this cat's cradle, this rat's nest, this 218-foot paraboloid with a central mast which was built to detect cosmic rays. I took the antenna over from Victor Hughes, converted it to 1.89 meters with a coaxial cable feed and, for stability, copied Ryle's receiver. I made it work nicely as a radio astronomy receiver.

Figure 10 shows a trace of a source going through the beam of the 218-foot dish, with Cygnus A on the right and Cygnus X on the left. It shows you, I think, that it used to work pretty well.

Figure 11 shows Cyril Hazard. I'm sorry about me, but that is Cyril Hazard, who worked with me and I expect a lot of you know him. I thought we ought to have a picture of him.