

NRAO ONLINE 24

Controversy over the Million Degree Corona – 1946: Martyn, Pawsey and Bowen and the “Two Paper” Imbroglio ¹

Introduction:

A triad of three prominent mid-20th century CSIRO radiophysicists – David Martyn, Taffy Bowen and Fred White—were involved in a chronic state of conflict from 1940 to Martyn’s death in 1970. Home (2000) has written about Martyn:

... [H]e never forgave White or E.G. Bowen (White’s successor [after Briton] as Chief of RPL) for the wrongs he believed they had done to him. He successfully blocked the election of both men to the Royal Society for many years [elections of White occurred in 1966, Bowen in 1975, after Martyn’s death in 1970 by suicide] and took evident pride in demonstrating the statistical inadequacies of work on rainfall records published by Bowen in the 1950s.

Over much of the period, White attempted to mediate in the conflict, following in the partially futile footsteps of the Chairman of CSIRO (see below), Sir David Rivett in 1947. In 1942, as White replaced Martyn as Chief of RPL (see the detailed account in NRAO ONLINE 7), White had to deal with the fall-out of Martyn’s dismissal. Schedvin (1987, *Shaping Science and Industry: A History of Australia’s Council for Scientific and Industrial Research 1926-49*. CSIRO PUBLISHING) has written: “White was only a year older than Martyn, and, if anything, less experienced. He had an intense dislike of the animosity surrounding Martyn’s fall from grace, and on many subsequent occasions in a long career with CSIRO he would go to great lengths to avoid open conflict [with Martyn].”² The conflict over the recognition and publication of the hot corona

¹ Sources NAA C3830, A1/1/1, Part 1 1945-1946 and A1/1/5 Part 2

² Schedvin (1987) has also provided details of Martyn’s relationship (1940-41) with Ella Horne (née Kruse), a recent German immigrant to Australia. (See NRAO ONLINE 7 for a detailed account.) Although she was initially suspected of having “pro-Fascist” sympathies, eliciting the interest of the Australian Commonwealth Investigation Branch, “Ella was portrayed as an attractive, stylish woman of uncertain morality, German in her sympathies but definitely not ‘pro-Fascist’ ... For his part Martyn was guilty of an indiscretion, but no more. There is no evidence that he revealed any secret information. He was boastful of the importance of his position [Chief of RPL], and always keen to attract attention. Given the extreme sensitivity of RPL, he would have been wise to have been more selective in his choice of friends. On the issue of security this is the most that can be said against him.” Madsen, Julius and Rivett took “a more censorious view”; Rivett thought that Martyn had “contravened a sacred trust”. Martyn was replaced by White in the period a complex series of events in 1941-1942. . In addition, Martyn’s management style was found to be a major problem by CSIR over a multi-year period.

from 1946, which continued for many years, was only one issue that caused conflicts between the three scientists in CSIR (after 1949, CSIRO) over the next two to three decades.

As Sullivan (2009, page 311) has discussed:

One of the main influences of radio observations on solar astrophysics of the mid-1940s was assisting in the general acceptance of a startlingly high coronal temperature of about 1 million degrees K. But the exact way in which the radio observers and their data interacted with the astronomical establishment and their optical data is complex and appears to have varied from one person to another.

Since 1939-1941, Grotrian and Edlén had recognised that the previously unidentified coronal lines observed during total eclipse could be identified with highly ionised lines of iron, nickel and calcium, the forbidden lines. The lines arose from atoms stripped of 10-15 electrons; if the corona was in thermal equilibrium, a surprising temperature of 10^6K was indicated. In addition, the line **widths** indicated a large temperature. For example, Woolley (1947, "Galactic noise." *Monthly Notices of the Royal Astronomical Society* 107: 308) reported on the evidence that the Fe IV line width implied a temperature of a few times 10^6K ; the line width was large due to the high random velocities of heavy iron (55.8 times the mass of an H atom). This was the state of knowledge in 1946. A major theoretical problem remained: to provide a mechanism to transition from a photosphere temperature of 6000K to a temperature of 1 to 2 million degrees K over a short distance from the photosphere through the hotter chromosphere to the outer corona. Even with a number of proposed solutions, the problem of the source of heating of the hot corona remains an active region of research today.

By the late 1940s, both Woolley, and especially David Martyn at Stromlo, began to realise the implications of the hot corona for the new radio observations. Sullivan has written:

Why the corona was so hot was not all understood, but the evidence was there. Martyn realized he could apply standard techniques in ionospheric theory to calculate the expected radio emission from the sun. Once he had adopted likely values for the electron densities in the corona, he found that the corona was opaque at Pawsey's frequencies [200 MHz]. The observed radio waves were therefore not at all from the 6000K optical surface (photosphere) of the sun, but from well above, out in the million-degree corona. When the sun was quiet [in the radio], this coronal emission constituted the entire solar signal; when active, the coronal emission was dwarfed. (Sullivan, 2009, p. 118)³

³ Sullivan (2009, Cosmic Noise: A History of Early Radio Astronomy. Cambridge University Press, Cambridge, UK, p.135) has pointed out that Vitaly L. Ginzburg had also made similar calculations about

A month after the submission of the Collaroy *Nature* paper (of early 1946), Martyn wrote Pawsey (the salutation was a familiar “Dear Joe”) on 22 November 1945⁴. (Chapter 14) Martyn had clearly read the preprint and was becoming interested: “I am beginning to get some definite ideas on a mechanism which may be responsible for the results you have been getting in solar radiation at 200 MHz. I think it quite possible that the radiation [of the enhanced levels of solar noise] may be polarised, and the purpose of this note is to suggest that, if at all possible, you might care to make observations on this point.” In fact, Martyn himself detected the circular polarised radiation at Stromlo using the RPL supplied equipment on 26 July 1946. He submitted a paper to *Nature* on 6 August 1946, “Polarisation of Solar Radio-frequency Emissions”, published on 31 August 1946.)⁵

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From the December 1945 report by Payne-Scott (see Chapter 19), it is now quite clear that Martyn had also developed his ideas about the level of quiet sun emission the previous year. Payne-Scott wrote in her “Conclusions- Suggested Origin of the Radiation” of the summary paper:

The radiation [of the sun] may come from the corona, which has recently been shown to have a very much higher temperature than the photosphere, and which, although transparent to visible light, may well be opaque to long radio waves. Dr D.F. Martyn, of the Stromlo Observatory Canberra, has suggested this origin. This theory does not seem to account for the greatly increased radiation when at the time the sunspots actually cross the meridian [the enhanced emission].⁶

the hot corona while considering the possibility of solar radar echoes. RPL only became aware of this in 1948, long after the October 1946 paper on the hot corona had been published in November 1946. In a letter from Bowen to Pawsey in London (NAA C4659 Part 8) on 26 July 1948, Bowen wrote: “I have just seen a remarkable paper by Ginzburg [from March 1946] ... The amusing thing is that it describes the possibility of a million degree radiation being received from the sun and makes a calculation of the depth of penetration of radio waves.” The translation of the abstract (from Russian to English) had occurred only a short time earlier.

⁴ All correspondence from NAA C3830 A1/1/1 Part 1, A1/1/5 Part 1 and the Sullivan archive.

⁵ Note the short publication delay of only about 3 weeks. Martyn’s publication was followed a week later by two confirmation observations of circular polarisation of the sun by Appleton and Hey (85MHz) and by Ryle and Vonberg (175MHz).

⁶ At this time, the separation of the two types of emission (corona and “sunspot”) had not become clear to Pawsey and Payne-Scott.

The following year, Woolley, Allen (Commonwealth Observatory, Stromlo) and Martyn (RRB Stromlo) were all interested in starting an observational programme at Stromlo. The joint project was not smooth sailing after the 200 MHz Yagi system)⁷ (from RPL) had arrived at Stromlo as Woolley wrote to White on 16 April 1946:

The set Pawsey mentioned has actually arrived, though the basis on which it has come does not appear to be quite clear. May I suggest that it ... be placed on loan to the Observatory [Commonwealth Observatory] by Radiophysics. The Observatory certainly desires to borrow an instrument for observing the sun in radio wave lengths.

You should know that Martyn has all along been interested in the subject and he and Allen have already formed certain views on the mechanism by which noise is produced. ... [A]t present Martyn's team includes the operation of this equipment in their duties.

White was worried about the managerial aspects expressed by Woolley. Bowen was also aware of the problem as Bowen wrote to White on 18 April 1946: ⁸

I am sorry that Dr Woolley has taken the grim view indicated in his memorandum, but we must admit that most of the arrangements with Mt Stromlo have been done informally between members of staff without any communication between the Chiefs. This is an arrangement of which I am very much in favour, as long as it works. [It appeared to Bowen that Woolley preferred "the more formal method" of communication between Chiefs. Bowen concluded:] I have already taken steps to inform Dr Woolley more fully of our plans and intentions regarding solar noise work by arranging for Dr Pawsey to visit him immediately after Easter. Pawsey will not only bring

⁷ Fritz Goro, the famous *Life* photographer had been in Australia in March 1951 (just after Bowen went overseas). See NRAO ONLINE 23 Additional Note 1. Goro (inventor of macrophotography- "extreme close-up images", 1901-1986) photographed at Dover Heights (Bowen), Potts Hill (Pawsey and Christiansen) and Canberra (Oliphant and Martyn). Later in September or October 1951, Bowen was in Cambridge Mass at Harvard University. Goro met Bowen, Ed Purcell and Harold "Doc" Ewen for a series of photos. The article in *Life* was published on 19 November 1952 "Radio Astronomy, Celestial Sound Reveal Invisible Stars and New Facts about the Sun"; the article shows unattributed images of Pawsey and Bolton. The scene at Harvard with Bowen, Purcell and Ewen is, however, attributed, perhaps due to the Nobel Prize in Physics won by Purcell in early November 1952. No images of Oliphant or Martyn appeared in *Life Magazine*. In NRAO ONLINE 13 (Oliphant) we describe the photo of Martyn with the 200 MHz aerial supplied by RPL for radio astronomy at Mt Stromlo. The published article has no attributed author; there is no mention of Goro. Dating of these events is from NAA C3830 Z1/9, Bowen to White from the US 19 October 1951. Bowen remarked in a sarcastic tone to White after he saw a copy of the Martyn photograph (which was never published) at Mt Stromlo from March 1951: "The very first picture said: 'DF Martyn, famous Australian Physicist, Fellow of the Royal Society, discoverer of circularly polarized waves from the sun'. His technique is masterly."

⁸ NAA A1/1/1 Part 1 1945-1946

him more into the picture but will also take the opportunity of addressing the seminar of observatory staff.⁹ [It was perhaps also unfortunate that Bowen himself was not more encouraging of the new enthusiasm at Mt Stromlo. At the beginning of the letter to White, Bowen wrote rather dismissively:] We regard the solar noise measurements being made at Stromlo as a useful adjunct to their visual observations and we ourselves do not get more than incidental benefit from the existence of the set there.

By late July 1946, a number of informal discussions had occurred among Pawsey, Bowen and Martyn. Bowen, the new Chief of RPL since June 1946, summarised the understanding he had reached with the Commonwealth Astronomer Richard Woolley. This letter (under Bowen's name, drafted by Pawsey and Martyn) is key evidence of the firm understanding that existed between Stromlo and RPL on 29 July 1946:

[To Woolley]: Dr Martyn recently discussed with Pawsey and myself [Bowen] the desirability of writing a letter to *Nature* on the concept of a shell surrounding the sun at which the density of electrons is such as to give zero refractive index for each particular wavelength and from which thermal energy would originate at a level dependent on the temperature of the shell. The present position with regard to radio-frequency observational data is that we now have six [actually close to eight] months observations of intensity levels on 200 MHz. These show a pronounced tendency to occur at a level corresponding to a temperature of about 2×10^6 K ... with a fairly sharp cut off below 10^6 K and a wide dispersion on the upper-side up to 10^8 ... **The concept originated at Stromlo, but Martyn thinks that the publication would be improved by including Pawsey's data.** (our emphasis) As regards authorship, we suggest ... the inclusion of Pawsey, an arrangement which is agreeable to both Martyn and Pawsey. If anyone else at Stromlo is involved, no doubt you will make the necessary arrangements with Martyn.

Immediately, Woolley replied with another plan that modified the nature of the collaboration. On 5 August 1946, he replied to Bowen suggesting that he [Woolley] and Martyn publish a

mathematical paper dealing with the transfer of radiation ... in the corona ... It appears to me that this subject can be most profitably written without reference to any observational data about the intensity of the "noise" radiation actually received ... but

⁹ NAA A1/1/1 Part 1 1946 Britton (Chief of RP) to Woolley at Stromlo, 18 April 1946 suggesting that Pawsey might visit Stromlo in the near future to give a colloquium "on the progress of solar and cosmic noise investigations. Pawsey would also like to take the opportunity of learning more about the visual observations associated with the work."

with an eye to the application of the mathematical results to observations of noise at a later date when the noise observations have been studied further than at present ... [Martyn] and I propose to write a mathematical paper now ... and [we] will not refer to details of the observations of noise carried out in your laboratory or here on Mt Stromlo.

Before Pawsey received this surprising letter from Woolley, he had written (8 August 1946) to Martyn with a detailed plan of the proposed paper. He emphasised that the 200 MHz data was solid, showing a sharp cut-off in the distribution of intensities at the low end. The data at 60 and 75 MHz were consistent with a cut-off at about a million degrees, but was of poor quality due to confusion with “cosmic” noise and also ionospheric effects at low elevation at sunrise at Dover Heights. A very important new fact was also emphasised by Pawsey:

On 200 MHz the interference pattern [with the sea-cliff interferometer at Dover Heights] has been observed to show shallow minima when the intensity is low ... [consistent] with the idea of a distributed thermal source ... I see no reason we should not proceed to write a letter to *Nature* as originally agreed. If it is not done soon it will almost certainly miss the bus as other workers will start to investigate causes for high radiation levels. Further, no immediate significant progress appears feasible on the observational side, and on the theoretical side a detailed study is required. Publication in this manner would have advantages, other than the claim to priority, in that it would provide an opportunity to collect low intensity observations from other workers, and could also be used to give notice that a theoretical paper was in ... preparation.

On 12 August 1946, Pawsey had seen the letter from Woolley to Bowen from 5 August. Communication became somewhat confused as letters crossed in the post. Pawsey had immediately telephoned Woolley to discuss the change in plans and wrote back to Martyn (“Dear David”). He agreed with a theoretical paper by Martyn and Woolley: “... I would be only too pleased to see this work proceed ...” But he was surprised:

With regard to the proposed letter to *Nature*, Woolley mentioned that he was against it at the time that you proposed a joint letter by Allen, you and me. I shall be seeing Woolley at [ANZAAS on 19 August 1946 in Adelaide] and propose to discuss things further with him. Perhaps you could tell him your present views. The reason I have been a little worried about this letter is that we had made definite arrangements to write it and then you [not Woolley?] suggested cancelling these, although the 200 MHz was quite unchanged and the 60 MHz evidence did not prove or disprove the general ideas present. However, let us make no decision until after my discussion with Woolley.

On 15 August 1946, Martyn replied to the letter of 8 August (Martyn had not seen the 12 August letter). Martyn had become nervous about the poor quality of the 75 MHz data, “which I interpreted as not fitting in to my concept.” But based on Pawsey’s reassurance, he was “happy to go right ahead at once. I shall assume this position is correct and draft a letter in both our names at once. It will be as short as possible in order to make it easy for *Nature* to publish quickly.”

However, a few weeks later (4 September 1946), Martyn delivered a “bombshell”. He had prepared a paper on his own which had been submitted on the same day to *Nature*, with no input from Pawsey! Martyn gave his reasons:

I have made considerable progress with the theory of quiet sun temperature [thermal] radiation since I wrote to you last. I have got out the effective solar temperature and variations of limb brightening over the radio spectrum... It was my original intention to present this in full detail only in a paper, but I am concerned about the consequences of Bowen’s discussion of my basic ideas in England. [Bowen was in the UK, US and Canada from September to December 1946 giving talks on radio astronomy – e.g. the Cavendish Laboratory¹⁰.] In the circumstances, it seems wise to publish a summary of the main theoretical conclusions at once in *Nature*, and I have prepared the enclosed note for this purpose ...

The way is now open for anyone (including yourself and team) to publish observational material confirming (or controverting) my theoretical finding.

Clearly Pawsey rang Martyn as soon as the surprising letter (as well as the text of the paper which arrived on 9 September 1946). Pawsey immediately saw a way to salvage the situation: send in a second paper (the “two paper solution”), a proposal accepted by Martyn. Pawsey wrote on 10 September 1946:

As agreed in our telephone conversation of [9 September, Monday], I am enclosing a draft of a proposed letter to *Nature* for publication immediately following the one you are drafting. This change of plan from the original proposal of a joint letter of less content that you now envisage is acceptable to me, subject to your making certain small

¹⁰NAA C3830 Z1/9/1946. Bowen reported to Pawsey news of the initial two-element solar interferometry that Ryle was carrying out at 175 MHz. Also, there was interest in the thermal radiation: “Ratcliffe is very interested in our theories of thermal radiation, but is not convinced about them yet. The main reason for the doubt on his and Appleton’s part is that they usually stop recording during quiet periods.” Bowen implied that the quiet sun was not detected since they were not observing during periods of no enhanced emission! However, Pawsey and Yabsley, in their definitive paper from 1949, “Solar Radio-Frequency Radiation of Thermal Origin”, re-analysed the Ryle and Vonberg data of 1947 at 80 and 175 MHz. The signature of the quiet sun was indeed detectable in the Cambridge data.

alterations in the [final section of your paper] dealing with the observational verification ... I think the material is first-rate and should be a decided stimulus to observation and interpretation.¹¹

Pawsey sent his paper (a telegram of only 393 words) to London via the ASRLO office at Australia House on 18 Sept. 1946; the ASRLO officer was Mr Lewis, whose first name was also Lewis! A letter also was sent to the editor of *Nature* (L.J.F. Bimble) on this date pointing out the rationale of the two adjoining papers, Martyn and Pawsey. "I have discussed the matter with Martyn and we consider that the value of our respective contributions would be materially enhanced if they could be printed one following the other in the same issue of the journal."

Martyn wrote a similar letter: "... [Pawsey's paper] appears to give considerable support to the theoretical deduction in my communication." Martyn also asked that the paragraph (see footnote 9) concerning experimental evidence be dropped. Martyn's final paper does not acknowledge Pawsey or the RPL; he thanks Woolley and Allen "for much advice on solar data". Pawsey wrote in the acknowledgements to his short paper: "I am indebted to Dr D.F. Martyn for pointing out to me the probable existence of high-level thermal radiation, and to members of the Royal Australian Air Force and of the RPL who took the observations.'" The details of the two papers are provided in Chapter 21 and NRAO ONLINE 20.

At this point, Bowen was in London, apparently worried that all was not well. He sent a telegram on 17 September 1946 to Pawsey back in Sydney¹²: "For reasons you will understand I would again urge early as possible publication of evidence for thermal radiation from the **corner** [*sic* corona]. Someone in Sydney wrote a series of question marks "???" after the last word "corner". On 26 September 1946, Bowen wrote a handwritten letter to Pawsey [again "Dear Joe"]¹³:

I am terribly sorry you have had so much trouble with Martyn. He has played exactly up to form and I have written a strong letter to White on the subject. Under the circumstances I am sure it is better to hold up publication of both his and your letters

¹¹ Later a number of CSIR colleagues, and especially Bowen, criticised Martyn for having failed to include any reference to the base-level in the radio data (in the paper of 2 November 1946), caused by the hot corona. Ironically the first version of the paper (NAA C3830 A1/1/5 Part 1) did in fact include a paragraph that Pawsey had initially suggested. The key sentence that was dropped in the second version was: "...[The results of Pawsey et al *Nature* paper with Collaroy data of 9 Feb. 1946] at 1.5 metres gives a temperature of slightly less than 10^6 K at periods of negligible sunspot activity." On 10 September 1946, Pawsey asked Martyn to drop this sentence since with the juxtaposition of the two papers, all the observational evidence was to be presented in the second (Pawsey) paper.

¹² Apparently, Pawsey had asked for advice from colleagues in the UK [including Bowen] about the question of publication of his own paper.

¹³ Pawsey family archive.

rather than let Martyn get away with it ... I have asked the Executive to give serious thought to stopping Martyn's letter ... Nothing could be the clearer that it is a combined effort in which yourself and Martyn are the principles ... I take the strongest possible view therefore that the letter in *Nature* should contain [his theoretical suggestion and your experimental verification], written by Martyn and yourself with the right acknowledgements. Martyn can then, to use his own words, be free to go ahead and publish any amount of further speculation on the subject under his own name. Let me say again how sorry I am that this has happened and how much I hope it can be rectified ... I have left it to the Executive to take the necessary action. I am sure you will keep closely in touch with White on the subject.

A month later (22 October 1946), Pawsey (Acting Chief of RPL while Bowen was overseas) wrote Bowen in Ottawa, Canada, with a report of activities at RPL¹⁴. Item number 6 was about the confused "two paper solution":

I am very sorry that you had to become involved in this rather unsatisfactory business. When the dispute originally arose I decided to divide the material into two letters and be rather punctilious about not including any ideas in my letter which could possibly be attributed to Martyn. [After your letter to White arrived¹⁵] [White] discussed the matter with me and I could see no alternative but to consistently back my own decision. I feel this was the best thing to do and since [Lewis] Lewis had already passed my letter into *Nature* I think the matter can now be left.

The archival evidence (e.g. the statement in the Payne-Scott December 1945 report) is that Martyn did originate the suggestion that a hot corona was likely well known before there was clear evidence of the base-level at 200 MHz. After the experimental evidence was solidified in early 1946, Martyn then constructed a detailed theory. Sullivan (2009, p.136):

Nevertheless, it seems that Martyn was indeed the one who brought in the previously astronomical evidence of a million-degree corona and who pointed out that the million-degree "effective" or "apparent" temperature cited by the RPL group could actually represent *thermal* emission from the solar atmosphere.

Martyn himself gave a plausible account of the sequence of events in a letter he wrote to Appleton during a visit to London in 1948. On 27 October 1948¹⁶, he wrote Appleton with his comments on the draft document for URSI Special Report No. 1 "Solar and Galactic Radio Noise", finally published in 1950. Sullivan (2009, p. 136) has provided an amusing description of

¹⁴ NAA C3830 Z1/9/1946.

¹⁵ Not located in the archive.

¹⁶ Sullivan archive, originally from the Appleton archive.

Martyn's actions: "Martyn claimed to be holding the epistemological high ground." Martyn wrote:

There is a natural tendency now to look on my theory as one designed to explain the observed facts, which followed rapidly upon its heels. In point of fact it was developed ... before the facts were known. It is a theory of prediction rather than explanation, and perhaps has correspondingly greater weight because of this.

However, the damage was clearly done. The factors were (1) Martyn's precipitous withdrawal from the collaboration on 4 September 1946 and (2) his apparent lack of acknowledgement of the contributions provided by Pawsey and Payne-Scott and others at RPL.¹⁷

The "Two-Paper" controversy continued for some years. Stanley Hey in his remarkable book *The Evolution of Radio Astronomy* (1973, London: Elek Science) misinterpreted the narrative:

The intense sunspot and flare emission were exceptionally startling phenomena, but it was characteristic of Pawsey that he should be seeking the answer to a simple basic question, namely, what really was the base level of radio output from the Sun in its quiescent state? Daily recordings enabled him to assess residual component in the absence of enhanced emission. The lowest level could be expected to represent thermal radio emission from the Sun. Curiously enough, the radio brightness temperature turned out to be about a million degrees at 1.5m wavelength. During a visit to CSIRO, D. F. Martyn, well-known for his theoretical ionospheric research in Australia, was shown Pawsey's analysis. Martyn pounced on it with the alacrity of a theorist presented with a key experimental result. He at once realised the explanation, the quiet Sun radiation was emanating from the solar corona where the electron density would be sufficient to render it opaque at metre wavelengths. Previous evidence of extremely high temperature in the solar corona, about a million degrees, had been derived from optical

¹⁷ An exception is the extensive publication by Martyn in 1948 "Solar Radiation in the Radio Spectrum I. Radiation from the Quiet Sun", in the *Proceedings of the Royal Society*, April 22, 1948. The author thanked Woolley and Allen as well as Pawsey: "The author also wishes to acknowledge the benefit from frequent discussion of solar noise problems with Dr J.L. Pawsey and the staff of the RPL of the Council." A publication by Woolley ("Solar Corona" in *The Australian Journal of Science* from 21 October 1947) has a section: "The Corona as a Source of Solar Noise". Woolley only referred to the Martyn publication of the previous November with no discussion of the Pawsey data. In addition, Woolley included a disingenuous statement: "It should be noted that the intensity of the quiet solar noise on 200 MHz observed on Mt Stromlo leads to a temperature of about half a million degrees for the corona ..." With the lower sensitivity of the Stromlo radio telescope, the noise level at 200MHz (few times 10^5 Jy) exceeded the expected level of the quiet sun (10^5 Jy). The summary paper (Pawsey, J. L., and Yabsley, D. E. (1949). "Solar Radio-Frequency Radiation of Thermal Origin." *Australian Journal of Scientific Research A Physical Sciences* 2: 198) contained no Stromlo data, due to the limited sensitivity with the smaller aerial.

spectral lines, although the heating process was a matter for conjecture.

Hey had misinterpreted Martyn's role. Not surprisingly, when Sullivan interviewed Bowen on 24 December 1973, Bowen was excited:

[Hey] got it dead right. Martyn was a rogue-- he tried to make out that he thought of it all first and then told Pawsey to take the measurements. Absolutely wrong-- Pawsey had done them first and he at once knew the answers ... Martyn had been the first Chief of the Division of Radiophysics and got the sack, and I replaced him. So it was a very difficult situation.

As we have seen, Bowen's memory of the events of almost thirty years earlier was faulty.

Bowen versus Martyn –Continuation, January-June 1947

The new year, 1947, brought no relief to the chronic conflict between the "Scots" Martyn and the "Welsh" Bowen. On 28 January 1947¹⁸, Bowen wrote Martyn continuing an in-person conversation from the previous Friday (24 January). "I would not have referred to our conversation of Friday [presumably a heated exchange] were it not for your insistence on the importance of the written as against the spoken word." Then Bowen quoted from a press **release** from the previous January (1946) which mentions a million-degree temperature observed on the sun; it is not at all clear if this was referring to the quiet sun! But Bowen claims it was decisive: "I think you will agree that there was some foundation for my claim that we knew about million-degree radiation from the sun some time before you came along." It is no surprise that a reply from Martyn has not been found.

The opening salvo that continued the conflict was launched the same day, 28 January 1947¹⁹, in a letter to the secretary of the CSIR, George A. Cook. Bowen had lost patience with Martyn, who appeared on the "Estimates of RPL" (i.e. on the official payroll) and had been seconded for duty at Stromlo for some years. Bowen:

Since my return from overseas [in December 1946] I have become conscious of the fact that these privileges [of a staff member of RPL] are being abused to the detriment of the work of the RPL and the research ability of the staff in a manner which is detailed in other correspondence ... Two [possible solutions] were proposed: (1) Martyn remains on

¹⁸ NAA C3830 A1/1/1 Part 2.

¹⁹ NAA C3830 Z1/29 (correspondence Bowen –Martyn).

the staff of the RPL and comes under my supervision for all aspects of his work. (2) He is removed from the Estimates of the RPL.

The next day (29 January 1947), Bowen wrote a 2 ½ page letter to White²⁰ with a laundry list of complaints. “I am sorry to say I have fallen out with Dave Martyn again.” His major complaint was the collaboration on solar noise and the “two paper controversy”. He brought up the lack of acknowledgements to RPL in the three *Nature* papers by Martyn of the previous year.

... As you know, Pawsey began the solar noise work almost entirely on his own in 1945. It became obvious that we would gain immensely by discussions with Stromlo, and we took the question up with Woolley and Allen. Martyn became interested, and he revealed an unexpected talent for interpreting solar phenomena. He saw the importance of the work and rapidly became our main contact at Stromlo. Since that time, we have adopted a policy of maximum collaboration, having full discussions on our experimental results as they appeared, making equipment for Stromlo ... This has undoubtedly been very profitable and allowed both of us to proceed much more quickly than would otherwise have been possible. We have been careful to acknowledge this fact in all our publications by direct reference to individuals at Stromlo ...

You are already aware of the other case in which I objected strongly to Martyn’s solo publication of work on thermal radiation, after he had definitely agreed with me to write a joint letter with Pawsey ... He gives no hint [of the impact the experimental evidence provided by RPL] in his write-up and, instead, makes very definite suggestions to the contrary.

Finally, I find that in his frequent visits to the RPL Martyn has been making much greater use of our facilities than I am prepared to grant. He uses our mathematicians and computing service, our reproduction facilities, and quite a large proportion of the time of our solar noise group is spent constructing equipment for him. The work of our own people is suffering as a result, and I cannot allow this to continue. I have therefore asked the Executive [the letter the previous day to Cook] for a clarification of Martyn’s position and his relation to the Laboratory ...

I have therefore decided to cut down dramatically on our collaboration with Martyn. I realise that this will often lead to delays in publication of interesting results, but I believe it is more important to do justice to our young men and give them the opportunity of doing some thinking for themselves. I am sure you agree with this point of view.

²⁰ NAA C3830 Z1/7/B Part 1

After a delay of a month (24 February 1947), White asked Pawsey to provide a copy of **all** the correspondence from late July to late September of the previous year. There were 12 letters involving Bowen, Woolley, Martyn, Pawsey, Cook, the Editor of *Nature* and Lewis Lewis at ASLO in London. Pawsey sent the list on 24 February 1947.²¹ At almost the same date, Cook wrote Bowen about Martyn; the CSIR Executive had considered the case and needed more information! They needed “specific charges in 1947, giving details of when Martyn has abused the privileges of the RPL to the detriment of that Laboratory.” Note surprising, Bowen essentially refused. On 25 February, he wrote Cook:

My principal concern ... was in seeking enlightenment on [Martyn’s] status and I had no wish to make complaints against him. I am still reluctant to do so and feel that no good purpose would be served by a detailed enquiry into past events. I sincerely hope ... it will not be necessary to have a round table conference as you suggest. I think it is well known that we have difficulty in getting along with Martyn, and that this was the reason for his secondment to Mt Stromlo in the first place. Differences have arisen with him again, and no one is more sorry about it than I am. I feel sure they have occurred because there is lack of a firm definition of his position and responsibilities, and I am only asking that these be clearly defined. If this is done I am confident that similar troubles will not arise in the future.

It is likely that this conflict was an almost “too hard” problem for the CSIR Executive. After an additional delay of about three months, the next move was made. A committee of three, with Madsen, Bowen and Woolley, would attempt to mediate and coordinate Martyn’s activities at Stromlo as a CSIR Officer of the Radio Research Board. The ground rules were summarised by Sir David Rivett, Chairman of CSIR, on 5 June 1947 to Bowen²²:

[Martyn] ... continue on the staff of the RRB; but as a CSIRO Officer he will be welcome at, and will receive full and free cooperation from, the RPL. In particular, he will work in intimate association with Dr Pawsey; but he will keep the Chief [Bowen] informed of all his activities and will be accorded the latter’s support just as though he were a member of the Division’s staff. Matters concerning RRB, RP and Stromlo will be discussed as frequently as may be necessary by a small Committee of Sir John Madsen, Dr Bowen and Dr Woolley; any differences between officers will, in the first place, be referred to this Committee.

²¹ NAA C3830 Z1/29

²² *Ibid*

Does this seem to cover what we did, or, should I say, what we attempted to do? It is always difficult to put these things into words, and perhaps it may be inadvisable to attempt it; but in this particular case I have a hankering after definition.

This statement indicates that the Martyn affair was a bureaucratic tangle; clearly Rivett was at a loss. Martyn received a similar letter. Likely, the chaos continued.

In May 1948, while Pawsey was in the UK, he corresponded with Bowen about the text of the extensive summary thermal radiation publication “Solar Radio-Frequency Radiation of Thermal Origin” by Pawsey and Yabsley, finally published in mid-1949 after an inordinate delay. Pawsey had sent a draft to Martyn for comments, who had suggested a change to a sentence in the version of the paper: “This led to the suggestion by Martyn that the steady background of radiation observed in the absence of sunspot activity was thermal in nature” be changed to “It was suggested by Martyn that a relatively high background of thermal radiation should exist.” Bowen responded on 18 May 1948²³ agreeing to the change. He added: “We are all very amused by the way that Martyn continues to ask for modifications in our papers where his name occurs. It is happening too often to be accidental.”

After the Rivett “summit conference” of early June 1947 and his letter to Bowen of 5 June, Bowen responded to Rivett on 10 June 1947 with a note of optimism. “I agree fully with the form you suggest and will do my best to adhere to the letter and spirit of it.”

However, something must have gone wrong; as expected, the conflict with Martyn did continue. At an unknown date later in 1947 or 1948²⁴, Bowen wrote a draft letter to White, again with an exasperated tone; the source of the immediate controversy is not known. This letter has been located in the National Archives of Australia, C3830, Z1/7/B, the only document in this category (“Collaboration with Mt Stromlo on solar noise and Radio Research Board”). This letter is not in the collection of “Collaboration with D.F. Martyn from September 1946 to June 1947, 35 pages Z1/29”. There are two pencil messages on the draft: (1) “**NOT SENT**” in Sally Atkinson’s handwriting in pencil and (2) “**Do not destroy but bury deep**” in Bowen’s handwriting (in pencil) at the top of the page.

This unusual letter is an unsent letter from Bowen to Fred White, 1947²⁵:

²³ NAA C4659 Part 8

²⁴ From the context and references the time was well after June 1947 and before March 1949.

²⁵ cf “The Lost Art of the Unsent Angry Letter”, by Maria Konnikova, *New York Times*, 22 March 2014. Such letters were written by President A. Lincoln, e.g. an angry unsent letter about R.E. Lee’s escape from Gettysburg to General George G. Meade (Civil War, 1863). Konnikova: “Lincoln was hardly unique: Among public figures who need to think twice about their choice of words, the unsent angry letter has a venerable tradition. Its purpose is twofold. It serves as a type of emotional catharsis, a way to let it all

I thought it might be useful if, for your personal information, I summarised our present position on collaboration with Stromlo and the RRB ...

First of all, everything appears to be going moderately well between Woolley and ourselves. We have arranged a series of joint colloquia to be held alternatively at Mount Stromlo and Sydney; we are getting all the data we ask for from them, and the RPL is proceeding with the construction of a 60 MHz set with polarisation aerials and an improved 200 MHz set for solar noise work at Stromlo ... Unless anyone throws another bomb [e.g. the 4 September 1946 letter- the “bombshell letter”- from Martyn to Pawsey, see above and Chapter 14], I think things should continue to go smoothly with Stromlo for some time. The only worry I have is that Woolley continues to avoid asking directly and specifically what he wants.

The discussions with Sir John [Madsen] and Martyn are another matter. [Bowen will not go into all the details], [But] I have made specific proposals of the basis on which collaboration should take place. A copy of these is enclosed [unfortunately not in the archive]. They are not acceptable to Madsen and Martyn, and the main objections they have made to me are: (1) They do not wish to state a definite programme and (2) They deny us the right of taking time to consider and analyse our experimental results before disclosure to them.

Indeed, Martyn asks and, in fact, practically demands, free and unrestricted access to the RPL and full discussion of the results of our work as it proceeds. Any disputes as to originality of ideas and authorship would be settled afterwards by the committee [of three] ... of Madsen, Woolley and myself. I object to Martyn having free access to the Laboratory because this has been the cause of all the trouble in the past. I insist on the terms of collaboration being clearly defined in writing and, in particular, reserve the right, which every experimentalist has, of not discussing our early experimental results until we have had a reasonable opportunity of analysing them for ourselves.

Quite apart from differences between Martyn and myself, I think you put your finger on the root cause of the trouble during our telephone conversation. CSIR is at present maintaining two organisations for radio research. There may be good reasons for this, but I can see no good reason why they should be doing the same work.

out without the repercussions of true engagement. And it acts as a strategic catharsis, an exercise in saying what you think, which Mark Twain (himself a notable non-sender of correspondence) believed provided ‘unallowable frankness and freedom’’. Other examples of non- sent letter writers were Churchill and Truman.

At present both the RRB and ourselves are doing ionospheric work and both doing solar noise. Couldn't there be a firm division of responsibilities? We would gladly give up our ionospheric work if, at the same time we were relieved of embarrassment on solar noise [i.e. RPL was allowed to be the only solar noise institute]. I would like to discuss this with you when you come to Sydney.

What was the impact, if any, of this "letter"? After the early 1950s, there were few RPL publications on ionospheric research. Pawsey's last publication was a 16-page paper "Ionospheric Thermal Radiation at Radio Frequencies" by Pawsey, McCready and Gardiner in 1951. See NRAO ONLINE 22

After mid-June 1947, Martyn wrote only one more paper directly related to solar noise research, "Solar Radiation in the Radio Spectrum- I. Radiation from the Quiet Sun" in 1948, an extensive elaboration of his *Nature* paper of November 1946. Most of the publications throughout the rest of his career were related to the ionosphere (e.g. tides in the ionosphere). His last publication that we have found on the Astronomical Data System listing is from 1964, with T.W. Davidson, "A Supposed Dependence of Meteor Rates on Lunar Phase" in the *Journal of Geophysical Research*, 1964, vol 569, page 398.

He moved from Stromlo (where he had been stationed since December 1944) to the Radio Research Board station at Camden in 1956²⁶; this became the CSIRO Upper Atmosphere, "specially created for him with the CSIRO" (Home, 2000). Martyn experienced a tragic end of life on 5 March 1970, committing suicide at his home in Camden NSW. He had been plagued by mental illness (Melrose and Minnett, 1998) for some years. See NRAO ONLINE 7.

²⁶ NAA C3830. E2/2 Part 2. 7 December 1953, White asked Bowen's opinion: "[The CSIRO Executive] will shortly be discussing ... the proposal from the RRB to purchase the large house at Camden as a headquarters for D.F. Martyn. Before the discussion takes place, I should like to hear from you whether space in this building or on the 60 acres surrounding it would be likely to be of use to the Division of RP ... If the RPL wanted space at Camden ... this might be an added reason for purchasing the building" Bowen replied immediately (15 December 1953) in a sarcastic letter: "If the purpose of the RRB proposal is [to buy the large house in Camden for Martyn], I would regard it as a very extraordinary proposal indeed. It might, however, serve as a wonderful precedent for the rest of us in old age. I am all in favour of the proposal as long as it is applied indiscriminately to all ex-Chiefs, whether they leave CSIRO with honour or without. [Bowen stated that RPL would have no interest in the Camden site, an ironic statement since RPL did consider a nearby site for the GRT a few years later (see chapter Chapters 36-41). Bowen did say that RPL was looking for an over-the-water site [e.g. a site on a sea-cliff] for a pulse transmitter to study the ionosphere.] "If, of course, CSIRO would contemplate the provision of a lake (preferably with fish) some two or three miles across near the site mentioned, we might be persuaded to change our minds."