the Mark I, Hazard and I debated whether or not to continue with this work which we had found so interesting. However at that time we were busy mapping the radio emission from the North Polar Spur (Hanbury Brown et al. 1960), and we decided that any attempt to map irregularities in the extra-galactic emission must wait until we knew more about the irregularities which we were finding in the emission from our own Galaxy.

THE INVENTION AND EARLY DEVELOPMENT OF THE INTENSITY INTERFEROMETER

The First Radio Intensity Interferometer

If you read the "Report on the Progress of Astronomy" written by J.S. Hey in 1949 (Hey 1949), you will see how little was then known about the "radio stars". To be sure, Bolton, Stanley and Slee (1949) had tentatively identified three of these "stars" with the Crab Nebula and the galaxies M87 and NGC 5128, but the two most intense, in Cygnus and Cassiopeia, were a complete mystery; in their positions there was nothing obvious to be seen. Many people thought that these radio sources might originate in some unrecognized type of star which, like the Sun, emitted intense bursts of radio waves, but which was very much more powerful. This idea was supported by the curious fact that the sources fluctuated rapidly, a phenomenon which we now know to be due to the ionosphere. No wonder we were keen to solve this mystery; it is not often that one gets the chance to discover a new type of object in the sky!

One obvious way of finding out more about the sources was to measure their angular size. If they were nebulae or galaxies, then we expected to measure minutes of arc; if they were stars, then we expected fractions of a second of arc. To measure minutes of arc looked fairly easy; all we had to do was to build an interferometer with a baseline of a kilometre or so. But if, as seemed more likely, we had to measure fractions of a second of arc, then we should need very long baselines indeed. For example, we knew that to measure the largest known stellar diameter (Betelgeuse, 0.047") would require a baseline of about 4000 km at a wavelength of 1 m and, in 1949, we could see no practicable way in which this could be done with a conventional interferometer. For one thing, we failed to see how to preserve the relative phase of the signals received at two such widely separated places. It was while trying to find some way of comparing two remote signals that I thought of the basic idea of an intensity interferometer. Late one night in 1949 I was wondering whether, if I were to take "snapshots" of the noise received from a radio source on oscilloscopes at the outputs of two spaced receivers, I could then compare these snapshots. The answer to that question led me directly to the idea of an interferometer in which the *intensities* of two noise-like signals are compared instead of their amplitude and phase. To put this proposal on a sound mathematical basis I sought the help of Richard Twiss, who made a remarkably thorough analysis and confirmed that the proposal was sound (Hanbury Brown & Twiss 1954).

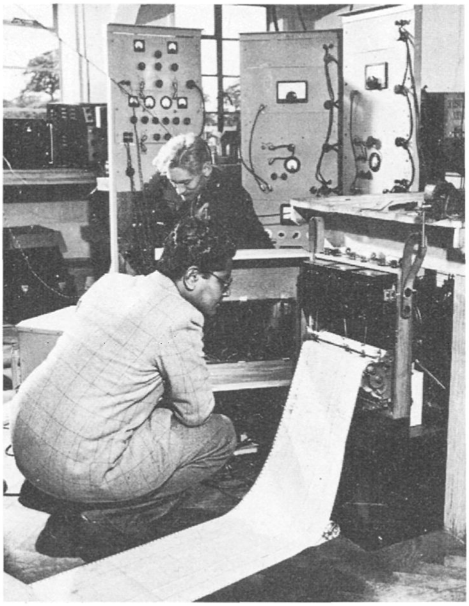
Our next step was to join with Roger Jennison in building a radio intensity interferometer to measure the angular sizes of the two intense sources in Cygnus and Cassiopeia. This instrument consisted of two *completely independent* receivers tuned to 125 MHz with a bandwidth of 200 kHz. The intermediate-frequency output of each receiver was rectified in a square-law detector (to simplify the theory!) and then passed through a low-frequency filter with a pass-band extending from 1 to 2.5 kHz. The two low-frequency signals were then brought together and their product or *correlation* measured in a linear multiplier. We had shown theoretically that this correlation, when suitably normalized, was proportional to the square of the fringe visibility in a conventional interferometer with the same baseline.

The reason for building this novel instrument was that it could be made to work with very long baselines. The two receivers, being completely independent, were simple to build, and the technical problem of correlating two low-frequency signals without upsetting their relative phase looked easy to solve. For short baselines we proposed to modulate one of them on to a radio-link, or to send it by land-line; for very long baselines, should they prove essential, we intended to record the signals on tape and then correlate the tapes. I had in mind from the beginning that we might need to work even across the Atlantic.

We built our first model of the interferometer in 1950 and tested it by measuring the angular size of the quiet Sun. It worked perfectly well and we decided to press on with the measurements of Cygnus and Cassiopeia. It took some time to build the final instrument and the measurements were done by Roger Jennison and M.K. Das Gupta in 1952. The antenna of the remote station was loaded on to a lorry and set up in a farmyard about a mile away. All went well, and in subsequent measurements the remote station worked its way slowly across Cheshire, farm by farm. But to our surprise both sources were resolved with baselines of only a few kilometres. Cassiopeia proved to be roughly circular in outline with an angular size of about 3.5 minutes of arc, while Cygnus was elongated and measured roughly 2 x 0.5 minutes of arc (Hanbury Brown et al. 1952). These radio stars were clearly not stars. Although our results were valuable, there was really no need to have developed the intensity interferometer; we could have done the same job with a conventional interferometer in half the time and with half the effort. We had built a steam-roller to crack a nut.

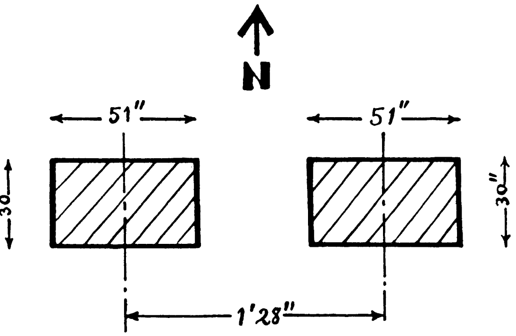
But, as it turned out later, building this novel instrument was not a waste of time. While Richard Twiss and I were watching it one day, we noticed that it worked perfectly well even when the ionosphere was making the signals fluctuate violently. Richard looked into the theory and it dawned on us that we had overlooked a most important feature of an intensity interferometer: it can easily be made to work through a turbulent medium. We realized that, if we could make an optical interferometer working on the same principle, we could overcome one of the main obstacles which had so far prevented the development of Michelson's stellar interferometer, namely the effects of turbulence in the atmosphere.

Figure 5 R.C. Jennison (at rear) and M.K. Das Gupta with the radio intensity interferometer (ca 1951).



Jennison and Das Gupta then completed a more detailed survey of the two radio sources, in the process discovering that the "elongation" of Cygnus A was (Fig. 6) actually two, distinct sources (Jennison & Das Gupta 1953; 1956). It was while trying to interpret the measurements on this first double source that Jennison developed his method of phase closure (Jennison 1958) which is so important today. Richard Twiss and I then turned our attention to the problems of making an optical interferometer.

Figure 6 The approximate distribution of intensity across the radio source in Cygnus found by Jennison and Das Gupta (1953).



The First Intensity Interferometer for Light Waves

It took us some time to work out a quantitative theory of an intensity interferometer for light waves; the trouble was that we had to start thinking about photons, which don't normally worry radio engineers. For example, at radio wavelengths the signal-to-noise ratio of our intensity interferometer was limited by "wave noise", and "photon noise" was negligible; but at optical wavelengths it would be limited by "photon noise" because the energy of the photon is so much greater. To our disappointment our theory showed that to work on bright stars we would need very large light collectors which looked to be absurdly expensive. It took us several months, however, to realize that all these collectors were required to do was to direct the starlight on to a photo-electric detector and that, compared with an optical telescope, they could be relatively crude and inexpensive.

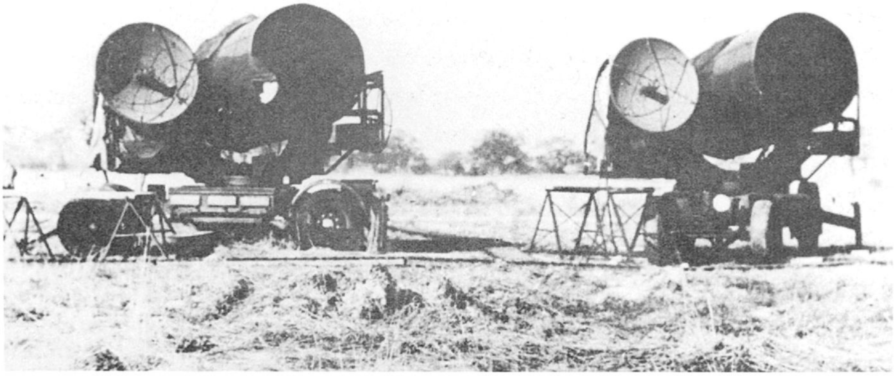
We then decided to check our theory by experiment. Early in 1955 we set up an optical system in a room at Jodrell Bank which had been built to house a spectro-heliograph. A beam of light from an intense "point source" was viewed by two photo-electric detectors whose separation could be varied. The outputs of these two detectors were taken to an electronic correlator with a bandwidth of 27 MHz. With this equipment we demonstrated that the fluctuations in the two spaced detectors were, in fact, correlated and that this correlation decreased with their separation as we had predicted theoretically.

In publishing the results of the experiment (Hanbury Brown & Twiss 1956a) we drew attention to the fact that if, as we had shown, the intensity fluctuations in a light wave arriving at two spaced receivers are correlated, then it followed that the time of arrival of the photons at two spaced receivers must also be correlated. In other words, the photons of mutually coherent light tend to arrive in pairs. Now in those days radio engineers didn't worry about photons, but physicists did, and to our surprise our work aroused vigorous opposition. Physicists wrote to us from all over the world to tell us that we didn't understand quantum theory; brandishing the sacred texts of quantum theory, they said that photons are emitted at random and cannot therefore arrive in pairs. At least two experiments were performed, in Hungary (Ádám et al. 1955) and in Canada (Brannen & Ferguson 1956), to show that we were wrong. We answered these objections as best we could (e.g. Hanbury Brown & Twiss 1956c) and in due course published a complete account of our work (Hanbury Brown & Twiss 1957a, b; 1958a, b). In settling the controversy we were helped by Edward Purcell who published a short theoretical paper giving welcome support to our conclusions (Purcell 1956). The most effective reply would have been to repeat our experiment with equipment which could count the coincidences between individual photons. Unfortunately we couldn't do this without a mercury isotope lamp which, at that time, was temporarily unobtainable in England. In fact it was not until 1957 that Richard Twiss managed to get hold of a suitable lamp and, with Alec Little, repeated our original experiment in Australia with equipment which could count individual photons (Twiss et al. 1957).

Encouraged by our laboratory experiment we decided in 1955 to build a pilot model of a stellar interferometer (Fig. 7) to measure the angular size of the brightest star in the sky, Sirius. To save time and money I persuaded the Army to lend us two of their largest (156 cm) searchlights; we removed the arc lamps, substituted photomultipliers, and mounted the searchlights on railway sleepers in a field at Jodrell Bank. The electronic correlator was installed in one of the laboratories of the control building of the 250 ft antenna (under construction at that time); in the absence of the control desk and computer, the equipment which directed the two searchlights was mounted in the control room so that it had a good view of the sky.

The weather in that part of the world is far from ideal for optical astronomy and, to make matters worse, Sirius reaches a maximum elevation of only 20° above the horizon. Indeed the experiment was the most tedious that I have ever undertaken. I had to keep the equipment running for 60 nights over a period of five months (November 1955 - March 1956) in order to get a total of 18 hours of measurements on Sirius. Nevertheless, the work was a success (Hanbury Brown & Twiss 1956b; 1958b); although the equipment was crude, it worked well enough. Since most of the time Sirius was twinkling violently, it certainly demonstrated that an intensity interferometer would work through a turbulent atmosphere. The measurements were made at four different baselines, the longest being about 9 m, and gave an angular diameter for Sirius of 0.0071" ± 0.00015".

Figure 7 Pilot model of stellar intensity interferometer used in 1955 to measure the angular diameter of Sirius.



This was, by the way, the first angular diameter of a main sequence star ever to be measured.

From our pilot model we learned a number of valuable practical lessons; for example, we appreciated the importance of screening the whole electronic installation, including the photomultipliers, against radio interference. We also realized that large exposed mirrors must be heated at night to avoid the formation of dew. But the most important result of this test was, of course, that it showed that a stellar intensity interferometer was a practicable proposition.

In 1958 we put forward a proposal that a full-scale stellar intensity interferometer capable of measuring all stars brighter than $+2^{\mathrm{m}}.5$ should be built. To cut a long story (Hanbury Brown 1974) short we built the instrument as a joint venture of the Universities of Manchester and Sydney; it arrived at the Narrabri Observatory of the School of Physics (University of Sydney) in January 1962. Originally I had intended to spend one or two years in Australia and then return to Jodrell Bank, leaving someone else to run the Observatory. But things didn't work out that way. So much money and work had gone into building the interferometer, and it was so very difficult to install and run such a complicated instrument in the bush, that there was really no choice. I had to stay and look after it.

RETROSPECT

I have always regretted leaving radio astronomy in 1962 — there was so much to be done and the new large instruments were coming into use. Nevertheless we only live once and, looking back, I am glad that I had the rare priviledge of working on a "young" science before it grew up into a "Big Science".

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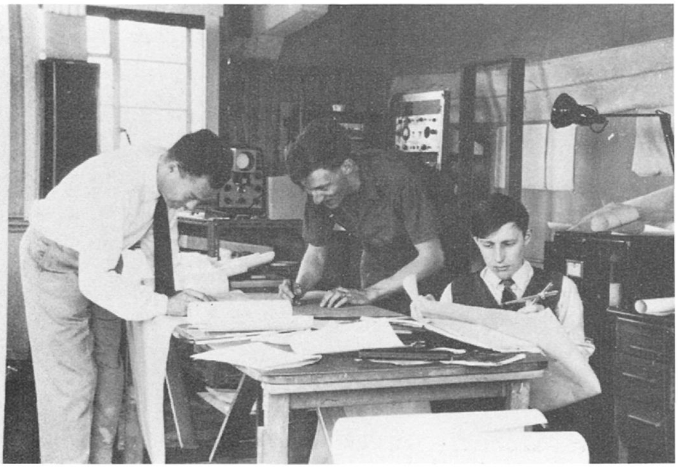

(l. to r.) Don Mathewson, Michael Large, and Glynn Haslam working on 408 MHz data taken in 1958 with the new 250 ft reflector at Jodrell Bank. It took roughly one week for one man to reduce one night's good observations. (courtesy Mathewson)