

NRAO ONLINE 43

GRT 1957 ---- Freeman Fox Proposal for the GRT

Epigraph:

Bowen to Bolton 21 October 1957:

I am afraid we are almost as much in the dark about the Freeman Fox report as you. Their Final Report has been coming “at the end of the month” since last June, but nothing ever seems to show up. Being 12,000 miles away from the scene of operations, there is practically nothing we can do except bite our nails and squawk, which we do often, with no tangible result.

Frustration with FFP

The year began with a growing sense of frustration on the part of CSIRO with the delayed FFP design study. A typical expression of the frustration was shown in Bowen’s letter to Bolton on 23 May 1957¹:

As you know, our design has been going extremely slowly. The final report has been promised for some time but shows no sign of appearing, and as the culprits are 12,000 miles away there is very little we can do about it but squawk ... Apart from the delays, what is depressing is we appear unlikely to get a good inter-comparison with an equatorial system. FFP went for the alt-azimuth design **on a hunch** [our emphasis] in the beginning and they have resisted doing anything quantitative about an equatorial design. This is a pity and we may have to spend still more time getting it properly worked out. Big dishes are a slow business, as nearly all of us are finding out.

On 21 October 1957², Bowen wrote Bolton: “[The] final report [from FFP] has been coming at the end of the month since last June”. A month later, Bowen told Pawsey³ “ ... FFP weeks have a habit of turning into months ...”

During 1957, Harry Minnett wrote two “off the record” reports to Bowen with his candid assessment of the situation at FFP (7 February and 7 August). Minnett anticipated the successes

¹ Bolton correspondence, CSIRO, CASS, Ron Ekers.

² *Ibid*

³ NAA, C3830, A1/3/11/1, Part 9. 14 November 1957.

and problems of the final report. On 7 February 1956, Minnett expected the report by late April at the earliest⁴. Minnett:

Roberts meant to convey that by the end of May [1957], he reliably expects to report to you on the feasibility and cost curve of the alt-azimuth mounting based on Wallis's master equatorial. ... [H]e has very strong grounds for believing that the design is feasible and that the dish diameter will be in the range 200-300 foot for a sum £400,000 (pounds sterling). Ralph Freeman agreed with me that it would not be possible to have completed a thorough study of equatorial mounts ... [Perhaps they would have] an opinion of equatorial feasibility and cost ... As you know, I am not very enthusiastic about the current equatorial mounting because ... there has not been sufficient study of basic geometrical and structural possibilities. Jeffery has simply done his best along one particular track ... Freeman [thought that in the future they could] explore the equatorial more thoroughly ... I can see no hope of changing FFP policy of finishing off the alt-azimuth before devoting any real effort to the equatorial ...

Also there was conflict. Minnett continued on 7 February::

[I fear] that it is difficult ... to get a statement of intentions from Gilbert Roberts or to alter his course of action. He gets quite upset at any suggestion that FFP are not doing the right thing, and some sharp words have been exchanged in the course of our talks. Ralph Freeman is much more informative and amenable to argument, but unfortunately he takes only a very general interest in the work. He is very busy with administration and ultimate action always depends on Roberts.

In spite of these pessimistic complaints, Roberts did, at least, send Bowen a thorough update on 27 May 1957⁵. This communication included a set of eight of the design drawings (e.g. layout of the 230 foot diameter dish structure). An interim document from Sir Howard Grubb, Parsons and Company from 22 March 1957 was included along with an interim report for the newly named "master equatorial" (previously termed the "equatorial master"). This small telescope, located at the intersection of the azimuth and altitude axes of the GRT, was to provide the coordinate transformation between hour angle and declination and azimuth and elevation. The concept document included detailed descriptions such as the servo autocollimator similar to the Babcock automatic photoelectric telescope guider (Babcock, Horace W. "A Photoelectric

⁴ However, the final report did not arrived in Sydney until November 1957.

⁵ NAA, C3830, A1/3/11/1, Part 8.

Guider for Astronomical Telescopes." *The Astrophysical Journal* 107 (1948): 73), used for guiding the 100 inch telescope on Mount Wilson.⁶

At essentially the same time (3 June 1957), White was losing patience⁷ as he wrote to Freeman:

... [W]e are finding the continued and protracted delays somewhat embarrassing. You are aware ... that the funds for this project have in part been donated by the Rockefeller Foundation and the Carnegie Institute [sic] in the US, while a substantial sum is to be provided by the Commonwealth Government. There have been, in addition, very large donations to the project from Australian sources.⁸ We are finding it increasingly difficult to explain ... why it is that the report from your firm has not yet come to hand, particularly as we have told these people first that we expected it by the 31st March [1957], then by the end of April, but now still find ourselves without any very definite information to give them. While we take comfort from the fact that little finality seems to have been reached in the US [delays in the 140 foot AUI/NRAO project] on similar design studies ... [i]t would be very much to our liking if you could give some indication as to when the final report is likely to come to hand.

Ralph Freeman responded immediately to White and pointed out that it was Gilbert Roberts who was in charge of the GRT project. The update of 27 May 1957 from FFP with the short summary and eight drawings had crossed in the post; the final report was projected for the second half of July. The excuse was that other firms were involved (Grubb, Parsons, Metropolitan-Vickers, Sir William Arrol and Company): "We realise only too well that the report would be of little value to you without an accurate estimate of the cost, and it is therefore very important that our own estimates should be verified by the manufacturers who have assisted in the design study, and who might be called upon to share in the building of the telescope." There was no contractual control over these firms, most of them were "fully engaged in defence programmes" and FFP just had to proceed "waiting our turn". Freeman pointed out "everything is being done to achieve finality as soon as possible".

⁶ Hanbury-Brown, Minnett and White (1992, Edward George Bowen, 14 January 1911-12 August 1991, *Biographical Memoirs of Fellows of the Royal Society*) article about Bowen have described the development of the ME: "In October 1956 [see Chapter 27 and NRAO ONLINE 42], however, Grubb Parsons Ltd. agreed to develop and cost a master equatorial system. They also suggested an important innovation for sensing the error between the pointing directions of the master unit and the slave reflector axis [mounted on the back of the reflector]. This idea was based on proven auto-guidance technology and was a significant advance on the untried [and complex] mechanical and hydraulic system of the [Barnes] Wallis proposal."

⁷ NAA, C3830, A1/3/11/1/, Part 8, White to Freeman.

⁸ This was a major exaggeration. The private contributions in Australia would only amount to few percent of the final cost.

Gilbert Roberts replied to Bowen on 16 July 1957, beginning the letter with a strained joke: “Many thanks for your letter of 25 June [acknowledging the ‘eight drawings’ letter], in which you say that you are studying our drawings with relish. I only trust this is not a euphemism for a pinch of salt!” Bowen had mentioned on 25 June 1957 that Roberts and/or Freeman might consider a visit to Sydney to discuss the project after the final design study was in hand. Roberts had originally considered the possibility of combining the Sydney trip with “one of our trips to Auckland, where ... we are consulting engineers for the [Auckland] Harbour Bridge”⁹. However, the possible visit to Sydney would have to be independent of the New Zealand trip. Roberts predicted that with the expected delivery of the final report in August 1957, a sensible time for a visit to Sydney would be late September 1957. However, the visit of Roberts was to only occur at the beginning of the following year, January 1958. Roberts knew the CSIRO was very anxious to get even a rough estimate of the costs involved. His estimate for a 210 foot antenna was £A 475,000, consistent with the funds available in Australia.¹⁰

Additional Funds for the GRT

However, by this time in mid-July 1957, Bowen had begun to question Roberts’s estimates, even with £A 524,000 on hand. On 23 July, he wrote Alfred Loomis a long letter with a plea for advice¹¹. Since Loomis was on the Board of the Rockefeller Foundation and the Carnegie Corporation of New York, he was quite familiar with the GRT background. After providing a comprehensive summary of the status of the US, UK and Australian projects for large paraboloids, Bowen described the FFP design study: “Apart from preparing the design, FFP will give us a size-cost curve from which we will have to make a firm decision on the size of dish in relation to the amount of money available to us.” With an expected size of about 210 feet, the GRT would be smaller than the Manchester aerial, but with a much improved surface and pointing accuracy; the dish would have full sensitivity at 21 cm and the pointing accuracy was expected to be a few arc min.

Bowen continued to Loomis:

⁹Kerensky, O. A. ((1979). "Gilbert Roberts. 18 February 1899-1 January 1978." *Biographical Memoirs of Fellows of the Royal Society* 25: 477-503) has described Roberts’s (1899-1978) role in the Sydney Harbour Bridge design in the late 1920s and early 1930s. In the 1950s, FFP were the consulting engineers for the Auckland Harbour Bridge with Roberts in charge. The bridge was opened on 30 May 1959. Roberts and FFP were also involved in the design of the West Gate Bridge in Melbourne, opened November 1978. During the construction a major accident had occurred on 15 October 1970, killing 35 men. A Royal Commission was commissioned in Melbourne later in 1970. Roberts provided testimony.

¹⁰ In his letter of 16 July 1957, Roberts praised Harry Minnett: “[He] has been of the greatest assistance to our study in many ways ...”

¹¹ NAA, C3830, A1/3/11/3, Part 4.

I would hate to see this process of whittling down the diameter go on to a point where we finished up with a dish less than 200 feet in diameter. The problem is mainly a financial one. An extra quarter of a million dollars from an outside source would put us firmly in the 210 to 250 foot bracket. Double that would really put us in business. The USA has been so generous on this great project that I certainly don't want to overdo it, but I am wondering if you know of any benevolent group who might be inclined to help still further.¹²

As Bowen explained to Loomis, he had asked in vain for support from the Sloan Foundation. He had tried his friend Rowan Gaither at the Ford Foundation¹³ “without putting the hard word on him”. The Ford Foundation was, at that time, not interested in the physical sciences, “but I wonder if you can think of any formula which would bring the giant telescope within their orbit ... I am writing this very much as a preliminary note to sound out your views. ... I do hope that I am not presuming too much in writing to you in this way”. No response from Loomis has been found; likely Bowen visited Loomis during his frequent visits to New York in the era 1950-1962.

A few days previously (19 July 1957), Bowen had written Warren Weaver, Vice-President of Natural and Medical Sciences of the Rockefeller Foundation, with an optimistic status report of the GRT; major concerns and uncertainties were hardly mentioned.¹⁴ “We are somewhat embarrassed by the fact that our progress in the project has been slow. At the same time, we do feel that a most comprehensive study of the many constructional problems has been made and this study could not have been in more competent hands. We will be in a position to start placing contracts for construction two or three months from now.” It was, in fact, to be mid-1959 (two years in the future) before contracts could be placed.

On 16 August 1957, Pawsey left for his National Science Foundation sponsored seven-month visit to the US (Chapter 28). Before he left, he worked on two projects involving the GRT: radio frequency interference mitigation and performance parameters of the GRT compared to cross type antennas. In Chapter 35, we describe the internal publication RPL 122 of July 1957: “Performance Parameters of Radio Telescopes for Observations of Extremely Distant Objects”.

¹² NAA A1/3/11/10, Part 2. A few weeks later, Minnett wrote Bowen from London. The final report was still three months in the future. “[The report] contains large sections on wind forces, basic design considerations of the 230 foot dish structure, surface and feed support, basic design considerations in the two types of mount and a **detailed discussion of the alt-azimuth design**. This is entirely written by Jeffery ...” Minnett pointed out that some of the aspects of the dish involving the radio performance had still to be specified, such as the useful area of the focal plane, the choice of the ratio of focal distance to diameter (f/D) and the effect of obstructions in the path of the incoming radiation.

¹³ Bowen knew Gaither (1909-1961) during WWII at the Radiation Laboratory at MIT (Assistant Director). He was President of the Ford Foundation from 1953 to 1956, then Chair of the Board from 1956-1958.

¹⁴ NAA, C3830, A1/3/11/3, Part 4.

During R.G. Casey's trip to the UN in New York in October 1957, he visited Dean Rusk, the President of the Rockefeller Foundation. Casey telegraphed from the Australian legation in New York to White in Australia on 7 October 1957:

I told Rusk that the probability was that the diameter of the telescope now looked appreciably smaller than 210 ft. He asked if there was any anticipated scientific disadvantage in reduced diameter ... which I said I could not answer but would ask you to answer. The unstated inference that I gathered from this was that if there was scientific disadvantage more money might be forthcoming [from the Rockefeller Foundation].

In a letter of 18 October 1957, Warren Weaver opened the door for additional funds in a letter to Bowen ("Dear Taffy"):

We would appreciate it very much if you would tell us how much you are giving away, scientifically, by virtue of such a reduction in diameter; and at least some rough approximation of the amount which it would cost to hold to the original size estimate [of 250 feet]. I must not give you the idea that we are automatically prepared to make a further contribution, in order to come back to the original size. Indeed I am not sure that any additional contribution would turn out to be at all possible. But, we would certainly wish to have all the facts before us.

Draft FFP Design Study

Finally, on 21 October 1957, Roberts posted the draft of the final report to Bowen, arriving on 6 November ("... [T]here have been innumerable obstacles to overcome and special points to investigate before we could be certain that our [FFP] proposals would really work".¹⁵) On 12 November, 1957 Bowen replied to Roberts, clearly relieved to finally have something concrete to show colleagues, as well as the foundations in the US, plus the CSIRO administration in Melbourne. Bowen had a number of questions and comments. He requested that Minnett return to Sydney for discussions¹⁶ (especially to provide a first-hand report on the proposed GRT servo control¹⁷). Bowen and colleagues were looking forward to the visit of Gilbert Roberts

¹⁵ NAA, C3830, A1/3/11/1, Part 9.

¹⁶ Minnett had been in London at FFP since early 1956; he was in Sydney from December 1957 to March 1958 and then back at FFP until 1961 (Thomas, B. M., and Robinson, B. (2005). "Harry Clive Minnett 1917–2003." *Historical Records of Australian Science* 16, no. 2: 199-220).

¹⁷ As Hanbury-Brown, Minnett and White (1992, Edward George Bowen, 14 January 1911-12 August 1991, *Biographical Memoir of the Australian Academy of Science*) have written: "By October 1956, ... Minnett had proposed a servo system [for the GRT] that avoided the stability problems arising from structural resonances, and had shown that [the servo system and the master equatorial] could accurately track astronomical sources under dynamic wind loads."

in early 1958. Would it be possible for some of the contractual work to be carried out by Australian companies in addition to UK firms? Bowen also foreshadowed the use of European contractors: “Would it be advantageous to place any of the contracts outside England and Australia?¹⁸” Bowen to Roberts 12 November 1957:

This is a problem on which we could make ourselves unpopular, but we are very naturally interested in getting the construction done in the shortest possible time at the minimum cost. ... [C]ertain types of construction work in Australia have been undertaken both cheaper and faster by European [i.e. non-British firms] contractors.

Bowen had numerous questions for FFP: How would I FFP proceed to the tendering process and place contracts? Will FFP supervise the construction? What will be the time of construction? Finally, Bowen wanted to know who was to have the final authority on the acceptance of the GRT. Would this be FFP or the contractor? All of these were relevant questions and cause for concern. Answers would only appear over the next few years.

The following day (13 November 1957), Bowen wrote Minnett with a copy of the letter to Roberts.¹⁹ He hoped that Barnes Wallis had seen the report. “He has not been mentioned much recently but I gather he will have seen a final copy and will, no doubt, have some fairly definite views about it.”²⁰ Bowen continued to vent his anger at the delays during 1957 caused by FFP

I need not say how relieved we were to see at last a draft copy of the Final Report. To be quite frank, I was running out of patience at receiving promises, none of which ever seemed to be kept. I will not enlarge on this except to say that a show like ours simply cannot run if something is promised for Wednesday the 23rd, but it doesn't in fact happen anywhere near that date. We would like to know, say on the 22nd, that it will not happen or at least on the 24th that it didn't happen. If we were Tibetan mystics endowed with second sight and an infinity of time, everything would be perfectly all right. **But we are just ordinary mortals and would like to be told by pen and ink, cable, carrier pigeon or what-have-you, but not please by telepathy.** [our emphasis]

As the preliminary versions arrived in early November, they were classified as “Draft” (6 November 1957) and then “Preliminary” (8 November 1957). A month later the “Final” Design Study arrived, leading to increased activity at RP in Sydney. In addition to letters to two senior RP personnel overseas (Pawsey in the US and Frank Kerr in Leiden and the US), Bowen gave

¹⁸ Strikingly, no requirement for Australian contracts were discussed.

¹⁹ NAA C3830 AA1/3/11/10, Part 2.

²⁰ On 13 November 1957, Minnett replied before he had received the query about Barnes Wallis from Bowen. “[Wallis's] initial reaction to [the FFP report] contents and conclusions has been most enthusiastic.”

reports to White (29 November 1957)²¹ and to R.G. Casey (9 December 1957)²². Bowen wrote Pawsey at Harvard (care of Donald Menzel, the Director of the Harvard College Observatory) with a revealing description of his opinion of the “Preliminary” version of the design study²³:

Broadly speaking, it looks as if they have done a thoroughly good job on the alt-azimuth design and a correspondingly poor job on the equatorial.²⁴ Their description of the alt-azimuth type is comprehensive ... [For problems with the alt-azimuth design they] suggest good or even excellent cures.

On the equatorial design they begin with a very good analysis of the three basic types, and then choose for detailed study what I frankly think is the worst of them ... [T]hey do not give an engineering description but a catalogue of reasons why it isn't as good as the alt-azimuth. ...

In other words, they have compared the best they can do with the alt-azimuth with the second or third best equatorial. For the same diameter the cost of the equatorial is only 10 per cent higher than for the alt-azimuth ...

I am getting more and more short-tempered with guys who make it their life's work to give evasive answers.

On 19 November 1957, Bowen sent a report to Kerr during his extended visit to Leiden. The report was surprisingly critical of Minnett and Pawsey. Bowen complimented Minnett (“great deal of hard work”) but complained that he had not “kept us properly informed of the magnitude of the job ... As long ago as last July he was confidently predicting we would have the report in two or at worst, three weeks' time. If we had been given a more accurate picture of what was going on we would still have had complaints about FFP, but we would have been a good deal less critical of Harry [Minnett] ...” Bowen also had harsh criticism about Pawsey:

What is needed then is for Joe [Pawsey] to put some enthusiasm behind steerable dishes and to express his enthusiasm loud, long and often. Alternatively, if he doesn't do this, then for the Radio Astronomy group to do it for him. The final decision on these

²¹ Ibid

²² NAA, C3830, A1/3/11/3, Part 3.

²³ NAA, C3830, F1/4/Paw/5.

²⁴ Hanbury Brown, Minnett and White, *op cit*. “Roberts and Wallis intuitively preferred an altazimuth mounting because of its structural simplicity compared with an equatorial, and a compact and extremely rigid design [for the GRT] was evolved.” Based on the proposed small rigid hub of the telescope, the complex servo-compensated aluminium structure was not required.

things is always made by the Executive, and if there are any dissenting voices or the dead hand of unenthusiasm [sic] around, they are sure to spot it and act accordingly.

Minnett Leaves FFP for Sydney, Bowen-FFP Conflict Continues

On 22 November 1957²⁵, Freeman sent Bowen a private letter, as Harry Minnett was preparing to return to Sydney. Freeman wanted to apply some “oil on troubled waters” regarding the personal relations between FFP and RP as well as an explanation of the appreciable delays experienced earlier in 1957.

[W]e shall say farewell to [Minnett, spelled Minett by Freeman] with genuine regret on next Wednesday, although I gather we may see him back in England for a time next year. Apart from his liaison duties, the value of his assistance to us on the radio telescope report has been enormous. He has successfully contrived to preserve the necessary independence and detachment of outlook whilst at the same time working in with our people just as if he were one of them. His attitude has been that nothing was too much trouble, and I think he feels that he has made worthwhile contributions to a good job; we at any rate are in no doubt about this. ... I hope you do not ascribe any blame for the time taken on the study to Minnett, for that would be quite wrong. Criticism, if there is any, must be levelled at us, but I do not think that would be justified either, unless we are to be criticised for having gone into the matter too deeply. ... [W]e had no precedent to guide us (I don't think you would count Jodrell Bank as a very useful precedent) and so none of the answers could be jumped at from previous experience. ... We had to endure a lot of disappointment with false starts and broken promises by some of these people [Grubb Parsons, Metro Vick, Arrols etc.] but we simply had to bear with them if we were to get the benefit of their knowledge.

On the same day as Minnett's departure (27 November 1957), Freeman sent Bowen another private letter asking Bowen to perform a favour²⁶, a gesture of goodwill:

A tremendous amount of hard and painstaking work, not only by our staff but also by quite a number of enthusiasts in Grubb Parsons, Metropolitan Vickers, etc. lies behind this report and there could be no better reward to all these people who have borne the brunt of this labour, from Gilbert Roberts down, than a few simple words of appreciation and praise from you ... Apart from the encouragement our own people

²⁵ NAA C3830 Z1/14/A.

²⁶ *Ibid*

would get from such a letter from you, we should be glad to be able to send copies to all the other people who have taken part in the study.

Unfortunately, we do not know if Bowen sent such a letter to FFP. We can only imagine that there was little incentive to do this after the FFP reaction to the 21 November 1957 (see below) letter from Bowen was received in Sydney . Relations likely remained sour.

There was to be a continuation of the “troubled waters” between RP and FFP, stimulated by an impatient letter from Bowen to Minnett (21 November from Sydney) which arrived on 26 November, the day before his departure to Sydney. Clearly, at this time, Bowen had had time for a discussion with the structural experts (Technical Advisory Committee) in Australia, Arthur Wills and Roderick. Without waiting a week or two for a one-on-one discussion with Minnett, Bowen fired off a letter with three somewhat contentious questions. Minnett immediately (27 November before this departure that evening from London) showed this letter to Roberts and Freeman at FFP; Freeman prepared a “quick reply so that he [Minnett] can take [the letter dated 27 November] with him.”

An example of one of the questions from Bowen was (21 November 1957):

In calculating the deflections of the dish, how is allowance made for the strength of the spiral members? One way of doing this would presumably be to postulate the equivalent membrane which would have the same characteristics. Presumably [Barnes] Wallis knows a great deal about this and may have a set of figures which would give the required answer.

Bowen concluded the letter (21 November):

These are only a few items picked up at random and there are obviously many more. I know that FFP would like to have their word regarded as Gospel, but we are a bunch of sceptics- by now a bunch of critical sceptics- and we want persuading on these points. I am quite sure that there are complete and satisfactory answers to any questions of this kind, but that certainty should not stop us asking them. I am sending these along now as examples of the kind of question which you and probably Roberts will be asked when you come out [Roberts was due in early 1958]. I am sure that you will both make a good fist of answering them. ²⁷

As might be expected, FFP were not pleased with these comments from Bowen on 21 November 1957. Before Minnett’s departure on the evening of 27 November, Freeman wrote, likely with the concurrence of Roberts on 27 November 1957):

²⁷ Old fashion British and Australia expression: “to do something well”.

The questions you [Bowen] asked [Minnett on 21 November] are outside his province and it would not be fair to expect him to stand up to cross-examination on such structural matters. ... The report clearly states [Freeman is answering one of Bowen's queries] that the equivalent-membrane was used to design the dish structure. ([Barnes] Wallis had nothing to do with this; the work was all done here [FFP]²⁸). The method is essentially a matter of trial and error, and a good deal of both was involved ... When a satisfactory solution was reached, showing that both deflections and stresses were within permissible limits under all assumed conditions of loading, the deflection calculations were performed again as a final check ... Judging by the elementary nature of these comments [from Bowen], and the fact that the answers to most of the questions implied in them are in fact to be found in the report, I can only suggest that the people from whom the questions emanated had not studied the report very carefully when they asked them. I would like to sound a note of caution against attempts to search out minor points of criticism before properly digesting the report as a whole. Such attempts would only tend to waste your time and ours ... We have now completed our work to the very best of our ability, having probed into all aspects of the project in much more minute detail than would have been justified in the case of a subject for which substantial measure of precedent or experience already existed.

Freeman ended the letter with a conciliatory tone:

If you embark on [a recalculation] and find that there is any data or information you need that does not appear in the report, we will do our best to supply the information. I would suggest, however, that no major operation of this nature [a repeat of the calculations] be put in hand until you have had a full discussion with Gilbert Roberts [in person].

Final FFP Design Report, Response in Sydney, High Frequency Use of the GRT

In early December 1957, the final FFP document arrived in Sydney: "Proposed Radio Telescope: Design Study", with the misleading date "September 1957" on the cover. The report consisted of 120 pages with 20 drawings. Already on 18 November, Lindsay McCready had prepared a succinct 10 page summary, based on the preliminary documents²⁹. For the first time, RP

²⁸ The parentheses are from Freeman. This statement suggests that Wallis had already reduced his active participation with FFP. (see Chapter 27 and NRAO ONLINE 42)

²⁹ NAA C3830 A1/3/1/1, IX.

personnel saw the details of the ME designed by Grubb-Parsons, using the concepts suggested by Barnes Wallis and now patented by Vickers-Armstrong.³⁰

December 1957 was a busy month for the RP management and for Pawsey (in the US) as they prepared their response to the FFP Design Study. For the first time in over two years the Technical Advisory Committee met on 6 December 1957 in Sydney, with Harry Minnett's personal report. The committee was faced with many questions and dilemmas; there was a looming deadline with Gilbert Roberts's two upcoming visits during the course of January 1958, bracketing his trip to New Zealand to consult on the Auckland Harbour Bridge project. On 6 December 1957, Taffy Bowen, Arthur Wills (Assistant Controller, Research and Development, Aeronautical Research Laboratories, Department of Supply), Bill Wittrick (aeronautical engineering, Sydney University), Hugo Messerle (electrical engineering, Sydney University), Lindsay McCready, Harry Minnett and Arthur Higgs met at RP.³¹

The committee considered:

1. Was the FFP design sound?
2. Did the FFP data support the final recommendations?
3. Which, if any, of the FFP conclusions should be checked?
4. What steps should be taken to "satisfy ourselves that the performance claimed by the designers will be ... realised"?
5. Could RP make these decisions during the following month, January 1958, during Roberts's two visits to Sydney?
6. What further work on the design was required?
7. What size aerial was consistent with the specifications and the current available funds (£A 500,000)?
8. Should further work on the under designed equatorial design be carried out?
9. Should FFP's decided preference for the alt-azimuth design be accepted, in the face of the uncertainties with the equatorial design?

In the end the basic question was simply would RP accept FFP's design, even if incomplete? ³²

The scaling laws were clear with a total cost which scaled as the 2.5 power of the diameter, with the dish surface and support tower scaling as the third power and the drive components

³⁰ British Patent Application No. 29248/1955. "Improvements in Telescope Mountings". Vickers-Armstrong (Aircraft) Ltd. Morpurgo (1972).

³¹ NAA, C3830, A1/3/11/4.

³² The committee assumed that CSIRO would take all responsibility for radio frequency performance issues. They also assumed that all structural issues required outside expertise during the evaluation process.

scaling as the second power³³. The funds available at this time would accommodate a 175 foot equatorial or a 210 foot alt-azimuth dish. Also a striking fact was obvious to Bowen and Pawsey: the predicted surface accuracy of the rigid design of the 280 foot alt-azimuth dish would meet the specifications of plus and minus 1 cm at modest zenith angles.

After much discussion, the consensus was that the master equatorial of Grubb-Parsons was a workable, sound design. Some changes were required: “the performance of the ME, and particularly of the error detection system, should be reviewed ... [T]he position with regard to checking the control system as a whole was satisfactory”.

There were major concerns about the equatorial system: “[T]he report did not provide adequate data on which a fair comparison between these two types might be made. A completely independent treatment for the equatorial would obviously require considerable further work and it was therefore decided to concentrate mainly on the proposed alt-azimuth design provided that checks of the deflection calculations ... and of the control system showed that the overall performance would be substantially as claimed.”

There were concerns about the spiral purlin system: in contrast to the Wallis design, 24 radial cantilevered ribs of tubular steel (50 cm diameter) were to be attached to the central hub. From the McCready summary: “A membrane of intersecting equiangular spirals is stretched over these ribs. For the chosen f/D [focal length to diameter] of 0.41, these spirals contribute about 30 per cent of the bending stiffness and practically all the shear and torsional stiffness.”³⁴

³³ Thus each cost was proportional to the diameter raised to the N th power where N was 2.5, 3 and 2, respectively.

³⁴Pawsey family archive. About this time (10 or 11 December 1957), Lindsay McCready sent Pawsey in Princeton a handwritten letter, containing a personal account of the 6 December meeting of the TAC. Minnett had done most of the talking during the four hour meeting. Bowen and Arthur Wills were certain that CSIRO should not just trust FFP on all structural aspects of the design: “Jodrell Bank would not be in their present trouble if they had outside advice ... However Harry [Minnett] is very enthusiastic about it and says that Grubb Parsons and Metro-Vick are quite satisfied with the feasibility.” As far as the equatorial versus alt-azimuth was concerned, McCready thought “that Taffy will, reluctantly, accept the latter provided people like Bruce Rule [Caltech] etc. do not have any strong views against the FFP design. He is keen, just as keen as ever, on equatorials but realises it will delay things. He is now prepared to cut many corners to get the thing started.” McCready had convened a meeting of the RP Steering Committee (Chris Christiansen stood in for the absent Frank Kerr, who was in the US) to look after the RF and user aspects of the GRT. Important decisions over the f/D , the nature of the telescope mesh, the control system, shadowing of the tripod and the feeds had to be discussed. There was a fascinating section entitled “Gossip and Scandal-GRT”: “Relations between Taffy and FFP are strained at present. Taffy acted, as ever, rather hastily to their delays in sending and completing the design study. Harry Minnett expects some fireworks when Roberts and Taffy meet [early 1958 in Sydney]. In addition Freeman has written a ‘fatherly advice’ type of letter to Taffy. I think it will blow over ... The overall impression is [however] most pleasing. Barnes Wallis is very pleased with the design.”

Starting on 11 December 1957, Bowen and Pawsey independently came to the same conclusion: the FFP design would enable the GRT to observe at shorter wavelengths than the originally anticipated 21 cm.

On 11 December Bowen wrote Pawsey about the meeting of the TAC on 6 December 1957, describing the expected events of the new year (1958) after Roberts's arrival. The four main points communicated to Pawsey were: (1) "The idea of mounting the ME near the axes of rotation and then making the dish follow the ME ... is a clear cut advance which might go a very long way towards making the alt-azimuth type of mount acceptable." (2) Based on the FFP design, the deflections and pointing accuracy were within specification. "[T]he instrument should ... give a good account of itself down to 10cm." (3) CSIRO could "exceed 200 feet with the money at hand". (4) A number of experts outside of RP and the TAC had been sent confidential copies of the design study: Lee Dubridge of Caltech with a request that Bruce Rule of his staff "give an off-the cuff" opinion on whether the deflection and pointing accuracy would be achieved, as well as Lloyd Berkner of AUI and Merle Tuve of the Carnegie Institution of Washington.

On 28 December 1957, Pawsey wrote back before seeing the 11 December letter. Pawsey mentioned that new aspects of the GRT problem had arisen since the specifications of 1955: changes in the radio astronomy perspective and new considerations uncovered by the FFP study. Major advances in lowering system noise for cm receivers (masers, traveling wave tubes, parametric amplifiers) would permit vastly increased sensitivity at shorter wavelengths.³⁵ He anticipated that with a diameter a little over 200 feet "we may find that this itself requires more money". Also he was pleased that the focus cabin was much larger with an increased weight capacity compared to the original specifications.

Pawsey provided a thorough evaluation of the design study: "I should say that I think Freeman Fox have done an exceedingly good job. This applies particularly to the design of the dish which I regard as the heart of the problem". Pawsey accepted that the alt-azimuth design was "first class" but FFP had "not been able to produce an equivalent equatorial [dish]. They stress the standard argument for the alt-azimuth that deflections can be partially compensated owing to the reproducible nature of the main stresses, those due to dead weight [i.e. gravity]".

Pawsey's major point was that the new design would produce a radio telescope that could be used at 10 cm and even 3 cm (10 GHz). Pawsey was convinced that the alt-azimuth configuration would enhance the high frequency use of the GRT. Pawsey also anticipated a property of the GRT that was to be fulfilled in the following 50 years: adding higher quality surface panels (called "mesh" in 1957) in progressive steps (starting from the 66 foot solid

³⁵ This important point was not anticipated in the period before the mid-1950s.

surface at the centre) to improve the high frequency efficiency. He anticipated that this could be done in piecemeal fashion. He favoured an improved mesh quality for higher frequency use (say 10 or 3 cm) in place of the originally specified mesh optimised for 21 cm.

Pawsey also stressed the importance of a provision to adjust the setting of the reflector. Given the rigidity of the planned dish, he saw no need to provide a permanent error detector to measure the instantaneous departure from a paraboloid, as had been discussed in the 1955 specifications. He suggested, instead, a process of infrequent panel settings (to be carried out by Minnett, Yabsley and Puttock in future years). "I envisage the proper procedure as an original setting which is checked at the time of construction and then improved at a subsequent date when the behaviour of the dish is known." Pawsey's predictions of the high frequency use of the GRT during the following decades did come to pass.³⁶

On 13 January 1958³⁷, Bowen responded enthusiastically to Pawsey's suggestions:

I am full in favour of exploiting the high accuracy of the FFP dish to the very limit, and one of the most important points we shall be discussing with Roberts is the extent to which the mesh can be made suitable for operation at 3 or 10 centimetres. We shall have to approach this one carefully and a great deal will depend on his initial reaction. My own view is that we should go to the limit to obtain operation at the highest frequency ... [We will plan] to go for the finest mesh that we can talk Roberts into.

Then on 20 March 1958, Bowen replied to Pawsey with news of Roberts's reaction:

We looked very hard at the possibility of getting down to 3 centimetres but found that we were caught in a rapidly ascending spiral of increasing wind loads [due the finer mesh panel]. We therefore settled for 10 cm operation as the lower limit. Roberts was tolerably certain we could not go to 3 centimetres without structural embarrassment, but he was quite happy about 10 cm.

As the year ended, RP gradually came around to the point of view of FFP; with the servo and the Master Equatorial problems in hand, an optimum solution was appearing. "FFP were confident by early 1957 that an altazimuth mounting was the best solution." (Hanbury-Brown, Minnett and White in the biographical memoir of E.G. Bowen, 1992).

³⁶ In the almost 60 years of operation of the Parkes Telescope, high frequency use at cm wavelengths has become routine as the surface accuracy of the panels was improved as well as the procedures for surface adjustment.

³⁷ Pawsey archive, CASS. Ron Ekers.