Journal & Proceedings of the Royal Society of New South Wales, vol. 151, part 2, 2018, pp. 181–208. ISSN 0035-9173/18/020181-28

Professor Ronald Bracewell

Interviewed by Ragbir Bhathal

This is an edited version of an interview between Ronald N. Bracewell and Ragbir Bhathal FRSN that took place on the 10th June, 2000, in Sydney (TRC-4596). Ron Bracewell AO was born in Sydney on 21 July 1921. Irene Kelly provided further information.

Bhathal: What was your impression of Joseph Pawsey[1] and Edward Bowen[2] at the CSIRO Radiophysics Lab in 1942, during the war?

Bracewell: I worked very closely with Pawsey and ultimately we wrote a book together. He was an admirable leader. His knowledge of physics was intimate. He didn't do high-powered theory but he understood electromagnetic phenomena in a very intimate way. He'd had experience with the television transmitter at EMI in England and he really understood how electrical things worked. He gave a course of lectures shortly after I arrived on transmission line theory. I still have the notes. I believe that set of lectures deeply influenced many of the people that went through Radiophysics, gave them a feeling for how you think about electrical things, as distinct from starting with Maxwell's equations and trying to deduce everything from that. There's a different way of doing experimental electricity and Pawsey conveyed that very well.

He developed loyalty. He was a group leader who managed a number of ongoing activities very well. Bowen arrived somewhat later. Bowen was given the job of taking the first powerful magnetron across the Atlantic to the United States. When he'd finished that, he came on to Sydney. At that time there was some uncertainty about the management of the place. D. F. Martin had been the first chief of Radiophysics. He had been pulled out and gone to work on ionospheric things and we had an interim chief, John Britten, who was not a physicist but was certainly a manager — on the other hand, not quite appropriate — and for a time Fred White[3] ran the place. Bowen turned out to be the long-term resolution of this period of uncertainty.

We all got to know him very well, a dynamic person, good background in physics, great experience with radar which he'd been involved in since 1935, so he really knew the subject that we were dealing with, and quite original. He just didn't take orders from people, he thought of things. The Parkes dish is an example of an initiative that he took. As a trivial example was this: When we heard about jet aircraft, jet engines, Bowen began to build in the workshop at Radiophysics indoors a big propeller about two metres long and hollow, a metal propeller. This was welded together and fabricated, not an easy thing to make. It had a big hub and it was suspended on a couple of saw horses and it could spin, you see. It was unusual in that the ends of this propeller took a sharp turn at right angles at the tips, very short, and there was a small hole. The idea

[1] Joseph Pawsley FRS (1908–1962). http://adb.anu.edu.au/biography/pawsey-joseph-lade-joe-11353

[2] Edward Bowen FRS (1911–1991). http://adb.anu.edu.au/biography/bowen-edward-george-17857

[3] Sir Fred White FRS (1905–1994). http://adb.anu.edu.au/biography/white-sir-frederick-william-fred-1035

was you would now feed petrol in through the axle of this propeller under pressure and it would squirt out through the small holes at the end in opposite directions arranged like a Z, you see, and you would set light to this petrol and it would drive the propeller.

You would now have a jet propulsion device without all this nonsense of turbines and expensive blades and so on and so on. If you can imagine this and a workshop full of fellows all watching this explosive device and who's going to light the wick, as it were.

It actually ran. I saw it running. It was terribly dangerous and it never came to anything but that was the sort of initiative he took, and he also was very early at work on the notion of building antennas which would adjust their own shapes by a servo-mechanism. He had a small test of that kind. He was a very ingenious fellow.

I applied for the CSIRO studentship and Bowen and Pawsey would have supported that very strongly. Bowen's support in fact was so strong — and this illustrates a little bit about his personality too — when I got to Cambridge, J.A. Ratcliffe[4] welcomed me in his office and as I came in he was closing his desk drawer. He had clearly just been reading the letter of recommendation which Bowen sent, just to refresh himself, and he said he was delighted to have me there. He said, "I'm particularly pleased to know that you've had some experience with the ionosphere". I said, "No, I've never done anything with the ionosphere". A frown crossed his face and he opened his desk drawer, pulled out this piece of paper and read it very carefully and he kept frowning and put it back in the desk drawer. Bowen had clearly perjured himself to ensure that I got in. I couldn't complain about that but it's a revealing item on personality.

Bhathal: What was your impression of Ratcliffe as a scientist and a person?

Bracewell: Admirable. He was in a tradition of experimental physics in the Cavendish lab. He was well acquainted with J. J. Thompson[5] and other significant figures in other sorts of physics, but they were all very closely grouped in one building. He thought very logically, he thought intuitively, physically. He thought in terms of actual things. At the same time, he'd had at high school a very good basic training in mathematics. He could do mathematics but he laughed when you did mathematics to prove something when he could see it physically at a glance and he would explain to you, "This is the real reason, you see. It's got nothing to do with Bessel functions. It's because … " and then he would make it all clear to you.

He'd had experience during the war making electronic devices, so he had a good background and he was an extraordinarily good teacher. He could clarify things. We learnt a lot of good things from him like how you behave when visiting lecturers come and so on. Someone would come to our weekly seminar and give a talk and Ratcliffe would ask very difficult questions, you see, almost rude, in fact, the complete opposite to what happens in the United States where, if the lecturer gets up and gets it completely wrong, no-one will say a word, where in England they'd jump on you. In Australia they don't do it in quite such a sophisticated way but they're pretty rude here too, which you have to be prepared for.

[4] J. A. Ratcliff FRS (1902–1987). https://en.wikipedia.org/wiki/J._A._Ratcliffe

[5] Sir J.J. Thompson FRS (1856–1940). https://en.wikipedia.org/wiki/J._J._Thomson

Bhathal: What was your PhD about? Can you tell us about it? Did you find anything significant?

Bracewell: Well, I was given a choice of jobs. Ratcliffe had a list of things he wanted to do. I may have arrived a week or two before some of the others, so he said, "You can work on metre wavelength stuff on the ionosphere or you can work on very long waves," which were wavelengths of about 15 kilometres. I had been working on the short-wavelength fringe for four years starting at 20 centimetres and 10 centimetres and 3 centimetres and then my friends were working on just a few millimetres wavelengths, and I thought, "Well, I've been at the cutting edge of the spectrum of that end. I'll go to the other end." That was my logic, totally baseless, so I got on to the 16-kilohertz propagation. There was a transmitter at Rugby with several towers almost a thousand feet high which had been used, or intended, for communications through the Empire. You could pick that transmitter up in Australia all the way round the world, you see, which is due to the fact that there's reflection from the ionosphere and not all that much loss. Just how this propagation took place depended on properties of the ionosphere that were not then known.

I picked up on that. There had been some previous work before the war started, then that terminated, so partly I was trying to explain earlier observations and partly get new ones. Ratcliffe would arrange for the Rugby transmitter, GBR, to transmit at certain times of the day and night. Tom Straker and I would go to out-of-the-way places where you could plug in the electrical things and take antennas with us and make measurements manually all through the night. What we found, which is significant, is that the D region actually consisted of two regions, D alpha and D beta, and that one of these began moving in the early morning at sunrise as seen from a height of 80 kilometres. Twenty minutes or so later another region would be affected when the sun was on the horizon. We could see that there are two regions, the lower of which is transparent to the ionising radiation understood to be ultraviolet, and it would go through that region twice and come out and start the first region going and then later the second region would go. This hadn't been at all clear before and I think this was one of the significant things done.

The other thing we did was to follow up the sudden ionospheric disturbances which cause short-wave fade-outs and we found that's due to the D region being pushed down by ultraviolet radiation accompanying solar flares, and when that ionisation gets pushed down, we see it as a reflecting layer, since we're using the long wavelengths, but to short waves all of a sudden you have ionisation in a region where previously there hadn't been any, so that causes the absorption. We did a lot of work on that too and that was probably the beginning of my interest in solar phenomena, because I'm supposed to be studying the ionosphere but as a matter of fact we've got a tool for studying solar phenomena also.

Bhathal: After returning from Cambridge, you continued at the CSIRO Radiophysics Lab from 1949 to 1954. What were the problems you were trying to solve at this time?

Bracewell: Well, I started working on a long-wave transmitter at Belconnen near Canberra, known as VHP, and began to make observations with Keith Bigg for two or three years. We travelled around New South Wales having a wonderful time and learning a lot about geography. That was the first sort of thing that I did. Radioastronomy was going on around me, as it had been in Cambridge. Martin Ryle's[6] group was just in the next lab to mine, so I was thoroughly familiar with all that, and here I'm in a radioastronomy environment again but I'm still doing very long-wave studies on the ionosphere.

Bhathal: Let's talk about your period between '49 and '54. How did you get interested in radioastronomy, because you were doing more ionospheric work?

Bracewell: Well, I was just in very close contact with them, you see. It was going on around me and I just watched what was going on. In Cambridge I was looking at it pretty closely and I would get letters from Sydney asking me what Ryle was doing. Ryle was a very secretive person.

Bhathal: So you were sort of a spy there?

Bracewell: It would seem that there are people who thought this. Well, Martin Ryle did. He had a fear of persecution and he had instructed his co-workers that when people came from Manchester to pay a visit that everything had to be removed from the desks and put in the drawers. He was really — there's a word for it — paranoid. Ruby Payne-Scott[7] wrote to me and said, "We are getting these bursts from the sun of very short duration, just a few seconds. Ask Ryle if he's getting them." They were just short blips and could be interference. So I asked Martin, "Are you getting these things?". He said, "Oh, yes, we see them all the time but they're due to aeroplanes going over. The difference between the Sydney people, those colonials, and us is that we have a loudspeaker attached to our receiver so we can hear what's happening. Whenever we see one of those, we can hear a plane going over."

I wrote back to Ruby and she said, "Yes, I've heard that but we're convinced they're coming from the sun. Could you get me some records?" I went and asked Martin if I could trace some records. All the records were four-inch wide paper tape and there's reams and reams of this stuff. I asked him if I could make some tracings. He didn't have any objection to that, so I spent half a day there. Everyone could see what I was doing. I sent these to Ruby and she said, "That's exactly what we get and that's due to the sun." When I got back to Sydney Ruby produced a letter from Martin Ryle saying, "If I'd known that Ron Bracewell was spying on us … "

Then in Sydney I was also very close … you see, for quite a while I was sharing a room with Chris Christiansen[8] and Harry Minnett[9]. Christiansen was involved in up-to-date observations. Harry Minnett was beginning to work on plans for the 210-foot dish, but he had done radioastronomical observations on the moon that I was thoroughly familiar with and had written it up in our textbook. Christiansen was studying the sun with his 32-element array at Potts Hill and the observations he was getting were being analysed in the next room, among other

[6] Sir Martin Ryle FRS (1918–1984). https://en.wikipedia.org/wiki/Martin_Ryle

[7] Ruby Payne-Scott (1912–1981). http://adb.anu.edu.au/biography/payne-scott-ruby-violet-15036 See also: Halleck, Rebecca (2018), Overlooked No More: Ruby Payne-Scott, Who Explored Space With Radio Waves, *New York Times*, 29 August. https://www.nytimes.com/2018/08/29/obituaries/ruby-payne-scott-overlooked.html

[8] Chris Christiansen (1913–2007). https://csiropedia.csiro.au/christiansen-wilbur-norman/

[9] Harry Minnett (1917–2003). https://csiropedia.csiro.au/minnett-harry-clive/

people by Govind Swarup[10], who came and spent several years with me ultimately at Stanford. I knew all this radioastronomy was going on in my immediate vicinity. Then Pawsey had asked me to write the textbook with him, so I became pretty familiar with all branches of radioastronomy, whether it was in the next room or not.

It was a very interesting experience writing that book. Pawsey had listed the chapters, then he would write a draft. His way of writing was to write very fast, very large writing, and get it typed up and then he would scribble all over it and get it typed up again and go through an infinite number of revisions; this is great if you have the power over typists, and we had a room full of typists in those days. My method of writing was to think very carefully so that I didn't have to rewrite the same sentence twice. If Pawsey wanted to make a correction and say this is black or white and he changed his mind, he'd cross that out and he'd write down, "This is white". My method would be to cross out the "black" and the "or" and minimise the amount of ink wasted.

This is very interesting. It was like a sort of feedback system. He would write it and then I'd rewrite it and then it would get typed and then he'd scribble all over it and he would leave great blanks and he said, "This reason for this is … ", and then there'd be a great blank and I'd go and fill that in in the library. That was a good experience and that's how I got into radioastronomy. I've sometimes wondered, though I don't know for sure, whether this might have been resented by some of the working radioastronomers. On the other hand, they had all had the invitation previously and they felt, and quite rightly, I can understand, that making the next observations and beating the competition to the next discovery is much better than doing something historical.

But for me Pawsey actually manœuvred me out of ionospheric things. He judged that there was very little future there. The first thing he did was in 1952 to put me on the organising committee for the URSI[11] meeting in Sydney, the first scientific radio union to meet in Australia, and that kept me occupied for many months. The committee consisted of Madsen[12], Pawsey, Bowen, Piddington[13], another senior person whose name escapes me, and I was the so-called organising secretary. These fellows would all sit around and they'd figure out what was to be done next, then they'd look at me. The good side of this was I had infinite resources. I could go to other people in the lab and say, "I have been told that you are now going to do this." That took me out of action for at least six months. Pawsey had looked for co-authors and hadn't had any success, so he got me, you see, and I just had to study all these things up and write it, so I really just slipped into it.

There was a great paper by [Lindsay] McCready, Pawsey and Payne-Scott[14] in which they observed the sun. The problem was to find out where on the sun the very strong radiation was coming from. The

[10] Govind Swarup FRS (1929–). https://en.wikipedia.org/wiki/Govind_Swarup

[11] The International Union of Radio Science https://en.wikipedia.org/wiki/International_Union_of_Radio_Science

[12] Sir John Madsen (1879–1979). http://adb.anu.edu.au/biography/madsen-sir-john-percival-vaissing-vissing-7456

[13] Jack Piddington (1910–1997). https://csiropedia.csiro.au/piddington-jack-hobart/

[14] McCready, L.L., Pawsey, J.L., Payne-Scott, R. (1947). Solar radiation at radio frequencies and its relation to sunspots, *Proc. Royal Soc. A*, 190: 357–375.

sun was going up by orders of magnitude in power output at metre wavelengths. The question is: was the sun as a whole brightening or was this coming from the sunspots? Well, they solved that question by observation that radiation was not coming from the sunspots but it was coming from the vicinity and there's a clear correlation. The sunspots are really quite tiny, only minutes of arc across, whereas the source of the radiation was bigger and coming from a high-temperature region in the immediate vicinity and above the spots. They were able to track these sources day by day and show that they were rotating in exactly the same way that the sunspots were, so they really clinched that question.

Bhathal: In 1954 you were invited to give lectures on radioastronomy at the University of California. How did this come about and was this the beginning of your move to the United States?

Bracewell: Pawsey had met Otto Struve[15] at some meeting and, on returning from the meeting to Sydney, he had a letter from Struve asking if he would like to go and give a year's lectures at Berkeley. Pawsey couldn't possibly do that because he was directing a sizeable group, so he asked me if I would be interested in doing that. It was particularly appropriate because I'd just finished writing the book and I could easily speak on the whole of radioastronomy. I consulted my wife, Helen, and we had at that time a daughter nine months old and experts told us that nine months was a very good age for travelling with children because they weren't yet independently mobile so you could travel with them in planes and so on. I said, "It's only for a year," and she said, "Well, okay." I dragged her away from her family just for a year.

At that time I had no intention of moving to the United States. At the end of the nine months lecturing, which I found very congenial and several of the people in that class went on to make names for themselves in astronomy, I took the summer off and made visits to other astronomical places, in particular Ann Arbor, where I had friends connected with solar astronomy, the Department of Terrestrial Magnetism in Washington, where I had other friends and also met Merle Tuve[16], the director there, and also visited the U.S. Air Force Office of Scientific Research, who ultimately became my sponsors for many years.

While I was there, Struve said the University of California at Berkeley should get into radioastronomy. What was my advice? What should they do? Well, the background I came from meant thinking of something new, the whole record of radioastronomy with people thinking of a new instrument or a new target and following that up. It was not continuing earlier work. I thought about that and it seemed to me that the Christiansen array of 32 dishes and the Mills-cross idea of having linear arrays perpendicular could be combined. I wrote that down and gave that report to Struve and at the same time I sent a copy of it to Pawsey suggesting that it was something I might like to do when I got back to Sydney.

If you look at that correspondence now you can see I was far from thinking of remaining in the United States.

Pawsey's reply was that Christiansen was going to do it, so that left me without any-

[15] Otto Struve FRS (1897–1963).
https://en.wikipedia.org/wiki/Otto_Struve

[16] Merle Tuve (1901–1982).
https://en.wikipedia.org/wiki/Merle_Tuve

thing to do but I returned to Sydney early in 1955 and spent some time there. I discussed this with Christiansen and Pawsey, what to do next, and it was quite noticeable to me that I was not asked to join Christiansen's operation.

What in fact happened was that after three months I went back to Stanford where they offered me a teaching job and there was a reasonable prospect that I might get funding to do what I was going to do. I got funded, Chris had to move to the University of Sydney but he got funding, and we both built essentially the same array, mine working at 10 centimetres and his at 21. They were complementary in the sense that the things we did were observed at two wavelengths, and so you got spectral information. That's how I wound up in the United States.

Bhathal: Was it a very acrimonious time?

Bracewell: Christiansen was somewhat combative. He was a person who was very sensitive to injuries done to the working class. We had examples of that. For example, the rain physics people used to fly planes through dangerous clouds. The more lightning they had in them, the better. They were supposed to be measuring all this. They lost a plane at sea, so, all of a sudden, one of our colleagues doesn't come back, but they don't find any wreckage, see, so nobody knows for sure whether he's dead. You would think it would be only reasonable to wait for a week or two until some wreckage washed up somewhere. But his salary was cut off the same week that he crashed and his widow is left in a serious state, which you would have thought the management would have postponed an action like that until they knew for sure, until a court declares you're dead. If you disappear, you're not legally dead.

Christiansen was the person who took the initiative over that sort of thing. That sort of thing caught his attention, and he battled very hard over that to get some justice for the widow. That was typical of Chris. I forget the exact details of the argument he had with Bowen. It was irrelevant because Bowen needed to get rid of him because he couldn't fund him. Chris didn't want to leave, but then he told me that once he'd had this shouting match with Bowen he knew he had to leave; he didn't need to be told to go. He accepted the job as head of the EE department. He came and visited me. I helped him to fund a trip to the United States. He came and stayed for several months in our group and I just paid him salary out of my grant. I was relatively affluent then. He was a visiting expert and I just put him on the payroll.

I also helped Bernard Mills[17] who was in the same position of having to raise money. Mills got money from the National Science Foundation and I was able to support his application for cash there at the very same time that Bowen was trying to get money for the 210-foot dish, and it's not to his advantage to have the National Science Foundation giving money to Mills. I think you will find in some of the other historical accounts more detail of how Bowen tried to squeeze Mills out from funding but didn't succeed.

That's another example of Bowen's slippery personality. I always got on with him extremely well. When he was appointed to the Australian embassy as scientific liaison officer, he used to come to Stanford quite regularly, interview some students, take care of any Australian students, of whom there were only two or three. We always interacted in a perfectly friendly way, but he did have

[17] Bernard Mills FRS (1920–2011). http://www.eoas.info/biogs/P000648b.htm

these other aspects that one can criticise. It didn't apply to me. I was never involved in any acrimony.

Bhathal: In retrospect, do you think the Parkes dish was a good idea, then?

Bracewell: I think it's quite clearly proved itself. It wasn't Pawsey's notion of what to do next. He would rather think of ingenious new departures, and the idea of building a big battleship because a destroyer had worked wasn't his … and the management program was completely outside his way of dealing with things. He dealt individually and in detail with a sizeable group and could do the thinking and leading very well, but the idea of getting manufacturers and surveyors and going through land deals and so on, that wasn't his idea of doing science. But it was well within Bowen's range because he had seen the way radar had begun at Bawdsey Manor[18]. They had taken on a very big job with a lot of people and you certainly wouldn't micro-manage them. They all did their own thing but nevertheless there are big projects like that.

Bhathal: You have made several significant contributions to science and engineering. We want to look at some of these areas of research. Perhaps we should begin with your work on imaging theory in radioastronomy. Could you tell us about this work and the problems you were trying to solve?

Bracewell: That became my main preoccupation when I got into the radioastronomy. We had these well-known people, Mills, Christiansen, Wild[19], Bolton[20] and their fellow workers, very busy generating mathematical problems on the side. One of the problems that came up was if you were mapping the sky with a relatively large beam. I think J. S. Hey's[21] first survey of the sky had been done with a 17-degree beam. Well, that's not what we call high resolution. It was quite clear to Hey that the map that he was generating was not exactly the same as the true distribution. There's the measured distribution and there was what used to be called the true distribution. What one had in mind was that if you knew what was there and then scanned it with a 17-degree beam, it would be blurred and the blurred distribution would be what you measured.

The other way of looking at that is to say we don't know what is really there. All we know is what we observe, and we have to work back from that. Kevin Westfold[22], who was a mathematician and went back to being a university mathematician later on, produced a solution to the inversion problem. Given the observations, what was the true distribution? That solution came out in the form of a series and it occurred to me that when you were summing a series, when you were presented with a series, maybe it doesn't have a sum and, if it does have a sum, it's not guaranteed that it will be the true distribution. I gave that a lot of attention and I managed to solve the problem and I found that the series doesn't always converge but I found the condition for convergence which is often met and, if it does converge, then it never, if ever, converges to the true distribution.

[18] RAF Bawdsey.
https://en.wikipedia.org/wiki/Bawdsey_Manor

[19] Paul Wild FRS (1923–2008).
https://csiropedia.csiro.au/wild-john-paul/

[20] John Bolton FRS (1922–1993).
https://csiropedia.csiro.au/bolton-john-gatenby/

[21] J. S. Hey FRS (1909–2000).
https://en.wikipedia.org/wiki/James_Stanley_Hey

[22] Kevin Westfold (1921–2001)
https://csiropedia.csiro.au/westfold-kevin-charles/

This is a big jump. It's no good trying to get a solution if someone can show that actually you can't get it. That all worked out very well and we were able to show that this mapping problem was analogous to an electric circuit problem of filters, which at first seems to have no connection at all, especially as the mapping is two-dimensional and the electric signals are one-dimensional, but it proved to have a close analogy and we could use some signal theory which existed and then put in the presence of noise which had been previously left out. If you put in the noise, if you're not careful you're going to amplify it. That was a very sophisticated problem which I was very happy to … I got a lot of papers out of that because there are so many variants on it.

It has now been applied in many different subjects, and things like true distribution have disappeared. There was the spectral sensitivity function which turned out to be the two- dimensional analogue of the transfer function of a filter. In optics where similar problems arise, the word "transfer function" has now displaced whatever was the local usage in several subjects. "Transfer function" has become standard terminology binding all these subjects together and "impulse response" is another such term, even used in optics. Impulse of course is a mechanical thing but "impulse response" is understood in optics now. No-one thinks any more that these are all different problems but at that time we were just groping in the dark. So that was one thing in imaging.

Another one was Pawsey, McCready and Payne-Scott had reasoned out that there's a Fourier-transform relationship between interferometer observations and what is really there in the sky. With the interferometer, a typical record just looks like a sinusoidal wiggle, has a certain amplitude, has a certain period and it has a certain location on the chart. Pawsey was able to make a connection between such an interferogram and what the distribution of emission over a sunspot would be. He latched onto this Fourier-transform thing and showed that if you made a lot of interferograms with different spacings, a Fourier transformation would get you back to the thing you were really looking for.

That had a lot of loose ends about it. First of all, it wasn't two-dimensional and also one is applying a linear point of view to something that's really not linear. The sky situation is not linear, partly because the sky is curved and partly because of other reasons. I worked on that and managed to get the result in terms of a two-dimensional Fourier-transform relationship between the brightness distribution over the sky and the complex visibility distribution on the so-called (u, v)-plane. When I first did this it came out with u and v being used for the coordinates in spatial frequency and I'm happy to see that the (u, v)-plane is still current terminology today.

That was fun too because in optics there were similar things going on where we were measuring "visibility," a word which we got from Michelson[23], who had done the same at Mount Wilson. To him the visibility of interference fringes was something he saw in an eyepiece and he judged whether the visibility was 100 per cent, which meant that the minima went down to nothing, or whether it was 50 per cent and so on. We got that word from there and Michelson knew about the Fourier-transform relationship, which was arrived at quite independently by Pawsey.

[23] A.A. Michelson FFRS (1852–1931). https://en.wikipedia.org/wiki/Albert_A._Michelson

But in optics they had what they called the modulation transfer function. That was essentially the same as Michelson's visibility. It just measured the depth of modulation of the fringes with respect to the mean level if there were no fringes.

But to make this Fourier-transform relationship work, you needed also to know the spatial phase. This never applied to Michelson because when he saw interference fringes in his telescope, they were always drifting because of the atmosphere, and if the atmospheric turbulence increased, these things would drift faster and faster and then wipe themselves out. We needed phase. When I put the phase in, I got this more general Fourier-transform relationship and what was previously modulation-transfer function MTF in optics could be generalised to optical-transfer function, which was the same as the MTF multiplied by e^{i} times whatever the phase angle was. That work happened in optics at about the same time as it was happening in radioastronomy. Now it's all been unified and we've got a good general Fourier-transform relationship.

A criticism was made by Emil Wolf[24], a great optical wizard who was co-author of *Born and Wolf*[25], a great textbook still in existence, and growing each edition. He said, "No, you have something wrong there". It had previously been worked out by Zernike[26], and he had had to make an approximation in order to get the same result I got. I didn't have to make an approximation. Therefore, Wolf said, I must have made a mistake, but he couldn't say where. It's not that way at all. In optics the field of view was always so small that it was customary in discussing aberrations and whatnot to make some simple approximations which were absolutely correct for optical levels. We couldn't do that in radioastronomy. You had to work not in terms of angles but in terms of direction cosines. When you did that, it all came out exactly right.

The Fourier transform was done by hand at first. These days we have the fast Fourier transform. The group at Cambridge included H.M. Stanier. Stanier had made observations of the sun as a whole with a pair of antennas and he would come back the next day and change the spacing and he had several interferograms in effect. Then he superposed those and he avoided the problem of the phase of interference by arguing that since the sun is symmetrical, you don't have to measure the phase; you know it's zero. You know that all these cosine waves that he is observing must all have a maximum in the middle. You could say that was the same thing, it's part of the history.

Much the same thing was done by Christiansen in two dimensions where he had scans with his east-west array and he argued the same. He had someone in the room next to ours adding these cosine waves up by hand. I don't know how Stanier did it. I don't think he had more than half a dozen components, but Chris would have had 16 and they're being added up by hand. I watched this very carefully being laboriously done. It was very well known to everybody. I mean, when you look at the integral statement of the Fourier transformation, you can see. Here's the thing you're looking at and we're multiplying it by

[24] Emil Wolf (1922–2018).
https://en.wikipedia.org/wiki/Emil_Wolf

[25] Max Born & Emil Wolf, *Principles of Optics: Electromagnetic Theory of Propagation, Interference and Diffraction of Light*, 7th Ed., C.U.P., 1999. Nobel Laureate Max Born FRS (1882–1970) was Olivia Newton John's grandfather.

[26] Frits Zernike FFRS (1888–1966).
https://en.wikipedia.org/wiki/Frits_Zernike

a cosine function: you can do it one way and then you can see what nature is doing to you and you know you've got to do the opposite. The beautiful thing is that the opposite of the Fourier transform is the same as itself. You could understand this very clearly. Pawsey understood this. It was in the air.

Bhathal: From radio imaging you went on to do work in medical imaging. Can you tell us about this?

Bracewell: Yes. Well, see, about 1956, I think, I wrote the two-dimensional paper on reconstruction from fan beam scans. The idea was that you had a fan beam which would scan over the sun, as Christiansen was doing, and then you would get another scan at a later time at a different angle and then you would get scans at more angles. The question was: if you had all these scans, would you be able to get back to the original distribution?

That's a very tricky question. Christiansen was working with scans at a limited range of angles. You would get a scan at midday and then in the later afternoon the sun has turned a little bit in the sky, so that the scan you get is at an angle with what it was previously. For the medical imaging, we knew when the work by Hounsfield[27] was done at EMI, the same place that Pawsey came from, he was taking 180 scans at one-degree intervals. We then realised that if you're going to get a decent image, you have to have all the scans, but in the beginning you could only get a few.

But the theory of that was pretty tough and, since I had been working on Fourier things, this was one of the subjects Ratcliffe was well up on because the Cambridge crystallographers had worked this all out and he understood this Fourier stuff very well. I started looking at that problem and I published in 1956[28] a solution showing how you would get from the scans back to the original distribution. What's more, although the theory is done entirely in terms of Fourier transforms, the final algorithm doesn't use the transforms at all. It enables you to use the actual data and combine them together and you don't use transforms at all. This was really marvellous. You couldn't understand it without thinking in Fourier space but in the end you didn't use it.

In 1961, by which time I was at Stanford, we had been obtaining scans over the moon and thought this might be an opportunity to give it a try. We used that algorithm and it worked out quite well and then the next thing I know is that particular paper of 1961, which had been cited in the science citation index maybe once or twice and then disappeared from view, all of a sudden it becomes my most cited paper and it's been cited in journals of neurophysiology and Lord knows what. I don't know exactly how it came to be known, but all of a sudden all these medical people are citing it as though it was mother's milk, you see. You didn't really know it unless you gave this citation. I'm sure the great bulk of them couldn't understand the paper; it was pretty mathematical.

Somehow it leaked out and it might have been that all this time at EMI they knew about this paper in the *Astrophysical Journal*[29]. They were very close to Cambridge and they were obviously employing people who'd worked on radioastronomy at Cambridge. Some of

[27] Sir Godfrey Hounsfield FRS (1919–2004). https://en.wikipedia.org/wiki/Godfrey_Hounsfield

[28] Bracewell, R.N. (1956). Strip integration in radio astronomy. *Australian J. of Physics*, 9: 198–217.

[29] Bracewell, R. N. & Riddle, A. C. (1967). Inversion of fan-beam scans in radio astronomy. *Astrophysical Journal*, 150: 427–434.

these radioastronomers, who never emerged as radioastronomers but had been trained there, might have seen my paper. They'd had a few years in which to let it leak out. It's possible that Hounsfield had misled the commercial opposition by pointing out that in order to solve this inversion problem, you had to invert a matrix with 180 by 180 terms. Inverting matrices is pretty tough even now but 180 by 180, who would do that? But he had a way of doing it, he said, and that might have been complete eyewash. He might in fact have been doing it with the aid of my paper. I don't know. I've never met him and I'm sure he would tell me, but that's my private suspicion.

Bhathal: You constructed the microwave spectroheliograph. What was the motivation for this work and how useful has that been?

Bracewell: Well, that's essentially what we were talking about earlier. It was my first project at Stanford. It had various outputs. We made a weather map of the sun each day for 11 years. We also got Christmas Day. I mean, it was a seven-day-a-week operation. Those maps were distributed by tele-typewriter the same day that they were made and went all over the world and NASA made use of them for predicting whether or not astronauts were going to be fried by cosmic rays from the sun and avoid having any sort of activity outside, shut the windows, get inside and so on, and later gave an acknowledgment of this contribution to the lunar landing. Of course, we weren't trying to do that but it's rather nice to think… combining those observations with those of Christiansen at 20 centimetres compared with 10, we get an idea of the spectrum and of the distribution of temperature and density in the regions immediately in the chromosphere and slightly above.

Bhathal: The spectroheliograph study led you to make a study of Cygnus A. What was the significance of that?

Bracewell: Yes, we looked at Cygnus, but we looked at several point sources. That equipment was not designed for that purpose; it was for looking at the sun, which is a very strong source. We could only see a few sources, especially Cygnus and Centaurus and the moon. We did that to see what would happen. Of those, Centaurus was the one that proved to be far and away more interesting. Other people were able to study sources and map them with much bigger installations, so that was of no great significance.

Bhathal: This led to the discovery of magnetic fields, didn't it?

Bracewell: I'll tell you the story there. Centaurus A had been discovered by John Bolton and that would have been probably 1949, plus or minus a little bit, and he had named it. It's a very strong source and about five degrees across, a really big thing. Now, we had three minute of arc resolution, which is incredible resolution, so we did a scan across Centaurus and we found that the central source, which up till that point was thought to be a point source, in fact had two components very close together, about five minutes of arc apart, that we could separate that with our three-minute beam. We found they were not equal. We found that one of them was narrow and strong and the other one was broad and relatively weak, though they had a similar total flux density.

Meanwhile, Alec Little, working with the Mills cross in cooperation with Richard Twiss[30], who happened to be visiting, had

[30] Richard Twiss (1920–2005). https://en.wikipedia.org/wiki/Richard_Q._Twiss

had exactly the same idea. At a different wavelength they get a scan across Centaurus and they also see this central source and it's divided into two bits but, because of lower resolution, these two sources appear to them to be the same strength and the same width. Then Alec Little came to Stanford and worked in my group for a good year and while he was there he got a master of science degree at Stanford. I had known Alec since he was a boy, you see, at Radiophysics, and we always got on very well together. We looked at these things and saw immediately there's something interesting going on here; we need to know more about this central source.

The first thing you don't understand is, if you look at the photograph of Centaurus, it's a great big nebula, pretty nearly circular, with a dark irregular band of stuff running across its middle. Here we have found two point sources which are inside the visible optical thing. They're pretty close together, you see. We know the right ascensions of these two things and we can compare that exactly with the optical position and we know precisely where these two sources are but only in one coordinate, left to right. Top to bottom we don't know whether the one on the left is up in the north-east or whether it's down in the south-east. We've got two sources but we don't know in which direction the line joining them runs.

I then went to Sydney for a time. I was visiting the physics department and I went and saw Bowen and told him about this and he said, "Well, I'm going up to Parkes in a few days.

Why don't you come?". He was flying up in a small plane with a pilot and I found this very interesting. You go over the Blue Mountains and you head down towards Parkes and when you get there, there are sheep all over the runway. You do a U-turn and scatter the sheep and then you land in the hole between them, you see. I thought that was pretty cute.

I then arranged with Bowen to go back to Parkes and observe Centaurus with a new receiver that was installed operating at 10 centimetres. Brian Cooper[31], my old colleague, came up with me. I had been working on advisory committees for astronomical observatories in the United States, so I knew very well how, when the people at the U.S. National Radio Astronomy Observatory built new equipment and then outside visitors, who had every right to visit a national facility, came in and used the new equipment, it used to cause a little bit of concern because the people who did the real work that made the observations possible would sort of get pushed aside. It was very well understood that visitors had somebody with whom they were associated who would tell them what to do next and so on and would share in any publications. In my case that would be Brian Cooper.

We went up there and he was doing his own thing and I was up in the middle of the night making these studies. The first thing I found was that the beam width is now five minutes of arc wide and I'm trying to resolve two things which are five minutes of arc apart. It soon occurred to me that if you scan through that, which the big telescope was quite capable of doing in a simple raster scan, you would never see two blobs. The position of the blob would change with each scan but you would never resolve the two. I found out that it would be possible for me to scan diagonally by turning on one of the drive

[31] Brian Cooper (1917–1999). https://csiropedia.csiro.au/cooper-brian/

motors so that the antenna would be driving east to west and then at a certain moment I could turn on the declination motor also and with both motors going, the dish would run down in a diagonal direction.

Then we would turn off that second motor and the antenna would continue out to the west, then we would reverse that motor, have it come back in, switch on the second motor, go up another diagonal. In this way, two things which in fact were seven minutes of arc apart measured diagonally could be resolved. I get a long series of two or three nights of scans showing these two things and I can see they have different diameters and different intensities. But in addition to that, a means of rotating the feed horn had been installed and not as of then used on any serious problem. It was purely a technical thing out of the workshop at that point. They'd made it, they knew it would work but it hadn't been used by any observer.

Here I find I'm in the marvellous position of being the first person to turn the horn feed antenna. We repeat these observations. When I say "we," I'm up there in the dark in the tower and there's an engineer who turns motors on and off and makes sure nothing hits the ground and so on, and there's another technician who is keeping the receiver working.

That's Tom Cousins, whom I had known from years before. He had built the receiver and his duty was to make sure it worked when someone else was using it. Cooper is also there but he's sound asleep. I turn the feed horn and I discover that one of the two components is strongly polarised, much stronger than anyone had ever seen, 15 per cent. The other one was barely polarised, not detectable. This is pretty exciting.

I wrote that up in the visitors' book before I left and I'm happy to see no-one ever tore that out. In fact, two different people have sent me photographs of it from time to time and it's still there. It says exactly what I've told you: who was there, who did what and what the results were and how exciting it was. After I left, Easter weekend was approaching and Marcus Price, a young American, was there on his own and he knew exactly what we had done. I mean, everyone sat round at teatime and talked about it. Well, as soon as everybody went away for Easter, he took the feed horn out, replaced it with a 20-centimetre feed horn, connected it to the existing 20-centimetre receiver and repeated these observations. Now, at 20 centimetres, the beam width is about 10 minutes of arc, so he was unable to resolve the central component. All he saw was the unresolved source. Nevertheless, he scans through it, turns the feed horn and scans through it again, and finds there's a certain percentage of polarisation, quite strong, just as he expected, but the direction of polarisation is not what I got.

In a talk that he gave and is written up in the proceedings of a meeting that took place at Charlottesville not too long ago — ten years ago perhaps — he said, "I realised poor old Ron got the direction wrong". He's a humorous sort of fellow. He goes back down to Sydney really excited and the first thing he encounters is he's reprimanded for having used the telescope without permission. Here he has made this great discovery and they're telling him, "You're not allowed to do things like that. You're an underling and there's no-one there in authority. How dare you do such things," you see.

Brian Cooper was pulled in and Brian thought that this disagreement in the two position angles of polarisation might be due to the Faraday effect caused by magnetic fields in the region either in Centaurus or in the intervening space. In order to establish that beyond doubt, he made observations at at least three different frequencies and showed that the rotation went in proportion to the wavelengths squared, which is what it was supposed to do, and he found that and he wrote that paper up. That's *Cooper and Price*[32]. Meanwhile, I had written mine up and handed it in for typing to the Radiophysics lab. They had typed it up, just handed it to Bowen. Bowen rewrote the first page and I simply adhered to that. I might have changed the grammar a bit. His motivations in dealing with the outside world from the point of view of a director are not quite the same as mine and I can understand why he is making these minor changes. He refers to this, that and the other in a way that makes Radiophysics look a little better relative to work going on at other labs. I've still got this handwritten stuff and the final version incorporates his modifications.

It went off to *Nature* and in due course I see it appears in *Nature*[33] but to my surprise, first thing is I've acquired a third author. I've been diluted by an extra 50 per cent, you see. Tom Cousins appears on it, never previously mentioned. The other thing is that *Cooper and Price*, the work that was done second, comes out in print first — some skulduggery went on here. Brian assures me that there was no skulduggery but it was very odd. For years, this didn't bother me at all, not a moment, because I knew that it would be apparent from the dates on which the observations were made and the date of receipt of these manuscripts at *Nature* would make it clear to anybody what the sequence was. It was only quite recently, just three or four years ago, when looking at this material again I realised the dates of the observations and the dates of reception are not there. How that got done I really don't know. I mean, that is really sophisticated. The fine detail of what happened there is not known.

The next thing, what really got me interested, was Alec Little then sent me a clipping from the *Sydney Morning Herald* with a picture of Bowen, saying, "Great discovery made by local people, marvelous," and Bowen saying, "Yeah, this is the biggest discovery we've ever made in Australia, polarisation in Centaurus," and it raves on and on. Alec Little's handwritten note attached to it says, "They really think your work was really marvellous. Pity they didn't mention your name." That really got my attention. That is why I wrote up my internal report. Since two or three other reports like this appeared in print, I thought, "I'll write up my version." I might even have sent you one. I know I sent it to various people. That's the story of Centaurus.[34]

Bhathal: In 1971 you constructed a second radio telescope.

Bracewell: That was much bigger, five 20-metre dishes extended over a bigger baseline and with a very narrow beam width. We made observations of a variety of galactic sources and published those. For technical details, I think I'll just refer you to the lists of publications. From that we got various

[32] Cooper, B.F.C. & Price, R.M. (1962). Faraday rotation effects associated with the radio source Centaurus A, *Nature*, 195: 1084–1085.

[33] Bracewell, R.N., Cooper, B.F.C., & Cousins, T.E. (1962). Polarization in the central component of Centaurus A. *Nature*, 195: 1289–1290.

[34] Bracewell, R.N. (2002). The discovery of strong extragalactic polarization using the Parkes Radio Telescope. *J. of Astronomical History and Heritage*, 5: 107–114.

other returns. For instance, going back to the cross antenna, we had the problem of making wave guide runs equal to about a millimetre in 100 metres. That's one in ten to the five. That is what we consider to be geodetic survey accuracy. But you can't really do that very easily and we worked out electrical ways of making that sort of length measurement.

Then we trained a lot of people who then went into radioastronomy in other places and various items of technique, not to mention these image-forming problems, were solved. Much the same applies to the five-element array of 20-metre dishes. All sorts of things were ironed out that were dubious when we began. It was a hard thing. It was one of the first aperture synthesis arrays working by earth rotation. Christiansen was the very first person to demonstrate earth rotation synthesis, then Martin Ryle came along and invented the term "aperture synthesis" — didn't mention Christiansen — but there were still loose ends when we'd come along and we managed to get that working.

About 1970 there had been an explosion of university radio telescopes. There was a move afoot to bolster the national radioastronomy centres. There were two at that time, one in Charlottesville and one in Puerto Rico at Arecibo, and people were arguing in Washington that we couldn't afford to have new expansions of expensive equipment in many universities, much the same as has happened in particle physics, and that the national centres should be boosted. I got a telephone call in 1970 from my sponsor, the National Science Foundation, saying, "I don't know how to tell you this, Ron, but the National Science Foundation is not going to sponsor your radioastronomy research any more."

There are some funny aspects to this. I wasn't the only one who got a call that day. Harvard was shut down, Michigan, Ohio State and one of the Florida universities, and I think Berkeley too. In the case of John Kraus[35], who was at Ohio State, the conversation went like this: "I don't know how to tell you this, John, but we're not going to support you any more," and there's no answer. "John, are you there?" There's absolutely no answer. The fellow got quite alarmed. He telephoned someone that lived in the same building and said, "Go and see if John Kraus just dropped dead." It took them quite a few years to shut Kraus down and the same is true of me. I fought back. I knew it was illegal. The National Science Foundation is there for the purpose of reading your proposal and rejecting it if they see fit, not to reject it before you've submitted it.

I fought back and I hung on for another three or four years, but this is the sort of thing that would happen. We needed more sensitivity, we needed some special sort of receiver. These words were on the tip of my tongue for years. We needed parametric amplifiers, we needed five, one for each dish, in order to boost our sensitivity and we'd be able to see a lot more objects. I wrote a proposal and this is the sort of thing that would happen. You've got to understand this peer review system means that your competitors are your peers. They're the ones who know what you're doing and any money they give you is less for them, you see. Here's a typical thing. NSF told me that a reviewer said, "It's a very poor proposal. Even the wavelength of observation is not mentioned." I replied, "Please note that it's mentioned in the abstract, on page 1, page 3, page 5,

[35] John Kraus (1910–2004). https://en.wikipedia.org/wiki/John_D._Kraus

page 7 and page 9. Please take a note of this reviewer's name." By this time NSF is explaining why I didn't get funded, you see, it is too late — a done deed.

Another reviewer said, "The trouble with this radio telescope is it's not sensitive enough, why should we give you things to make it more sensitive?". You see, the logic tends to irritate you. Anyhow, the writing was on the wall and year by year I would lose another colleague and I began to notice that each time one of my colleagues left and went somewhere else, it wasn't as traumatic as it seemed. They all got good jobs mostly in radioastronomy elsewhere, but not all — some in industry. Each time one went away, the quality of life went up because we wouldn't be sitting around figuring out what would go in on our next proposal. Finally when my secretary went, the quality of life really hit the ceiling. I didn't have to rush in to the office and create work. Radioastronomy built up to a big maximum and then slowly faded away and went to the national centres.

I have another strange thing to tell you. In these days when I was trying to raise support from my friends to work on NSF, I went to Charlottesville and was talking to the director there, Mort Roberts[36], who had been a student of mine at Berkeley, and I'm telling him what I'm telling you, you see, and he's very sympathetic. Then, as I get up to leave, he says, "By the way, do you have any students that we could take?" He'd completely missed the point. I'm not going to be able to produce any more students. I've produced my last student, and it really hasn't struck him that closing down universities in favour of national centres, of which he is it, is going to cut off their source of supply and they have to become universities themselves. It was a turning point in life but we survived.

Bhathal: You received a patent for your work on the Hartley transform. Can you tell us about this and its usefulness in scientific work and how different is this from other methods?

Bracewell: The Hartley[37] transform is like the Fourier transform. It is fully equivalent. If Smith gives us a function, you take the Fourier transform, I take the Hartley transform, we both have identical information. Anything you can do I can do and vice versa. The difference comes about in the calculations that you will have to do. You will be doing complex arithmetic and I will be doing real arithmetic. When you come to put your stuff into the computer, you'll find that your computer doesn't multiply complex numbers, it multiplies real numbers. That will be very irritating. In fact, when you've finished doing all your complex multiplication, you'll find that you've done twice as much as you had to do. The reason for that is that the Fourier transform of a map has symmetry. The value at any point in the (u, v)-plane is the same as the complex conjugate of the value at the opposite point, or the point that's 180 degrees away.

You've done twice as much work as you have to do and that's because complex numbers are a creation of the brain. They are not in nature and nor did Fourier think they were either. He multiplied by sines and cosines, not by this e^i theta stuff that is very convenient for human beings who understand complex arithmetic. But it's not essential and the computers don't like it. The

[36] Mort Roberts (1945–2010).
https://www.nrao.edu/archives/Roberts/roberts.shtml

[37] Ralph Hartley (1888–1970).
https://en.wikipedia.org/wiki/Ralph_Hartley

effect of the Hartley transform was to permit you to do the FFT in half the time. When I sent in my paper on that, the reviewers really couldn't understand how that could be. They knew they'd been perfecting the FFT for some years, and only discovered a little later that Gauss had done it back in the early 1800s, and they could not understand how you could cut the time. They'd been whittling it down 5 per cent, 10 per cent, year by year and they'd really got to the bottom. They were doing prime factor algorithms, they figured out all sorts of quaint things, but they couldn't find out where the paper was wrong.

I got three lengthy reviews criticising minor things here and there and I dealt with all of those. Then I got three more lengthy reviews, in the same vein. I knew my analysis was correct. They couldn't find out where the paper was wrong. They knew it was wrong. Then finally I got a letter accepting it and saying it would come out in April. Two years go by. This is my record on delays in publication. Before the month of April comes, I get three more negative reviews from the IEEE. I don't know how this … I thought I'd better not ask, I'll wait until April comes. When April comes, there it is in print[38]. Where these three extra reviews come from I don't know, but a lot of people were inconvenienced by it. Then when the university Office of Technology Licensing said, "Why don't we take a patent out on it?" that went through and also made a milestone. It was the first patent with an embedded copyright. What this means I don't want to explain to you now, but it had never been done before.

A howl went up from people. "How can you possibly be taking financial benefit from something given by God to everybody? It's not your property. You didn't invent it," and things like that. It was really funny for a while. It didn't do me any harm. Although the cash take was to be divided three ways between the dean of engineering, the chair of electrical engineering and me, they never told me that they don't do this three-way division until the Office of Technology Licensing had paid off their out-of-pocket expenses, which were the salary of the person principally responsible for at least six months, maybe a year, and the fees of the attorney. Nothing came at all for several years and then I got maybe a thousand dollars out of it.

The fact that it works twice as fast has become irrelevant because each year the speed of computing goes up, practically doubles, and I'm pretty sure now that hardly anybody is using the fast Fourier transform. The slow Fourier transform is just as good. It takes a tenth of a microsecond and it used to take 10 microseconds. What's the difference, you see?

Who is using the Hartley transform now I don't know. Hundreds of papers appeared, literally hundreds, and we know for all the people that write papers there are other people who are using it, but where and who they are I have no idea. All I can say is that I've caused Mr Hartley's name to be transmitted in perpetuity. Oxford University Press commissioned a monograph *The Hartley Transform*[39].

Bhathal: With the advance of the space age, you were involved in some experiments on the Sputnik. Can you tell us about this work?

[38] Bracewell, R.N. (1984). The fast Hartley transform. *Proc. of the Institute of Electronic and Electrical Engineers*, 72: 1010–1018.

[39] Bracewell, R.N. (1986). *The Hartley Transform*. New York, O.U.P.

Bracewell: Well, we were having dinner one night with Villard, Peterson and Eshelman, colleagues in electrical engineering, and the telephone rang and said, "The Russians have just sent up a sputnik and it's arriving over California in half an hour," or something. "What do you know about it?" I said, "Well, I'll tell you when we've gone and had a look." We're all having dinner and the men all jump up and zip off to a field shack and get antennas ready. They told us the frequency, about two megahertz, and we hear it go over. No question. It's going "beep, beep, beep." Then the question was: was this a Russian fraud? Was it really up there or was it only apparently up there? Well, after it's been around two or three times, it's quite clear that it's up there all right.

I got interested in that and with Owen Garriott[40], who later became an astronaut in his own right and spent a lot of time up in space, we took recordings of the satellite transmission and found it was modulated in a funny way and we could attribute part of this to the ionosphere and Faraday rotation and partly due to the rotation of the satellite itself. It's rotating. It's doing a very funny sort of rotation. We got the theory of that all worked out. I don't know how you heard about this, but Explorer One was launched after the east coast scientists at the Naval Research Laboratory had failed two or three times to launch their grapefruit-sized satellite. There was no shame in that because they were competing with Germans who'd been doing it for twenty years.

They got Werner von Braun and said, "Listen, what are you going to do about this?". He had very cunningly put a Redstone in a garage and was waiting for this day and when the day came, he wheeled it out, lit the wick and there's a satellite in orbit. That's experience, you see. At the Jet Propulsion Laboratory they were trying to launch much smaller things and they built the Explorer One, which was about two metres long and about 15 centimetres diameter, and on the launching pad they set it into rotation. They had a rotating turntable, so it is actually spinning before launch. Their idea was that it would behave like a rifle bullet. It would keep pointing in the same direction. That was important because it had two dipoles at right angles and the thing is spinning but it's circularly polarised and so the spin doesn't matter.

To their surprise, when it comes back — after one trip around the earth it comes back over the Jet Propulsion Lab — it's like a ski thrown across the ice. It's slithering across. The next time it comes back it's turning head over heel and doing dreadful things. Since we'd been thinking about rotation, it didn't take long to figure out what's going on there. If you have an object that's long and narrow like that and spinning, as it loses energy, since it can't lose angular momentum, the energy has to be taken from some mode of vibration or rotation that does not contribute to the angular momentum. If you have something that's pencil shaped spinning around its long axis, it will wind up rotating about an axis perpendicular to the axis of the pencil. But if you have a disc-shaped satellite, it will rock as it spins but the rock will die out, leaving the pure spin, so it will stabilise itself. That turned out to be of some excitement for the time being. It was rather enjoyable to do.

Bhathal: In the late 1970s you invented the spinning nulling two-element interferometer, which I understand is to be used by NASA in its program to search for earth-

[40] Owen Gariott (1930–).
https://en.wikipedia.org/wiki/Owen_K._Garriott

like planets. How does this instrument work and what was the motivation that made you invent this?

Bracewell: Barney Oliver[41] was interested in the search for extraterrestrial intelligence and got elected head of the IEEE, the world's largest professional organisation. He spent his year as president travelling around the United States giving lectures on how to design a huge antenna array that would pick up messages from extraterrestrials. Then he got support from NASA to hold a summer school which attracted science teachers from all over the United States to work on some project. He had them designing this huge array, hundreds or thousands of dishes as big as the biggest dishes then available.

I was asked to spend some time working with this group and I was overwhelmed by the size of the project and I thought there might be some other way of doing it. I proposed that an interferometer … well, what I'm trying to do is to detect planets, not the extraterrestrial intelligence people themselves, but to detect those stars which might have planets. The difficulty here, the technical difficulty, is to blot out the light from the sun and see the very much fainter light from the planet. Barney Oliver himself had found out that you could design a telescope mirror not much more than a metre in diameter which would be so free of side lobes that you could see planetary light even in the presence of the strong light from the star.

But I thought if we could use an interferometer, then we could have the components in anti-phase suppress the light from the star and then arrange for the maximum sensitivity to be in the general location where you imagine planets might be. At that time the Space Shuttle was under development and we knew exactly what the size of the instrument bay would be, so I designed this so it would fit in the instrument bay of the Space Shuttle. That was really amusing because I already knew from experience of friends that if you wrote a proposal to NASA, five years would go by before it could be all engineered and designed and then another five before it could fly, and it never occurred to me that this was anything more than a talking point, a sort of joke even.

We worked out a lot of the details of that. If you have an interferometer with null reception along a line, you can put that line straight through the star. Mind you, that's going to take very precise pointing, but that had already been reached, or was nearly good enough. It certainly can be done. If you then rotated the interferometer and let it spin, the planet would go round and round in the pattern of the interferometer and cross through the null line twice per revolution. If there was a planet there, you would see modulation at a frequency which you yourself had injected into the instrument. That was the idea.

I think I got something like $10,000 or $15,000 from NASA to pay salary to a visiting scientist, R. MacPhie, who came to work with me at Stanford at that time. After we had written our report, NASA gave our report to Lockheed and gave them $100,000 to report on our report. Their report on our report was based entirely on what we told them face to face. This was a sociological comment. It turns out that the nulling interferometer has now become a reality. The multi-mirror telescope on Mount Graham was disassembled about a year ago and, when the last two remaining mirrors were in place, was used as an interferometer at infra-red

[41] Barney Oliver (1916–1995). https://en.wikipedia.org/wiki/Bernard_M._Oliver

wavelengths, just as planned, to produce a null on Betelgeuse and was successful in suppressing the starlight and, lo and behold, they were able to see the infra-red glow from a cloud of dust similar to that which creates the zodiacal light on earth.

It not only proved the principle of the instrument but also made a minor astronomical discovery, and that's from the ground. The idea is that that will ultimately be flown in space, not only in space but out near Jupiter. I mean, it's a really large undertaking. It's in the hands of Roger Angel[42] and Nick Woolf at Tucson and is one of the two serious contenders for NASA's next major investment.

Bhathal: What are the dimensions?

Bracewell: As I saw it, it was going to be about 10 metres long. Angel and Woolf, since they don't feel they're restricted to the bay of the space shuttle, have doubled the length and added two components, so it's now a four-element which has certain technical advantages, and they've also moved to a wide band of infra-red which will enable them to see water vapour, oxygen and CO_2. Not only will they pick up the radiation from any planet they happen to detect but they will also get an indication of the atmospheric components. If they see oxygen, for example, that would be a strong indication of life because oxygen in the earth's atmosphere is entirely due to biological activity. Before there was any life on earth there was no oxygen in the atmosphere.

Bhathal: You were also interested in the SETI program[43], the search for extraterrestrial intelligence programs. You were a little critical of the Drake equation. What is wrong with the Drake equation from your perspective?

Bracewell: The Drake equation[44] seems to get quoted by all writers in the SETI business but it can't possibly be right. First, I'll tell you what it does and then I'll give you a homely example. The idea is we want to know the number of intelligent societies in the universe, N. We say that N is equal to the rate of formation of stars multiplied by the probability that the star will have a planet at about the right distance from the star where water will be in liquid form, and we multiply that by the probability that life will start and then the probability that it will reach technological level, and you throw in a few more factors such as the longevity of a technological civilisation and then you get an answer.

Somehow, they always seem to arrange this so that it's a pretty good number, looks very encouraging. Here's my criticism. The fact of the matter is we don't know the values of any of these parameters. Suppose I were to say to you, "Let's estimate the number of cats in Sydney. N is the number of cats in Sydney. It's equal to the rate of formation of cats (the birth rate) multiplied by the longevity of a cat multiplied by the probability that the cat will be fed so it doesn't die at birth and multiplied by other probability factors connected with the funding of the RSPCA and things like that". If you're going to have to guess the rate of formation of cats, you might as well just guess the number of cats. What's the difference? I mean, how many kittens are born per day? You'd better tell me how many mothers there are before you start guessing that.

[42] Roger Angel FRS (1941–). https://en.wikipedia.org/wiki/Roger_Angel

[43] https://www.seti.org

[44] Frank Drake's equation, 1961. https://en.wikipedia.org/wiki/Drake_equation

Furthermore, there's more than one way of getting cats, you see. There are cats that grow up in loving households and there are other cats that are fed by secretaries and grow up under the buildings and then there are feral cats, you see. It's not all produced by one procedure, there are several. My fundamental criticism of the Drake equation is it does not contain any plus signs, so it cannot be right.

Bhathal: But cats are living systems, whereas the Drake equation refers to physical things. Is there a problem there?

Bracewell: Well, it refers to life, you see. It's the number of technological civilisations. There's a very good chance that we're the only one in the whole universe. Of course, you might have a personal bias against that. If you're conducting observations intended to locate these people and they're not there at all, you wouldn't be very receptive to the notion that we're the only ones.

Bhathal: You wrote a book called *The Galactic Club*[45]. Can you tell us what you were trying to tell your readers?

Bracewell: I was pursuing the consequences of different rational lines of reasoning. You could say that since our sun has planets, other stars would have planets. Until recently that was really not known for sure, and in fact it's not known that other stars have planets like the terrestrial planets. All they're observing now are planets like Jupiter which we don't think are very good habitats for life. But let's assume that other stars do have planets like ours. Our planet developed life, so maybe they did too. In that case some of them would have developed life long ago and some will just be in the age of monkeys, some will be in the age of trees and fungi and others have no life at all.

Of these various other supposed living societies, some would be more advanced than we are, and in fact not just by a hundred years. A little more than a hundred years ago, we didn't have radio, so it would be pointless to try to communicate by radio in those days.

But there'll be societies somewhere else which are thousands of years ahead of us. When we see the fantastic burst in information sciences lately, you can only imagine that they will be … we cannot imagine the abilities that they will then have. The notion that we should take the initiative in making this contact doesn't seem very plausible. It seems that they would have very much more power and they will be taking the initiative. In fact, when we first make contact with an external civilisation, why should that be the first time it's happened? It would have been done before, perhaps many times. The first small group to get into contact would then begin to organise making contact with other groups.

That's the galactic club, you see. It's the supposed group of intelligent civilisations that are already in contact. Perhaps they are trying to contact us. They have had experience. When you say, "Well, where are they? There's no sign of them," you could say perhaps they checked on us a couple of hundred years ago and they found no signs of radio waves, which would be a very easy thing to discover because you would simply listen with a sensitive antenna and you would hear none, so you would know we had none. They might say, "Okay, cross them off the list and put them back on in another couple of hundred years," or another thousand years.

[45] Bracewell, R.N. (1974). *The Galactic Club: Intelligent Life in Outer Space*. Stanford, Stanford Alumni Association.

I mean, what's the hurry? We've had life on this earth for 3,000 million years, maybe 4,000 million, so what's the hurry? If they wait for another hundred years, we'll be better prospects for contact.

That's the idea of the Galactic Club. But now go back and suppose that our original suppositions are not right. We're assuming that we would be average. There'll be some ahead of us, some behind us. That's an assumption. Let's test it out on some particular case. Here we have life on earth. We have everything ranging from ants to elephants, so therefore we should be average. We're not. We're at the top of the tree. We are unique on this earth. This assumption of mediocrity, as it's known, fails in the one case we can test it. It's just as conceivable that in the whole universe we are the top of the tree and we are in fact the instrument by which the whole of the universe is going to be populated with intelligence. Our destiny might be unique and we are the beginnings of the galactic club if there ever is going to be one. I was trying to balance these two alternative views and the one that we're unique is not popular among the SETI world, although I must say they're all friendly. In fact, we're a small club, you might say.

Bhathal: You mentioned in your book how long it would take to populate a galaxy. How long will it take to populate a galaxy?

Bracewell: Well, not very long by cosmic standards. It's about 100,000 light years across the galaxy. If you travelled at the speed of light, it would take you 100,000 years. If you travelled at a tenth of the speed of light, that would be a million years. If you travelled at 0.1 per cent of the speed of light, that's 10 million years. Now, 10 million years is microscopic compared with the time that life has existed on earth. We'd be there. To think about that another way, the earth is populated with human beings. Only Antarctica had no humans and a few Pacific islands when the explorers began covering the earth a few centuries ago.

Well, how did all these people get there? You could imagine that life could originate in Africa. It could also originate in Siberia. The bears could have come out of the Arctic forest, adopted an upright stance and begun hunting mammoths and developed language and so on. In South America the three-toed sloths might have descended from the trees and adopted an upright stance, developed language, grew very long legs for chasing after the game animals. You'll notice that, although they had the opportunity to do this, what actually happened was that the superior form that originated in Africa, so we believe, migrated on foot to these far corners of the earth. The people that Charles Darwin found in Patagonia had walked 10,000 miles from Africa not as individuals, but their grandparents and so on had marched. If you say they wouldn't travel much more than a mile a year, why would they? They settled in a certain place until they've eaten everything that's eatable and then some of the boys and girls go over the mountain and start another colony somewhere. To cover 10,000 miles from Africa to Patagonia would take 10,000 years on this very rough calculation, and 10,000 years, you see, is negligible compared with the million years that it took for human beings to evolve from their antecedents. The notion that we might populate the galaxy has to be taken very seriously. That's why of course we're all interested in the SETI activities because, if they were to produce evidence of intelligent life, this would help us understand what this universe really is about.

Bhathal: What's your answer to the question, where are they? That's the question you posed.

Bracewell: Yes. Well, there could very well be some other communities like our own in the galaxy, so we say, "Why haven't they arrived here?". Well, why would they? We have no idea of their values or their form of life. We see that there have been human beings who like to explore, mostly driven by greed for gold, some for other purposes, but if you look at the motivations of the explorers, it had largely to do with commerce if they knew roughly where they were going or some other commercial motive. We have pure explorers. I dare say one can think of a few but you have to think pretty hard. You might say those Franciscan monks that came from Europe and explored South America were doing it out of a pure sense of exploration, but even that is not clear. They brought military men with them and they had religious beliefs, it wasn't just scientific curiosity.

There are plenty of reasons why there could be other people out there and they haven't arrived or maybe they're due here in another thousand years, as I mentioned earlier. I don't think that it's a very cogent negative comment at all.

Bhathal: Forty years have passed in the search for ETI in the radio spectrum but nothing has been found. Should we be looking at other parts of the electromagnetic spectrum, for example, the optical infra-red spectrum?

Bracewell: There's a lot to be said for optics and there are several other modalities that you can think about. For instance, the magnetic field lines that extend out of the solar system go somewhere. If you were to shake one of those field lines, there'd be a ripple come through our solar system. It wouldn't be visible and it wouldn't be audible in the sense that radio is, but you can send influences by a variety of strange ways and for which we might in due course have ways of detecting. I don't think personally that expanding into the optical will make much difference. After all, infra-red and radio are much the same, so maybe there's a factor of two you might gain.

No, I think that the way this is going to open up in the future is that we're going to find there is life pretty widely distributed but it's going to be in bacterial form, elementary, and that it's basically everywhere. This would explain why when the earth was extremely hot and totally unlivable and being beaten up by meteor impacts, then when the meteors mostly landed and been cleaned up by hitting the earth or the moon — most of them would hit the earth compared with the moon because the earth is more massive — as soon as this surface of the planet became habitable, there's life there. That suggests to me that the strange coincidences that are required for life to generate itself didn't happen on the earth. Something happened somewhere else and that creatures or some sort of particle capable of living on the earth fell onto the earth from outside.

Bhathal: You have at one time suggested that advanced civilisations might be using probes. Can you tell us more about this idea of yours?

Bracewell: Well, I think it's a very good idea. It was based on the assumption that there is a galactic club. Now, there's an engineer in charge of making contacts with other civilisations and he's got a budget: he has to spend his money in the most efficient way. It's true these people might be incredibly rich compared with us but he's being told, "Go and contact more civilisations," and since they already have contacted a lot, it's not all that important to them, but he's got to do it cheaply. He thinks about the SETI by radio and he knows the sort of record that has: he has to consider sending out a probe. Now, he would send out, let's say, one probe a year. It wouldn't cost much. It would be about the size of a human head or, if their heads are smaller than ours, about the size of their head, and relatively cheap to launch. It would take hundreds or maybe thousands of years to arrive but it would be launched in the direction of a star like our sun which they know has a reasonable chance of having a planet, and very possibly they are able to see these planets.

The probe arrives and it spends a year or so travelling through the solar system and its duty is to attract our attention. If we have radio communication, which we do, that visiting probe would be able to receive the transmissions from our radio transmitters and television transmitters. It now knows that there are intelligent people here. We may not be intelligent but at least we're technological. Its next duty is to inform us that it is here visiting us. This may seem to raise problems of language but in fact is very simple. All it has to do is to pick up, say, the 6 o'clock news, amplify it and re-radiate it. People all over the country would hear not only the person reading the news but with a time delay of some minutes or even hours, depending on how far out in the solar system it is, they would hear an echo. This would be the first time they ever heard it and people all over the country would hear it at the same time and it would be obvious to technical people that it was an echo and of a very unusual kind. The location could very soon be established and it would seem to me then to be up to us to acknowledge receipt of this message.

There would be some people arguing that we shouldn't acknowledge receipt because, if we gave away our position, they might land and dig up our gold and so on. However, they already know we're here and you might not want to respond, but somebody would. The way to respond is simply to re-echo what they have already echoed. They see that you're on to it and you understand it and then they're going to exchange information. This seems to really involve a problem of language but in fact it's a question of television. They already know the parameters of our television transmissions — not the same in Europe as the United States but they can easily cope with that.

They will send us, as I imagine it, a picture of a constellation, say Orion, and we see this on our television screen as a recognisable constellation. Now, it might be back to front or upside-down but we'd still recognise it. I can just imagine a group of important people standing around looking at this and they're saying, "Yes, it came from Orion, but which star?". Maybe Betelgeuse would then begin to blink. That's where they've come from.

They've told us without any knowledge of English, you see, only with pictures. Then you could imagine that we have a marvellous movie in which they zoom in on Betelgeuse and then we see the planets and then we zoom in on the planets and then we zoom in on the people. What a wonderful thing that would be. I think it's very exciting. I'd be delighted to be present when that happens.

Bhathal: You have been conveying an appreciation of the role of science in society, the public. Could you tell us what you have done and how successful have you been in this venture?

Bracewell: Well, I have given lots of lectures and written lots of things. In a way you could say it's disappointing, especially as you see that many scientists have put considerable effort into writing books, writing newspaper articles, yet when we look at the people in government and in law, we don't see that major problems of society which have scientific components in their solutions, we don't seem to notice that the law-makers are very well grounded in the relevant science. You could say that about global warming at the present time, for instance, but about many other things. We don't seem to have a population led by people who understand things that are clearly of vital importance to us.

Problems of health which would be helped if we made new discoveries sometimes don't receive the support they need. Money is spent instead on treating symptoms. You could say that about tobacco. It's killing a lot of people and we think of ways of alleviating the symptoms but what we do is not entirely logical. It's not just plain science, it's logic.

After thinking about this for a long time, I don't see how it's going to change. I think in the end we're going to get what we deserve. We're going to bumble into things and when there's blood on the road a millimetre deep everywhere, we will start working on speed limits for cars and things like that. But if you don't personally see people being run over, the fact that there are thousands of them doesn't get your attention, so we don't do the logical things. We wait until we feel personal pain and then we do something. In many cases that could be too late, especially on global matters where population concerns and food supply are going to create devastating things in other parts of the world before the advanced countries who are in a position to do something about it technically begin to even pay attention.

I'm a little bit gloomy about science education. The science sections in the newspapers have improved a lot but people are just not interested in reading that sort of material. It really looks as though we are waiting for an oligarchy of very wise people to run the place but, since that's been tried in past years and we know that doesn't work, I think we have to get back to what Winston Churchill said, that democracy is a lousy way of running a country but it's the best considering the alternatives.

Bhathal: I was interested to read that you gave the 1996 Bunyan lecture on the destiny of man[46]. What is the destiny of man?

Bracewell: Well, of course we don't know. There are two extremes. One is that we're going to survive and as consecutive difficulties arise, we'll deal with them and can continue on — in my opinion, controlling the population is the most serious one — or we'll fail to do it. I saw a rather sad quote from Jacob Bronowski[47] who said that our destiny might be controlled by failure of the human mind. He doesn't mean the failure of individual minds. He means failure of the mind of the community. That is really a worrying thing. I myself don't see a way in which we're going to cope with the fact that half the people are below average and that of the representatives that are elected, half of them or maybe more are below average.

[46] https://web.stanford.edu/dept/astro/bunyan/bunyan-1996-poster.pdf

[47] Jacob Bronowski (1908–1974). https://en.wikipedia.org/wiki/Jacob_Bronowski

Bhathal: You have written several books. I wonder whether you could give us an insight into some of these books. What was your motivation in writing them?

Bracewell: When I got my copy of *Pawsey and Bracewell*[48] — that would have been in 1954 — I got a very warm feeling. I had put in a lot of work on it and really worked hard but when I saw this and felt it in my hands, after having read many other books written by other people, to have one that you contributed to yourself, I had a good feeling. That continues to be the case. I haven't brought out very many books but it gives you a pleasure to do that. It's a sort of reward. It doesn't seem to matter too much how many people read your book. You will notice a lot of books are printed and clearly the authors got this warm feeling that I'm referring to but, even if it only sells 500 copies, I think they get the same pleasure as one that sells five million.

Bhathal: After a lifetime devoted to science, what do you consider the major achievements in your life?

Bracewell: Well, you have to be pretty modest in things like this, but you can discuss it. My contribution to medical imaging could perhaps be measured in dollars or in human life or something like that, yet it's out of all proportion to my estimate of the work I put into it. Something might be valued in accordance with something measurable, like man years of life saved. Who knows? But was it a major achievement? In any case, one only makes a contribution. If I hadn't done what I did, it wouldn't have delayed things more than a couple of years at the very most. It's not a great achievement.

On the other hand, things that I thought were pretty clever and difficult and I got done, I'm not sure whether you'd call it an achievement, you see — never heard of or never had any impact. It's an interesting question. You could certainly ask people what their major achievements were and very likely get a list in many cases, and then you could analyse that list and see how much of it they contributed themselves and what impact it had on society, how difficult was it in the sense that if they had not done it, it wouldn't have been done for years. Take Wegener's hypothesis of the drifting continents.[49] That was a fantastic achievement as measured by the fact that it took 30 or 40 years and no-one else did it in the meantime. That tells you it was hard. It's a good question but there's really …

Bhathal: Maybe we should rephrase it this way then. What do you want to be remembered for?

Bracewell: You could rephrase it again and say, "What will I be remembered for?" I could make a guess at that but anything you wanted to be remembered for, you'd have a lot of luck if it worked out that way. I don't think you have much control over it. Well, some people do. Some people invent myths about themselves and plant stories, but most of us we don't have any control. I mean, look at any number of scientific things like the Fraunhofer lines. He's remembered for discovering the Fraunhofer lines. Pity they were discovered by Wollaston[50]. There are many cases like that. People are actually remembered. They didn't think they'd be remembered for that and probably couldn't care less, although I dare say if they knew,

[48] Pawsey, J.L., & Bracewell, R.N. (1955). *Radio Astronomy*. Oxford, Clarendon Press.

[49] Alfred Wegener (1880–1930). https://en.wikipedia.org/wiki/Alfred_Wegener

[50] W. H. Wollaston FRS (1766-–1828). https://en.wikipedia.org/wiki/William_Hyde_Wollaston

they'd be … Einstein's name will never be forgotten, nor will Archimedes, but other people who have done remarkable things are just completely forgotten. I don't worry too much about what I'm going to be remembered for.

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