

Bowen's Role Elucidated in the Delay of the Bracewell Publication, Centaurus A *Nature* Publication, 1962

Centaurus-A: RP Publications from 1962 with the Parkes Radio Telescope¹

Bracewell has described the “convoluted publication saga” of the Cen-A polarisation discoveries made in April 1962 with the Parkes telescope by three groups. A major uncertainty through the years, one that perplexed Ron Bracewell for years, was both the order of discovery and publication. He suspected that the CSIRO hierarchy had intervened with the editors of *Nature* in rearranging the order of publication. Also, the role of John Bolton in the delay has remained a mystery. All these questions can now be answered based on the Atkinson archive. As we will see, she could have informed Bracewell about the resolution of these questions in 1996 well before his death in 2007, but did not.

Bracewell described his experiences of 1962 in a lengthy and lively paper published in 2002, “The Discovery of Strong Extragalactic Polarisation using the Parkes Radio Telescope” (**J Astron History and Heritage**, Vol 5, page 107, 2002).

Bracewell was on sabbatical at the University of Sydney School of Physics, from Stanford, starting 25 September 1961, returning to Stanford in California in late May or early June 1962. Bracewell was invited by Bowen to observe at Parkes, arriving at Parkes 14 April 1962.² The observations started on the Sunday, 15 April, and continued for the next two days (16 and 17). (Easter was the following Sunday, 22 April). With a beam size of about 6.7 arcmin at 10cm, he was able to infer that the inner source was double with a separation of about 7 arcmin. The remarkable discovery was made that the NE component was 15 per cent linearly polarised, with no detectable polarisation for the weaker SW component.³ Bracewell continued:

¹ Also see *Four Pillars of Radio Astronomy*, Mills, Christiansen, Wild, Bracewell (Frater, Goss and Wendt, 2017) p.142-143.

² From the Joe and Lenore Pawsey Family Collection: On 6 April 1962, Bracewell wrote Pawsey in the US from Sydney: “After Taffy came back I got approval to have a shot at Centaurus in cooperation with Brian Cooper, next week.”

³ Pawsey heard about this result while he was in the hospital in Washington in April 1962 (Chapter 40). McCready wrote him on 24 April 1962 in Washington DC, a rather confused letter (Pawsey family archive): “Did you hear that Ron Bracewell has discovered that radiation from Centaurus A is polarised? He was using the 210-ft dish with Brian Cooper. They were working at 10cm and the two peaks showed opposite polarisations, each about 30 per cent. [As we have seen this assertion was an exaggeration.]

This was the first detection of linear polarisation using the Parkes telescope... The somewhat tricky observation established that the compact components were on the route to the outer extended lobes of Centaurus A. Immediately after I left Parkes, an American, Marcus Price, spent Easter weekend [perhaps starting on Saturday 21 April] at the Radio Telescope.

Price observed at 21cm and found 7 per cent polarised fraction in the central double (not resolved with the 14 arcmin beam). The 90 degree discrepancy with the position angle at 10cm (from Bracewell) led to a multi-frequency program carried out by Cooper and Price within the next few weeks. These data led to the discovery of Faraday rotation (the position angle of the polarisation rotated, proportional to the square of the wavelength). "...[H]ence ... magnetic fields within the Galaxy could be determined."

Bracewell returned to Sydney, writing a draft of a paper for publication. Bracewell:

I listed the authors as myself and Brian Cooper, in accordance with my understanding with Taffy Bowen. As was the custom at Radiophysics, I handed the draft, including mention of permission to use the dish, to Bowen, and it [manuscript] came back with a page of his [Bowen's] handwriting that superseded my [Bracewell's] introductory paragraph. I copied this out in a legible hand, added some extras, and gave it to the [publications] office for typing. I will refer to this as Paper I, as it was the first written [text] reporting observations of Cen A polarisation using the Parkes Radio Telescope.⁴

Bracewell then returned to Stanford in the US the following month. Bracewell continued:

While these... developments were in train I was eagerly anticipating the publication of my Parkes paper. Instead it was Gardner and Whiteoak's paper that first appeared in print, even though it was the third Centaurus-A polarisation study carried out at Parkes in 1962 and the third paper written (Paper III). This important paper (Gardner and Whiteoak, 1962), which exemplified the power of the new 64-m Radio Telescope, was received by *Physical Review Letters* on 1962 July 11, and was published in the

Headline [underline by McCready]: 'University of Sydney School of Physics makes important discovery on Parkes Radio Telescope!!' "As we will discuss below, the *Sydney Morning Herald* article only mentioned CSIRO personnel. Due to Pawsey's absence from Sydney, he did not provide comments on the paper; later in 1962 after returning from the US (as his health deteriorated rapidly), he did emphasize his pride in the success of the Parkes telescope in these ground breaking studies of polarisation and Faraday rotation.

⁴ The published paper had Bracewell's address listed at Stanford and a footnote: "At present on sabbatical leave at the School of Physics, University of Sydney."

September 1 issue.

Strangely, Cooper and Price's paper, reporting the second Centaurus-A polarisation study carried out at Parkes in 1962 (Paper II), was the second paper published, appearing in the September 15 issue of *Nature* (Cooper and Price, 1962). Two weeks later my own long-awaited paper finally appeared in the 1962 September 29 issue of *Nature*, with its figure of 15% polarisation (Bracewell *et al*, 1962). Immediately I noticed that Tom Cousins had been promoted to co-authorship, and although this was irregular — in the sense of never happening to a paper I wrote before or since — it did not strike me as out of keeping with the hierarchical structure that I was familiar with after a dozen years or so at Radiophysics. Besides, Tom had made the crystal mixer that was used! What did surprise me though was a “Note added in Proof” stating that C.H. Mayer had ‘... detected a similar degree of polarisation’ at 3.15cm⁵. Clearly, submission or publication of our paper had been delayed for reasons that, at the time, were not apparent, notwithstanding the fact that the Australian Scientific Liaison Office in London used to read the proofs of papers submitted to *Nature* in order to minimize the delay. In a letter to me, co-author Brian Cooper (1962), said that he could not understand why the NRL result should have caused any delay, but Haynes *et al.* (1966: 251-252) later laid the blame squarely with John Bolton: “... Bolton, furious at this unscheduled use of the telescope, intervened to delay submission of the paper... [and] Bolton, ever the *eminence grise*, arranged that this paper should appear in *Nature* two weeks before the report of Bracewell's earlier observation.”

If this is a realistic account of Bolton's role (and it is the only such mention that I have seen or heard), then it is ironic that Bolton should rate these observations so highly, for when asked what he considered was the greatest discovery made with the Parkes Radio Telescope, he immediately identified the occultation of 3C 273, but was quick to add: “I would place, certainly on an equal footing, the discovery of polarisation in the extragalactic radio sources as one of the really fundamental discoveries.” (see Bhathal, 1996: 113). The Parkes telescope saw “first light” in 1961 October, was opened on 1961 October 31, and detected strong polarisation on 1962 April 15. The occultation of 3C 273, the first accredited quasar, was observed by Cyril Hazard, a visitor from the Physics Department at Sydney University, on 1962 August 5. [Hazard et al in 1963]

⁵ Bracewell reported to Goss in a letter from 22 February 2000, that he was not pleased to see the Mayer “Note added in Proof”: Bracewell wrote “What has been unclear is how the order publication of Papers I, II and III came to be inverted, how the authorship of Paper I came to be enlarged [Cousins’ name was added- likely by Bowen- after Bracewell gave the draft to the RP Publications office] and how Paper I came to be saddled with a **non-positive** [opinion] *Note added in Proof.*”

[Bracewell continued in his 2003 publication about the events of 1962:] Obviously, the chronological reversal of the publication order of Papers I, II and III was a surprise to me but I thought the dates of observation and submission would speak for themselves. However, in 1996 I noticed that none of the papers included the dates of the observations; nor did the two papers published in *Nature* state the dates upon which they were received. As the discovery paper was the last of the three to appear in print, although written up and submitted promptly, there has been some confusion on the part of subsequent authors as to the true sequence of events. Yet the priority of the initial observations is clearly confirmed by the dates, and acknowledgments to Brian Cooper, Tom Cousins, Les Fellows, G. Henderson and Jim Roberts, recorded in the Parkes Visitors' Book. This was the first entry by a visiting observer in the Book, and it reads:

1962 April 14-18. I came as a guest investigator to study the central source of Centaurus A...I measured polarisation parameters over the field, finding a degree of polarisation much higher than has been observed in galactic or extragalactic sources. It is a great privilege to use the magnificent instrument' ...R.N. Bracewell, Radio Astronomy Institute, Stanford University.

Undoubtedly, a contributory factor in this later confusion was an article that appeared in the *Sydney Morning Herald* on 15 September 1962, which reported that

Two C.S.I.R.O. scientists, using the new radio-telescope at Parkes, have discovered what they believe is a possible clue to the origin of the universe. They have discovered the existence of magnetic fields in outer space... The two scientists who made the discovery are Mr Brian Cooper, of Sydney, and Mr Marcus Price, an American, both of C.S.I.R.O.'s Radio-physics Division ... The scientists discovered that radio waves were 'linearly polarised' — the electrical vibrations lay in a definite plane. When the scientists changed the frequencies on which they were receiving signals, they found the plane had rotated.⁶

⁶ Alec Little, recently returned from Stanford to join Mills in the newly formed radio astronomy group at the School of Physics at Sydney University, wrote to Bracewell in California on 18 September: "Apparently your discovery is being turned to good advantage, but is it a pity that you didn't get a mention!"

Another, complementary, view of the events of 1962 was provided by Brian Cooper in November 1998⁷:

Ron Bracewell has recently published a personal account [an earlier version of the 2002 paper circulated at Stanford see above, the JAHH paper of 2002] of the circumstances surrounding his discovery of strong linear polarisation in the 10cm emission from the central component of Centaurus A. His account has been given the number "Glint 744" in the archives of Stanford University. Some controversial points arise in Ron's account, and having been personally involved in those observations and also in the subsequent work with Marc Price which revealed Faraday rotation of the polarised emission, I would like to record my own recollections of that very interesting period, early in 1962, when observations with the 64m telescope were just getting into full swing.

In the Australian Archives, NSW Section, Ref. number C4632/2, there are various memoranda relevant to that period. One of them, dated 6 February 1962, announced the setting up of a programming committee for the 64m telescope. The memo also solicited requests for observing time. The committee comprised J.G. Bolton, B.F. Cooper, J.A. Roberts, and F.J. Kerr. When the committee was set up, I took it that my function would be to make requests for receiver setting up time, but when the first program appeared I found that John had "volunteered" me, with George Day assisting, to do some preliminary observations of Centaurus A at 20cm. That would be from March 12 to 15. Then, from April 15 to 18 [1962], Tom Cousins and I were to observe the Galactic Centre after setting up the 10cm receiver. I was a bit surprised at being cast as an observer but went along with it anyway because I thought it would be interesting.

My recollection of the allocation to observe Centaurus A was initially to follow up the observations which John had made at Owens Valley. There his observations had been incomplete due to the source extending down to the southern horizon at Owens Valley. Observations of the central structure would also be of interest, but there was as yet no inkling of the looming importance of polarisation. I don't think George Day ever participated in the Cen A work because, by then, Marc Price had arrived on the scene. Unfortunately, the Parkes log books which would show who actually did what in that first quarter

⁷ An unpublished two page report sent by Brian Cooper to Bracewell in early November 1998. The original report is dated 1 November 1998. "Parkes, Centaurus A , and All That"

are at present in limbo somewhere between Parkes and the National Archives. Marc Price also took over from Tom Cousins for the Galactic Centre observations, which we later reported in the book, "The Galaxy and the Magellanic Clouds."

Time for polarisation observations had been requested by Jim Roberts to follow up the work on Jupiter polarisation, which he had commenced at Owens Valley [in California], and also for Jim, Joe Pawsey [still in the US] and Max Komesaroff to look at galactic polarisation. A small feed rotator had been installed in the focal plane, and Jim was responsible for checking it out. That feed rotator would later be replaced with a larger version.

When Ron Bracewell requested time to look at Centaurus A at 10cm there was no real problem fitting him in since Sag [*sic* Sgr A] A didn't rise above 30 deg. elevation until about midnight, whereas Cen A rose 4 1/2 hours earlier. Then, as I remember it, when Marc and I turned up at the telescope at around midnight to take over from Ron, we found him gazing with great glee at scans through Cen A showing a dramatic degree of linear polarisation of the northeast component.

Ron, Marc, and I finished our observations just before Easter, and the telescope was then supposed to close down. However, Marc Price, in residence at Parkes over Easter, seized the opportunity to check on the degree of polarisation of Cen A at 21cm. As Marc has recorded in *Serendipitous Discoveries in Radio Astronomy* [p. 300, edited by Kellermann and Sheets, 1983] not only [did he find] that the polarisation was still strong at 21cm, but he also concluded that "poor old Ron" had got his feed angle wrong by 90 degrees. Here I think Marc was trying to make a dramatic point, since the polarisation position angle had actually changed by 60 degrees in going from 10cm to 21cm [due to Faraday rotation in the intervening medium].

I returned to Parkes after Easter to find Marc in earnest discussion with a number of people, notably Frank Kerr, about the possibility of Faraday rotation, which Marc had been reading up on. As an unimaginative engineer I *was* inclined to suspect an instrumental effect, but obviously some observations at a number of frequencies needed to be done. When we tuned the 21cm receiver over its frequency range, it immediately became apparent that the change from 10cm to 21cm had not caused the polarisation vector to twist merely through 60 degrees in the direction we presumed, it had actually twisted through an impressive 120 degrees in the opposite direction! I

needed no more convincing that Nature was at work here, and over the next few days we put together whatever feeds and receivers could be improvised for the range 15 to 30 cm. We were pleased to find that the rotation was following a wavelength squared relationship in accordance with the Faraday effect. Marc deserves great credit for being alive to such an important scientific possibility at an early stage of his astronomical career, and I was privileged to participate in that particular discovery.

Returning to the controversy surrounding the order of publication of Ron's paper and the Cooper-Price paper, there was a delay of about five months between the work done at Parkes and the publication of both papers. I don't know why the publication of two short papers took so long, but as Ron points out, we don't know when they reached *Nature*. I don't recall shepherding either paper through the publication process, and I don't know if John Bolton deliberately sent Ron's off two weeks later than the Cooper-Price paper, or whether he just let it get buried on his desk. [As we have seen, Bolton's involvement in the publication was minimal.] Anyway, it is clear to anyone reading the papers that Ron's observations came first.

Ron also mentions that I wrote to him shortly after his return to Stanford saying that word had reached Sydney of Cornell Mayer's observation of polarisation in Centaurus A at NRL. A note to that effect was added in proof to Ron's paper. I don't recall how the official news came, but there has been quite a bit of speculation since then on how soon unofficial tidbits of information might have percolated along the grapevine that binds the radio astronomy community together. People will form their own opinion and I will leave them to it.⁸

The Centaurus A files found in the Atkinson collection were examined by Goss in July 2014. In 2013, the previous year, while visiting the National Archives of Australia, he noticed that two important files were missing in the large collection C3830 D5/4/. This collection contains the publications files from RP from 1945 to 1981: starting with RPP No. 1- D5/4/1, the October 1945 campaign at Collaroy by Pawsey, Payne-Scott and McCready (published in *Nature*, 9 February 1946, vol 157, p158) continuing to D5/4/2450 (RPP 2450) published in August 1981, "The Position Angle of Jupiter's Polarised Synchrotron Emission and the Jovian Magnetic Field Configuration" by M.M. Komesaroff and P.M. McCulloch, published in *Monthly Notices of the Royal Astronomical Society*, vol 195.p 77, 1981. This massive collection of files was sent to the National Archives in 1996. Two of the D5/4 collection were

⁸ R.M. (Marc) Price had provided an additional, entertaining, account in the Kellermann and Sheets volume (1983), "The First Years in Parkes", p 300.

surprisingly missing in the archives at Chester Hill, Sydney: D5/4/714 and 727, Paper I and Paper II, i.e. the two Cen A papers of 1962, Bracewell et al followed by Cooper and Price,

The reason the files were missing was, as expected: Sally Atkinson had taken them out of the file system at the RPL in Marsfield before the massive collection was moved to the National Archives of Australia. The outer file folder in the Atkinson papers is shown in Fig. 1; the handwriting is Atkinson's. The almost unreadable faint text reads: [for D5/4/714 and 727] "(Marked for destruction by the Archives)⁹". The struck-out text (done by Atkinson) indicates that the material was originally slated to be returned to their sister files, the ones that were in fact moved from Marsfield to the NAA at Chester Hill. At some point, Atkinson changed her mind and wrote (twice): "To be Kept." The files were taken home by her and remained there until her death in late 2012. Goss then discovered these in Marsfield in July 2014.

The content of the two files answers a number of questions. The question of the delayed publication is no mystery: Paper I (RP 714-Bracewell et. al.) was sent to *Nature* on 6 July 1962 and published 12 weeks later on 29 September, while the Cooper and Price paper (RP 727) was submitted to *Nature* four weeks after Paper I and published two weeks before Paper I. Ron Bracewell's suspicions were confirmed.

Atkinson included in each folder her notes regarding dates of submission, dates of editing and dates of publication. (The published papers did not list the dates of observations or the date of receipt of the manuscript in London.) Some of her notes were written by Atkinson in shorthand. Her handwritten text has provided a source of her activities: "... [F]ollowing conversation with RN Bracewell when he suspected that RPP 714 must have been held onto during course of ...[unreadable]." Likely, Bracewell had been in Sydney and rung Atkinson. As we indicate below, Atkinson never sent these lists to Bracewell.

The major cause of the delays for RPP 714 arose due to the interference of Bowen. In a letter to the Editor of *Nature*, L.J.F. Bimble¹⁰, Bowen wrote on 7 August 1962, less than a week after Paper II was submitted. Bowen wrote:

I should really have sent you a personal note with it just to explain that the 210-foot telescope at Parkes is going extraordinarily well and is now producing results of quite outstanding quality. Although the paper by Cooper and Price is not the first to come from work on the telescope, it describes the most exciting result to date. It really is a minor "classic" in its way

⁹ The meaning of this text is unclear: Were the files originally to be destroyed? And when was this text, apparently an afterthought, added? The answer remains a mystery.

¹⁰ Editor of *Nature* from 1938 jointly with A. J. V. Gale until 1961, then sole Editor until his death in 1965

and is likely to be referred to as a basic paper in radio astronomy for many years to come. I fully expect we shall have small gems of this kind coming along at quite frequent intervals.

A week later (15 August 1962), Bimble replied: ... “I had recognised that this was a very important piece of work, and it is being published as quickly as possible.” Bowen’s pressure worked, and the paper was published 4 ½ weeks later, followed by the Bracewell et. al. paper two weeks afterwards. John Bolton was not involved with the correspondence with *Nature*.

Others at the time in 1962 suspected some interference. For example, Brian Cooper (a co-author on both Papers I and II), wrote Ron Bracewell on 19 September 1962, just after Paper II was published.

This [the Cooper Price paper] took 5 [actually 6] weeks, but of course Taffy asked them to get a move on it. [Apparently, Brian saw or heard about the letter to Bimble.] As far as I am concerned there has been no skulduggery, but if you are sufficiently interested you can probably get the facts from Bimble.

Marc Price wrote Bracewell on 16 September 1998 (from Albuquerque NM at University of New Mexico) as they discussed the events 36 years earlier: “There is little doubt in my mind that Paper I was held up somehow. I do not know if this involved the magazine [*Nature*], its editors, or the Scientific Liaison in the UK. It does seem clear that it would have needed Taffy’s and/or other high level involvement.” Both the Cooper and Price comments indicate that they had been suspicious of Bowen’s intervention.¹¹

A tragic letter was written to Ron Bracewell by Sally Atkinson, likely in 1996 (but undated) at the time of the phone conversations between the two. Again, an **unsent letter** (see footnote 25 NRAO ONLINE 24 “Controversy over the Million Degree Corona – 1946: Martyn, Pawsey and Bowen and the ‘Two Paper’ Imbrogio”¹²) played a role. If the letter had been posted, it would have provided some answers to questions Bracewell had posed for over three decades. Atkinson has written in pencil at the top of the letter: ‘**Not sent**’. See Fig. 2 for the original unsent letter.

Dear Ron,

Some years ago you mentioned an early paper of yours ‘Polarisation in the Central Component of Centaurus A’ by R.N. Bracewell, B.F. Cooper and T.E. Cousins (RPP 714) which, from memory, you said had been held up at Radiophysics so that another paper ‘Faraday Rotation Effects Associated with the Radio Source Centaurus-by B.F. Cooper

¹¹ Cooper and Price letters in the Bracewell archive, provided to Goss in the years 1998 to about 2000.

¹² However, in the case of the Atkinson un-sent letter no anger was involved.

and R.M. Price (RPP 727) could be given precedence in publication. Both these papers were published in *Nature*.

Recently on going through our early publication files I came across the correspondence dealing with the submission of these two papers and attached is a chronological list of relevant dates. [These are not shown in the letter; likely there was an attached list of dates.] You can draw your own conclusions. You will see that the time taken by the Publications Section to prepare the manuscripts for publication was from 7/6/62 to 7/7/62 for your paper (RPP 714) and from 25/7/62 to 2/8/62 for RPP 727. This time difference could of course, have been caused by delays in the preparation of the figures. The amount of text in each paper is approximately the same.

I am sorry not to have done anything about your query until now. I hope all goes well with you and your family.

Best wishes – Sally Atkinson

Sadly, Bracewell never received this news before his death 12 August 2007, our emphasis ¹³

¹³ Libby and Miller Goss had a long visit with Helen and Ron Bracewell in January 2007 in Palo Alto on their way to Sydney from New Mexico. Many topics were covered; at some point Libby Goss was fast asleep on the Bracewell sofa. Miller and Ron discussed the Centaurus A story at length. Ron died in August 2007.

Fig 1. The top page of the Atkinson archive material about the Centaurus publications of 1962, Photographed by Goss in 2014 July in Ron Ekers' office. The faint text reads 'Marked for Destruction by the Archives'.

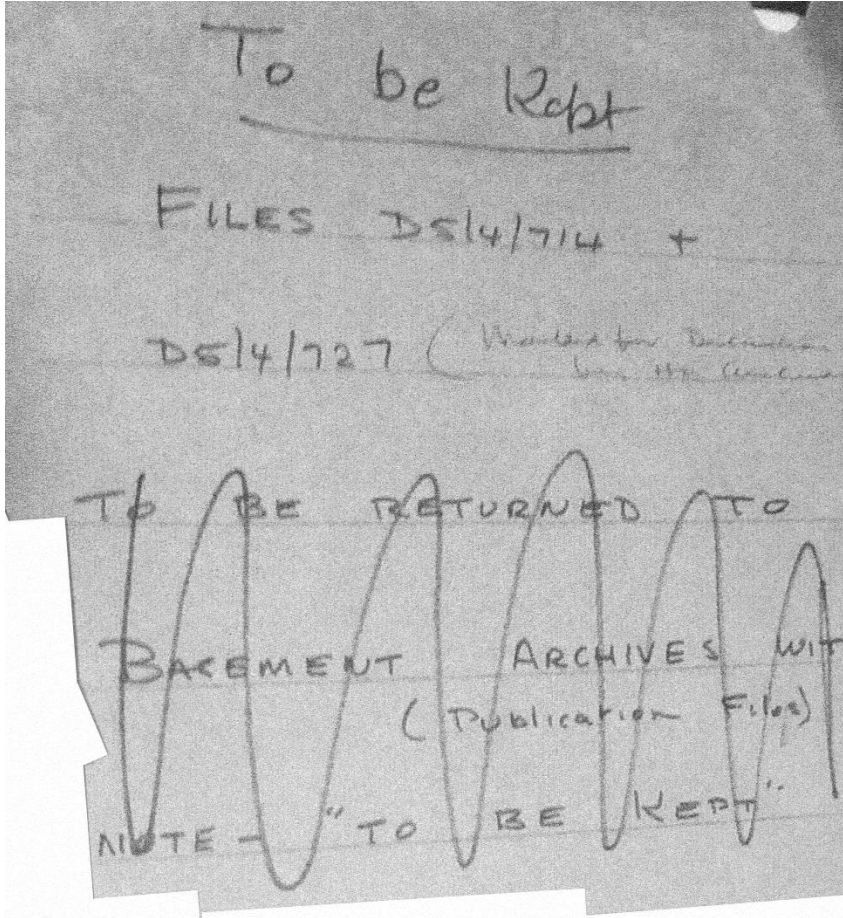


Fig 2. The 'not sent' letter from Atkinson to Bracewell.

