NRAO ONLINE 60

Coordination of Solar Noise Research and Interferometer Techniques with Cambridge 1948-1949

Sullivan (2009, Cosmic Noise: A History of Early Radio Astronomy. Cambridge University Press, Cambridge, UK, page 144) has described Pawsey's attempts to coordinate Sydney (RPL) and Cambridge (Cavendish) radio astronomy activities, starting in 1946.[1] During the first year of solar noise research in both Sydney and Cambridge, Pawsey had received a letter from Ryle (likely late August 1946) with a copy of the Ryle and Vonberg *Nature* paper "Solar Radiation on 175 Mc./s" (submitted on 22 August and published 7 September 1946, a time interval of only two weeks[2]). Pawsey acknowledged this preprint on 2 September 1946 in a letter to Ryle. Pawsey pointed out that Ratcliffe had been sent a copy of the CSIR *Proceedings of the Royal Society* paper by McCready, Pawsey and Payne-Scott on 2 August 1946; he had been asked to pass the paper on to others at the Cavendish. "You will see that there has been considerable similarity between the work we have carried out here and that which you have done at Cambridge."[3]

Then on 10 September 1946, Pawsey wrote Ratcliffe (quoted by Sullivan, page 144):

> I got rather a shock when I received Ryle's note enclosing a copy of the letter to *Nature* contributed by himself and Vonberg. As you will see from the paper describing our work, which should have reached Cambridge just after the dispatch of Ryle's letter, the Cavendish and Radiophysics Laboratories have unfortunately succeeded in duplicating a very considerable part of the work. Our paper was communicated to the Royal Society by Sir David Rivett before July [1946] … I do not know what we can do about this duplication or how we can avoid it in the future. My only suggestion is that you have a talk with E.G. Bowen, Chief of this Division, who is at present in England. I have written him to get in touch with you … I sent you a food parcel in August so that with any sort of luck you should get it about Christmas time. According to accounts here your menu is pretty dull. Out here the food situation is above reproach but there are still grave

[1] NAA C3830 A1/1/1 Part 1.

[2] The first Sydney solar noise paper was published on 7 February 1946, after being submitted about 14 weeks earlier.

[3] Pawsey also described the circular polarisation work done by David Martyn, published in *Nature* (31 August 1946), the week before the Appleton and Hey followed by Ryle and Vonberg papers, also reporting detections of circular polarisation in the *Nature* publication (vol 158, p 339) published on 7 September 1946.

> deficiencies with respect to manufactured goods and the discomfort in travel persists [a continuation of wartime restrictions].

On 17 September 1946, Ratcliffe replied:

> I have just returned from a holiday … and on my return I found your most interesting paper and letters awaiting me. First let me say how admirable I thought your paper on solar noise work was. There have been so many people scratching at this subject and making a few half-hearted measurements, that it is nice to see someone who has done it so thoroughly as you. As you know, we have embarked on a big programme of this kind and, as you mentioned in your letter, there has been, and probably will be, a certain amount of overlap between us. I do not think there is any harm in this. The methods which we are using are in many respects very different. You gain our extra sensitivity by means of a large aerial gain; Ryle gets it with his special integrating technique [the use of the noise balancing system]. You do your "Michelson method for the diameter of the source" by using the reflection in the sea; Ryle does it by spacing his aerials. With the spaced aerial arrangement Ryle can find at the same time the polarisation; moreover he is not restricted to horizontal viewing. He is not intending to concentrate entirely on solar noise, but has ideas about using the same equipment, both with similar and different aerial systems, for exploring galactic noise … I think you will agree with me that this question of the emission of radio wavelengths from the sun is such a big one that it needs several workers on it. I do not view with any more dismay the possibility of overlap here than I do in the case of ionosphere research, for example. Now that the Air Mail works so quickly (Ryle showed me your reply yesterday to his letter) we will make a special attempt to keep you fully in touch with what we are doing.

Pawsey wrote Bowen on 24 June 1948 about F. Graham Smith's plans at the Cavendish. As Goss and McGee (2009) have discussed in detail, Graham Smith knew nothing of this project at that time. During an interview with Goss In 2008 at Jodrell Bank, he was surprised to hear about the letter of 60 years earlier.

After Pawsey visited Cambridge in 1948, Smith left solar noise research to concentrate on the position determinations (his PhD research) of the "radio stars", leading to the identification of Cyg A with a faint galaxy at a large redshift (Chapters 18 and 21).

Pawsey wrote Bowen- 24 June 1948 letter:

> In my discussions at the Cavendish I found [F.G.] Smith, one of Ratcliffe's men who work on solar noise, contemplating carrying out a series of observations on the form of bursts

> which is very similar to that done by Ruby [Payne-Scott]. It is a clear case where the discouraging sort of duplication of work could occur if we do not get together before hand. I indicated the general lines of Ruby's program without giving much of the results and we agreed that I should write you with the following suggestions. We should ask Ruby to prepare an outline of the paper she intends to write and send it to Ratcliffe for Smith's information in the near future. Smith would hold off immediate work on this subject, and when he has results to publish would of course expect to acknowledge any relevant work of Ruby's. By this arrangement we gain freedom from fear of immediate prior publication by Smith and consequent undue haste in publication. His gain is obvious. At the same time it emphasises that competition is likely and I should strongly recommend Ruby to get on with the job promptly. I think it should take full precedence over any interference work [the Potts Hill swept lobe interferometer then under construction] she may have on hand.

The solar noise proposal as put forward by Pawsey was doomed from the start due to various reasons. Smith was leaving the field and the Cavendish group did not believe in the reality of isolated (Type III) bursts (NRAO ONLINE 30) and in particular the rapid drift in frequency from high to low frequencies. In addition, the conditions of the coordination were ill defined. This complex proposal was stillborn. In practice, the solar groups in both locations just ignored the proposal in spite of the continued correspondence that lasted from 1948 to 1951.

Coordination of Position Determinations with Michelson Interferometers , Smith , Payne-Scott and Mills (1948-49)

A very helpful component of the coordination was the exchange of two reports about the use of spaced interferometers for position determinations. The letter from Ryle to Bolton in June 1948[4] included a copy of a report by Graham Smith from 18 June: "The Determination of the Positions of Discrete Galactic Sources". This report was published in an extended form in 1952 by Smith (MNRAS vol 112, page 497 submitted 18 April 1952) "The Determination of the Position of a Radio Star". The 1948 report contained a discussion of the geometry of the system (see FIG 1 at the end of the text for Smith's figure) as well as the sources of errors such as errors in aerial alignment. A special problem was the calibration of the instrument. At this time the only radio source with a known declination was the sun; the quiet sun was too faint and too resolved. "Sunspot radiation often gives a steady enough signal for calibration purposes." For a

[4] Ryle also sent a copy of the report to Pawsey in care of the Australian Scientific Liaison Office in London.

declination of + 40 deg, the interferometer at 80 MHz with a baseline of about 500 metres provided declinations with an error of about 15 arc min, while the errors in right ascension were 20 seconds of time (5 arc min at modest declinations).[5]

The Australian response arrived with a delay of about one year. On 28 June 1949, Pawsey wrote Ratcliffe ("Dear Jack") with the report of Payne-Scott and Mills "Notes on Interferometer Errors".[6] These two were then busy debugging the Potts Hill 97 MHz interferometer (three elements. baseline of 40, 240 and 280 metres) for both solar and Cyg A observations. Pawsey wrote an accompanying letter:

> … [T]hey have been impressed by the rather tricky nature of the possible sources of error in the case of attempting a precise position finding. In consequence they suggested that it might be a good thing to maintain rather close contact with Ryle and his group with the idea that each might be able to help the other. [Please pass] on to Ryle with a request … for… collaboration. Incidentally the 'dawn' [sea-cliff] technique is just as bad or worse but the errors are different.[7]

Many sources of error were considered in the RPL report; examples were refraction by the troposphere and ionosphere, misalignment of the E-W line of the interferometer, geometric errors in the baseline, changes of cable length due to temperature changes etc. Surprisingly, the simplicity of the troposphere correction for a plane-parallel atmosphere was not recognised in Australia until a few years later. By 28 June 1951, Payne-Scott and Little wrote: [8]

> A uniform plane sheet of refractive material does not introduce a path difference between parallel rays, and hence the refraction due to a uniformly stratified region about a plane earth can be ignored. [9]

They also considered the effect of curvature of the atmosphere (e.g. at low elevations); for their data on the sun at plus and minus 2 hours of transit this effect was negligible for the troposphere. For the ionosphere this was not the case. Errors due to the ionosphere were as large as one arc min, but again negligible with a fringe spacing of 40 arc min.

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[5] By 1951 Smith's observations at 1.4 and 3.7 m yielded improved accuracies of 12 arc sec in right ascension and 1 arc min in declination for Cyg A.

[6] NAA, C3830, A1/1/1 , Part 4

[7] Mills added a final section to the report giving an interim position for Cyg A. – the declination was within an arc min of the correct value but the right ascension was in error by 15 arc min.

[8] It is possible that this is the first published recognition of this fact.

[9] Mills (1952) also came to the same conclusion with the 100 MHz interferometer at Badgery's Creek. The baseline was 270 metres, comparable to the length of the baseline at Potts Hill.

Ryle replied with a delay of three months; there was confusion whether Ryle or Ratcliffe would reply to Pawsey; also the staff at the Cavendish were on summer holidays. On 28 September 1949 Ryle wrote:

> We are very interested that you had been using the spaced aerial technique and had looked at some of the errors … The general conclusions [of F.G. Smith from June 1948] are very similar as regards tropospheric or ionospheric refraction and in the corrections for height and direction of the aerial system.

He criticised some of the details of the report, including the treatment of errors in height of the aerials and E-W alignment. Ryle was surprised that the cable expansion at the Sydney site was so extreme (12 cm per hour in the day and 4.5 cm per hour at night). Perhaps the difference was the superior quality of the "Jerry" cable [surplus German from the war, Sullivan, page 386] at the Cambridge site. Also Ryle was concerned by the techniques used to measure the absolute phase of the fringes at each aerial as well as the method of determining the effective frequency of the system at Potts Hill. No detailed response from either Payne-Scott or Mills to the criticisms made by Ryle have been found in the National Archives of Australia.

Pawsey's Coordination in 1951-

Pawsey's attempts to coordinate research interests continued through early 1951. On 3 November 1950[10] (two years after his return from the 13 month overseas trip of 1947-1948), he wrote Ratcliffe:

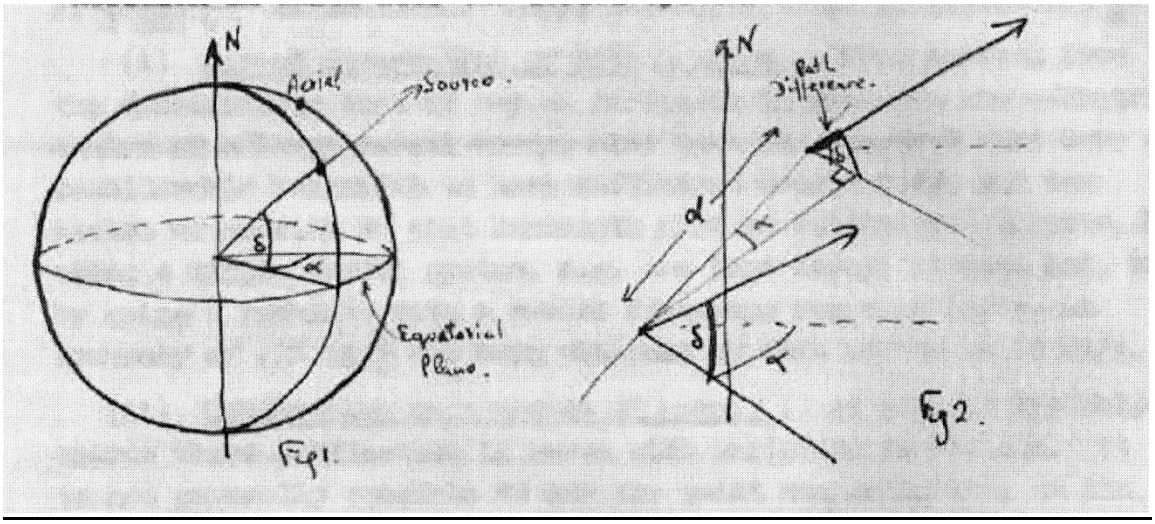
> When I was in Cambridge [1948], we discussed ways of avoiding clashes of interest between our two laboratories and we agreed that it was desirable to keep each other informed of the major experimental projects (involving expensive equipment) in radio-astronomy … The other should not follow a similar plan without prior discussion.

RP had not started 45 MHz research: "This is a wavelength which we held off by agreement with Ryle [in 1948] so he could have a clear field." Pawsey gave a list of current RP projects including the Potts Hill swept-lobe interferometer, the Penrith and later Dapto swept-frequency instruments, and the multiple beam interferometer (the Potts Hill grating array of Christiansen) and a series of cosmic noise experiments (including the 3 element 100 MHz instrument of Mills). He mentioned that a survey of cosmic noise sources was to be done in both the southern hemisphere and the "region visible from both Cambridge and Sydney- have you any proposals for collaboration here?"

[10] NAA C3830 A1/1/1 Part 5

Ryle replied on 7 February 1951[11], with his usual apology for the long delay in responding. There had been confusion about whether Ratcliffe or Ryle would answer. “I am extremely sorry for this; it was not intended to signify lack of wiliness to co-operate!” He mentioned the work of Machin and O’Brian at 81.5 and 210 MHz on the quiet sun. There was a little overlap with “your rapid interferometer work on 100 Mc/s”. We have, however, concentrated on very accurate measurements, without the facility for great rapidity.” For 45 MHz solar observations there was little “chance at the moment because of the lack of staff”.

Fig 1 below. F.G. Smith’s figure in his report: “The Determination of the Positions of Discrete Galactic Sources”. This report was published in an extended form in 1952 by Smith (MNRAS vol 112, page 497 submitted 18 April 1952) “The Determination of the Position of a Radio Star”. The 1948 report contained a discussion of the geometry of the system.



[11] Ibid Part 6