NRAO ONLINE 54

HI (Hydrogen 21 cm Line) -Time Line of Early Australian Research: 1946 to 1986 [1]

1946-- Reber met Hank van de Hulst, preparation of "Radio-Frequency Investigations of Astronomical Interest" to be sent to *Observatory* January 1947[2]

Summer 1946 – visit of Henk van de Hulst to Reber's home radio observatory Wheaton Illinois. Although Reber took a "dim view" of van de Hulst's suggestion to look for the 21 cm line, he "did not , however, completely discard the idea… he started the development of a 21 cm spectrometer….But although various components were built, the project ended prematurely when Reber dismantled his antenna and moved to the National Bureau of Standards in [June] 1947." (Sullivan, W. T., III. (2009). Cosmic Noise: A History of Early Radio Astronomy. Cambridge University Press, Cambridge, UK p.397).

22 October 1946 – Jesse Greenstein sent a draft of the review paper ("Radio-Frequency Investigations of Astronomical Interest" published in February 1947) to Reber for comments. He hoped to send the paper to *Observatory* which would publish the manuscript promptly. On 24 October 1946, Reber replied from Wheaton to Greenstein at Yerkes Observatory in Williams Bay, Wisconsin. He approved the manuscript. In addition, he had a few remarks about a paper by Charles Townes who was working on using free-free emission to understand the galactic background at frequencies from 9.5 MHz to 3 GHz (published in March 1947, "Interpretation of Radio Radiation from the Milky Way", *Astrophysical Journal*, vol 105, p.235).

3 November 1946 -- Reber to Greenstein. For the *Observatory* paper, Reber had re-evaluated the calibration of his earlier 1944 160 MHz solar data. The correction for the intensity was about a factor of 300! With this correction the brightness temperature for the sun at 160 MHz was about a million degrees. Earlier in 1944-1946, Reber had then asserted that his 160 MHz

[1] See Sullivan (2009, *Cosmic Noise- A History of Early Radio Astronomy*, hereafter [CN], chapter 16 "The 21 cm Hydrogen Line"). Source material NRAO Archive Reber Collection, Joe and Lenore Pawsey Family Collection, National Archives Australia, NAA C3830 A1/3/17 Part 1. Many details are discussed in Chapter 20. The inquisitive reader might be interested in the insightful analogy that Henk van de Hulst exposed to Grote Reber on 9 September 1948 as he departed from the US after a two-year visit to Yerkes Observatory. He compared the new 480 MHz image of the northern sky to a treasure map on a hidden island. See the entry below for 9 September 1948. There are a number of references to ESM (Electronic Supplementary Material) texts in the main book online.

[2] Sullivan (2009, p 68) has described the Observatory paper as the "a review of radio astronomy…, the first of its kind and in fact the first collaboration between an astronomer and a radio observer. By that time 48 citations were necessary for an exhaustive coverage of the world-wide literature; in addition, Reber and Greenstein reported many ongoing unpublished studies of cosmic noise". The two authors emphasised that major problems remained with the calibration of the existing radio data; only order of magnitude agreement was expected.

data was consistent with "Southworth's microwave data as showing a blackbody spectrum consistent with a temperature of 6000 K." (CN, p. 66 fn31). This new finding in November 1946 removed the contradiction with the radio bright corona discovered earlier by Pawsey and colleagues at Collaroy in 1945 at 200 MHz (see Chapter 14). Now in addition, Charles Townes (CN, p.135) had discovered the calibration error with the Reber 160 MHz solar data. In the first draft of his *Astrophysical Journal* paper (see above), he had asserted that the excess radio noise found at 160 MHz by Reber was consistent with the million degree corona. However, in a version of Townes' paper submitted to the *Astrophysical Journal* in September 1946, this claim was rejected by the journal editor and referees. After removing the text about Reber's solar detection, the final version of his paper was submitted on 20 December 1946 and published in March 1947. (Sullivan, 2009, p.135).

4 November 1946 --Greenstein to Reber. Greenstein would update the calibration of the 160 MHz data in the proofs of their publication. Thus, a new conclusion was possible: the Reber 160 MHz solar data was consistent with the Australian data at 200 MHz.

13 November 1946 --Reber to Greenstein. As a postscript to a letter about a possible Office of Naval Research proposal for a radio observatory project, Reber mentioned a spectral line project. The text indicated that Reber was confused: "I think I can borrow a search receiver with which to look for radiation from the **negative hydrogen ion** (our emphasis). I don't remember the exact frequency you gave in our paper. Was it 1440 MHz? And what likely line width? If I recall correctly this emission band should not only be in the sun's atmosphere but also n the gas in space in the direction to Sagittarius."

1947-- Pawsey to the US in November

10 July 1947--Pawsey to Reber. Acknowledges receiving reprint of Reber-Greenstein " Radio-Frequency Investigations of Astronomical Interest" in *Observatory* February 1947.

18 July 1947-- Reber to Pawsey. Moved to National Bureau of Standards, Central Radio Propagation Laboratory (until northern hemisphere spring 1951)

21 July 1947--Bowen to Reber. Receipt of Reber-Greenstein publication. Likely, neither Pawsey or Bowen read the brief discussion in the paper about the 21 cm H line prediction by van de Hulst.

20 August 1947--Reber to Pawsey. He was to meet Pawsey at his new address in Washington at the NBS- National Bureau of Standards (Central Radio Propagation Laboratory) in November 1947.

5 December 1947--Pawsey gave colloquium at NBS about radio astronomy in Sydney.

1948-- Pawsey learns about the HI Line from Reber at the Central Radio Propagation Laboratory of the National Bureau of Standards, Washington, D.C.

23 January 1948—Conversation Reber and Pawsey. A key event occurred in January as Pawsey continued his conversations with Grote Reber at the NBS. On 23 January 1948, Pawsey (in DC) wrote Bowen in Sydney. "..[T]he general position [in the US] is there is a considerable degree of interest [in the RPL results], but practically no observational work going on [in the US]. …Mr Reber gave me some very valuable information. He tells me that there is an absorption line of hydrogen atoms on a frequency of 1420.4 MHz; and also one for deuterium at 327 MHz. This is derived from theory and from laboratory work which he thinks is published but which I have not seen. It may be in the *Physical Review* and is probably by the Columbia University people [I.I. Rabi and colleagues, see Chapters 38 and 40]. If this is correct, there may be very considerable interest in searching for either cosmic or solar noise absorption or emission bands at this frequency."

15 April 1958-- Pawsey's report "Solar and Cosmic Noise Research in the US and Canada". The 3 ½ page report was sent to the RPL in Sydney for distribution among scientific staff. "….. [T]he Australian work, Stromlo and Radiophysics, has not here had any serious competition in the solar field. In the cosmic field our work has supplied a very vigorous stimulant to work which was progressing slowly chiefly under the impetus given it by an amateur, Reber, working alone in his spare time. The position now is that the astronomers of the US, who form a group who maintain very close contact with one another, have now become thoroughly interested in the implications but have not yet taken the plunge of tackling a totally new technique. Meanwhile, the physicists who at the close of the war had the skill and inclination to undertake the radio side but failed to interest the astronomers then, now have other interests. The result is that we have a first class opportunity to establish the lead which we at present hold." Pawsey repeated the message from 23 January 1948 (message to Bowen) about the HI line at 21 cm. "It opens up the possibility of determination of constitution of matter and of Doppler velocities in a manner analogous to optical spectroscopy." He ended with a pessimistic note: "Lamb, of Columbia, for example, did not expect we should be able to find lines owing to low probabilities of emission or absorption and 'smearing', due to changes due to magnetic field and so on."

Pawsey had written a section of his report containing a number of frank assessments of colleagues he had met in the US and Canada. The most insightful praise was reserved for Grote Reber: "Among the radio workers, Reber impressed me very favourably. I forgive him his imperfections in his papers when I consider how he worked, along with no encouragement, working in his spare time and buying equipment with his own money. He lacks the research background which many of us have, but I believe he 'has what it takes' to make a success of things. He is a young bachelor and has a delightfully direct personality. My feeling is that if there is anything we can do to help him along, let us do it. He will give back as much as he gets…. In Toronto Williamson is a young astronomer who is thoroughly interested in one

subject, [radio astronomy]. [see NRAO ONLINE 26, “Pawsey Connections with Canada 1941-1957”] In fact, he is acting as a sort of promotor of such research. He was formerly at Cornell and says he and [Charles] Seeger prompted Burrows to start. He has now written a review for the Canadian Astronomical Journal to attempt to push the subject among Canadian astronomers.” Pawsey was also impressed with the quality of the observatory directors he met: Kuiper (Yerkes), Oort (Leiden), Strömgen (Copenhagen), Spitzer (Princeton), Goldberg (Michigan) and Shapley (Harvard). Finally, Harvard solar physicists he met were to play a role in his future endeavours, W.O. Roberts and Jack Evans.

27 April 1948- Pawsey (now in the UK) to Williamson, University of Toronto

Pawsey had visited Williamson earlier (“the most helpful contact”, who promised a report on the HI line with the assistance of a student Reeson). In the 27 April letter, Pawsey asked for an update: “When we last met you and I and Reeson (or Ressor?) were discussing the radio frequency spectrum of hydrogen and you were planning to try to find out the details and write it down. What luck have you had? I thought I would try a little investigation on my own and asked the Columbia people. I got nowhere. My conclusion is that a lot of people know a lot about separate details, but I found no one with a general picture. Hence I think the information you were planning to get would be thoroughly useful and possibly would make a paper for publication. At worst it could form the base of a first class report for the Cornell project [Willamson had a part time appointment with the Cornell group of radio astronomers, Barrows, Seeger et al.].” Pawsey repeated his offer for Williamson to spend some time working at RPL in the future. (NRAO ONLINE 26).

18 May 1948 --Letter from Bowen in Sydney to Pawsey in the UK. “This possibility [of a line due to HI] is certainly an interesting one but, in view of the present state of knowledge, I doubt very much whether we should yet devote a special effort to it. A search for the atomic and deuterium lines could be made with the Georges Heights equipment but this would involve dislocation of other work which is scarcely justified at present. At the moment Harry Minnett is chasing up on the references you supplied and we are hoping that Williamson will live up to the promise he made you to let us have a survey of the whole subject.”

18 May 1948- Report from Pawsey in the UK to Westfold, McCready and members of the Radio Astronomy group at RPL “on planning research along ‘theoretical grounds’ or ‘exploratory grounds’ and on conditions of success (see Chapter 33). Item number 3 in the ‘exploratory’ list was added after the typed version was prepared: In Pawsey’s characteristic handwriting he wrote: “Can we obtain the atomic hydrogen spectral lines or other [lines]?“ The document

would have been shared among the radio astronomy group at RPL after it had been sent to RPL from the UK.

13 July 1948 --Williamson at Cornell (possibly a summer visit from Toronto) to Greenstein at Caltech. Apparently, Williamson was gathering data for the spectral line report promised for Pawsey. "The Cornell Microwave Project is committed to making a search for the presence of a spectral line in the galactic radiation (and perhaps solar, also), due to the hyperfine transition in the ground-level of neutral hydrogen. Up to the present time, the only data which we have for some estimates of frequency and transition probability quoted from van de Hulst…. [I]t occurred to us that you might know of some other determinations, either theoretical of experimental, or someone who could direct us to such data. If so, it would be helpful indeed, [as we design the receiver, since we need to know the frequency to within about one percent.]" Greenstein answered on 20 July 1948. Surprisingly, Greenstein did not refer Williamson to the recent *Observatory* article from February 1947 by Reber and Greenstein, "Radio-Frequency Investigations of Astronomical Interest". (Also, it appears that Williamson was unaware of the publication.) Greenstein replied that he had no data on the exact frequencies and suggested that Williamson write Rabi at Columbia. (The *Observatory* article had reported: "Van de Hulst has suggested the hyperfine-structure transition of the ground state of hydrogen, located near 1410 MHz.") Greenstein also suggested that Williamson look at "the furor of papers that appeared last year in the *Physical Review*" about the hydrogen lines.

7 September 1948 --Ralph Williamson at the David Dunlop Observatory in Toronto to Pawsey in the UK. "… I feel that radio astronomy would probably be my first choice [for future research], if I had an opportunity of pursuing it in an environment favorable for true scientific work." He asked Pawsey for additional details about the offer to work in Sydney at RPL. "I should be grateful for some definite information on how the CSIR radio astronomy program is organized , particularly as to division of responsibility for originating and published research, and as to the possibility of varying antennas and set design for specific problems. I am beginning to assemble some data on possible radio transitions of astrophysical interest, and if you like, will send you a rough draft when it gets to that stage." As we point out in NRAO ONLINE 26, Williamson and Pawsey continued to correspond later in 1948 and into 1949. Williamson never sent the spectral line text; in addition, in the end he turned down Pawsey's offer of a position at RPL after extensive correspondence.

9 September 1948--Henk van de Hulst on board the RMS New Amsterdam from New York to Rotterdam to Grote Reber at NBS. The ship was close to the coast of England when the letter was written. Van de Hulst wrote to Reber:

> Here comes a late letter of thanks for the fine hospitality reception and sightseeing you gave me last week. [Van de Hulst had been in Washington DC as he visited Reber at the

> National Bureau of Standards. Van de Hulst and his wife were finishing a two-year visit to Yerkes Observatory. Reber had left Wheaton Illinois for NBS in June 1947.] I had so much science and engineering poured into me during the last week that it just had to get settled first. The two last days [in New York] before we left, I spent at Columbia [visiting Rabi and colleagues] and Princeton [visiting Spitzer and Martin Schwarzschild].

At this point in the letter, van de Hulst created a noteworthy analogy between a terrestrial map and a radio map of the sky.

> Oort will be very interested to hear about your present work, and especially to see the maps [480 MHz] you gave me. It resembles somewhat a classical treasure hunt; some map, not too accurate of an island with a mark where the hidden treasure is, but nobody knows precisely where, nor whether it really is treasure, But it is exciting, anyway…. Best wishes, Grote, and thanks again. Let me know if there is any problem connected with your work where you would suggest work of a plain astrophysical nature to be done. Suh suggestions ae always welcome, without obligations. Thanks again, and see you some other time.

1949 Mills Considered HI Research- Early 1949

Mills wrote in 2006 about his discussion with Pawsey in 1949 ("An Engineer Becomes an Astronomer", *Annual Reviews of Astronomy and Astrophysics*, vol 44, p 1). Would he be interested in a program to do Michelson interferometry of the discrete source or try to detect the HI 21 cm line that had been predicted by van de Hulst? "If I had been a trained astronomer and therefore aware of the possible great importance of the H line, no doubt this would have been my choice. But I looked on it as merely a technical challenge, whereas I was intrigued by the mystery of the discrete sources and had no hesitation in choosing this option."

Mills elaborated his thinking in 1949 (Sullivan, 2009, p.125) in an interview with Woody Sullivan in 1976 as he and Pawsey discussed the two options: "One was a search for the hydrogen line. Pawsey was very interested in it at the time. And the other was trying to locate very precisely the positions of radio sources. And it was a difficult decision to make. I eventually chose the precise positioning because I was more familiar with some of the techniques, and it looked as if it was something that would lead to an immediate result, whereas the other [HI line detection] was extremely speculative." Sullivan pointed out that given the resources and available expertise, the RPL would have soon detected the line if the decision to go ahead had been made in 1949. A promising opportunity was lost in 1949; the Australians were however slatted to make major contributions to galactic HI research in the post 25 March 1951 era (date of Ewen and Purcell detection at Harvard, see below).

1950 Discussions of the Possibility of the HI Line

Early 1950-- At RPL a translation of the 1949 Shklovskii paper "Monochromatic Radio Emission from the Galaxy and the Possibility of its Observation**"** became available**,** reported on 4 September 1951 at a Radio Astronomy meeting at RPL.

1 July 1950--Bolton and Westfold visited Leiden for a month.

3 October 1950 – Reber (NBS) to Struve (at University of California, Berkeley). "Since most of the universe is made of hydrogen, this element seems to offer some possibilities. There is one transition in hydrogen near 15 cm [sic] and another in hydrogen II [ionised a recombination line?] near 90 cm which offer possibilities…. The transitions at 15 cm and 90 cm [this latter line was also not detected in the laboratory?] have been verified in the laboratory but not in the sky."

19 November 1950 Reber (at the National Bureau of Standards in Washington DC., departed to Hawaii in northern hemisphere spring 1951) to Greenstein at Caltech. "Consideration is being given to constructing equipment to look for this phenomenon [HI hyperfine line] at 1420 MHz. If it exists, interesting information could be secured about magnetic fields, line of sight velocity etc."

9 December 1950 -- Reber at the National Bureau of Standards Central Propagation Laboratory (in Sterling Virginia, 40 miles west of Washington DC, site of Dulles Airport] to Henk van de Hulst to be at Harvard in (northern hemisphere spring and summer) 1951. "When you are here [at the NBS] I would like to get together with you on the hyperfine spectrum of hydrogen."

1951 Year of Discovery

17 March 1951--Kerr (visiting Harvard) to Pawsey: Status report on Ewen-Purcell endeavours at Harvard and Owren at Cornell (see Sullivan, 2009, p 417). "Ewen, of the Harvard Physics Dept., had developed some very nice equipment. (His main interest is in electronics and he has used all his gadgeteer's skill… [He uses a 4 by 5 foot horn and] "no sign of the line has been found". This was to change in only 9 days! The Cornell attempts of an HI detection of this era did not succeed. [Chapter 20]

30 March 1951--Letter from Kerr to Pawsey with a hand drawn copy of the successful Harvard HI detection at 1420 MHz on 25 March 1951 at the Lyman Laboratory of Physics. The Leiden group are also trying to detect HI. “Van de Hulst [of Leiden but visiting Harvard] is of course very pleased about the success of his prediction [from 1944 and published in 1945, in Dutch]”.

12 April 1951-Pawsey at meeting on 12 April with minutes from 24 April 1951. Present: Pawsey, Higgs, Piddington, Christiansen, Wild and Bolton. Two groups were lashing together 21 cm receivers: (1) Piddington and Hindman and (2) Christiansen and Bolton. Later Christiansen and Hindman joined forces as they merged receiving equipment.[3] Bolton dropped out due to illness, hospitalized for some time (18 May 1951, letter from Pawsey to Bowen) with pneumonia and kidney or bladder problems. However, by 7 June 1951, Pawsey wrote Bowen with the news that Bolton had recovered. He had been seriously ill for one week, then recovered at home. In early June, he was said to be “in the country on holidays”. [4]

20 April 1951--Pawsey to Purcell. Congratulations to the Harvard group for the initial detection. Two groups at CSIRO, RPL, were working to confirm results. The CSIRO group were to communicate with the Harvard group and if positive results they would “publish a confirmatory note in the same journal as that in which the Harvard work is described”. RPL groups were “using a balancing technique alternating between two frequencies, one including the desired spectrum line”.

2 May 1951-- Frank Kerr at Harvard to Pawsey. Kerr suggested to Pawsey that he wanted to change his research interests at RPL. “I have in fact got very interested in the subject, through close contact with van der Hulst and Ewen, and would welcome the opportunity of entering this field [HI observations of the galaxy]. The main reason I had not mentioned this before as a possible project for me was not through any lack of interest but because I had thought you might well regard it as [interfering] in the field of those already engaged in galactic work. …[A]s you imply in your letter, the study of this line would seem to offer a full-time job for somebody for quite a long time … One thinks in term of a thorough survey of the sky ... Possible ... Zeeman splitting effects should also be looked for……” Kerr returned to Australia on 22 September 1951, after a few weeks in Europe where he met Oort, van de Hulst (in Leiden the latter for the second time), Ryle and Lovell (in the UK).

9 May 1951--Purcell to Pawsey. Two pages from Ewen’s thesis were included showing the doppler shift of the HI radiation due to the motion of the earth: “The results ... leave little doubt of the identity of the line and its galactic origin.” Purcell was favourable to a possible joint publication if the CSIRO group had success in the next months before the Harvard results would be published.

[3] Hindman provided the receiver front end and Christiansen the signal generator for 1420 MHz.
[4] NAA C3830, Z1/9

11 May 1951-- Paul Wild of CSIRO to van de Hulst at Harvard. Wild was working on his paper for the *Astrophysical Journal* "The Radio-Frequency Line Spectrum of Atomic Hydrogen and its Applications in Astronomy" which would be submitted on 8 September 1951 and published in March 1952. After congratulations to van de Hulst "on your work which lead to this exciting new discovery", Wild asked for a copy of the Dutch publication which was apparently not available in Australia, *Nederlandsch Tijdshrift voor Natuurkunde* from December 1945. "Herkomst der Radiogolven uit het Wereldraum", translated by W.T. Sullivan, III, 1982, p. 302 "Origin of the Radio Waves from Space". Also, Wild hoped to receive a translation of the publication which would be "even more desirable" than the Dutch text. Wild's original determination of the transition probability was in error, corrected by Ewen and Purcell after the paper was submitted in September 1951. On 22 October 1951, Purcell wrote Kerr with the correct determination, a factor of 4 larger at $A= 2.85 \times 10^{-15}$ sec^{-1}. (Shklovskii had made the same error.) Wild included the revised version of the transition probability in the final publication.

11 May 1951-The Dutch group of Muller and Oort (Leiden and the Netherlands Foundation for Radio Astronomy- SRZM) succeeded in confirming the HI line with the Würzburg aerial at Kootwijk in the Netherlands. On 1 June 1951, Frank Kerr (CSIRO visitor at Harvard) provided details and drawing in a letter to Pawsey in Sydney. Kerr had seen a letter from Oort to Henk van de Hulst at Harvard, posted on 28 May 1951 in Leiden. Again, the large velocity shifts due to galactic rotation were observed. The Leiden group had been working on a 21 cm line system for about a year in mid-1951; their system was almost operational at the time of the Harvard detection in late March. (see Sullivan, 1982. Classics in Radio Astronomy, p 299, "Prediction of the 21 cm Hydrogen Line" and p 325, "Detection of the 21 cm Hydrogen Line")

18 May 1951--Pawsey replied to 9 May 1951 letter from Purcell. Christiansen was to make his first observation on 18 May 1951. But success was almost two months in the future (6 July 1951). Again, the Australians hoped to participate in a joint publication. "I am very grateful ... for giving us pre-publication information which has enabled us to make an early start in what, with any luck, should prove a most fruitful field."

18 May 1951--Pawsey to Bowen in London. There were still no positive HI results. "I am going out [to the Potts Hill field station] to see the first tests tonight...I have been in touch with Purcell and he is a gentleman." (Pawsey and Purcell had been colleagues at the MIT Radiation Laboratory in WWII. Later in October 1951, Bowen would visit Purcell and Ewen in Cambridge, Mass, Chapter 20 and photo of Bowen and Harvard colleagues.) On 7 June 1951, Pawsey again wrote Bowen with the discouraging news of the two groups working in Sydney; they were still unsuccessful. But the Dutch had detected the line in the meantime on 11 May 1951.

14 June 1951--Purcell submitted the Harvard paper to *Nature* (Ewen and Purcell, "Observations of a Line in the Galactic Radio Spectrum", published on 1 September 1951, vol 168, p 356).

Adjoining papers were: (1) “The Interstellar Hydrogen Lines at 1420 MHz, and an Estimate of Galactic Rotation” by Muller and Oort. At a new galactic longitude of about 30 deg, the radial velocity of the gas was about 55 km/s. (2) A telegram from 12 July 1951 from Pawsey, announcing the Sydney detection from 6 July 1951, made with the 18 by 16-foot dish at Potts Hill (see Chapter 20) by Christiansen and Jim Hindman.

19 June 1951 and 27 June 1951--Exchange of letters with Ryle and Pawsey. Ryle was impressed: “It is a most important advance. Are you planning any such observations?” Pawsey replied: “The Harvard and Dutch work … is of first-rate interest. I hope to follow up later as soon as we can get clear as to what is required.”

13 July 1951-Pawsey to Bowen. The RPL detection was reported (“after much trial and tribulation”). He described the RPL telegram to *Nature* (see above). Response on 23 July 1951 from Bowen in London with reserved praise. “It is nice to know the effect exists and it clearly opens up many new lines of investigation.”

14 July 1951--Pawsey to Oort with news of the RPL detection. “I think [our detection] represents an outstanding example of planning and of experimental work to enter the field of cosmic radio waves in this way.”

20 July 1951--- Oort in Leiden to Pawsey in Sydney with congratulations in a hand written letter from his holiday in Friesland (the Netherlands). “I hope that an agenda of cooperation on certain programmes between your Radiophysics Laboratory and our still small group can be established.”

31 August 1951--Pawsey wrote Harold (“Doc”) Ewen at Harvard, responding to Ewen’s letter of 22 August. “I am sorry that… you are not in a position to try to come out here [to RPL]. If …you reconsider this decision I should appreciate it if you could let me know so that we might try for you to work here…We have [21 cm HI] equipment which is of the breadboard variety and is made to work by sheer willpower ... Christiansen is engaged on a preliminary survey of the visible sky using a 17-foot aerial. We are also planning to make better equipment but this will take some considerable time.” Ewen would visit Sydney during the URSI in 1952.

4 September 1951--Meeting of Radio Astronomy group at RPL. Present: Pawsey (chair), Bolton, Christiansen, Davies, Hindman, Little, McCready, McGee, Mils, Murray, Piddington, Shain, Warburton and Wild. “Van de Hulst first considered the likelihood of observing this line in the sky in December 1945, in a paper written in Dutch, and Reber told me [Pawsey] about it in Washington in 1947. At the beginning of 1950 we had here a translation of a 1949 paper by Shklovskii mentioning the matter. Kerr wrote that Ewen had succeed in observing the line in March 1951 and Purcell, in charge of the Harvard work, wrote inviting us to verify the observation. Christiansen and Hindman did this on 6th July 1951. Muller and Oort were successful in Holland in May [11]. A short note regarding our success will be appended to the American and Dutch letters in Nature [on 1 September 1951].”

8 October 1951-- Reber (in Hawaii, Maui) to Ewen and Purcell at Harvard. “This epoch-making discovery will require a reorientation of thinking about the material of interstellar space. I wish to congratulate you on an excellent piece of work. May I suggest you now try for the second isotope at about 327 MHz- [deuterium].”

24 October 1951 --Pawsey to Bowen in London. Christiansen would discontinue HI research as he returned to solar work, the Potts Hill grating array at 20 cm (“progressing favourably now”). “… Frank Kerr and Jim Hindman are carrying on with the [HI] observing programme with McCready’s help. It is clear that the observations require the utmost in sensitivity and I hope that some novel ideas which we are trying out here will give us a little more than other people are obtaining with conventional schemes.”

31 October 1951--Pawsey to A.H. de Voogt Chair of URSI Commission V; Pawsey suggested that the HI frequency by given a special allocation with a protected frequency- 1420.4 MHz with a bandwidth of 3 MHz. On 14 November 1951, de Voogt reported to Pawsey that he had taken up the issue with the CCIR (International Radio Consultative Committee).

19 December 1951--Reber to Pawsey from Maui Territory of Hawaii. In the northern hemisphere spring, Reber left the National Bureau of Standards in Washington, D.C. “ Last spring I got tired of working for Uncle Harry [Truman, the US President] and his boys so I took a vacation in Hawaii. Things looked so good I decided to stay … If any of your fellows are out traveling I’d be very happy to have them call on me at Maui. The place is truly a land of eternal springtime. Furthermore, the milkway [sic] rises from and sets into the sea at this latitude as it should be a good place to do Lloyds mirror [sea cliff] experiments. Thus the future looks quite bright indeed from both a personal and scientific view point.” Reber hoped to see the Australian colleagues at the August 1952 URSI meeting in Sydney and the Rome IAU in September 1952.[5]

1952-1953 Consolidation of HI Research

1 July 1953--Pawsey to Oort in Leiden**.** The Australian HI group were planning for the future**.** They were struggling to relate velocity at a certain longitude in the Milky Way to possible distances from the sun or the galactic centre. They asked the Leiden group to assist. “Like you we stopped observations to build equipment and are finding it a big job. However, we hope to have some at least operating by August [1953]. The new 36- foot antenna was in fact operation

[5] NAA A1/1/1 Part 6.

already in January 1953 with a beamwidth of 1.4 deg. The new survey of the southern Milky Way would begin later with 4 channels of velocity resolution 8.5 km/s.

12 July 1952--Van de Hulst in Leiden to Pawsey, a handwritten letter. A galactic rotation curve was provided, circular velocity in the galaxy as a function of radius. The equation for a radial velocity at the sun at a certain distance from the sun and fixed longitude was provided. Van de Hulst provided a number of graphs showing the complex dependence of radial velocity on distance at various longitudes. For longitudes within plus and minus 90 deg of the galactic centre there was the well-known ambiguity with two possible distances for fixed velocities- the near and far distance.

7 November 1952 --Telegram of congratulations from the Sydney HI gang (Pawsey, Christiansen, Hindman, Kerr and Wild) to Purcell for his share of the 1952 Nobel Prize in Physics. The prize was shared with Felix Block of Stanford for "for their development of new methods for nuclear magnetic precision measurements and discoveries in connection therewith."

13 December 1952-- Newsletter No 1 -- "Newsletter on Work in Progress on the 21 cm Line ". The newsletter was planned in August 1952 at the URSI General Assembly in Sydney. The initial newsletter was produced by the HI group in Leiden. There was a US report by Doc Ewen, with brief mention of the work by John Hagen at the Naval Research Laboratory and Barrows at Cornell. He presented details of the Carnegie Institution of Washington work by Merle Tuve; Ewen had visited this group as they turned on their 1420 radiometer the previous week. Frank Kerr reported from RPL in Sydney that initial Australian survey carried out by Christiansen and Hindman had been published in 1952. The new system for HI work consisting of the new transit 36-foot telescope (resolution 1.4 deg with a new 4 channel spectrometer with 8.5 km/s velocity resolution) was under development. This system would be partially completed the following month (January 1953). In March 1953 the first extragalactic HI detection would be made by Frank Kerr, Jim Hindman and the young Brian Robinson. The Dutch group of Lex Muller, Jan Oort and Henk van de Hulst reported on the activities of the Netherlands Foundation for Radio Astronomy (SRZM). The frequency scanned spectrometer was swept over a 3 MHz range (630 km/s) with a resolution of 6.3 km/s. The 7.5 m Würzburg (former German WWII radar) aerial was used. Based on the assumed galactic rotation curve (presented in the report), the frequency shifts were converted to radial velocities and then possible distances from the sun and also within the Milky Way. The distance to the galactic centre was assumed to be 8.8 kpc, revised from the earlier 9.4 kpc; the revision had been suggested by the new Leiden Kootwijk data. The profiles in the galactic anti-centre (new 1958 coordinates, longitude 170 to 195 deg) and in the centre (350 to 15 deg) showed narrow profiles due to the decreased differential rotation. The southern Milky Way (longitudes 255 to 355 deg) was not observable from latitude of the Netherlands (52 deg). "Our first program is to complete the first survey of the galactic plan, to determine [the rotation curve of the galaxy] from the radio measurements themselves and to measure the distribution in latitude at various distances at selected longitudes. As

subsidiary programs Oort and Seeger are making some calculations for a new model of the galactic system, while Blaauw intends to redetermine the constant A [providing the relation between distance from the sun and radial velocity in the solar neighbourhood.]

1953 The new 36-foot telescope at Potts Hill

January 1953- 36-foot used for test observations, initial HI observations made with single channel spectrometer

On 26 March 1953--- Kerr to Harlow Shapley, recently retired Director of the Harvard College Observatory:

Kerr was quite pleased to indicate to Shapley that the Large and Small Magellanic Clouds had been detected at Potts Hill by the group of Kerr, Hindman and Robinson. This was the first detection of HI in an external galaxy, the nearby neighbours of the Milky Way (50 kpc). The first data consisted of simple detections of both clouds; the determination of the rotation of the clouds would follow later. (In 1955, Kerr and de Vaucouleurs published a paper on the kinematics in the *Australian Journal of Physics*, vol 8, p 508.) Kerr informed Shapley: "Knowing your great interest in the Magellanic Clouds, I will keep you in touch with future observations. The astronomical knowledge which I acquired at Harvard is going to be valuable during the coming year or so."

12 November 1953, 27 November 1953, 10 December 1953-- Meetings of the Hydrogen-Line Planning Committee at RPL. Starting on 12 November, a series of RPL regular meetings began to discuss the status of 21 cm HI astronomy. Pawsey was the chair with members Higgs, Kerr, Robinson, Hindman, Stanley, Murray, Fryar, McCready, McGee, Robert Price (MIT visitor, Fulbright Fellow). Meetings occurred at regular intervals until at least mid-1954, when Kerr became convenor and Hindman the secretary. At the first meeting on 12 November 1953, Kerr reported on successful HI observations of the Magellanic Clouds with the 36-foot aerial. Kerr suggested that he planned to also carry out observations of other external galaxies. (These likely did not succeed since no publications were to appear.) The long-range plans were to make an extended survey of the Magellanic Clouds and surrounding regions. In addition, a more extensive survey of the southern Milky Way was planned, especially the longitudes that were not accessible from Leiden (new 1958 galactic longitudes 255 deg to 350 deg). Gordon Stanley reported on 327 MHz observations for deuterium using the 16-foot dish at Dover Heights. Later attempts would be made using the 80-foot hole-in-the-ground antenna (see below). The minutes added the following:

> Dr Pawsey stressed that the search must be carried out thoroughly. A negative result is worth publishing if it can be stated that the line temperature is below X deg over a certain section of the sky. Discussion ensued on the useful information which could be obtained if the D-line is detected, Sources could be seen on 327 MHz at distances which

> are optically thick on 1420 MHz. Assuming that no D-line is detected, it is planned to change to 1420 MHz and test the receiver on the galaxy. Then the filters will be set to 40 km/s [velocity resolution] and a frequency sweep of 420 km/s will be followed over the range of the 80-foot dish [for extragalactic HI observations].

Two weeks later (27 November 1953), the Hydrogen-Line Planning Committee continued with Dick McGee reporting on the status of the swept frequency 327 MHz line receiver for the 80 foot-aerial. An improved system was to be tested using observations of the galactic centre later on 27 November, with the system running overnight as the Puppis region of the galaxy passed overhead. (The galactic anti-centre could not be reached due to the declination range limits of the 80-foot aerial from -30 to -40 declination, see below.) Brian Robinson presented a feasibility study of possible HI observations of M31 (and other smaller galaxies) with the 36-foot aerial; the Andromeda Nebula (declination +41 deg) was observable in the far north sky from Sydney.[6] "It was planned to extend this method to estimate the profiles of galaxies in the declination range of the 80-foot dish for discussion at the next meeting."

The last meeting of 1953 occurred on 10 December. Kerr reported on new observations of the Small Magellanic Cloud with improved sensitivity compared to the earlier data in March 1953. Stanley reported on continuing deuterium observations with the 80-foot aerial. A slightly revised rest frequency based on laboratory experiments was now used (327.384 MHz -error 3 kHz- compared to the previously adopted value of 326.5 MHz.) "The Deuterium search will be continued by R. Price with the frequency sweep reduced to 400 kHz. G. Stanley will return to setting up equipment for 1420 MHz observations."

On 10 December 1953, Brian Robinson presented a summary of possible, future HI observations of prominent galaxies in the declination range of the 80-foot aerial. Eight galaxies were suggested: NGC 300 was the most favourable, a Sc galaxy. Other candidates were NGC 1365 (barred spiral Sb), NGC 55 (Sp) and NGC 1097 (Sb). Robinson estimated the width of the HI profile (typically plus or minus 200 km/s). Many of these galaxies would become well known HI objects in later years. The system set up was tested on 8 July 1954 with the first attempted observations in a 4-day period in November. McGee, who had replaced Robinson in the project after his departure for the UK in mid-1954 (footnote 9), observed a handful of galaxies with appropriate declinations with the transit 80-foot aerial.[7] The declinations ranged from-30 to -40 deg (the latitude of Sydney is about -34 deg, thus about plus and minus 5 deg in zenith angle; the beam size was about 35 arc min). Observations were carried out on 15, 16 17 and 18 November. As an example, on 17 November 1954, McGee carried out a single frequency scan for HI in NGC 1365 in Fornax. Other galaxies observed were NGC 1316 and NGC1097 in Fornax and NGC 300 in Sculptor. The log book gives no indication of any successful HI detections. Each galaxy required that McGee adjust the feed position for the appropriate declination. On 16

[6] Results were to be reported by Robinson at the next meeting.
[7] NAA C4641/3, McGee "Log Book 1420 MHz Search for Red Shift Project, Dover Heights June 1954"

November, Pawsey provided comments about the degradation of the system for the planned observations of NGC 1365, the well-known barred spiral galaxy: “Dr Pawsey suggested that sensitivity will be down”, due to the mismatch between the available spectral resolution and expected width of the HI line. (The redshifted line was observed at about 1414 MHz; the channel width was 1.4 MHz- 300 km/s[8].) In the end, these attempts to initiate extragalactic HI research failed due to the transit nature of the dish (leading to restricted observing time), restricted declination range and inadequate velocity resolution. Thus, the short integrations with the single channel receiver were inadequate. Successful extragalactic astronomy (apart from the nearby Magellanic Clouds) had to await the opening of the Parkes telescope in the post-1961 era. (Chapters 31-32).

1954 Success at Potts Hill with the 36-foot Aerial. Ambitious Attempts at Dover Heights with the 80-foot Hole-in-the-Ground Aerial

Meetings of the Hydrogen-Line Planning Committee on 26 January 1954, 1 March, 29 March, 9 June 1954

26 January 1954—Hydrogen-Line Planning Committee. Deuterium 327 MHz searches were going well with observations of the galactic centre and the Puppis region. “These records are tantalising.” However, progress was slow since the 80-foot antenna was a transit instrument. Only one scan across the frequency range was possible while the source was in the beam of the aerial. “No alternative aerial, which is likely to be a great improvement, is readily available.” The sensitivity was about 4 degrees antenna temperature. So far, the search had lasted since 10 December 1953. Pawsey suggested that the D-line survey should continue for three months, then “if the result is negative to give a definite limit as to the strength of the line.” A search had been made by Robinson with the 36-foot aerial of the Andromeda Nebula in HI, with no success. The observations would continue with higher sensitivity and at positions where the expected HI signals at the maximum rotational velocity should be more intense. These negative results at 21 cm were never published; Brian Robinson was to leave for Cambridge in the UK within a few months (see footnote number 9). The southern Milky Way HI survey would soon begin.

1 March 1954--- Hydrogen-Line Planning Committee. During this meeting the detailed plans for the southern HI survey were discussed. The galactic plane and “a small region around it” would be observed with the 4-channel receiver. Each channel had a separate chart recorder attached.

[8] This resolution was likely unfavourable since the total velocity span of the HI was over 300 km/s with an angular size of roughly 15 arc min. Typical profiles of the galaxy as observed with high angular resolution exhibited velocity widths of about 100 km/s. In addition, McGee observed NGC 1365 with a frequency that was in error by about 225 km/s (based on an inaccurate optical determination), again leading to a diminished HI signal.

Calibration of pointing was done on the sun (using the 600 MHz system which was attached outside the 1420 MHz feed). The pointing precision was 0.1 to 0.2 deg with a beam width of 1.4 deg. The region of the galactic plane was 215 deg to 35 deg (new 1958 longitude) which provided 40 deg of overlap with the Leiden survey at each end of the survey. The galactic latitude interval covered ranged from 1.5 to 9 deg, with a wide variation due for reasons connected to the programming of the survey. The overlap with Leiden was required for the required consistency checks between the two surveys. The intensity scale was checked by frequent observations of standard HI regions and zero-point checks at the south celestial pole.

Gordon Stanley continued with progress reports about the D-line survey. The present integration time for the observations was only 20 seconds, as the beam swept by a given point on the sky in 10 minutes between half power points. “The frequency sweep is 400 kHz carried out in 15 minutes and timed so that the frequency is 327.4 MHz when the centre of the beam pointed to the likely source” (e.g. Sgr A).

29 March 1954--Hydrogen-Line Planning Committee. Bob Price gave a progress report on the D-line work at Dover Heights (80-foot). He had some suggestive records but the signal to noise was marginal. Pawsey: “It is time to decide whether or not to continue with search for this line.” To make significant progress it would be necessary to spend another two months observing. Pawsey suggested “changing some of the observing conditions as to eliminate systemic errors (e.g. “to change start of sweep to move the peak to the return sweep”.) At Potts Hill with thee 36-foot aerial, Kerr was starting a new campaign of long observations of the southern galactic plane. Hindman had a number of suggestions, especially the need for temperature control of the observing building.

9 June 1954---Hydrogen-Line Planning Committee.[9] The brief meeting of 9 June 1954 contained descriptions of a future project to determine redshifts of distant galaxies with the 80-foot, a project of Stanley and Price. In November 1954, McGee (see above) would observe a handful of spiral galaxies with no success. The short integration times with the transit dish was an unsurmountable handicap.

By June 1954, the deuterium project of Stanley and Price had also come to an end. The HI Newsletter (No 4 June 1954) stated: “Gordon Stanley and Bob Price, at Dover Heights, have carried out an unsuccessful research for the 327 MHz deuterium line. Using their 80 foot paraboloid, which was dug out of the Dover sand, they investigated regions near the galactic centre, and in Puppis They estimated that any deuterium radiation in these directions must be less than a degree or so.” Stanley and Price published their negative result in *Nature* (1956, vol 177, p. 1221, “An Investigation of Monochromatic Radio Emission of Deuterium from the Galaxy”). They wrote:

[9] Brian Robinson was not present as he was on his way to Cambridge in the UK. He had been awarded a Rutherford Memorial Scholarship to begin a PhD with Ratcliffe and Weeks at the Cavendish Laboratory, working on the E region of the ionosphere.

> [The observations were made by]… automatically scanning with the receiver through 200 kc./s [about 200 km/s] as the direction of the galaxy passed through the antenna. Such methods failed to detect any radiation. In the later records graphical integration was performed to increase the sensitivity … The abundance of deuterium should therefore be less than 1/1000 of hydrogen. [10]

At the 1955, Manchester IAU Symposium (see Chapter 26), Pawsey mentioned the Stanley and Price Dover Heights result in the Discussion of the paper of Getmanzev, Stankevitch and Troitzky (from Gorky State University), "Detection of the Spectral Line of the Galaxy on the Wave-Length of 91.6 cm (in *IAU Symposium No 4: Radio Astronomy*, editor van de Hulst, 1957, p 90). These USSR authors claimed a detection which was, however, not shown. Pawsey pointed out the Dover Heights limit was not inconsistent with the claimed detection by Getmanzev et al.

15 to 18 November 1954- McGee attempted HI observations of a handful of spiral galaxies at Dover Heights with the 80-foot dish. (see above text for 10 December 1953).

The major survey with the 36-foot was published by Kerr, Hindman and Gum in September 1959, "A 21 cm Survey of the Southern Milky Way" in *Australian J of Physics,* vol 12, p 270.

No additional meetings of the HI Planning Committee were held at RPL after 9 June 1954[11]. HI work continued of course. With the opening of the Parkes telescope in 1961, HI galactic and extragalactic research would become prominent, under the leadership of Frank Kerr and Brian Robinson. (see Chapter 32).

[10] For a summary of this complex field in 2021, see https://web.williams.edu/Astronomy/research/deuterium/DeuteratedMolecules.htm. There is a vast literature of modern data concerning the radio line of deuterium at 327 MHz: e.g. (1) Blitz and Heiles using the 25 m aerial at Hat Creek (*Astrophysical Journal*, vol 313 L 95, 1987) with a possible detection in the galactic anti-centre with a D/H of about 5 x 10^{-5}, (2) Anantharamaiah and Radhakrishnan (*Astronomy and Astrophysics* , vol 79, p.9, 1979) using Ooty with a D/H limit of 6 x 10^{-5} for the galactic centre, (3) Lubowich, Anantharamaiah and Pasachoff using the VLA in D array (*Astrophysical Journal*, vol 345, p 770, 1989) with various limits at the galactic centre in the range 4 x 10^{-4} to 10^{-3}. Then the astronomy world was stunned in 2005 when Alan Rogers and collaborators (Dudevoir, Carter, Fanous, Kratsenberg and Bania) discovered deuterium in the galactic anti-centre with a specially constructed radio telescope at Haystack Observatory (*Astrophysical Journal*, vol 630, L 41, 2005). The D/H ratio was found to be 2.3 x 10^{-5}, with a 5-sigma detection.

[11] Based on investigations of NAA C3830 A1/3/17 Part 1 (1951-1955) and Part 2 (1956-1968). A1/3/17 series is titled "Research and Development- Cosmic Radio Astronomy- 1420 Hydrogen Line"

1982, 1983, 1984 and 1986 Reflective Memories of the HI Events of 30 Years Earlier, Bolton, Bowen and Wild[12]

24 August 1982---Bowen to Kerr relating to his planned article for the 1984 Sullivan anthology: ***The Early Years of Radio Astronomy, Reflections Fifty Years after Jansky's Discovery***

This letter demonstrated how Bowen's account (to be published in 1984, "The Origins of Radio Astronomy in Australia" see below) incorrectly portrayed Pawsey as reluctant to explore the possibility of detecting the HI line. (See ESM_20.1.pdf for additional details, "Review of Recollections of Bolton and Bowen".)

Bowen wrote Kerr on 24 August 1982 as he prepared the contribution to the Sullivan volume:

> Well before Ed Purcell's discovery, I used to be urged by Rabi to look for nuclear resonances in the galaxy. He was referring, of course, to the original work of the Columbia group on nuclear resonance, with things like Caesium high on the list of materials known to resonate at radio frequencies. This was very early in the piece and if you are interested in dates, I could probably turn something up out of my personal diaries. This suggestion was made to me more than once, and when I got back to the Lab, I discussed it with Joe and urged him to give it a go. As usual, Joe was a thorough sceptic and reluctant to do anything about it. However, he eventually asked Paul to draw up a list of likely frequencies, which were mostly those already stimulated in the laboratory. If necessary, this list could be dug out of the files. As far as I recollect, there was no mention of Hydrogen and certainly no mention of van der Hulst's wartime suggestion, which I personally had not heard about prior to Purcell's discovery.
>
> I mention this partly because the story was once passed around RPL that it was Paul who first thought of looking for line radiation. This is simply not true. It was first mentioned to me by Rabi and later by Zacharias of Columbia University [sic, MIT, the inventor of the caesium clock]. [13]

1982 ---Bolton in *Publications of the Astronomical Society of Australia*, vol 4 (4), p 349, 1982 "Radio Astronomy at Dover Heights": We include this quote to demonstrate how Bolton's account incorrectly portrays Pawsey's reticence to begin HI investigations. Both Bowen and Bolton elided that Pawsey had been keen to start HI in 1948-1949.

[12] There is considerable overlap with Chapter 20 and ESM_20.1.

[13] Bowen to Kerr, NAA C4661/1/11 as he prepared his contribution to the Sullivan volume, 24 August 1982.

Bowen wrote as he described the events of 1949 at RPL (this text also appears in ESM_20.1):

> … Kevin [Westfold] had time to read the current literature, amongst which was Shklovskii's 1949 original article in Russian on the 21 cm hydrogen line - which he translated. He gave the translation to Pawsey [This text has not been located in the RPL, NAA, National Library of Australia or the Pawsey family archives by Sullivan, Wendt or Goss], with the suggestion that someone in Radiophysics should build some equipment to look for it - unfortunately to no avail. From Leiden in the following year we reported on the preparations in progress by the Dutch for 21 cm line equipment. Later that year F.J. Kerr, who was spending a year at Harvard, took me to see the equipment that Ewen and Purcell had under construction. Independently he had proposed to Pawsey that Radiophysics should take some action. Regrettably, this was not to happen until after the Dutch and Americans privately communicated their detections of the H-line to Pawsey and invited a joint publication ***with some southern hemisphere observations.*** (his emphasis).

A number of statements in the above text may well be erroneous. e.g. there is no archival evidence of Westfold's suggestion[14] or that Kerr suggested a search to Pawsey before the Harvard detection of 25 March 1951. At minimum, Bolton's statement overlooks Pawsey's continued interest in the HI line from 1948, and the lack of interest among the group in setting their own projects aside to pursue this one.

24 March 1983---Bolton to Bowen with comments on a draft of Bowen's upcoming article in the Sullivan volume of 1984:

Bolton wrote to Bowen:

> If anyone reads both of our articles, they will find that Joe did not communicate with you. Joe knew of the HI-line in mid-1949[15] before Westfold left the lab for two years at Oxford ... I guess we just stuck to "proper channels" when Joe was around. How fortunate I was that Joe was overseas when I wanted to go to New Zealand for the position work [in 1948].

[14] In the Bolton retrospective of 1994 by Kevin Westfold ("John Bolton, Some Early Memories" in the John Bolton Memorial Symposium of 9-10 December 1993, *Australian Journal of Physics*, vol 47, p 535, 1994), no mention of the hydrogen line appeared in his detailed text about his early years working with the RPL group.

[15] As shown in Chapter 20, Pawsey heard about the HI line in early 1948 from Reber in Washington, DC.

Bowen in the Sullivan volume of 1984 *The Early Years of Radio Astronomy, Reflections Fifty Years after Jansky's Discovery,* "The Origins of Radio Astronomy in Australia "

> A significant "miss" about this time was a failure to follow through on a hint about line radiation. Well before 1949 I had been urged by Rabi and Zacharias of Columbia University to look for line radiation in the Galaxy. Unfortunately, we were to focus our attention on the lines known to be most easily excited in the laboratory, like those from caesium and related elements. We simply did not know about van de Hulst's wartime suggestion of hydrogen as the most likely candidate.[16] However, in 1951 Ewen and Purcell were kind enough to let us know of their discovery of the H line prior to publication and we were able to verify it **three weeks later**. [In fact about 3 ½ months]

30 July 1986---Wild to Sullivan (also ESM_20.1):

We cite an alternative view from Paul Wild, sent to Sullivan on 30 July 1986, in relation to an early draft of his book *Cosmic Noise*[17]:

> One comment of a slightly sensitive nature. Joe Pawsey is no longer with us to give his version of those days. I therefore think one should scrupulously check what his critics have to say. In this regard my good friend John Bolton had a somewhat obsessive dislike of interference by Pawsey (to whom John really owed a great deal). I therefore believe that some of the things he says about Joe need to be taken with a pinch of salt. In particular, the Bolton quote on page 21 [of Sullivan's draft] "After a week or two … reassignment" and again the penultimate sentence on page 10 on Pawsey's refusal to allow Bolton to search for the hydrogen line [18]—both sound so un-Pawsey-like that I

[16] As discussed, we think that at the time Pawsey wrote Bowen in January 1948 he was not aware of the brief summary in Reber and Greenstein or the van de Hulst paper (see Chapter 20). So, this statement is likely technically true, but it obscures the fact that Bowen was reluctant to pursue what appeared to be speculative research in the late 1940s.

[17] Sullivan archive, JP Wild to WT Sullivan III 30 July 1986.

[18] For example, in an interview with Sullivan at Parkes on 15 March 1978, Bolton said: "… One of the things that annoyed Westfold and I [sic] very greatly was that we weren't allowed to look for the hydrogen line very early in the piece … We could never get across to Joe that this was atoms emitting a line frequency ..." Papers of Woodruff T. Sullivan III, "Interview with John G. Bolton, 15 March 1978," *NRAO Archives*, accessed April 21, 2021, https://www.nrao.edu/archives/items/show/903

doubt their accuracy. I cannot imagine Pawsey stopping anyone from doing anything like that if they really wanted to.

True they may be expected to keep a program going, but you could always do what you wanted on the side— “on the wrong job number” as Pawsey put it. He was a strong believer on devoting part of one’s efforts on “long shots” or “wild cats”.

I therefore believe an opposite point of view should be put to those otherwise undefended statements. I am willing to be quoted on this matter. Incidentally, I have discussed this matter with Christiansen and he is in entire agreement with what I have written above and I am sure would also be prepared to be quoted on it.