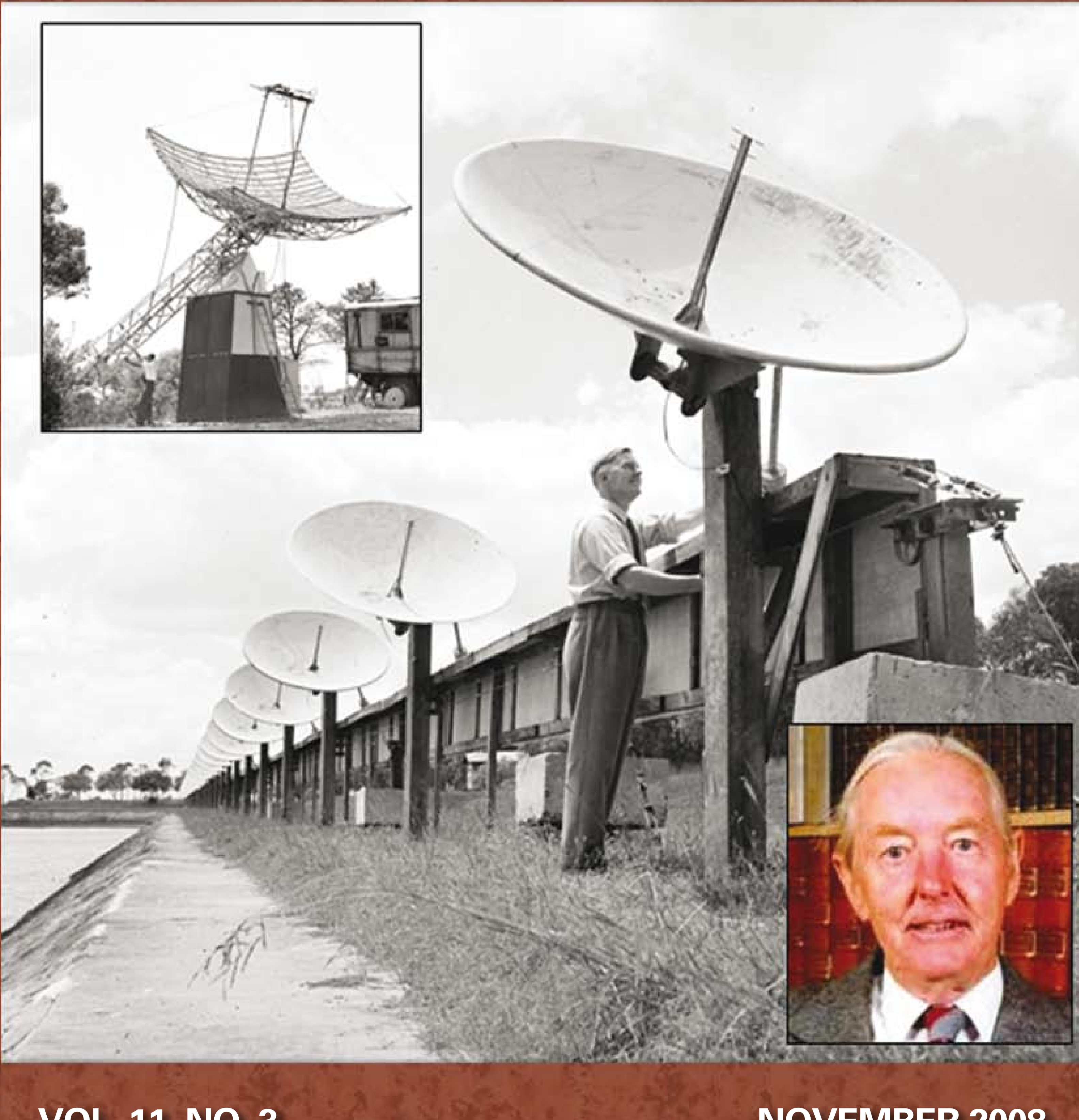
JOURNAL OF ASTRONOMICAL HISTORY AND HERITAGE

Christiansen Memorial Issue #1



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Papers on all aspects of astronomical history are considered, including studies that place the evolution of astronomy in political, economic and cultural contexts. Papers on astronomical heritage may deal with historic telescopes and observatories, conservation projects (including the conversion of historic observatories into museums of astronomy), and historical or industrial archaeological investigations of astronomical sites and buildings. All papers are refereed prior to publication. There are no page charges, and *in lieu* of reprints authors are sent a pdf or Word camera-ready version of their paper.

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COVER PHOTOGRAPH

This is the first of two issues celebrating the major contribution that Professor Wilber N. (Chris) Christiansen (1913-2007) made to international radio astronomy. The cover montage shows the East-West solar grating array that he designed, which was installed at the Division of Radiophysics Potts Hill field station in suburban Sydney in 1952. One of the inserts shows Professor Christiansen in his later years, while the other depicts the recycled 16 x 18 ft ex-WWII experimental radar antenna that Christiansen and Hindman used in 1951 to investigate the 21cm hydrogen line following the announcement of its discovery by Ewen and Purcell. The papers by Wendt, Orchiston and Slee on pages 173 and 185 in this issue of *JAH*^{2} discuss the solar grating array and Christiansen's pioneering H-line observations, while Swarup's paper on page 194 reflects on his initial work with Christiansen at Potts Hill and how subsequent contacts once he was based at Stanford, and later in India, impacted on his research as a radio astronomer.

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W.N. CHRISTIANSEN AND THE DEVELOPMENT OF THE SOLAR GRATING ARRAY

Harry Wendt
Centre for Astronomy, James Cook University, Townsville,
Queensland 4811, Australia.
E-mail: Harry.Wendt@jcu.edu.au

Wayne Orchiston
Centre for Astronomy, James Cook University, Townsville,
Queensland 4811, Australia.
E-mail: Wayne.Orchiston@jcu.edu.au

and

Bruce Slee
Australia Telescope National Facility, PO Box 76, Epping, NSW 2121,
Australia and Centre for Astronomy, James Cook University,
Townsville, Queensland 4811, Australia.
E-mail: Bruce.Slee@csiro.au

Abstract: By 1950 the C.S.I.R.O's Division of Radiophysics was emerging as a leader in solar radio astronomy. Early observations at radio frequencies were hampered by a lack of angular resolution. In seeking a method to produce regular high-resolution observations W.N. Christiansen devised the solar grating array. This unique instrument was constructed on the banks of the Potts Hill water supply reservoir in suburban Sydney and operated from 1951 to 1957. This paper discusses the inspiration for the design of the solar grating array, its physical characteristics and the contribution made to international solar radio astronomy through the observational programs carried out at Potts Hill.

Keywords: W.N. Christiansen, radio astronomy, solar grating array, Division of Radiophysics, C.S.I.R.O.

1 INTRODUCTION

By 1950 the C.S.I.R.O.'s Division of Radiophysics had already established itself as a leader in the new field of solar radio astronomy (Orchiston et al., 2006; Sullivan, 2005). Some highlights from the early research program were the observation of the million degree temperature of the solar corona (Pawsey, 1946); the association of enhanced radiation with sunspots, established through sea interferometry (McCready et al., 1947); and measurement of delays in the arrival times of bursts at different frequencies, suggesting the motion of the burst source through the decreasingly-dense coronal atmosphere (Payne-Scott et al., 1947).

A limitation of the early radio observations was the poor angular resolution of the instruments used. One way of gaining improved resolution was to exploit the technique of sea interferometry (see Bolton and Slee, 1953), and observations were also made during partial solar eclipses in 1948 and 1949 in an attempt to obtain even better resolution at a number of different frequencies (see Orchiston et al., 2006; Wendt et al., 2008a). It was during the partial solar eclipse of 1 November 1948 that W.N. ('Chris') Christiansen obtained his first major exposure to solar radio astronomy (see Christiansen et al., 1949a; 1949b).

2 INSPIRATION FOR THE GRATING ARRAY

Christiansen had joined Radiophysics in 1948 from Amalgamated Wireless (Australasia), where he had worked on aerial design. However, he was unique amongst the Division's early recruits in that he harboured a long-term ambition to become an astronomer (Sullivan, 2005: 14).

Christiansen was appointed to a senior role within Radiophysics, filling a vacancy created by Fred Lehany's transfer to the Division of Electro-technology. He was soon installed as the lead researcher of the solar program at the newly-established field station at Potts Hill in the western suburbs of Sydney. The main radio telescope there was a 16 × 18-ft ex-WWII experimental radar which had been relocated from the Georges Heights field station to Potts Hill in time for the 1948 solar eclipse (see Orchiston and Wendt, n.d.).

The accurate measurement of the distribution of radio emission across the solar disk was of prime interest as it provided information on the structure, density and temperature of the solar atmosphere, but particularly the chromosphere and corona. By measuring the distribution at different frequencies it was possible to compare the observations with various theoretical models (e.g. see Martyn, 1946; Smerd, 1950). These models predicted a progressive rise in the observed brightness temperature as the wavelength of emission increased from centimetre to metre wavelengths. The rise in brightness temperature was due to the area of origin varying from the comparatively cool chromosphere (10^4 K) to the hot corona (10^6 K). Also of interest was the prediction of increased brightness at the limb of the Sun, particularly at decimetre wavelengths. Meanwhile, earlier investigations had shown that at this wavelength the solar radio emission could be divided into two main components. The first, believed to be of thermal origin, was associated with the quiet Sun, and the second was a slowly-varying component that was correlated with the total area of sunspots visible on the solar disk. Both components provided information on the distribution of radio emission across the disk of the Sun.

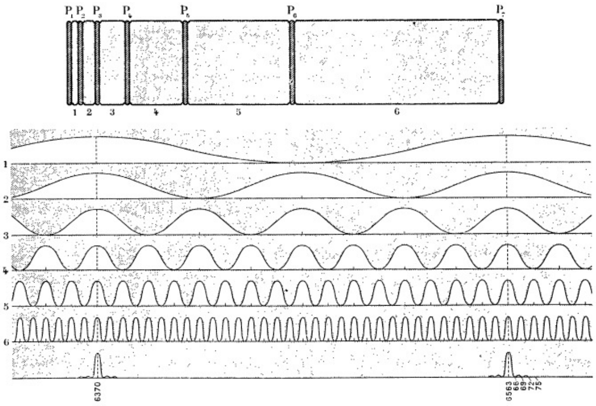


Figure 1: An illustration of Bernard Lyot's optical narrowband filter. The wavelength in Angstroms is shown on the X-axis. The two widely-separated narrowband responses at 6370 and 6563 Angstroms (bottom) are the result of summing the six different band-pass frequencies. The filter configuration is shown in the upper diagram. It was this work that gave Christiansen his inspiration for the design of the solar grating array (after Lyot, 1945: Figure 1).

Observations carried out during the partial solar eclipses of 1 November 1948 and 22 October 1949 (see Wendt et. al., 2008a), were successful in associating the enhanced emission with sunspot groups, but were inconclusive in detecting limb-brightening. Christiansen was looking for a way to perform high-resolution solar observations that did not rely on these relatively rare eclipses and would also not involve great expense. He was primarily interested in high-resolution observations at wavelengths of around 20 cm. To achieve a resolution of 3 arc minutes at these wavelengths would have required an aerial >1,000 wavelengths in diameter if a conventional parabolic design were to be used. Clearly this was not practicable.

Figure 2: View looking east showing W.N. Christiansen and the 32-element solar grating array at Potts Hill in the western suburbs of Sydney (courtesy ATNF Historic Photographic Archive).

At this time, John Bolton, Gordon Stanley and Bruce Slee (1949) were using sea interferometers to investigate discrete sources of cosmic radiation at the Radiophysics Dover Heights field station and Martin Ryle and D.D. Vonberg (1946) at Cambridge were making similar advances using a standard two-element interferometer. H.M. Stanier used a two-element interferometer to obtain the brightness distribution across the solar disk in 1950, and Alex Little and Ruby Payne-Scott (1951) were making good progress in measuring the accurate positions of solar bursts with a swept-lobe interferometer located at Potts Hill in the western suburbs of Sydney.

Stanier's use of the two-element interferometer to determine the solar brightness distribution was the first practical application of the Fourier imaging technique in radio astronomy (although this approach had been suggested by Joe Pawsey, Payne-Scott and Lindsay McCready in 1947). In doing so, Stanier made the simple assumption that the Sun was circularly symmetrical so that the distribution could be calculated from one scanning angle, but this also implied that all the components of the interference pattern were even (cosine), and therefore only the amplitudes and not the phases of the interference fringes needed to be measured. The use of the circularly-symmetrical assumption, and possibly the presence of localised active regions on the solar disk during observations, contributed to Stanier's failure to detect limb-brightening.

K.E. Machin (1951) followed up on Stanier's work, improving on the technique and conducting observations at 81.5 MHz. He was followed by P.A. O'Brien (see O'Brien, 1953a, 1953b; O'Brien and Bell, 1954; O'Brien and Tandberg-Hassen, 1955), who, during 1951-1952, used a two-element interferometer at a number of wavelengths and a variety of spacings and observing angles to calculate the two-dimensional brightness distribution across the solar disk. This was the first time that two-dimensional Fourier synthesis had been used to produce an image of the Sun.

In late 1949, Christiansen and Don Yabsley began experiments using two ex-WWII TPS-3 aerials as a two-element interferometer in an attempt to detect limb-brightening (see Christiansen, 1949). In February 1950 Ryle wrote to Ron Bracewell, stating he was very interested to hear that the Australians planned to carry out spaced-aerial work at 600 and 1,200 MHz to look for limb-brightening, and particularly if "… a Fourier analysis …" was to be used. He pointed out that Stanier (1950) had performed this experiment at 600 MHz and not detected limb-brightening.

It was around this time that the initial idea for the construction of a solar grating array occurred to Christiansen, and he then abandoned further work with Yabsley on the two-element interferometer.[1] Ultimately Christiansen devised an approach that was analogous to a diffraction grating. He realised that by using a number of aerials arranged in a straight line at uniform spacings, the combined response of the array would produce multiple narrow beams which would be separated from each other as the inverse to the spacing between the aerials. As the Sun's disk is 30 arc minutes in diameter, the array could be configured so that only one of the beams could be positioned on the Sun's disk at any given time. Christiansen's inspiration for this configuration came indirectly, and was influenced by his background in antenna design:

> The idea occurred while reading a description of Bernard Lyot's optical filter in which narrow frequency pass-bands are produced at widely different frequencies. This may seem particularly indirect when the

> analogy which is more obvious is the optical diffraction grating, but to me as an antenna designer the cos n.cos 2n.cos 4n series of the Lyot filter immediately suggested an antenna array and an array of arrays. (Christiansen, 1984: 118).

The analogy Christiansen drew from Lyot's (1945) paper is best demonstrated by Figure 1, which shows that the sum of each of the different band-pass filters produces the two widely-separated narrowband responses.

It is likely that Christiansen was also familiar with a lecture on the topic that was given by Bruce Billings at the American Astronomical Society meeting on 29 December 1946 (see Billings, 1947).[2] Christiansen (1950) first presented the idea for his 'Multi-beam Interferometer' to RP's Radio Astronomy Committee when it met on 14 March 1950.

3 CONSTRUCTION OF THE GRATING ARRAY

Keeping the cost of the design to a minimum was one of Christiansen's prime concerns. He was only given permission by Taffy Bowen and Joe Pawsey to construct the array provided that the cost could be kept under £500, or ~AU$12,500 in today's terms (Christiansen, 1984: 118).[3] The mechanical engineering for the array was performed by Keith McAlister, who proved extremely resourceful in meeting the project's cost target.

The construction of this innovative solar radio telescope commenced in 1951. However, Christiansen was temporarily diverted from this task when Pawsey asked him to confirm the detection of the 21cm hydrogen line by Ewen and Purcell (see Wendt et al., 2008b). Nonetheless, the first Potts Hill solar grating array was completed in February 1952,[4] and Christiansen immediately began a program of daily observations. These observations were generally made over a two hour period centred on midday.

The array consisted of 32 aerials (Figure 2), which were evenly spaced at 23-ft (7m) intervals along an east-west baseline of 700-ft (213m) located at the southern end of the northern reservoir at Potts Hill.

The array was constructed by Radiophysics staff. Initially a series of 32 wooden posts was aligned by Joe Warburton and Rod Davies using a theodolite, and Davies (2005: 94) was later to comment: "At that time we didn't know that Ph.D. meant Post-hole Digger!"

Each aerial comprised a 66-in (1.7m) solid metal parabolic reflector plate. A dipole receiver and reflector were mounted at the prime focus. In this form all of the aerials were horizontally polarised. To observe circularly-polarised radiation the aerials could be configured so that there was a 90° phase difference introduced between adjacent pairs of aerials. In this way the complete system could resolve circular polarisation into its right-hand and left-hand components. Each of the aerials was equatorially mounted and could be manually stepped in right ascension via a series of holes in the mounting post and a locking peg to allow tracking of the Sun. During observations the aerial positions were changed approximately every 15 minutes by having someone run down the length of the array and adjust each of the 32 antennas by hand!

The aerial outputs were combined using a branching system of transmission lines. To keep costs down, the transmission lines were a braced open-wire system separated by a ¼ wavelength and supported by polystyrene insulators and spacers (see Figure 3).

Figure 3: Another view of the east-west array, showing the bracing weights for the open-wire transmission lines. The parabolas were equatorially mounted, with the declination set for the given day. The right ascension was changed in 15 minute steps using holes and a locking pin on the mount (courtesy ATNF Historic Photographic Archive).

To achieve the branching configuration the transmission lines were stacked vertically in five levels and connected via short vertical connectors. A schematic of the transmission-line system is shown in Figure 4.

The directivity of the array can be calculated from

$$\Phi(\theta) = \frac{\sin^2 Np}{N \sin^2 p} \qquad (1)$$

where $\Phi(\theta)$ is the power received from the source relative to the power received from one aerial; N is the number of elements in the array; and $p = \pi d \sin \theta/\lambda$, where d is the spacing between elements, θ is the angle between the perpendicular to the baseline and the direction of the source, and λ is the wavelength (after Christiansen and Warburton, 1953a: 192).

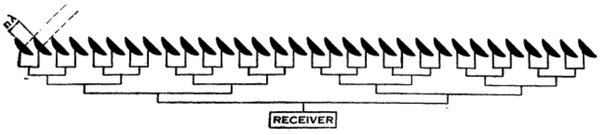


Figure 4: Schematic diagram of the branching transmission lines used for the 32-element solar grating array (after Christiansen and Warburton, 1953a: 192).

The array produced a series of fan-shaped beams each of which, at 1,420 MHz, had a calculated beamwidth of 2.9 minutes of arc at the half power points. The spacing between beams was 1.7°, which meant that at any one time only one beam would fall on the 30-arc minute solar disk. Figure 5 shows the beam response produced by the array.

A superheterodyne receiver was connected to the array transmission lines via a radio-frequency switch that contained a rotating condenser which switched the signal at a rate of 25 Hz between the transmission lines and a dummy load. The modulated signal was then passed to a crystal detector which was coupled to a line-tuned heterodyne-oscillator and a 30 MHz amplifier with a 4 MHz bandwidth. After the 30 MHz amplification was a further detector, a 25 Hz amplifier and a phase-sensitive detector. This then fed a recording milli-ammeter.

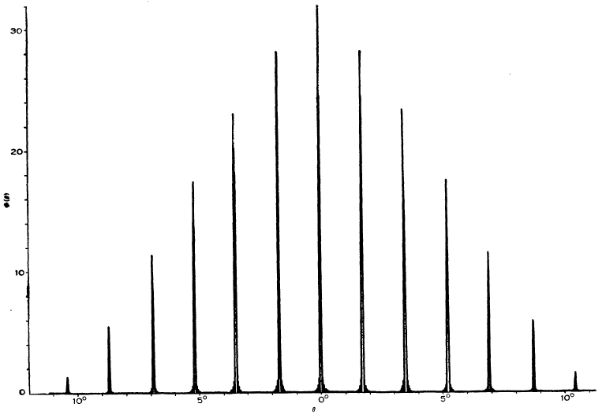


Figure 5: Beam response diagram for the 32-element array at 1,420 MHz (21cm). The power received from the source is shown on the Y-axis and the direction of the source relative to the array beam, on the X-axis. The beamwidth of each fan beam is 2.9 arc minutes, and the beams are separated by 1.7°. The overall response envelope of the individual beams is equivalent to the response of one of the individual aerials in the array (after Christiansen and Warburton, 1953a: 192).

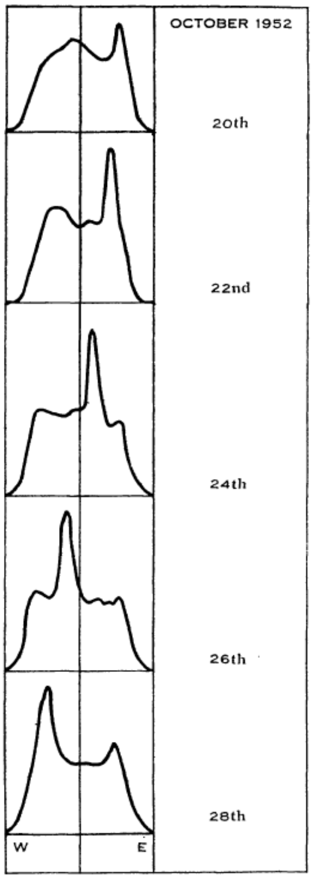


Figure 6: Daily records of one-dimensional brightness distribution across the solar disk between 20 and 28 October 1952. Each scan is just over 30 arc minutes in width, with the power received shown on the Y-axis. The successive scans show a source of enhanced emission associated with a sunspot group that was initially near the eastern limb of the Sun and progressed towards the western limb as the Sun rotated (after Christiansen and Warburton, 1953b: 198).

When the array was configured to measure polarisation the output recording characteristic would change. For linearly- or randomly-polarised radiation, successive records would be substantially similar in strength. For circularly-polarised radiation, successive records would show a diminished response depending on the sense of polarisation.

The high resolution beams of the grating array produced a one-dimensional response scan across the solar disk at 1,420 MHz. Using a succession of daily scans it was possible to determine how the one-dimensional profile changed over a number of days as the Sun rotated. Figure 6, for example, shows a succession of daily scans taken between 20 and 28 October 1952.

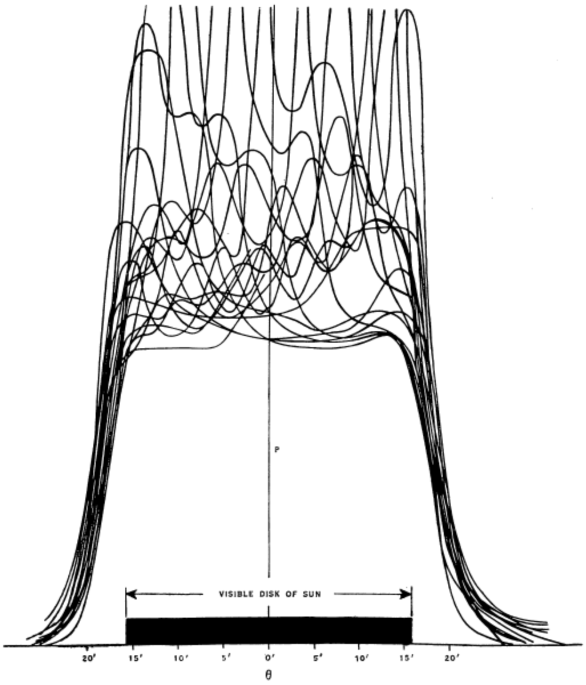


Figure 7: Twenty individual daily one-dimensional brightness distribution scans superimposed. The visual solar disk is indicated by the black bar on the X-axis. The inside envelope of the scans indicates the quiet base component of the solar emission (after Christiansen and Warburton, 1953a: 200).

One early finding that greatly simplified the analysis of the observations was that the centre of the radio record corresponded with the centre of the optical Sun, and that bright areas near the limb did not materially change the size of the radio disk. By superimposing the individual daily scans obtained over an extended period and ignoring localised areas of enhanced emission (termed 'radio plages'), a base level of radiation quickly became evident (e.g. see Figure 7). This base level indicated by the envelope of the successive scans is due to the quiet Sun, while the areas of enhanced emission above the envelope are due to the slowly varying component. Christiansen and Warburton (1953b) determined that the base level temperature of the Sun at 1,420 MHz was 7×10^4 K in 1952, during the period when the observations were made.

Another feature that is clearly evident in Figure 7 is that the source of the radio emission is larger than the width of the optical disk. For simplicity a circular symmetry was assumed for the purposes of the analysis, although there were already indications that the actual distribution was elliptical. Initially it was thought that the effect of this assumption would be small. However, it was fairly quickly recognised that taking the non-circular symmetry into account would be essential. Even allowing for an asymmetrical distribution of solar emission, it was very clear from the observations that limb-brightening was present. The brightening of the limb is due to the greater optical depth of the corona, which has a much higher temperature than the photosphere. Figure 8 shows examples of the radial distribution based on the one-dimensional scans, and these clearly contain evidence of limb-brightening—as predicted in a number of different theoretical models, including that proposed by Christiansen's Radiophysics colleague Steve Smerd (1950). Unfortunately, as the distributions were only measured at one frequency, it was not possible to determine which particular parameters of the models best matched the observations (although the latter were consistent with Smerd's model for a 10^4 K chromosphere and a 0.3-3.0 × 10^6 K corona.

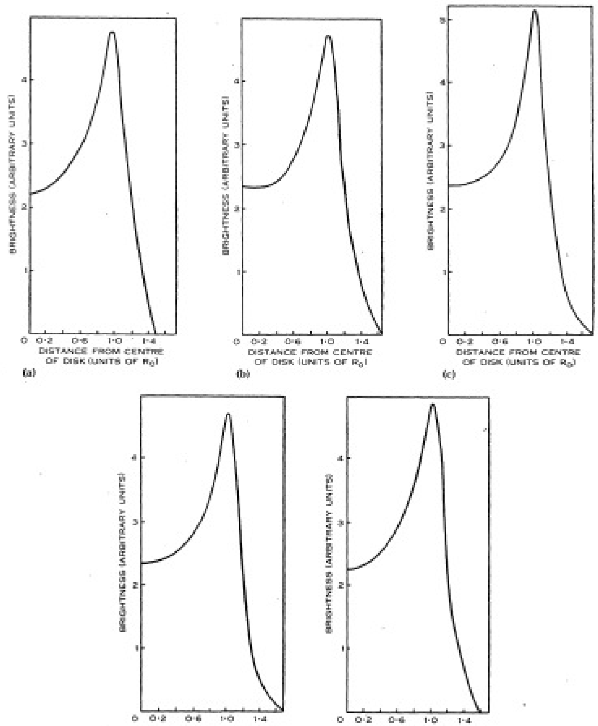


Figure 8: Examples of radial distributions of brightness across the solar disk based on one-dimensional scans. R_0 is the radius of the visible optical disk (after Christiansen and Warburton, 1953b: 268).

One of the limitations of observations using the east-west array was that fan beams could only scan the Sun in one dimension. In order to calculate the distribution of radiation across the solar disk it was therefore necessary to assume a symmetrical distribution, yet visual observation had revealed that the Sun is an oblate spheroid, and solar eclipse observations indicated that the solar corona was far from symmetrical (e.g. see Blum et al., 1952).

To overcome these limitations Christiansen realised that by using a second array, arranged in a north-south direction, the Sun could be scanned at a variety of angles.

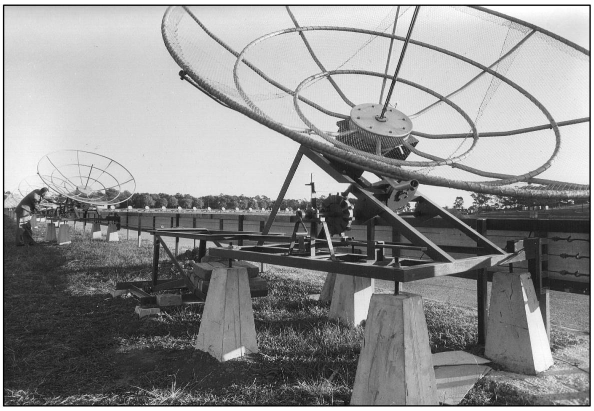


Figure 9: Close up of the north-south grating array, looking south, showing the robust equatorial mounting and the use of mesh rather than a solid reflector (courtesy ATNF Historic Photographic Archive).

4 THE NORTH-SOUTH GRATING ARRAY

A north-south grating array was then constructed on the eastern side of the same reservoir where the east-west array was located, but the aerial design for this new array was quite different. Instead of 32 elements, the north-south array had 16 elements, each consisting of open mesh parabolic dishes supported by robust equatorial mounts (see Figure 9).

The new array was also somewhat shorter than the east-west array, being 760 wavelengths (160m) in length as opposed to the 1,028 wavelengths (214m) of the east-west array. This meant that the array produced a slightly wider beam of 4 minutes of arc. The open transmission-line feeds were retained, and these can also be seen in Figure 9, with the east-west array in the distant background. Figure 10 shows an aerial view of the two arrays. This photograph was taken from the northeast, looking southwest.

Daily observations were made using both arrays from September 1953 to April 1954 (Christiansen and Warburton, 1955a). By observing over a long period Christiansen and Warburton were able to make use of seasonable variations in the Sun's orientation with respect to the two arrays and achieve a coverage of 140° out of a 180° range of scanning angles, as indicated in Figure 11.

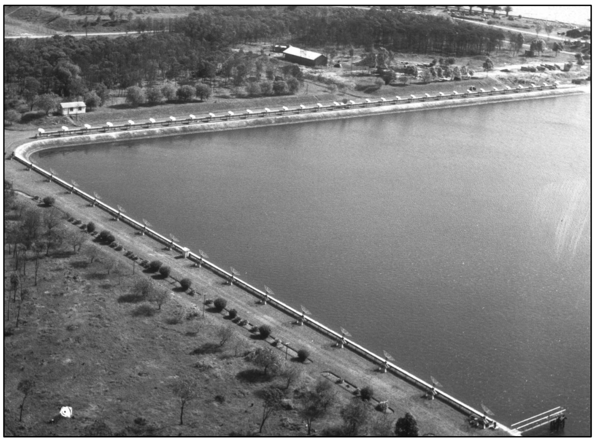
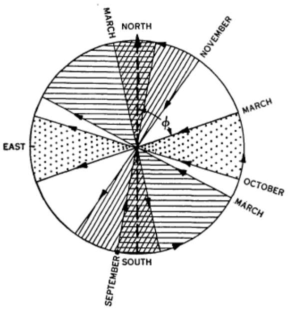


Figure 10: Aerial view of the 32-element east-west and the 16-element north-south arrays, looking southwest (courtesy ATNF Historic Photographic Archive).



SHADING	INTERFEROMETER USED	OBSERVATION PERIOD (LOCAL TIME)
▨	N.— S.	0800 → 1200 HR
░	E.— W.	1000 → 1200 HR
▤	N.— S.	1200 → 1500 HR

Figure 11: By observing the Sun over an extended period a large variety of different scanning angles could be achieved. This figure shows the coverage of the scanning angles by month for the two arrays (after Christiansen and Warburton, 1955a: 479).

Figure 12 shows an example of the scanning of the Sun with both the east-west and the north-south arrays. A source of enhanced emission on the solar disk is evident on all scans. The observations made with the east-west array were taken at an hour angle when the inclination of the aerial beams was fairly constant relative to the Sun's central meridian, and hence successsive scans are almost an exact replication. By contrast, the north-south array observations were made over a wide range of hour angles, and during the period of observation the scanning angle changed through a range of ~50°. As the hour angled changed during the observations the rate at which the solar disk passed through the beams also changed. This is evident in the lower plot in Figure 12 where, from left to right, the Sun passes more slowly through each scan until the central scan, then the process is reversed.

Over the course of a day a wide variety of scan angles could be observed and these could be extended further by observing over a period of months. Figure 13 shows the result of one-dimensional scans taken at different times on a single day, thus achieving different orientations relative to the Sun's axis of rotation.

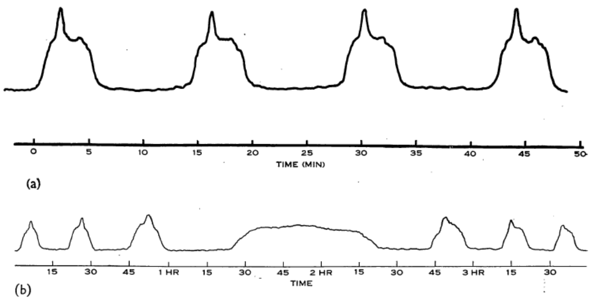


Figure 12: An example of the Sun passing through several of the beams of the east-west (a) and north-south (b) grating arrays. A source of enhanced emission on the solar disk is evident on each of the scans (after Christiansen and Warburton, 1955a: 477).

In order to produce a two dimensional image, a cosine Fourier analysis of the individual one-dimensional distributions for the different scanning angles was performed. It is important to note that by using the cosine Fourier analysis Christiansen assumed that the Sun was symmetrical, and phase was ignored. The numerical value for each scan was then plotted radially corresponding to the direction of the scan and then strip integrated with the strip summations being perpendicular to the scan angle. The cosine Fourier transform of the strip integrals was then taken to give radial cross-sections of the brightness distribution. The final two-dimensional distribution was then constructed by plotting each of the radial cross-sections and plotting contour lines joining points of equal intensity. This process took months of calculation and plotting by hand in order to produce the single two-dimensional image shown in Figure 14. For comparison, Figure 15 shows a photograph of the Sun taken during the total solar eclipse of 30 June 1954. The use of the symmetry assumption leads to the two-dimensional symmetry evident in Figure 14.

Although not widely acknowledged (see Christiansen, 1989), creation of the image in Figure 14 was the world's first application of Earth-rotational synthesis in radio astronomy.

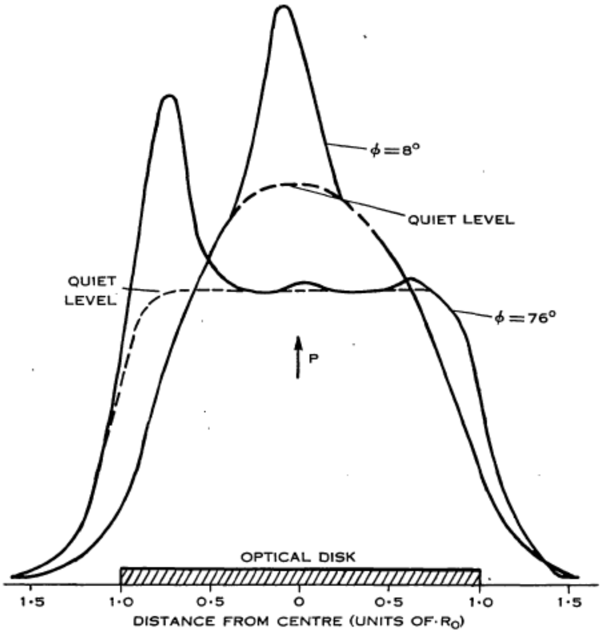


Figure 13: An example of one-dimensional scans taken for two different scanning angles by observing at different times on the same day. The angle *Φ* represents the scan angle of the aerial beam with respect to the Sun's central meridian, P (after Christiansen and Warburton, 1955a: 482).

The hand calculations that led to Figure 14 were performed by Christiansen and Warburton, assisted by Govind Swarup, using electronic calculators (but not computers; see Swarup, 2008). Bracewell (1984) has stated that the graphical method that was used for this reconstruction was adopted from his method of chord construction, although his contribution was not acknowledged in the published results. Bracewell had been assigned by Pawsey to work on the issue of fan beam reconstructions, and he shared an office with Christiansen and Harry Minnett at the time. Bracewell (1956) subsequently published a paper on strip integration based on this work. This paper includes a description of the use of the projection-slice theorem which would be used to underpin modern imaging techniques, including computerised tomography and medical imaging .

Figure 14 shows a strong correlation with the optical view of the corona seen at times of total solar eclipse. Furthermore, the elliptical radio source extended 1.6 times further at the equator than at the poles. In addition, the limb-brightening effect was not evenly distributed, with the strongest brightening at the equator and very little at latitudes beyond ±55°. Christiansen and Warburton noted that this latitude corresponded to the latitude at which structural changes in the corona could be observed at times of sunspot minimum. Also, there was a strong correlation between the outline of the 8,000 K contour and the photographic image. Christiansen and Warburton (1955a) concluded that the majority of the radiation at the centre of the image emanated from the chromosphere, while the limb-brightening was due to the greater optical depth of the corona, with its higher temperature gradient.

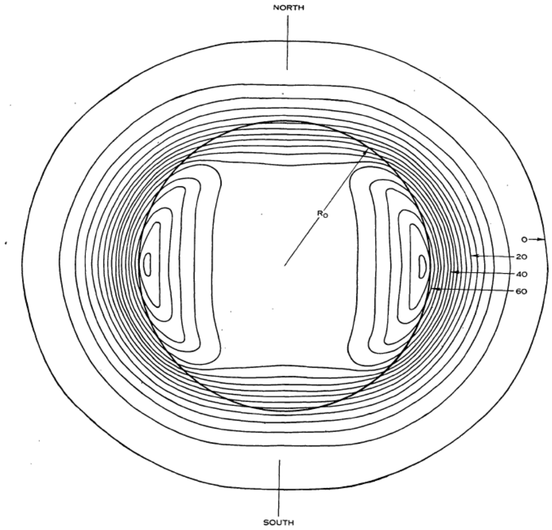
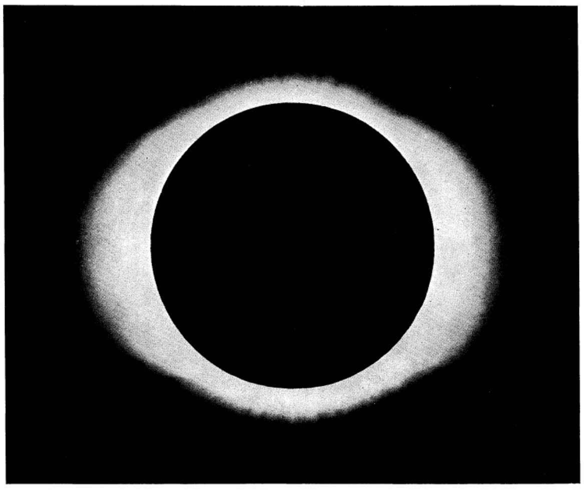


Figure 14: An example of the derived two-dimensional image of the radio brightness distribution across the Sun at 1,420 MHz. The central brightness temperature is 4.7×10^4 K and the maximum peak temperature is 6.8×10^4 K. Contours are spaced at equal intervals of 4×10^3 K. Observations were made during the period April 1952 to April 1954 (after Christiansen and Warburton, 1955a: 482).



Photograph of the Sun at the total eclipse June 30, 1954 (M. Waldmeier).

Figure 15: Photograph of the Sun taken during the 30 June 1954 total solar eclipse, representing a comparable period in the solar cycle to the radio observations (after Christiansen and Warburton, 1955a: Plate 2).

Observations over 1952, 1953 and 1954 showed no change in the shape or temperature of the quiet component of solar radiation at 1,420 MHz (Christiansen and Warburton, 1955a), thus providing support for the assertion by Jack Piddington and Davies (1953) that earlier reported changes in the base level of solar radiation were due to lag effects of changes in sunspot activity. Piddington and Davies (ibid.) concluded that the enhanced radio emission persisted for some time after sunspots had disappeared from the solar disk.

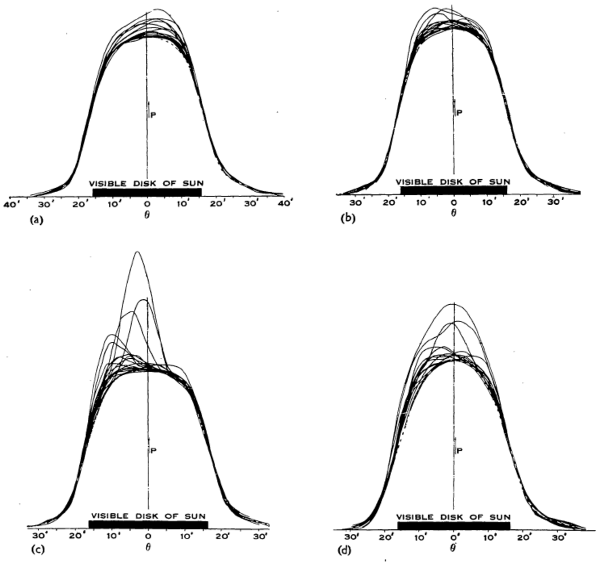


Figure 16: Superimposed one-dimensional brightness distributions at 500 MHz taken between July 1954 and March 1955. Observations were made during (a) 18 July to 5 August Φ = 3°; (b) 9 August to 1 September Φ = 11.5°; (c) 15 December to 3 January Φ = −3°; (d) 7 February to 4 March Φ = 26°; Φ represents the angle in arc minutes between the Sun's central meridian and the aerial beam (after Swarup and Parthasarathy, 1955b: 490).

5 THE 500 MHz GRATING ARRAY

During the U.R.S.I. General Assembly in Sydney in 1952 the French representatives invited Christiansen to work with them for a period, so in 1954 he moved to the Meudon Observatory (near Paris), on secondment from Radiophysics for one year. In Christiansen's absence, Swarup and R. Parthasarathy (1955b), who were working at Radiophysics under Colombo Plan Fellowships, modified the receiving equipment on the east-west array in order to carry out observations at 500 MHz (λ = 60cm). At this frequency the width of the fan beam was reduced to a theoretical value of 8.2 minutes of arc at the half power points, with a beam spacing of 4.9°. Swarup and Parthasarathy checked the actual beam response using Cygnus-A as a reference source and found the beamwidth to actually be closer to 8.7 minutes of arc.

From July 1954 to March 1955 Swarup and Parthasarathy used the east-west array to measure the one-dimensional distribution of radio brightness across the solar disk and to look for limb-brightening. By observing over a period of months they were able to scan the Sun at angles from 60° to 90° with respect to the central meridian. Figure 16 shows examples of superimposed observations over a period of several months.

One major interest at the time was the comparison of these results with the earlier observations by Stanier (1950) at the same frequency. Stanier had carried out his research closer to sunspot maximum and he did not detect limb-brightening. Figure 17 shows the two different radial distributions detected in 1950 and 1954/1955.

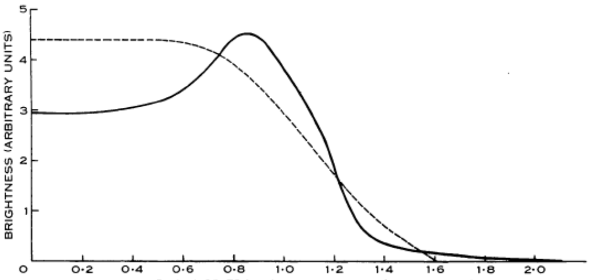


Figure 17: Radial brightness distributions at 500 MHz comparing Stanier's result (dashed line) and Swarup and Parthasarathy's observations (after Swarup and Parthasarathy, 1955b: 493; cf. Swarup and Parthasarathy, 1955a).

The interferometer observations made by O'Brien and Tandberg-Hassen (1955) had also detected limb-brightening, and Swarup and Parthasarathy's results were in good agreement with this. They also noted that like the higher-frequency observations by Christiansen and Warburton, the radio Sun did not appear to be circularly symmetrical. Although they were observing only with the modified east-west array, they were able to achieve a variety of scan angles by viewing at different times during the day and throughout the months. Figure 18 shows the different brightness distributions for aerial beams oriented at 90° and 64° to the Sun's central meridian. This indicated that the maximum width of the source occurred in the equatorial regions.

Swarup and Parthasarathy (1955b) calculated that the base apparent temperature of the quiet Sun at 500 MHz was 3.8×10^5 K, whereas Stanier obtained a figure of 5.4×10^5 K. Comparing their result with the previous eclipse observations, Swarup and Parthasarathy concluded that there was evidence to suggest a change in the base level temperature of the quiet Sun as a result in the decrease in the solar cycle.

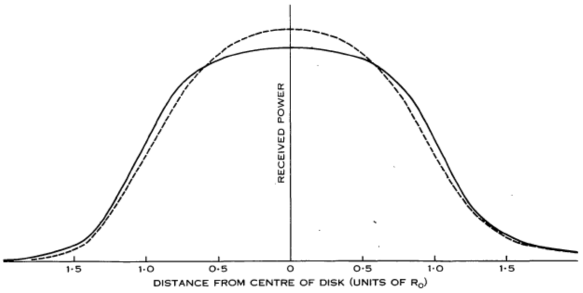


Figure 18: Brightness distributions at 500 MHz for the aerial beam at 90° (solid line) and 64° (dashed line) relative to the Sun's prime meridian. The observations show the maximum width of the emission occurs in the equatorial regions (after Swarup and Parthasarathy, 1955b: 491).

In 1957, Christiansen, Warburton and Davies published the fourth and final paper in their solar series based on observations made with the first solar grating array. This paper examined the slowly varying component based on the observations obtained during 1952 and 1953 (Christiansen et al., 1957). They concluded that the lag effect first suggested by Piddington and Davies (1953) was not sufficient to provide the sole explanation for the decline in base temperatures, and that it was likely that both the quiet component and the slowly varying component changed in the course of the solar cycle. They also concluded that the original correlation method proposed by Pawsey and Yabsley (1949) gave results that were quantitatively correct.

The paper by Christiansen, Warburton and Davies (1957) provided a clear illustration as to why the sunspot area correlation was in fact only a partial correlation. This is shown in Figure 19 where three groups are indicated: old sunspot regions, new regions and regions that have reached maximum intensity. The diagram shows that for new sunspots there was a delay before there was any correlation between sunspot area and the strength of the radio emission (see the path marked 'new region' in Figure 19). The larger active regions (see the path marked 'region at maximum activity' in the figure) showed some correlation between sunspot area and strength of emission, while the old sunspot groups ('old regions', in the figure) had almost disappeared in area, but continued to be associated with relatively strong radio sources. Although this analysis of the partial correlation with sunspots appears to require a great leap of faith, Christiansen, Warburton and Davies reached the conclusion that the radio emission was probably associated with plages rather than with sunspots. Plages occur in areas in the photosphere and chromosphere where sunspot groups grow and decay in the presence of strong localised magnetic fields. Christiansen and his two collaborators based this conclusion on a comparison of Mount Stromlo Observatory spectroheliograms and their solar grating array observations. Figure 20 shows an example of the comparisons, where the vertical lines indicate the maximum points during the one-dimensional radio scans.

A similar conclusion was reached earlier by Helen Dodson (1954) after comparing her McMath-Hulbert Observatory optical observations with Arthur Covington's Canadian radio observations, and she discussed this with Pawsey following an introductory lecture at the August 1955 IAU Symposium on Radio Astronomy at Jodrell Bank (see Allen, 1955: 262).

Using an analysis of the relative rates of rotation of the optical and radio sources, Christiansen et al. (1957) concluded that the radio emission emanated in a region about 24,000 km above the photosphere. They also found a strong correlation ($r^2 = 0.85$) between the size of the plages and the size of the radio sources and noted that it appeared that the sources behaved like thin disks lying parallel to the photosphere.

In 1958 Swarup and Parthasarathy published their second and final paper on the 500 MHz observations made with the modified east-west grating array. This paper dealt with their observations of the localised bright regions of radiation during the period July 1954 to March 1955. They found similar characteristics to those discussed by Christiansen et al. (1957): the sources of radio emission were closely correlated with chromospheric plages, were of the order of 3–6 arc minutes in size and appeared to be localised in regions ~35,000 km above the photosphere. Perhaps their most interesting finding was evidence of some variability in the localised sources. Figure 21 shows an example of the variation in the signal strength as the Sun passed through two adjacent beams.

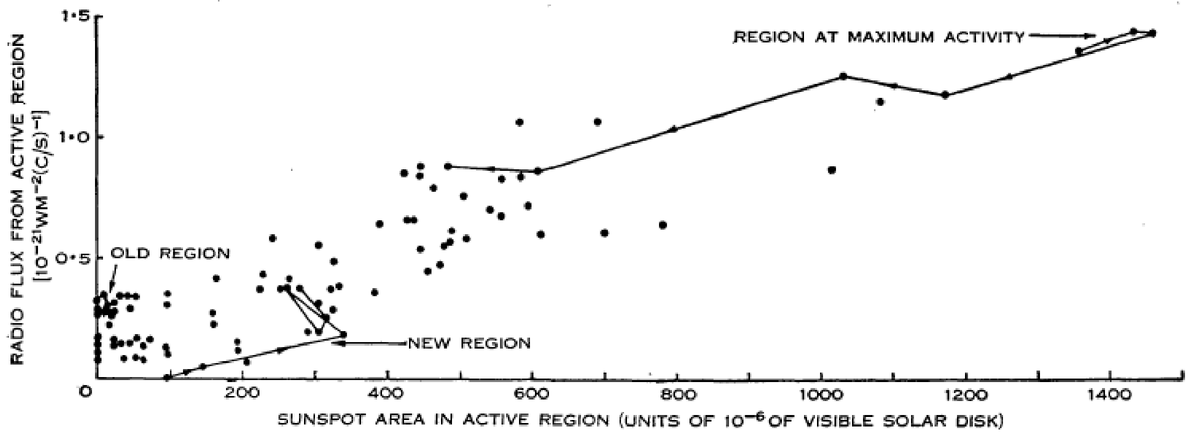
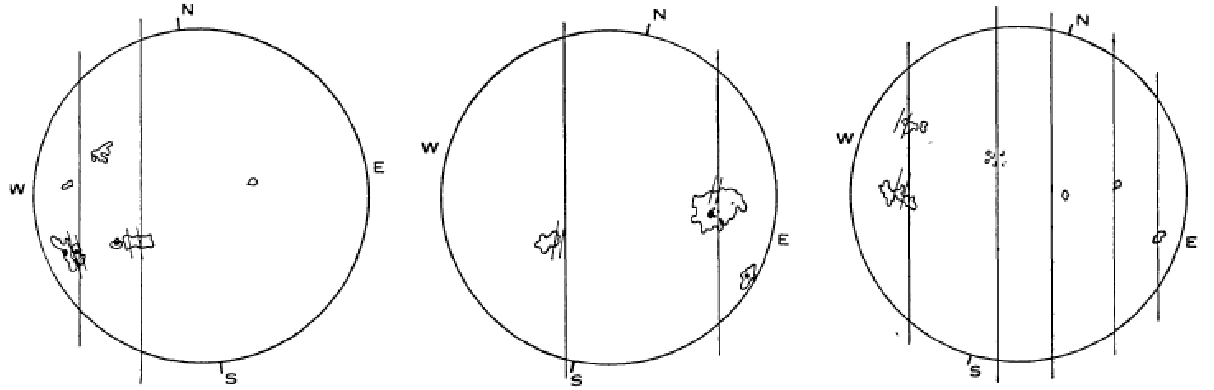


Figure 19: Scatter diagram of sunspot area versus radio flux. The day-by-day development of one new sunspot and one mature sunspot are shown by the lines connecting the points, with arrows marking the directions of development. Christiansen et al argued that the correlation of new and old sunspot areas with radio emission was not strong and only mature groups showed a strong correlation (after Christiansen et al., 1957: 511).



JULY 3, 1952
SPECTROHELIOGRAM 1400 U.T.
RADIO OBSERVATION 0200 U.T.

AUG. 23, 1952
SPECTROHELIOGRAM 0800 U.T.
RADIO OBSERVATION 0200 U.T.

SEPT. 13, 1952
SPECTROHELIOGRAM 1300 U.T.
RADIO OBSERVATION 0200 U.T
NO SUNSPOTS

Figure 20: Mount Stromlo spectroheliograms showing calcium K-line plage regions. The vertical lines indicate the positions of maximum emission on the Potts Hill scans. Note the strong correlation with plage regions (after Christiansen et al., 1957: 506).

These variations were observed on six occasions and lasted for periods of up to half an hour. This provided strong evidence for a non-thermal origin of some of the energy produced since a thermal change to an area the physical size of a radio plage could not occur that rapidly.

Swarup and Parthasarathy's paper was to be the last one based on solar observations made at Potts Hill.

6 THE PROTOTYPE CHRIS CROSS ANTENNA

Christiansen returned from France in 1955. However, during his absence he had determined to build a new array. The seed for this array had been sown in 1953 following a discussion with Bernard Mills. As Christiansen later recalled:

> While visiting Potts Hill one morning in 1953, Mills asked me why we did not couple the two arrays to produce high resolving power in two dimensions. During the ensuing discussion it was agreed that for this to be effective the centres of the two arrays must not be separated (as they were in the Potts Hill antenna), and also that some means had to be devised to multiply the outputs of the array. By the next morning Mills had devised the Cross Antenna consisting of a pair of thin orthogonal antennas with their outputs multiplied to give a single narrow response. (Christiansen, 1984: 122).

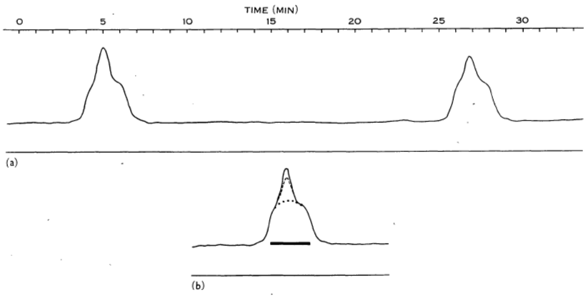


Figure 21: The Sun passing through two adjacent beams (top) and the two responses superimposed (bottom). An area of enhanced emission is present on the solar disk. The lower dotted line in the bottom graph (b) shows the level of the quiet Sun, while the upper dotted line shows the right-hand scan superimposed on the left-hand scan. This indicates the difference in solar intensity between the two scans, which were taken 22 minutes apart. The change in the level of enhanced emission during this short interval suggested that the source of the radiation must be non-thermal, as such a rapid change in the temperature of an area of this physical size could not occur (after Swarup and Parthasarathy, 1958: 345).

Figure 22: The prototype of the larger 5.8m aerial (left) that would be used in the new crossed array at Fleurs being tested at Potts Hill in 1956 adjacent to the north-south grating array (courtesy ATNF Historical Photographic Archive).

Mills went on to build the Mills Cross prototype at Potts Hill (Mills and Little, 1953) and ultimately the full-scale version at the Fleurs field station (Mills, et al., 1958). Christiansen decided to abandon the Earth-rotational synthesis technique he had developed, largely because it was too time-consuming to be useful for observing short-term changes in solar radiation. Instead he returned to the idea of the crossed array. Potts Hill did not have sufficient vacant land on which to build an array with a common centre, so Christiansen decided to also move his activities to Fleurs, where a new array was ultimately constructed.

A prototype of the aerial design that was to be used at Fleurs was tested at Potts Hill. Figure 22 shows the larger prototype aerial located next to the original north-south array, and a close-up of one of these new antennas is given in Figure 23.

Figure 23: Close-up of the 5.8m Fleurs Chris Cross prototype antenna undergoing testing at Potts Hill (courtesy ATNF Historical Photographic Archive).

7 TRANSFER OF THE EAST-WEST GRATING ARRAY TO INDIA

The new Fleurs array—known affectionately as the ‘Chris Cross’—began operation in 1957, and produced daily 1,410 MHz maps of the Sun (see Orchiston, 2004). With this development the Potts Hill grating arrays became redundant, and they were earmarked to be scrapped. Fortuitously,

> Pawsey liked to visit all the RP field stations unannounced to see what his staff were doing … and during one of his surprise visits to Potts Hill I asked whether these dishes [in the east-west array] could be gifted to India. He readily agreed to this suggestion, as did E.G. (Taffy) Bowen, Chief of the Division of Radiophysics. On 23 January 1955, I wrote to K.S. Krishnan about the possibility of transferring the thirty-two dishes from Sydney to the NPL (National Physical Laboratory) in New Delhi. (Swarup, 2006: 25).

Although Australia agreed to donate the equipment under the Colombo Plan Scheme, there was a substantial delay before the equipment was actually shipped to India as there was no agreement as to who should bear the cost of shipping—which at the time was about 700 Australian Pounds (ibid.). Eventually the C.S.I.R.O. agreed to meet the shipping costs and the 32 antennas were dispatched to New Delhi in the late 1950s.

In mid-1963 the array was transferred from the National Physical Laboratory in New Delhi to the Tata Institute of Fundamental Research and was set up at Kalyan, near Bombay, for solar observations at 610 MHz. The original 32 aerials were configured as two arrays, one of which consisted of 24 aerials oriented along a 630 metres east-west baseline and the remaining 8 aerials along a 256 metres north-south baseline. Known as the Kalyan Radio Telescope, this new instrument began operations in April 1965 (for further details see Swarup, 2008).

The fate of the Potts Hill north-south array is less clear although it appears that at least some of the aerials were either transferred or donated to universities within Australia. In March 1961, Professor G.R.A. Ellis from the Department of Physics at the University of Tasmania asked if it was possible to obtain any old aerials from Radiophysics. Pawsey (1961) replied stating that “… some time ago we gave one or several (old dishes) to Reg Smith.” Dr. Smith, from the Department of Physics at New England University in Armidale, was conducting ionospheric research at this time, although the results of this work were never published.

8 CONCLUDING REMARKS

Dr W.N. (‘Chris’) Christiansen played a key role in the early development of solar radio astronomy. His first (east-west) Potts Hill solar grating array, which had the ability to produce high-resolution one-dimensional scans across the solar disk in a short interval of time, was unique. This array was used very effectively to investigate the 1,420 MHz brightness distribution across the solar disk, and it provided valuable data on the structure of the solar atmosphere.

Once a second, north-south, grating interferometer was operational at Potts Hill the two arrays were used to produce a map showing the two-dimensional distribution of radio brightness across the solar disk. In constructing this contour map, Christiansen and his collaborators made the first application of Earth-rotational synthesis in radio astronomy. Although O’Brien, working at Cambridge, had earlier used a two-element interferometer to produce a two-dimensional image, Christiansen's Earth-rotation technique proved to be a far simpler method.

After 1,420 MHz solar astronomy was abandoned at Potts Hill, Swarup made sure that Christiansen's legacy would live on in India in the form of the Kalyan Radio Telescope.

9 NOTES

1 Don Yabsley subsequently left the Radio Astronomy Group to work on the development of air navigation technology.

2. A copy of Billings' paper and lecture slides were held on file at Radiophysics and are now in the National Archives of Australia in Sydney.

3. Although this is the accepted figure, on a different occasion Christiansen recalled that the cost needed to be kept below £180 (see Bhathal, 1996).

4. The completion of the east-west array in February 1952 meant that it was operational in time for the Tenth General Assembly of the International Union of Radio Science (URSI) which was held in Sydney between 8 and 22 August 1952. A field trip to Potts Hill and an inspection of the solar grating array was included in the program.

10 ACKNOWLEDGEMENTS

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Harry Wendt, B.Sc. (Hon), M.Sc. (Astronomy), recently completed a Ph.D. at James Cook University, Townsville, Australia. His thesis topic was "The Contribution of the Division of Radiophysics Potts Hill and Murraybank Field Stations to International Radio Astronomy".

Dr Wayne Orchiston is a Senior Lecturer in Astronomy at James Cook University, Townsville, Australia. His research interests include Australian, New Zealand and French radio astronomical history. He has published extensively, and has edited the book *The New Astronomy. Opening the Electromagnetic Window and Expanding our View of Planet Earth* (2005). He also has a book on early Australian radio astronomy, co-authored by Woody Sullivan and Jessica Chapman, which (hopefully) will be published by Springer in 2009. Wayne is a member of IAU Commissions 40 (Radio Astronomy), 41 (History of Astronomy) and 46 (Astronomy Education and Development). Currently he chairs the IAU Working Group on Historic Radio Astronomy.

Dr Bruce Slee is an Honorary Associate at the Australia Telescope National Facility in Sydney, and an Adjunct Professor in the Centre for Astronomy at James Cook University. He was one of the original discoverers of the first discrete radio sources and participated in one of the first metre-wave sky surveys for radio sources, followed by measurements of their angular sizes with medium base-line interferometers. His other interests include scattering in the interplanetary medium, radio emission from active stars, absorption in the interstellar medium, pulsar research, surveys of clusters of galaxies, radio relics in clusters and, in more recent years, the history of Australian radio astronomy. Bruce is a member of the IAU Working Group on Historic Radio Astronomy, and he continues to research topics which have engaged his attention for more than 60 years.

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W.N. CHRISTIANSEN AND THE INITIAL AUSTRALIAN INVESTIGATION OF THE 21cm HYDROGEN LINE

Harry Wendt
Centre for Astronomy, James Cook University, Townsville,
Queensland 4811, Australia.
E-mail: Harry.Wendt@jcu.edu.au

Wayne Orchiston
Centre for Astronomy, James Cook University, Townsville,
Queensland 4811, Australia.
E-mail: Wayne.Orchiston@jcu.edu.au

and

Bruce Slee
Australia Telescope National Facility, PO Box 76, Epping, NSW 2121,
Australia and Centre for Astronomy, James Cook University,
Townsville, Queensland 4811, Australia.
E-mail: Bruce.Slee@csiro.au

Abstract: On 25 March 1951 H.I. Ewen was working on his doctoral thesis at Harvard University when he detected the 21cm hydrogen emission-line (H-line). Within four months of the initial detection, small groups working in Australia and in The Netherlands were able to confirm Ewen's detection, thereby heralding a new chapter in international radio astronomy. This paper examines the Australian efforts that culminated in the confirmation of the H-line detection, and led to an initial survey of the southern Milky Way which produced the first indication of the spiral arm structure of our Galaxy.

Keywords: radio astronomy, hydrogen emission-line, Division of Radiophysics, W.N. Christiansen, J.V. Hindman

1 INTRODUCTION

By the early 1950s the CSIRO Division of Radiophysics had established itself as a leader in the new field of radio astronomy (Sullivan, 2005: 11). At this time the Division was operating a number of field stations in and around Sydney focused on both solar and cosmic research programs (see Orchiston and Slee, 2005).

When the discovery of the 21cm hydrogen emission-line (henceforth H-line) was announced, Radiophysics scientists quickly mobilised to confirm the discovery. The initial confirmation detection was made by W.N. 'Chris' Christiansen (Figure 1) and J.V. 'Jim' Hindman (Figure 2), who were working at the Potts Hill field station in the western suburbs of Sydney (Davies, 2005; Wendt, 2008). Following the confirmation, Christiansen and Hindman went on to conduct a preliminary survey of the southern Milky Way and found evidence of the spiral-arm structure of the Galaxy (Christiansen and Hindman, 1952a; 1952b).

2 THE H-LINE PREDICTION

Nearly all of the early major discoveries in radio astronomy, including Karl Jansky's original detection of cosmic radio emission, were serendipitous. Serendipitous discoveries in radio astronomy have been extensively discussed in the literature (see Kellermann and Sheets, 1983). Perhaps the best example of an exception to this phenomenon was the discovery of the 21cm hydrogen emission line. As Woody Sullivan (1982: 299) has noted, the prediction of the H-line was remarkable on two counts; both for its scientific prescience and for the conditions under which it was produced. Hendrik Van de Hulst was a student at the time of the Nazi occupation of Holland and his supervisor from Utrecht University had been interned. Van de Hulst spent three months visiting Leiden (van Woerden and Strom, 2006: 17, Note 2), where under Jan Oort's guidance he examined the possibility of radio line emission from neutral hydrogen. In a paper published immediately after the war ended, van de Hulst cautiously noted the possibility of detecting an emission line:

> The ground state of hydrogen is split by hyperfine structure into two levels with a separation of 0.047 cm^{-1}. The spins of the electron and proton are pointed in the same direction in one state and are opposite in the other state. A quantum of wavelength 21.2 cm is emitted due to a spontaneous flip of the spin. (van de Hulst, 1945).

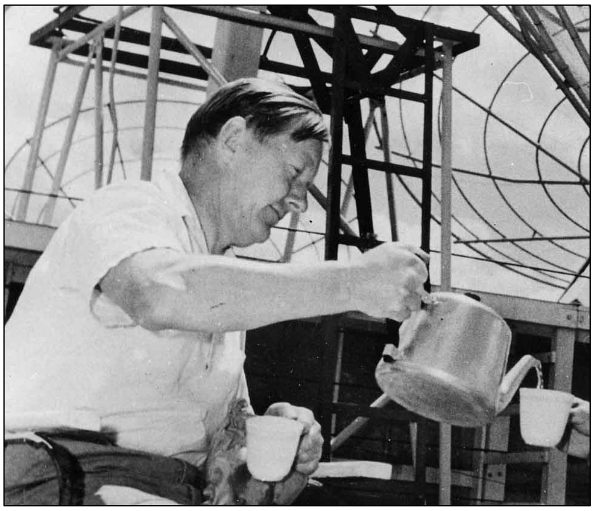

Figure 1: W.N. Christiansen at Fleurs Field Station in 1957 (courtesy: ATNF Historical Photographic Archive).

Van de Hulst noted that the transition to ground state was a forbidden transition and therefore it was necessary to assume a probability for the spontaneous transition to the preferred ground state. Provided that the life time of the hydrogen atom in the upper hyperfine-structure level was less than 4×10^8 years, there was a possibility of detection. He also noted that the sensitivity of radio receivers would need to be improved by a factor of 100 over the 1940s levels of equipment for the emission to be detected.

The actual value of the emission frequency from the spin flip transition to the ground state is 1,420.4 MHz (λ = 21.1 cm) and is due to the hyperfine structure transition being 5.9×10^{-6} eV (Wild, 1952). This is an extremely small energy level when compared (for example) to the Lyman-alpha transition of 10.19 eV which produces an emission at the much shorter wavelength of 122 nm. The probability of transition to the ground state is 2.9×10^{-15} $\sec^{-1}$ (~10^7 years), and is within van de Hulst's original limit.

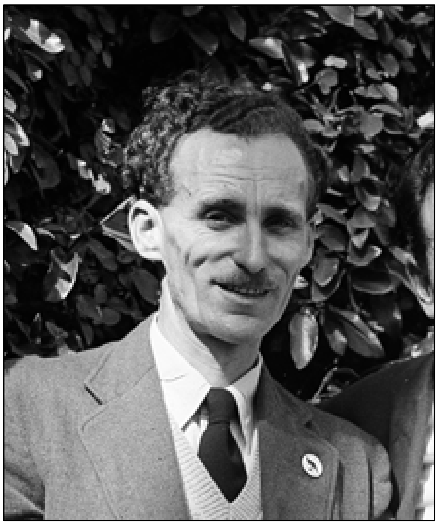

Figure 2: J.V. Hindman in 1952 (courtesy: ATNF Historical Photographic Archive).

3 RADIOPHYSICS PRE-DISCOVERY

The leader of the Radiophysics Radio Astronomy Group at this time was Joseph L. Pawsey. He was well aware of the potential that detecting radio spectral lines could provide. He was also familiar with the predicted 1,420.47 MHz hydrogen emission and also the prediction of deuterium emission at 237.38 MHz.

It was Grote Reber who had alerted Pawsey to the theoretical predictions and to the possibilities of detection during a visit Pawsey made to the U.S. in early 1948. Given the important implications that the detection of a radio-frequency spectral line would bring to radio astronomy, he alerted E.G. 'Taffy' Bowen (Chief of the Division of Radiophysics) to this potential in a letter dated 23 January 1948. Pawsey (1948) also included a section titled, "The Search for Atomic Spectral Lines in Noise" in the trip report he wrote following his visit to the United States. After a discussion of the potential in the report he concluded:

> The position is therefore quite uncertain. 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 fields and so on. (ibid.).

During his U.S. visit Pawsey also visited Harvard and met Oort who was visiting Yerkes Observatory at the time. However, there is no mention of any discussion on the H-line potential with these parties.

Bowen responded to Pawsey's U.S. visit report in a letter dated 18 May 1948. In this he noted:

> This [atomic spectral lines] possibility 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 hydrogen and deuterium lines could be made with the Georges Heights equipment[1] but this would involve dislocation of other work which is scarcely justified at present. At the moment Harry Minnett is chasing up 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. (Bowen, 1948).

The report from Pawsey triggered some activity in Radiophysics. In early 1949, Paul Wild produced an internal report titled, "The Radio-Frequency Line-Spectrum of Atomic Hydrogen. I. The Calculation of Frequencies of Possible Transmissions." This report was a comprehensive survey of the earlier theoretical work on the subject, and Bowen noted in a letter to F.W.G. White (Chief Executive Officer of the C.S.I.R.O. and former Chief of the Division of Radiophysics) on 21 March 1949: "There is nothing very original about it but it serves to indicate the direction in which this work might go." (Bowen, 1949).

White replied to Bowen's letter on the 28 March 1949 and noted:

> I have looked through it [the report] and find that, even to one who is not a spectroscopist, it is relatively easy to follow. The end results are certainly very interesting, and I hope that experimental data can now be found to which these can be related. (White, 1949).

As Sullivan (2005: 14) has reported, in 1949 Bernie Mills had considered taking on the H-line search as an in-dependent line of research, but dismissed it as too speculative. John Bolton and Kevin Westfold had also considered searching for the H-line (Robertson, 1992: 82). They had a copy of a Russian paper translated in an effort to obtain more details, however no search was under-taken. John Murray (2007) also recalls that on a number of occasions at meetings of the Solar Noise Group Ruby Payne-Scott proposed a search for the H-line.

Despite this early insight, there was no detection attempt made by the Radiophysics Group. Westfold has attributed the lack of an immediate investigation to Pawsey's conservative nature (Robertson, 1992: 82). As late as February 1952, in a meeting of the Radio Astronomy Sub-Committee on Galactic Work, Alex Shain raised the possibility of looking for line spectra as part of the group's research efforts. In attendance at this meeting were Pawsey, Bolton, Mills, Minnett, Jack Piddington and Shain. The outcome was recorded in the minutes as: "It was decided, however, not to plan for this as it could be easily fitted into other projects." (Mills, 1951).

4 THE H-LINE DISCOVERY

On 25 March 1951, H.I. Ewen working on his doctoral thesis in the Lyman Laboratory at Harvard detected the 21-cm hydrogen emission-line (Ewen and Purcell, 1951). In a remarkable coincidence, van de Hulst was visiting Harvard at the time and discussed the detection with Ewen and his supervisor, E.M. Purcell. Van de Hulst indicated that the Dutch group under Oort and C.A. Muller had been attempting to detect the H-line for some time. By Ewen's own account (2003) he was unaware of the Dutch group's work and had dismissed the possibility of the Dutch actively pursuing a detection attempt because he had interpreted van de Hulst's comments in his original paper as indicating that a detection was highly unlikely. In fact, Ewen thought it likely that his thesis would indicate a negative result. Ewen believed that if anyone would undertake a detection attempt it would be a group from the Soviet Union on the basis of I. Shklovsky's (1949) independent prediction (with which Ewen was familiar).

Also visiting Harvard at this time was Frank J. Kerr from the Radiophysics Laboratory in Sydney (Kerr, 1984: 137). Kerr was on a fellowship to Harvard to undertake studies in astronomy at the Harvard College Observatory under Donald Menzel. Kerr had written to Pawsey on 17 March 1951 drawing his attention to the fact that Ewen and Cornell University's Leif Owren had made unsuccessful attempts to detect the H-line (Kerr, 1951). Owren had used an 8-ft parabola, and a receiver similar to Ewen's but with less sensitivity.

On making the initial discovery Purcell and Ewen shared details of the discovery with the Dutch group and were keen to obtain an independent confirmation of the detection. Kerr sent Pawsey an airmail letter dated 30 March 1951 alerting him to the discovery and asking if the Radiophysics group could assist in the confirmation, even though no prior work had been conducted at Sydney. The letter included a hand-drawn sketch of the H-line response on Ewen's receiver (Figure 3). In a letter dated 20 April 1951, Pawsey wrote to Purcell saying that because of the "great potentialities" he had assigned two separate groups to attempt the independent detection and they were optimistically hoping to get results "… in a few weeks". He also asked about Purcell's plan to publish the discovery, and suggested that the Radiophysics team would privately advise the Americans of any detection and then publish a confirmation note at the same time Ewen and Purcell published their result.

5 THE RADIOPHYSICS DETECTION

In his letter to Purcell, Pawsey had referred to "two independent groups" working on attempting a confirmation. A meeting had been held on 12 April to coordinate the activities of the Radiophysics Group in attempting a confirmation observation, and in attendance were Pawsey, Arthur Higgs, Piddington, Christiansen, Wild and Bolton. The minutes state:

> It was agreed that parallel investigations to check delectability of lines were desirable in order to obtain independent checks but that, in order to avoid cut-throat competition, the groups who were experimenting in the same field, e.g. Piddington, Christiansen and Wild, should consider themselves, at least on the 1420 Mc/s line, as a single group and possible publication should be joint.
>
> Wild outlined the theoretical results he had obtained (mainly in RPL. 33 and 34). The chief point of interest is the existence of fine-structure lines at 10,905, 3,231 & 1,363 Mc/s with "inherent" line widths of the order of 100 and 20 Mc/s respectively.
>
> It was agreed to recommend Wild to write up this material for publication.
>
> Christiansen and Bolton outlined schemes for attempting to detect the 1420 Mc/s line with which they were proceeding (also corresponding deuterium line). They hope to have equipment for tests to start in a week or so.
>
> Piddington outlined a different scheme with which he was proceeding. (Pawsey, 1951b).

Elsewhere, Orchiston and Slee (2005: 139) state that Christiansen and Hindman had been working independently at the Potts Hill field station before they discovered they had both been tasked by Pawsey to work on the same problem. This is likely a reference to the early parallel work by Piddington and Christiansen, since at the time Hindman was working with Piddington. But it is unlikely that they did not know about each other's work; rather, this was a deliberate strategy by Pawsey, as the minutes of the 12 April meeting reflect.

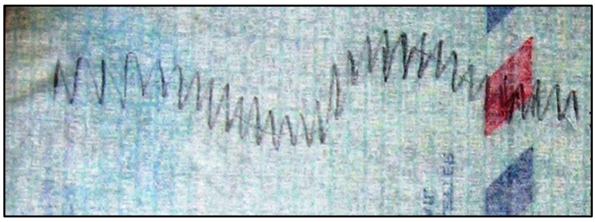

Figure 3: Hand-drawn sketch by Kerr of the H-line response detected by Ewen, included in a letter to Pawsey dated 30 March 1951 (National Archives of Australia – 972420 – C3830 – A1/3/17 Part 1).

After a short period, Christiansen took over the leadership of the group, with support from Hindman. It is unclear when Bolton's detection attempts were abandoned. However, later, in 1953-1954, an unsuccessful attempt to detect the deuterium line was made by Gordon Stanley and Robert Price using the 80-ft 'hole-in-the-ground' antenna at Dover Heights (see Stanley and Price, 1956; cf. Orchiston and Slee, 2002).

Purcell replied to Pawsey in a letter dated 9 May 1951. He welcomed the efforts of the Sydney group and provided further details of the detection and the receiver equipment. He also indicated that he and Ewen intended announcing their discovery in *Nature* "fairly soon", but would allow time for a reply before proceeding. Pawsey replied on 18 May 1951, saying that Christiansen would be, "… attempting the first observations tonight …" and since he (Pawsey) would be away for the next fortnight Christiansen would communicate directly if the attempt was successful, although he noted it would likely take several weeks. He also suggested that Ewen might wish to publish a detailed report in the newly-created *Australian Journal of Scientific Research.*

Christiansen and Hindman (1952a: 438) were able to construct a 'makeshift' receiver very quickly thanks to a great deal of improvisation. The receiver was in principle similar to that used by Ewen and by Muller and Oort. Coupling the receiver to the 16-ft × 18-ft paraboloid at Potts Hill (Figure 4), they were able to confirm the detection by the beginning of June.

Figure 4: The 16-ft × 18-ft paraboloid at Potts Hill (courtesy: ATNF Historical Photographic Archive).

Figure 5 shows a block diagram of the major components of the receiver. It consisted of a superheterodyne receiver with double-frequency change. It had two intermediate-frequency channels. The first operated at 30 MHz with a bandwidth of 2 MHz and the second at 5 MHz with a bandwidth of 0.05 MHz. A second heterodyne oscillator was used to continuously sweep the tuning of the receiver back and forth over a 1 MHz range. The signal from the hydrogen emission-line was detected as a small increase in signal when the pass-band of the receiver swept over the H-line frequency. As the signal increase was very small an additional balancing method was used to improve sensitivity. This was done by switching the first heterodyne oscillator at 25 Hz between two frequencies 0.16 MHz apart at around 1,390 MHz. This caused the centre frequency of the band-pass to alternate between the two frequencies and therefore allowed comparison between the signals. Any difference between the signals appeared as a 25 Hz component of the rectified receiver output. This component could then be recognised by using a selective amplifier and a phase-sensitive detector which was synchronised with the 25 Hz generator. As the receiver was tuned over the 1 MHz frequency band where detection of the H-line was predicted to appear, the energy produced by the H-line was first detected in one band-pass of the two switch components 0.16 MHz apart. This caused an in-phase 25 Hz signal. It was then detected in the other component as an out of phase signal. This caused a characteristic sine-wave signal on the recorder output as illustrated in Figure 6.

The receiver for the H-line detection was assembled in approximately six weeks. As Christiansen has commented:

> Our research was done crudely but it was good fun and the results were exciting. When Purcell's research student Ewen came over and saw the gear I had, with cables lying all over the floor and ancient oscillators, he said, 'My God. I can understand why you could do it in six weeks and it took me two years.' (Chrompton, 1997).

And,

> The fellow [Ewen] who discovered it [the H-line] in the USA came out and when he saw the equipment that Hindman and I had used for it he said, 'I can't believe it.' It looks like old rubbish lying on the floor – absolute 'string and sealing wax'. (Bhathal, 1996: 37).

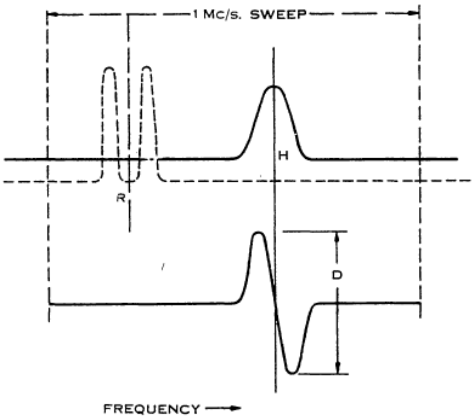

Figure 6: Illustration of H-line receiver operation and theoretical output signal. R = receiver pass-bands, H = H-line signal, D = recorder signal output (after Christiansen and Hindman, 1952a: 440).

And,

> We knew when we started that our gear was so rotten it mightn't work at all. Without exaggeration it was held together with string and sealing wax; Pawsey said it kept going through sheer will power. To make matters worse sparrows kept nesting in the aerial. We were stuck out at Potts Hill reservoir and it rained like all hell all the time. After observing for 10 days, without any luck we got fed up and went home, leaving the machine switched on. The next morning we found what we were after sitting up on the chart. (Christiansen, 1954).

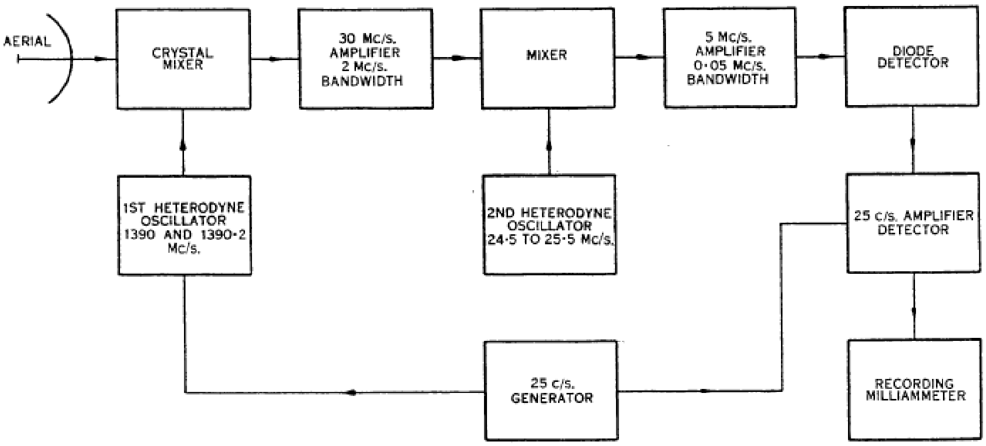

Figure 5: Block diagram of the Potts Hill H-line receiver (after Christiansen and Hindman, 1952a: 439).

Figure 7 shows an example of the H-line observation obtained by Christiansen and Hindman. This can be compared and contrasted with Ewen's original observations, an example of which is shown in Figure 8.

Ewen and Purcell's discovery was published in the 1 September 1951 issue of *Nature* in a letter dated 14 June 1951, and was followed by a confirmation paper from the Dutch group dated 26 June (Muller and Oort, 1951). After the Dutch paper was a short cabled communication dated 12 July which reported the Australian detection of the H-line. This read:

> Referring to Professor Purcell's letter of June 14 announcing the discovery of hyperfine structure of the hydrogen line in galactic radio spectrum, confirmation of this has been obtained by Christiansen and Hindman, of the Radio Physics Laboratory, Commonwealth Scientific and Industrial Research Organization, using narrow-beam aerial. Intensity and line-width are of same order as reported, and observations near declination 20° S. show similar extent about galactic equator. (Pawsey, 1951a).

The following day Pawsey sent Bowen a letter advising of the confirmation:

> Christiansen has worked … for the last two months trying to get this gear working and it is a very creditable performance on his part. The line is really exceedingly weak and it is necessary to make the right compromises all along the way in order to make the spectrum line evident. (Pawsey, 1951c).

6 INITIAL H-LINE SURVEY

Following the initial confirmation, between June and September 1951 Christiansen and Hindman proceeded to make a preliminary survey of hydrogen emission in the southern sky. The detailed findings of this survey were published in the *Australian Journal of Scientific Research* (Christiansen and Hindman, 1952a), and a summary paper appeared in *The Observatory* (Christiansen and Hindman, 1952b).

By taking a series of measurements in progressive steps of right ascension they were able to obtain a series of line profiles by declination. Figure 9 shows an example of a series of records taken along the Galactic Equator.

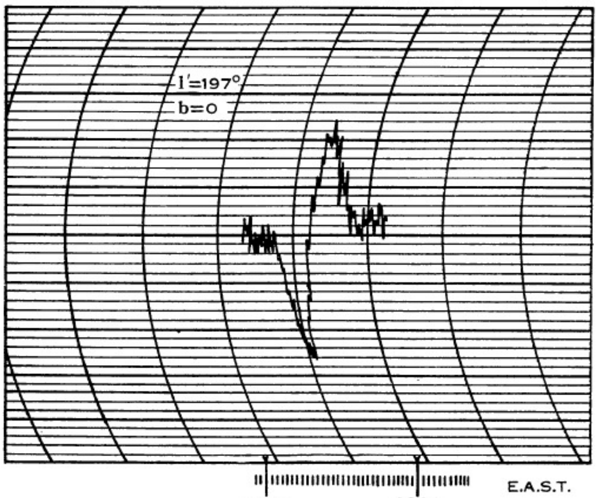

Figure 7: Example of H-line observation in the Taurus region (after Christiansen and Hindman, 1952a: 444).

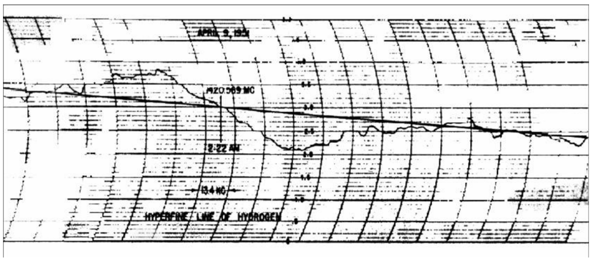

Figure 8: Example of the original H-line detection chart record made on 9 April 1951, approximately two weeks after the initial discovery (after Ewen, 2003).

From these individual records, the maximum deflection could be measured and hence a series of brightness intensities could be calculated. Figure 10 shows an example of the profile of peak brightness for the declination +10°.

By combining these profiles a contour chart of peak brightness was constructed. A peak brightness corresponding to a brightness temperature of approximately 100 K was observed. Figure 11 shows the final contour map of H-line emission. From this map it was evident there were marked variations in the peak brightness along the Galactic Equator. Christiansen and Hindman (1952a) noted that there were two likely causes of these variations, the first being due to line broadening caused by rotation of the Galaxy and the second—and more interesting possibility—was as the result of structural features in the Galaxy.

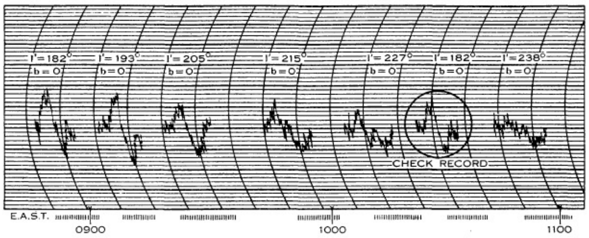

Figure 9: A series of six records taken along the Galactic Equator. A check record was performed near the end of each observing run as a check on receiver stability (after Christiansen and Hindman, 1952a: 445).

The line profiles were calculated based on the receiver response in the two swept band filters. Figure 12 shows examples of arbitrary line profiles and their corresponding receiver outputs.

The process of reconstruction of the line profiles from the receiver records was essentially the reverse of that shown in Figure 12. Figure 13 shows examples of the smoothed records and reconstructed line profiles from the Galactic Centre, the Anti-centre and Cygnus regions.

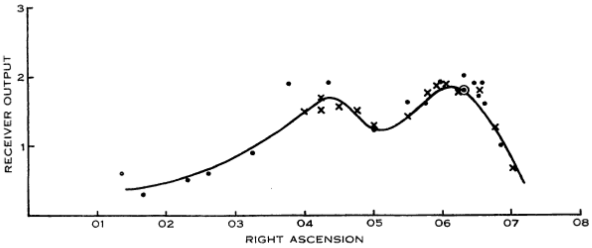

Figure 10: An example of the peak brightness profile in a strip along a declination of +10° (after Christiansen and Hindman, 1952a: 445).

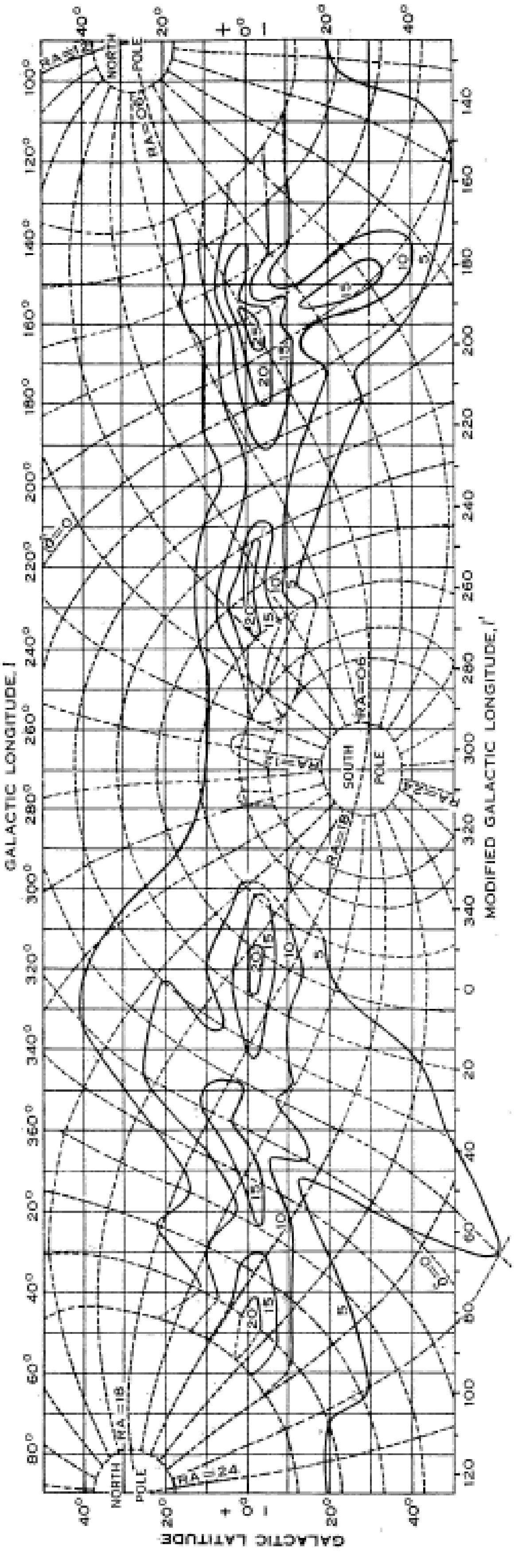

Figure 11: Southern sky contour map of H-line emission. The peak brightness of 25 units corresponds to a brightness temperature of approximately 100 K (after Christiansen and Hindman, 1952a: 446).

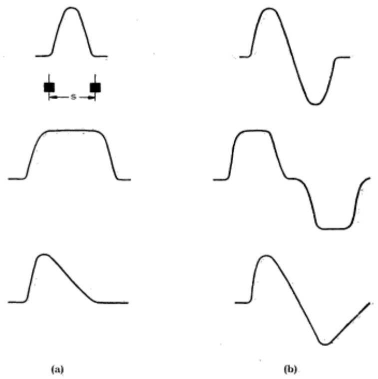

Figure 12: Example line profiles (a), and the corresponding receiver outputs (b). The sweep (s) of the two pass-bands (black boxes) is shown in the top left (after Christiansen and Hindman, 1952a: 442).

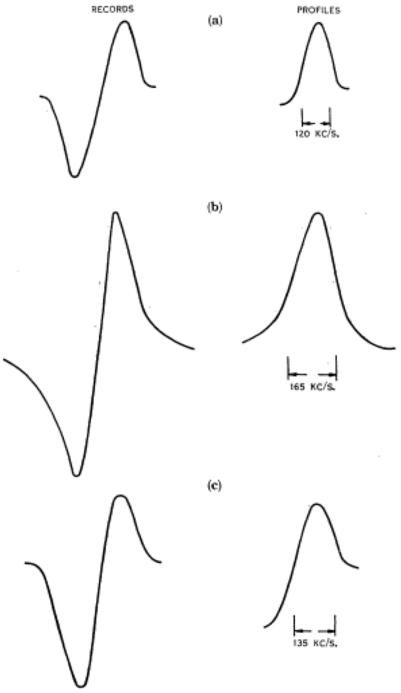

Figure 13: Examples of smoothed records and the calculated line profile in the region of the Galactic Centre (a), the Anti-centre (b) and the Cygnus region (c) (after Christiansen and Hindman, 1952a: 447).

Based on the broadening of line profiles, random velocities of the order of 12 to 18 km/s were estimated to be present in the neutral hydrogen clouds. In a number of cases double line profiles were also detected as shown in Figure 14.

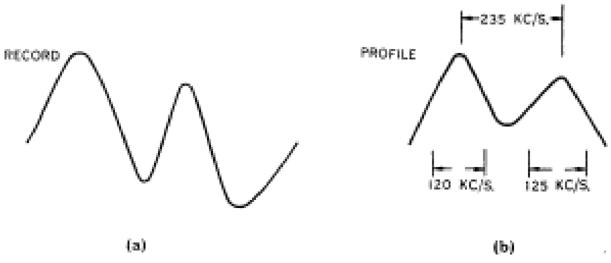

Figure 14: An example of the smoothed record and the resulting double line profile (after Christiansen and Hindman, 1952a: 448).

The existence of these double line profiles indicated regions with different radial velocities. Assuming a circularly symmetrical rotating galaxy, the radial velocity (v) of different regions is given by:

$$v = r.A.\sin 2l' \qquad (1)$$

where r is the distance of the source from the Sun, A is 6×10^{-16} sec^{-1}, and l' is the modified galactic longitude with respect to the galactic centre. From this equation, given a radial velocity estimate derived from the Doppler frequency shift compared to the rest frequency, a distance to the source can be estimated. The estimate for the two major regions showing double lines was 1,000 and 4,000 parsecs. Given the large size and the constant separation of the double lines as shown in Figure 15, the structure was suggestive of spiral arms in the Galaxy.

Further evidence supporting the detection of galactic structure was found by comparing the theoretical effect of galactic rotation with the actual observations. Assuming a uniform medium producing radiation, it is possible to calculate the brightness profiles for different hydrogen densities. Figure 16 shows the theoretical plots where (n) is the number of ground state hydrogen atoms per cm^3.

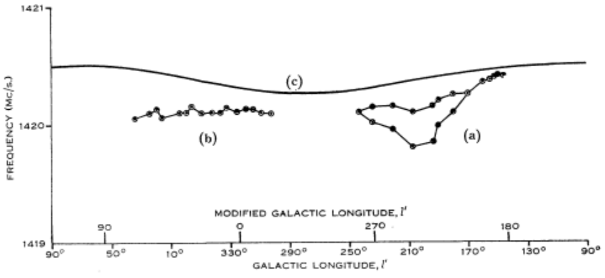

Figure 15: Plot of centre frequencies for line profiles showing double line profiles (a) and single line profile (b) regions. Line (c) is the expected frequency variation due to the Earth's relative motion (after Christiansen and Hindman, 1952a: 448).

The plot showed reasonable agreement with a density of somewhere between 1 and 0.5 atoms per cm^3. However, there were clearly regions that had factors other than rotation causing brightness variations. Also, by comparing the overall hydrogen emission to the general radio emission, which would not be effected by rotation, it is clear that there was general agreement between structural areas as shown in Figure 17. These factors suggested the existence of spiral arms in the Galaxy, and Christiansen and Hindman concluded that a much more detailed investigation was warranted.

Overall there were clear indications that the hydrogen-line emission occupied roughly the same distribution on the sky as the visible Milky Way. This association and the ability to penetrate the obscuring medium to discover Galactic structure heralded the beginning of a very important branch of investigations in radio astronomy. It also marked the beginning of a major international collaboration, particularly with the Dutch group working at Leiden, and was characterised by close cooperation that started with the prepublication communications by Ewen and Purcell to both the Dutch and Australian groups.

It is coincidental that in the same period that the breakthrough discovery of a radio frequency emission line occurred, the first optical evidence for spiral arm structures in our Galaxy was also published (Morgan et al., 1952).

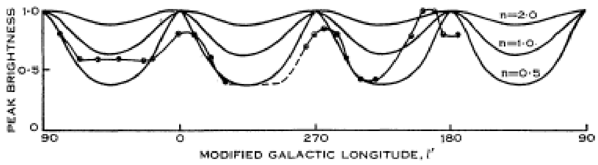

Figure 16: Calculated brightness peaks due to galactic rotation for given hydrogen densities (n). Dots indicate actual observations (after Christiansen and Hindman, 1952a: 450).

Immediately following the Australian confirmation of the H-line, Wild decided to update and publish the internal report he had written prior to the detection of the H-line (Wild, 1952). This was a comprehensive review of the radio-frequency line spectrum of atomic hydrogen and is largely in accordance with modern theory. The report provided a very solid theoretical base for planning of further observations by the Australians. The one exception in this analysis was the conclusion that the 1,420 MHz emission would be the only detectable line emission and that it would be unlikely the higher order recombination lines would be detectable. It would be nearly two decades before the recombination lines were finally detected in the Soviet Union (Sullivan, 1982: 300).

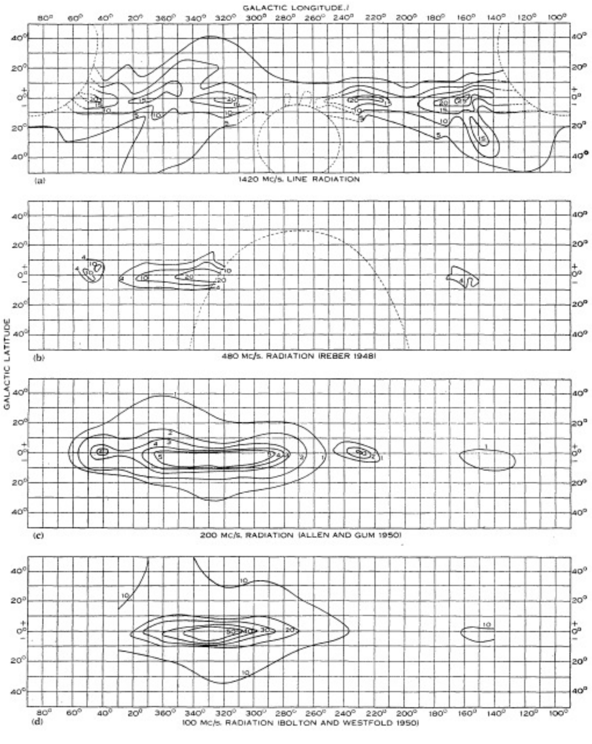

Figure 17: Comparison of H-line emission (top) and 480 MHz, 200 MHz and 100 MHz (bottom). Structural similarities are evident (after Christiansen and Hindman, 1952a: 451).

7 THE 1952 URSI CONGRESS

In recognition of the growing contribution of Australian researchers to the new field of radio astronomy, the Tenth General Assembly of the International Union of Radio Science (U.R.S.I) was held in Sydney from the 8 to 22 August 1952 (Haynes et.al., 1996: 222). Among those attending the Congress were Ewen from Harvard and Muller from Leiden. This meant that all those that had been involved in the initial detection of the H-line were able to meet for the first time (see Figure 18).

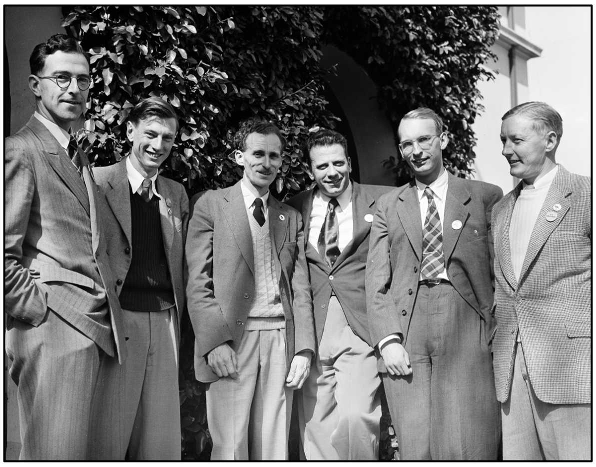

Figure 18: Gathering at the 1952 U.R.S.I. meeting in Sydney of those involved in the initial detection and confirmation of the H-line. From left to right: Kerr, Wild, Hindman, Ewen, Muller and Christiansen. Note also the special U.R.S.I. 'Kangaroo' lapel buttons being worn (courtesy: ATNF Historic Photographic Archive).

At the Congress those in Figure 18 decided to arrange a regular exchange of information by way of a newsletter that tracked the progress of the various groups undertaking H-line research. The first issue appeared in December 1952 and was circulated to those listed below in Table 1.

Table 1: Initial distribution list of the H-line newsletter.

H.I. Ewen	Ewen Knight Corporation, Massachusetts, U.S.A
B.J. Bok	Harvard Observatory, U.S.A
C.R. Burrows	Cornell University, U.S.A.
H. Tatel	Carnegie Institution, U.S.A.
J. Hagen	Naval Research Laboratory, Washington, U.S.A.
F.J. Kerr	Radiophysics Laboratory, Sydney, Australia
J.L. Pawsey	Radiophysics Laboratory, Sydney, Australia
O. Storey	T.R.E., Malvern, U.K.
A.C.B. Lovell	Jodrell Bank, U.K.
M. Ryle	Cambridge University, U.K.

8 CONCLUDING REMARKS

The achievement of Christiansen and Hindman in constructing a spectral-line receiver in such a short period and then using it to produce the first evidence of spiral arm structures in our Galaxy based on neutral hydrogen measurement was quite remarkable.

In retrospect, the H-line confirmation was however also a missed opportunity for Radiophysics (Sullivan, 2005: 14). Had a serious effort been made to detect the 21-cm emission line when the possibility was first raised it appears very likely that the Group would have been successful. The Group's early success in both solar and cosmic research and the wealth of discoveries made in the late 1940s and early 1950s meant that they were reluctant to pursue the more speculative search for the emission line even though they were aware of the significance that such a discovery would bring to radio astronomy.

The announcement of the discovery of the H-line also marked the first major international collaboration in radio astronomy.

After completing the initial H-line survey, Christiansen returned to his solar research program, thus ending the initial phase of Australia's H-line investigations. By this stage Kerr had returned from Harvard and he and Hindman focused on the construction a new and more reliable receiver and on a new 36-ft transit parabola for use in a dedicated H-line survey of the southern sky. They were also joined by a new graduate student, Brian Robinson, who would go on to build a distinguished international career in radio astronomy (see Whiteoak and Sim, 2006).

7 NOTES

1. Georges Height field station was located on a headland opposite the entrance to Sydney Harbour (see Orchiston, 2004). The equipment referred to was the 16-ft × 18-ft paraboloid which was subsequently relocated to Potts Hill for observations of the 1 November 1948 partial solar eclipse. At the time the aerial was fitted for simultaneous recording at 200, 600 and 1,200 MHz (see Orchiston, Slee and Burman, 2006; Wendt, Orchiston and Slee, 2008).

8 ACKNOWLEDGEMENTS

We are grateful to Kathryn Brennan from the National Archives of Australia in Sydney for help in accessing archival material and supplying Figure 3, and the Australia Telescope National Facility for providing Figures 1, 2, 4 and 18. We also wish to thank Ron Stewart (James Cook University), Richard Strom (ASTRON, The Netherlands) and Richard Wielebinski (Max-Plank-Institut für Radioastronomie) for reading and commenting on the manuscript.

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Harry Wendt, B.Sc. (Hon), M.Sc. (Astronomy), has just completed a Ph.D. at James Cook University, Townsville, Australia. His thesis topic was “The Contribution of the Division of Radiophysics Potts Hill and Murraybank Field Station to International Radio Astronomy”.

Dr Wayne Orchiston is a Senior Lecturer in Astronomy at James Cook University, Townsville, Australia. His main research interests relate to Cook’s voyage, Australian and New Zealand astronomical history, with emphasis on the history of radio astronomy, comets, historically-significant telescopes, early astronomical groups and societies, and transits of Venus. He has published extensively, and has edited the book *The New Astronomy. Opening the Electromagnetic Window and Expanding our View of Planet Earth* (2005). He also has a book on early Australian radio astronomy, co-authored by Woody Sullivan and Jessica Chapman, which will be published by Springer in 2009. He is the Chair of the IAU Working Group on Historic Radio Astronomy.

Dr Bruce Slee is an Honorary Associate at the Australia Telescope National Facility in Sydney, and an Adjunct Professor in the Centre for Astronomy at James Cook University. He was one of the original discoverers of the first discrete radio sources and participated in one of the first metre-wave sky surveys for radio sources, followed by measurements of their angular sizes with medium base-line interferometers. His other interests include scattering in the interplanetary medium, radio emission from active stars, absorption in the interstellar medium, pulsar research, surveys of clusters of galaxies, radio relics in clusters and, in more recent years, the history of Australian radio astronomy. Bruce is a member of the IAU Working Group on Historic Radio Astronomy, and he continues to research topics which have engaged his attention for more than 60 years.

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REMINISCENSES REGARDING PROFESSOR W.N. CHRISTIANSEN

Govind Swarup
National Centre for Radio Astrophysics, TIFR, Pune 411007, India.
E-mail: gswarup29@gmail.com

Abstract: In this short paper I describe my initiation into the field of radio astronomy fifty years ago, under the guidance of Professor W.N. ('Chris') Christiansen, soon after I joined the C.S.I.R.O.'s Division of Radiophysics (RP) in Sydney, Australia, in 1953 under a 2-year Colombo Plan Fellowship. During the early 1950s Christiansen had developed a remarkable 21cm interferometric grating array of 32 east-west aligned parabolic dishes and another array of 16 dishes in a north-south direction at Potts Hill. Christiansen and Warburton used these two arrays to scan the Sun strip-wise yielding radio brightness distribution at various position angles. During a three month period I assisted them in making a 2-dimensional map of the Sun by a complex Fourier transform process. In the second year of my Fellowship, Parthasarathy and I converted the 32-antenna east-west grating array to study solar radio emission at 60cm. During this work, I noticed that the procedure adopted by Christiansen for phase adjustment of the grating array was time consuming. Based on this experience, I later developed an innovative technique at Stanford in 1959 for phase adjustment of long transmission lines and paths in space. In a bid to improve on the method used by Christiansen to make a 2-dimensional map of the Sun from strip scans, I suggested to R.N. Bracewell in 1962 a revolutionary method for direct 2-dimensional imaging without Fourier transforms. Bracewell and Riddle developed the method for making a 2-dimensional map of the Moon using strip scans obtained with the 32 element interferometer at Stanford. The method has since revolutionized medical tomography. I describe these developments here to highlight my initial work with Christiansen and to show how new ideas often are developed by necessity and have their origin in prior experience! The 32 Potts Hill solar grating array dishes were eventually donated by the C.S.I.R.O. to India and were set up by me at Kalyan near Mumbai, forming the core of the first radio astronomy group in India. This group went on to construct two of the world's largest radio telescopes, the Ooty Radio Telescope and the Giant Metrewave Radio Telescope. Chris Christiansen was not only my *guru* but also a mentor and a friend for more than fifty years. I fondly remember his very warm personality.

Keywords: W.N. Christiansen, history of radio astronomy, history of science in India, solar radio emission, the Ooty Radio Telescope, the Giant Metrewave Radio Telescope

1 INTRODUCTION

Radio emission from the Sun was discovered during the Second World War in 1942 independently by Hey (1946) and Southworth (1945), in 1943 by Reber (1944; 1946), and in 1945 by Alexander (Orchiston, 2005). Appleton (1945) also described reports of radio noise received by ham radio operators at wavelengths between 7.5 m and 30 m during the previous sunspot maximum. In mid-1945, three separate reports describing the discovery of the solar radio emission by Hey, Reber and Alexander were received at nearly the same time by the C.S.I.R.O.'s Division of Radiophysics (RP) in Sydney (Ruby Payne-Scott, 1945), and these inspired Pawsey and collaborators to initiate systematic research activities in the new field of radio astronomy, starting in October 1945 (Pawsey et al., 1946). They discovered solar radio emission arising from the corona, a slowly varying component related to sunspots, and strong radio bursts associated with flare activity (see Christiansen, 1984b; Orchiston, Slee and Burman, 2006; Pawsey, 1950).

Two major instruments were built in Australia for detailed investigation of the solar radio emission. In 1948 Paul Wild erected a solar spectrograph at RP's Penrith field station[1] in order to study solar radio bursts (see Stewart et al., 2009), and in 1952 W.N. (Chris) Christiansen (1953) set up an innovative multi-element 21cm grating interferometer at the Potts Hill field station[1] in order to investigate radio brightness distribution across the Sun. This interferometer consisted of 32 parabolic dishes each 6ft in diameter aligned along a 700ft east-west baseline. One year later a 350ft north-south array comprising 16 dishes was installed at Potts Hill (see Wendt et al., 2008).

This pioneering development in radio interferometry by Chris led subsequently to the construction of many major solar radio telescopes around the world, including the 21cm Chris Cross antenna at Fleurs, near Sydney (Christiansen et al., 1957; Orchiston 2004), the 9.1cm Stanford Cross antenna in the U.S.A. (Bracewell, 2005; Bracewell and Swarup, 1961), radio interferometers at 7.5cm and 3.2cm in Japan (Tanaka, 1984), the 50cm Kalyan Radio Telescope in India (Swarup, 2006), the 120cm Miyun Radio Telescope in China, the 10.7cm solar radio interferometer in Canada (Covington, 1984), 3.2cm and 1.7m solar arrays in France (Denisse, 1984), and a 107cm solar grating array near Lake Baikal in Russia (Salomonovich, 1984). Based on their work in Australia and at Cambridge, Chris and Jan Hogbom were the main proponents for the construction of the Westerbork Synthesis Radio Telescope (WSRT). Success of the WSRT fostered the development of ever more powerful synthesis radio telescopes: the VLA in the USA, the AT in Australia, the GMRT in India and now the LOFAR in the Netherlands and the SKA in Australia or South Africa! Chris must have been very proud to see these developments.

I learned the powerful technique of radio interferometery from Chris in 1953 and have not looked back (Swarup 2006). To recapitulate, I first describe my early years. After receiving an M.Sc. in Physics from Allahabad University in north India in 1950 I joined the National Physics Laboratory (NPL) in New Delhi to work on paramagnetic resonance under the guidance of its Director, Sir K.S. Krishnan. In August 1952 Krishnan attended the Congress of the International Union of Radio Science (URSI) held in Australia. He was very impressed by outstanding discoveries being made in the new field of radio astronomy by scientists of RP, under the leadership of Joe Pawsey. Krishnan decided to initiate radio astronomical research at the NPL. With a recommendation from Krishnan, I obtained a two-year Fellowship under the Colombo Plan Scheme, and joined RP in March 1953. Pawsey suggested that I work with Christiansen for the first three months and then with Bernie Mills, Paul Wild and John Bolton for three months each. During the second year of the Fellowship, I was asked to work independently on a major project along with my Indian colleague, R. Parthasarathy, who had also joined RP in early 1953. We decided to convert the above-mentioned east-west grating array to operate at 60cm (instead of at 21cm) in order to study solar radio emission at this longer wavelength.

During my stay in Australia, I had close contact with Chris not only academically but also culturally and socially. He was not only my *guru* but became a close mentor and remained a friend for the next fifty years.

2 THE SOLAR GRATING INTERFEROMETER AT POTTS HILL

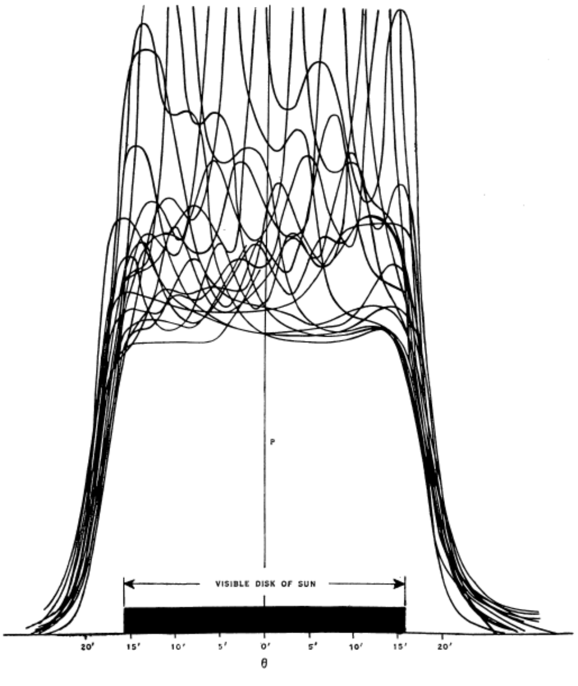
As mentioned above, in 1952 and 1953 Chris set up innovative multi-element east-west and north-south grating interferometers along the banks of a Sydney water supply reservoir at Potts Hill (Christiansen 1953; Christiansen and Warburton 1953a). A close-up of the east-west array is shown in Figure 1, while Figure 2 provides an aerial view of both arrays. Chris' objective was to study the daily radio brightness distribution across the Sun, and by March 1953 he and Joe Warburton had obtained a large number of daily strip scans across the Sun at a wavelength of 21cm (1,420 MHz), where the resolution was 4 minutes of arc. By superimposing the daily records, they determined the contribution of the quiet Sun from the strip scans at various position angles with respect to the polar axis of the Sun (see Figure 3).

As suggested by Pawsey, I joined Chris to work under his guidance for three months. Chris asked me to assist in preparing a two-dimensional radio brightness distribution map based on observations that he and Joe Warburton obtained. Using an electrical calculator, I first determined the Fourier Transform (FT) of each of the strip scans obtained at various position angles, plotted the values on a large piece of graph paper, made contour plots manually, determined manually strip scans of the two-dimensional plot at various position angles, calculated the FT of each of these and finally determined the two-dimensional distribution of 21 cm radio emission across the solar disk. Ron Bracewell described short cuts to me for faster calculation of the FTs. Nevertheless, it was a very laborious process, but thanks to Chris' gentle guidance it ultimately led to success! The map that was published by Christiansen and Warburton (1955) is shown in Figure 4. Years later it occurred to me that a much simpler procedure can be used to determine a two-dimensional distribution directly from strip scans without doing any FTs, as described in Section 4 below.

The above-mentioned two-dimensional map showed limb brightening at 21cm, as predicted earlier by Steve Smerd (1950), who assumed a higher electron density in the solar corona near the equatorial regions. However, measurements made by Stanier (1950) at Cambridge conflicted with Smerd's prediction and showed no evidence of limb brightening at 60cm.

Figure 1: View looking east showing the east-west grating interferometer on the southern bank of one of the two Sydney water supply reservoirs at Potts Hill (courtesy: ATNF Historic Photographic Archive, B2638-2).

Figure 2: Aerial view looking southwest across the two water supply reservoirs at Potts Hill. The 32 antennas comprising the east-west solar grating array can be seen on the southern edge of the foreground reservoir, and the 16-element north-south grating array is clearly visible along the eastern edge of this same reservoir (courtesy: ATNF Historic Photographic Archive, 3475-1).



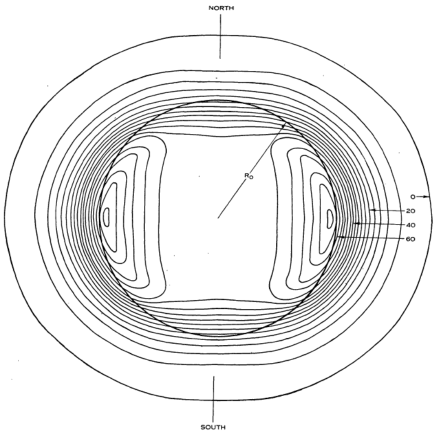


Figure 3 (left): Twenty individual daily one-dimensional brightness distribution scans superimposed. The visual solar disk is indicated by the black bar on the x-axis (after Christiansen and Warburton, 1953b: 200).

Figure 4 (above): The two-dimensional distribution of radio brightness across the Sun at 1,420 MHz. The central brightness temperature is 4.7 × 10^4 K and the maximum peak temperature is 6.8 × 10^4 K (after Christiansen and Warburton, 1955a: 482).

In 1954, Christiansen went on a one-year sabbatical to work at the Meudon Observatory in France, and the Potts Hill grating array observations were discontinued. Parthasarathy and I were interested in Stanier's earlier finding, and so we suggested that the east-west grating array should be converted from 21cm to 60cm (500 MHz), so that we could investigate this anomaly. Both Chris and Joe Pawsey enthusiastically supported our proposal. Chris explained the intricacies involved in matching the transmission lines of the 21cm grating array, particularly to ensure that the lengths of the lines from the central point of the array to each of the 32 dipoles was within a few mm. This involved a cumbersome procedure whereby a 21cm signal was transmitted from the junction of each adjacent pair of dishes, the signals were received at the dipole feeds of the adjacent dishes using a movable probe, their phase was then measured using a slotted line, and finally appropriate corrections were made to ensure equality of the lengths of the transmission lines to within a few mm. In 1954, while we were involved in this exercise I asked Pawsey whether I could short the outputs at each dipole successively and measure the positions of the short at the central point of the transmission line network of the entire array. Pawsey replied that my suggestion would not work as any mismatch in the long transmission lines would add to the resulting phase from the dipoles. Six years later I took care of Pawsey's objections by conceiving a round trip phase measurement scheme and modulating the signals at the outputs of the Stanford array parabolic dishes (see Swarup and Yang 1960, and Section 4 below). At the time, Pawsey (1960) wrote me:

> I had already heard of your phase measurement technique and think that you have made a real breakthrough in this technique. Congratulations! Chris regards the idea as the key to really large Mills Crosses. Without a good checking technique, they could not operate.

Once the Potts Hill east-west array was operational at 60cm we were able to dispute Stanier's finding (see Swarup and Parthasarathy, 1955) and show the presence of limb brightening at this wavelength (see Figure 5). We also studied localized radio bright regions associated with the slowly varying component and determined their emission polar diagrams by measuring the intensity with the rotation of the Sun (Swarup and Parthasarathy, 1958). For us this was a great experience as we were initiated into the wonderful world of radio astronomy: constructing dipoles, matching transmission lines, building a 500 MHz receiver, making observations, reducing data and finally, deriving meaningful astronomical conclusions.

3 TRANSFER OF THE THIRTY-TWO DISHES TO INDIA

Upon his return from France in early 1955 Christiansen decided to build a new cross-type array at RP's Fleurs field station near Sydney. Affectionately known as the 'Chris Cross', this consisted of two orthogonal grating interferometers, which were used to make daily solar maps at 21cm (see Christiansen and Mathewson, 1958; Christiansen et al., 1957; Orchiston, 2004). As a result, both of the grating arrays at Potts Hill, and associated equipment, became surplus and were to be scrapped. I therefore asked Pawsey and Chris whether the 32-element east-west grating array could be gifted to India. They readily agreed to this suggestion, as did E.G. (Taffy) Bowen, Chief of the Division of Radiophysics. On 23 January 1955, I wrote to K.S. Krishnan about the possible transfer of the 32 dishes to the NPL in New Delhi (Swarup, 1955). On 22 February he replied: "I agree with you that we should be able to do some radio astronomy work even with the meager resources available." (Krishnan, 1955). C.S.I.R.O. then approved the donation under the Colombo Plan scheme, but with the proviso that India must bear the cost of their transportation (which amounted to about 700 Australian Pounds, as I recall). I returned to join the NPL in July 1955, but the transfer of the dishes was delayed by bureaucratic correspondence. So in August 1956 I decided to join the Harvard College Observatory as a Research Associate in order to study dynamic spectra of solar bursts using the 100-600 MHz swept-frequency radio spectrograph that had just been installed at Fort Davis, Texas. One year later, I moved to Stanford University as a Research Assistant, and began research for a Ph.D. degree (Swarup, 2006).

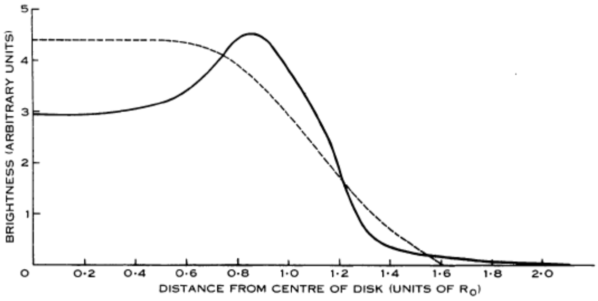


Figure 5: Brightness distributions at 500 MHz showing Stanier's result (dashed line) and Swarup and Parthasarathy's observations (after Swarup and Parthasarthy, 1955b: 493).

4 THE STANFORD MICROWAVE SPECTRO-HELIOGRAPH ANTENNA

In late 1955 Ron Bracewell resigned from RP and joined Stanford University in order to teach in the Department of Electrical Engineering and build a solar radio telescope. The outcome of the latter venture was the Stanford Microwave Spectro-heliograph Antenna, which operated at 9.1cm and consisted of east-west and north-south grating arrays arranged in the form of a cross (see Figure 6). Each array consisted of 16 parabolic dishes 10 feet diameter, spaced at 25 feet intervals (Bracewell and Swarup, 1960). The voltage outputs of the two arrays were multiplied giving a pencil beam of 3.1 arc minutes. In September 1957, soon after joining Stanford, I made a detailed study of a Cross antenna versus a T-shaped antenna and showed that both provided the same resolution but that the latter, although more economical, was much more sensitive to phase errors, which resulted in spurious sidelobes. Bracewell (2004) subsequently wrote to me: "I had a letter from Christiansen sometime later that he believed that the T-idea came from Stanford."

In September 1957 Chris visited Stanford and I got valuable tips from him concerning the Stanford Cross Antenna project. Bracewell asked me and K.S. Yang (another graduate student) to design and adjust the waveguide transmission line system. We equalized the lengths of the transmission lines and the resulting phases of the signal outputs of the 32 antennas using the technique developed by Christiansen for the Potts Hill array (Section 2). After more than six months of hard work we were able to make maps of the Sun but we found huge spurious sidelobes. Bracewell asked us to make fresh phase measurements. Again we found large sidelobes and we concluded that the spacing and physical location of the antennas could be in error. Bracewell decided to survey their positions himself and to make corrections as required but asked us to make the phase measurements again. How strenuous and boring, getting up early in the morning in order to make phase measurements before the length of the probes was affected by temperature changes caused by sunlight, not to mention having to attend classes at 9a.m.! Hence, I conceived the idea of transmitting a signal at 9.1 cm from a central point of the transmission line network to all the antennas, modulating and reflecting the voltage signal from the output of each antenna and measuring the round trip phase of the modulated signal, thus avoiding Pawsey's objections (Swarup and Yang 1960). The idea was conceived while I was a graduate student, when time was of the essence, so prior experience (at Potts Hill) and necessity became the mother of invention! The concept of round trip phase measurements has been widely used for phase adjustments to all of the synthesis radio telescopes built in the world over the last fifty years, klystrons of the Stanford linear accelerator, clocks and local oscillators in Space, and many other applications.

Figure 6: Panoramic view of the 9.1 cm Stanford Microwave Spectro-heliograph Antenna (courtesy: Stanford University Photographic Department, Negative No. 9448).

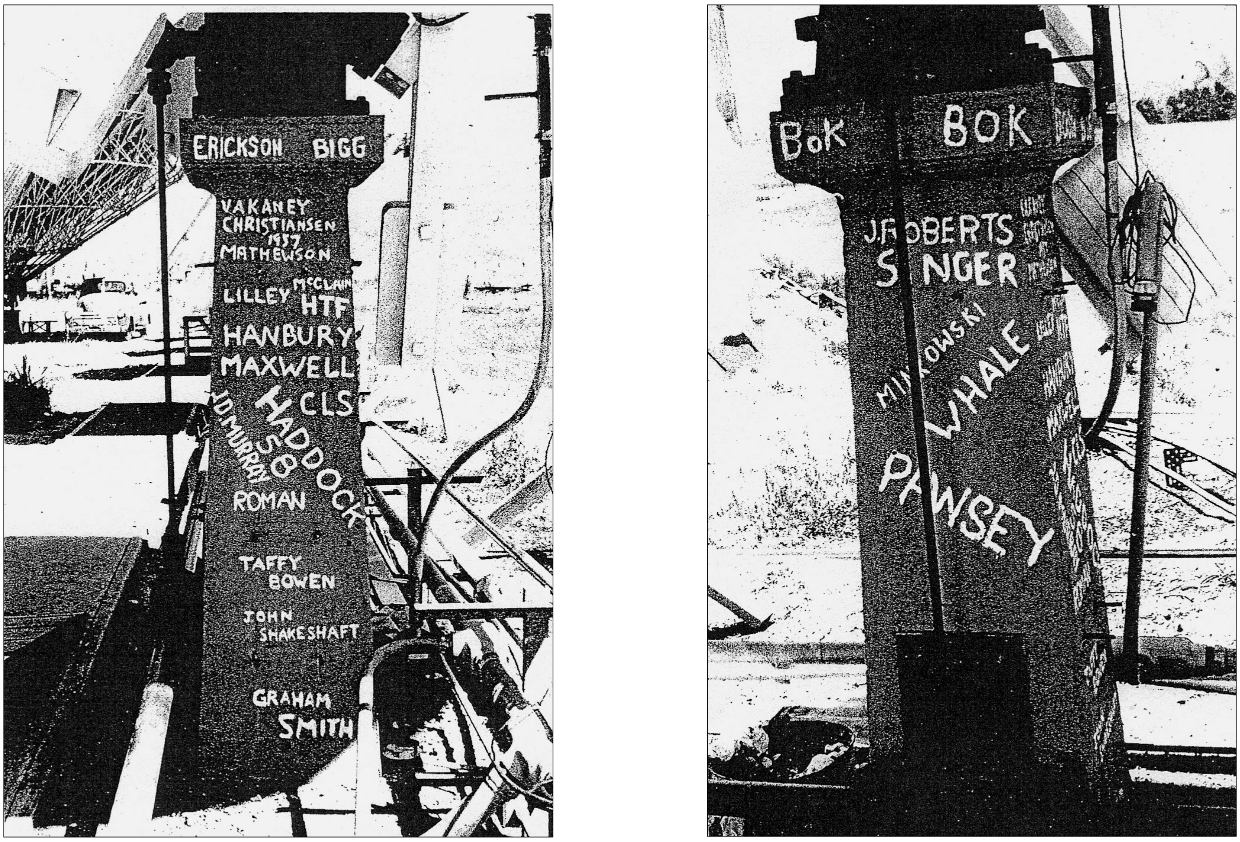


Figure 7: Two of the Stanford pillars with carved names of notable optical and radio astronomers.

By 1962 we had made large numbers of daily two-dimensional maps of the Sun at 9.1cm using the Stanford Cross. I found that the radio emission from the quiet Sun seemed to show a north-south asymmetry, and I wondered whether this arose from phase errors in the transmission line network of the Cross, including the rotating phase shifters in the north-south lines. Since the total power outputs of the two grating arrays of the Cross antenna were expected to be much less sensitive to phase errors, I wondered whether a much simpler procedure could be used to determine a two-dimensional distribution directly from the strip scans rather than the method recommended to me by Chris (and outlined previously in Section 2). I quote from one of the two letters that Bracewell later wrote to me about this: “I do remember being in someone’s car while our group was driving to have lunch when you proposed superimposing the scans in real space without any Fourier Transform at all.” (Bracewell, 2004; cf. 1992).

Since I was to return to India from the U.S.A. after nearly seven years, I did not pursue the idea, but the method subsequently was used by Bracewell and Riddle (1967) to make two-dimensional maps of the Moon using strip scans obtained with the Stanford arrays. It is interesting to note that this technique is widely used today in X-ray imaging and has revolutionized medical tomography (see Bracewell, 2005). I describe this here to highlight the initial work done by me under guidance from Chris. New ideas often have their origin in prior experience!

Before leaving the Stanford story there is one further recollection that I wish to share. There were a number of concrete pillars at the Stanford radio astronomy site that supported the different antennas, and Ron Bracewell would encourage visiting radio astronomers from around the world to carve their names on these. Many of my former RP colleagues’ names are to be found on these pillars, including Chris, Taffy Bowen, Don Mathewson, Joe Pawsey, Jim Roberts and John Murray (e.g. see Figure 7). The survival of these historic pillars was at stake when Stanford University decided to abandon the radio astronomy site in 2006 and remove all evidence of its former scientific past, and it is heartening to know that these pillars have all survived and will not be destroyed (pers. comm., Miller Goss, 2008).

5 FORMATION OF THE RADIO ASTRONOMY GROUP AT THE TATA INSTITUTE OF FUNDAMENTAL RESEARCH

Chris, Joe Pawsey and Frank Kerr were very supportive when it came to forming a radio astronomy group in India (Swarup, 2006). By the 1960s, T.K. Menon, M.R. Kundu and I all had more than eight years experience working at leading radio astronomy observatories and institutions abroad, and at the time T. Krishnan was working with Chris at RP in Sydney. On 22 September 1960, Chris wrote to me about Krishnan and said “…you two and Menon and Kundu should get together for a united attack on the monolith of Indian bureaucracy…” About a month later, on 26 October 1960, Pawsey wrote: “… you four could make an effective group … but keep off fashionable ideas …” Later, on 29 June 1961, he wrote: “… don’t, for example, buy a 60-ft. dish because someone gives it to you cheap … America is stiff with 60-ft. and 80-ft. dishes … by organizations who had no special ideas of what to do with one.” (Pawsey, 1961). Pawsey arranged for Krishnan to attend the Berkeley IAU General Assembly in August 1961, and during the meeting Krishnan, Kundu, Menon and I wrote a proposal to start a radio astronomy group in India and we submitted this to five major scientific organizations and agencies in India, including Dr Homi Bhabha, founding Director of the Tata Institute of Fundamental Research (TIFR) in Mumbai (Swarup, 2006). Our proposal was approved by Dr Bhabha, and I returned to India on 31 March 1963 in order to join the TIFR.

At that time there was a raging controversy between the Steady State and Big Bang Cosmologies. In June 1963 I suggested measuring the angular sizes of hundreds of extragalactic radio sources to arc second accuracy by lunar occultation observations in order to test the predictions of the two theories. For this purpose, I proposed the construction of a large parabolic cylindrical antenna placed on a suitably inclined hill in South India, so as to make its axis of rotation parallel to that of the Earth (Swarup, 1963). This concept was enthusiastically supported by Kundu and Menon, and Dr Bhabha approved the proposal but asked us to first form a core group. In August 1963, V.K. Kapahi and J.D. Isloor joined the group as Research Associates after their graduation.

As a first step, a solar radio interferometer was constructed by the group at Kalyan (near Mumbai) during 1963-1965, using the 32 dishes from Chris’ Potts Hill grating array. The resulting Kalyan Radio Telescope (Figure 8) was used to determine the two-dimensional distribution of radio emission from the quiet Sun at 49cm (Swarup et al., 1966). Considerable limb brightening was found at this wavelength.

Figure 8: View of the Kalyan Radio Telescope.

In late 1963 I discussed the occultation project with Chris during a brief visit that he made to the TIFR while he was on his way to the Netherlands. He described the 21cm Westerbork Synthesis Radio Telescope (WSRT), which was under development at the time in the Netherlands. An even less ambitious synthesis radio telescope operating in India at a longer wavelength would have required access to considerably more expertise and technology than was then available in India. Many components would have to be imported, but there was a serious foreign exchange constraint in India at that time. Hence we continued to pursue the cylindrical radio telescope project for lunar occultation and other investigations. N.V.G. Sarma and M.N. Joshi from the NPL, who had respectively worked at Leiden for two years and obtained a Ph.D. in radio astronomy in France, joined the TIFR group in late 1964. Then M.R. Kundu returned from the U.S.A.

in early 1965 and provided considerable support for the project. In January 1965, Ramesh Sinha and I located a suitably-inclined hill at Ooty in southern India, and construction of the 325 MHz Ooty Radio Telescope (ORT) was completed by February 1970 (Figure 9). This comprised a 530m long parabolic cylinder which was 30m wide (Swarup et al., 1971). The angular sizes of ~1,000 discrete radio sources were then measured with a resolution of between 1 to 10 seconds of arc for the first time, and these supported the Big Bang theory. T.K. Menon joined the group in 1970, and although he and Kundu eventually returned to the U.S.A., both played a very important role in the growth of the TIFR radio astronomy group, and particularly in the training of students. Since 1970 the Ooty Radio Telescope has been used for a wide variety of investigations (see Swarup et al., 1991), and it is currently making interplanetary scintillation observations of more than 900 sources every day (Manoharan: www.ncra.tifr.res.in).

During 1975-1984, a 4 km long synthesis radio telescope was set up at Ooty by combining the ORT with six much smaller parabolic cylinders measuring 23m × 7.5m (Swarup, 1984).

In early 1984 a proposal was prepared for the Giant Metrewave Radio Telescope (GMRT) (Swarup, 1984), and this was sent to several respected overseas radio astronomers. On 30 July 1984, Chris wrote to me: "… I think that you are doing the right thing in continuing your work at the lower end of the radio frequency spectrum. This part of the spectrum has been relatively neglected. India is a good place to do such work because of its relative radio "quietness" and you have developed good techniques for such work ..." (Christiansen, 1984a). Dave Heeschen, Director of the National Radio Astronomy Observatory in the U.S.A. wrote in September 1984: "... The GMRT ... would almost certainly be a uniquely powerful telescope for many years to come ... The GMRT would be a major step forward in Radio Astronomy that would benefit the science and radio astronomers everywhere ..." A site was located about 80km north of Pune in western India by 1986. The GMRT was approved by the Government of India in early 1987, after the Prime Minister, Rajiv Gandhi—who was an active radio ham—was satisfied after asking three penetrating questions. A detailed design was finalized by 1990 (Swarup et al. 1991). The GMRT consists of 30 parabolic dishes each 45m diameter and of innovative design (Figure 10). The GMRT became operational in 1999 and has been used by hundreds of astronomers from India and more than twenty different countries.

6 CONCLUSION

Although I worked with Chris only for a part of my two years at Radiophysics during 1953-1955, it was a very fruitful and valuable interaction. Later I had valuable discussions with him and received very helpful advice from him during his visits to Stanford and India. I also met him and his wife, Elsie, at their home in Sydney in 1953 and at several international meetings, particularly those of URSI. As the President of URSI, Chris strongly supported URSI's programmes for the growth of radio science in developing countries. Pioneering contributions by Chris to many aspects of radio interferometers led to the construction of several major radio telescopes throughout the world, and the book by Christiansen and Högbom titled *Radio Telescopes* (1969) has been widely used by students, antenna designers and astronomers.

Figure 9: The Ooty Radio Telescope, a 530m × 30m cylindrical parabolic antenna in Southern India.

Figure 10: Panoramic view of some of the Giant Metrewave Radio Telescope antennas.

7 NOTES

1. Potts Hill and Penrith were two among a network of 20 field stations and remote sites maintained by RP in or near Sydney during the late 1940s through into the 1960s. For a review of these field stations and remote sites see Orchiston and Slee (2005). The history of Potts Hill—the sole RP field station at which I was based—is also discussed by Davies (2005) and by Wendt (2008).

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Govind Swarup, F.R.S., was formerly the Director of the National Centre for Radio Astrophysics in Pune and Professor of Eminence at the Tata Institute of Fundamental Research in Mumbai (India). He has published more than 115 research papers concerning radio emission from the Sun, pulsars, radio galaxies, quasars, cosmology and radio astronomy instrumentation. He has edited four books: *Quasars* (co-edited by V.K. Kapahi*)*, *History of Oriental Astronomy* (IAU Colloquium 91; co-edited by A.K. Bag and K.S. Shukla), *Proceedings of the 6th Asian-Pacific Regional Meeting on Astronomy of the IAU held at Pune, India* (co-edited by V.K Kapahi, N.K Dadhich and J.V. Narlikar), and *The Universe at Low Radio Frequencies* (co-edited by A.P. Rao and Gopal-Krishna). He was President of the IAU Commission 40 (Radio Astronomy) during 1979-1982.

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THE EARLIEST TELESCOPE PRESERVED IN JAPAN

Tsuko Nakamura
Information Sciences, Teikyo-Heisei University, Higashi-Ikebukuro 2-51-4,
Toshima, Tokyo 170-8445, Japan
E-mail: tsukonk@yahoo.co.jp

Abstract: This paper describes the antique telescope owned by one of Japan's major feudal warlords, Tokugawa Yoshinao. As he died in 1650, this means that this telescope was produced in or before that year. Our recent investigation of the telescope revealed that it is of Schyrlean type, consisting of four convex lenses, so that it gives erect images with a measured magnifying power of 3.9 (± 0.2-0.3). This also implies that Yoshinao's telescope could be one of the earliest Schyrlean telescopes ever. The design, fabrication technique, and the surface decoration of the telescopic tube and caps all suggest that it is not a Western make at all, but was produced probably under the guidance of a Chinese Jesuit missionary or by the Chinese, in Suzhou or Hangzhou in Zhejiang province, China, or in Nagasaki. Following descriptions in the Japanese and Chinese historical literature, we also discuss the possibility that production of Schyrlean-type telescopes started independently in the Far East nearly simultaneously with the publication of *Oculus Enoch et Eliae* by Anton Maria Schyrle in 1645.

Keywords: Antique Schyrlean telescope, Japanese telescope, Tokugawa Yoshinao, cultural transfer

1 INTRODUCTION

Although the invention of the telescope has often been attributed to the Dutch optician Hans Lipperhey in 1608, the fact seems more likely to be that the telescope was not invented by any single particular person (van Helden, 1977). To our knowledge, the first introduction of a telescope into Japan was in 1613. Both Japanese and English sources record without contradiction that the telescope was offered to the first Shogun Tokugawa Ieyasu (1542–1616) by a captain of the British East India Company for establishing a bilateral trading relation, and it had a gold-coated tube with a silver mounting.[1] Considering that it took a few years at that time for a sailing ship to reach Japan from Europe via the Cape of Good Hope, this is a good example of very quick cultural transfer (Sluiter, 1997). The whereabouts of this first telescope are now unknown. After that, a considerable number of European telescopes were brought to Japan by British, Portuguese, Spanish and Dutch traders as flattering gifts for high-ranking Shogunal officers and warlords, responding to their requests (e.g. Shirayama, 1990); unfortunately, none of these seems to have survived.[2]

In 1964, a small antique telescope was displayed for the first time at an exhibition of telescopes held in Tokyo to commemorate the 400th anniversary of the birth of Galileo Galilei (National Science Museum, 1964). It would seem that the telescope did not attract any interest from the organizer or the audience, because neither a photograph nor a mention of it was included in the exhibition catalogue. Only a one-line description appeared, which was included in a mimeographed list of the displayed items, which was distributed to only a limited number of people. This stated that the telescope was originally owned by Tokugawa Yoshinao, but was now in the possession of the Tokugawa Art Museum in Nagoya city.

Thanks to the generosity and kindness of the Museum, in 2003 and 2005 I was given the chance to subject Yoshinao's telescope to detailed examination, in collaboration with staff from the Museum. This paper reports the results of our investigation. Firstly, in Section 2 we investigate the origin and the history of the telescope, and how it came to end up in the Tokugawa Art Museum. Then in Section 3 we describe the apparent structural and optical characteristics of the telescope in terms of surface decoration, fabrication technique, magnifying power, and so on. Finally, in Section 4 we discuss the historical implications of this telescope when viewed against the background and the perspective of the history of the telescope in general.

2 THE AUTHENTICITY OF YOSHINAO'S TELESCOPE

The Tokugawa is the Shogun's family. The original owner of the telescope is believed to have been Tokugawa Yoshinao (1600–1650), who was the ninth Prince of Ieyasu. Yoshinao's officially-recorded death on 5 June 1650 (in the Gregorian Calendar) implies that his telescope was made in or prior to that year. However, in the case of such an old cultural asset, establishing the authenticity of the reputed ownership is very important, in order to validate the conclusions derived from these studies.

Yoshinao was given by his father, Ieyasu, a large and prosperous *han* (clan) in the Owari-Nagoya district (which includes the current city of Nagoya), and he became the first *Hanshu* (feudal Governor) there. Because of his scholastic interests, he also inherited many rare books and curiosities from his father. These treasures have been safely retained by his descendants for more than 380 years without loss or damage from fires or wars, and now comprise the backbone of the prestigious collection stored in the Tokugawa Art Museum.

Yoshinao's telescope is in a catalogue compiled about a century ago which lists the properties that Yoshinao's son, Mitsutomo, inherited from his father. This catalogue is an edited version of the original inventory. Because the original inventory books maintained at the time when Yoshinao died were later reorganized into different classified catalogues to accommodate the growing collection, we lack the original description of the telescope.[3] However, since various records of the family indicate that swords, armour and tea ceremony instruments which make up a major part of the Museum collection have been kept with the original inventory and Yoshinao's telescope has always been stored with them, there is no room for doubting the authenticity of this telescope. Hence, we conclude that Yoshinao's telescope was certainly made in or before 1650.

3 CHARACTERISTICS OF THE TELESCOPE

3.1 Outer Appearance

Figure 1 shows the fully extended tube, the inner tube, the eyepiece and two caps of Yoshinao's telescope, along with the wooden box in which it was stored. The surface of the box reads "Oyuzuri To-onmegane" in golden Chinese letters, meaning "The inherited telescope". In spite of the telescope's apparent antiquity, it gives us an impression of having been preserved in good condition, without any parts going missing.

The telescope consists of five-stage draw-tubes, including the ocular part. The contracted and extended lengths of the telescope are respectively 41cm and 119cm, while the outer tube diameter is 50mm. Figure 2a shows the objective lens, and we can infer that it is about 40mm diameter since the inner diameter of the first-stage draw-tube was 41mm (unfortunately, we were not allowed to separate the lens from the tube and measure it). Directly in front of the lens there is an aperture stop, which is made of a brown-color mottled tortoiseshell and is attached to the tube with a copper ring. The diameter of the aperture measures 24mm. Although in Europe Galileo was already familiar with the optical effects of aperture stops to suppress chromatic aberration (Dupré, 2003), we imagine that the use of a tortoiseshell aperture stop in Yoshinao's telescope was simply intended as special decoration that would appeal to high-ranking people rather than for optical purposes since tortoiseshell was very expensive imported material at that time. It is noted that some Japanese telescopes produced by Mori Nizaemon during the 1720s also adopted this tortoiseshell decoration, suggesting that they were possibly influenced by Yoshinao's telescope. Figure 2b shows the eyepiece section of the telescope. The effective diameter of the eyepiece lens was 11mm, and the white eye-ring is probably made of ivory (which was also an expensive material).

Figure 3a shows part of the decoration on the surface of the telescope tube. The tube itself is made of paper, which is painted with semi-transparent *urushi*-lacquer. This is very traditional in Chinese and Japanese handicrafts, although the *urushi* work on later Japanese telescopes looks like non-transparent Western lacquer and appears quite different from that on Yoshinao's telescope. In Figure 3a, along the circumference of the tube, we see contiguous silver patterns symbolizing perhaps shrimps or scorpions, which were probably made by pressing with a kind of stencil. Such a symbol is not so common, but is seen on some of later telescopes (such as Iwahashi's telescope).

Figure 3b shows the inside of the tube. One can see plenty of fine wooden annular rings running parallel to the axis of the telescope (and the small piece attached at the lower left could have been for adjusting a loose movement of the tube). It was not hard for a skilled Japanese carpenter to cut out such material thinner than a piece of paper with a sharp plane. The tube of the telescope is fabricated by multiple layering of paper sheets on the wooden surface with strong natural glue. This technique is the same as that adopted in later Japanese telescopes made before the 1860s and is known as *Ikkan-bari*, which is Chinese in origin. It was considered that the oily resin coming out from the wooden surface would have kept the draw-tubes of the telescope moving smoothly for an extended period of time. The *Ikkan-bari* technique is referred to again in Section 4.

Figure 1: Overall view of Tokugawa Yoshinao's telescope preserved at the Tokugawa Art Museum in Nagoya city, Japan.

Figure 2a (above): The objective lens with a tortoiseshell aperture stop.

Figure 2b (right): The ocular part whose ring is made of ivory.

Figure 3a (left): The surface decoration of the telescopic tube.

Figure 3b (above): The inside view of the tube made of thin wooden sheets.

Figure 4a (left): The outside view of the telescope cap.
Figure 4b (above): The inside of the telescope cap with colored water-marble decoration.

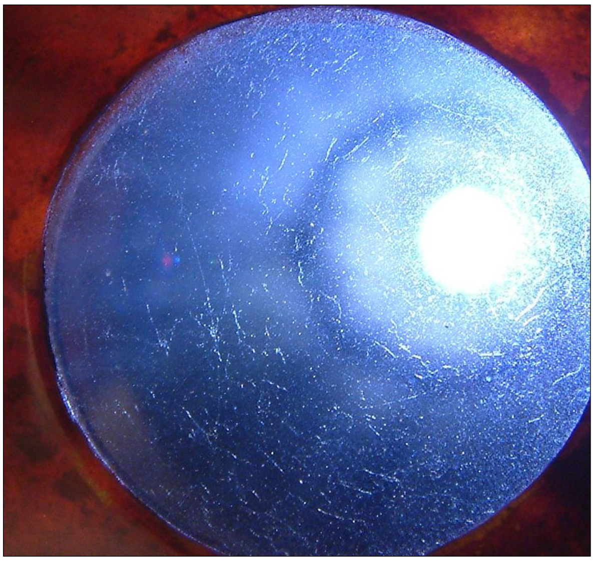

Figure 5a (above): Glass material of the objective lens seen through a back-illuminated light.

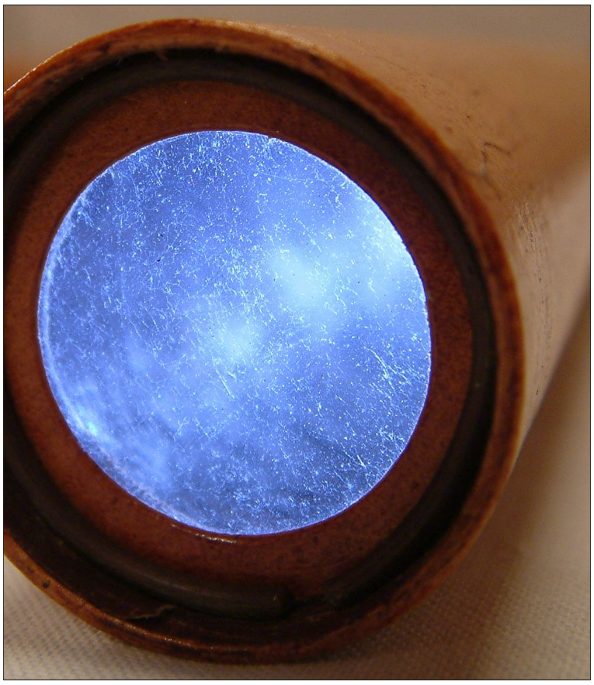

Figure 5b (right): Glass material of an ocular lens seen through a back-illuminated light.

Figure 4a and 4b are the outside and inside views of the telescope caps. As mentioned later, *Ikkan-bari* used to be basically a method to make a cap of a small vessel. The bottom of the inside of the cap (Figure 4b) is decorated with a red, blue and white-colored water-marble pattern. An historical description of colored Japanese water-marble first appeared in *Kiyu Shoran* (Kitamura 1830),[4] although monochromatic charcoal water-marbles in Japan date back to around the tenth century (Kawakami, 1987). Recognizing that Yoshinao's telescope is more than 350 years old, the three-colored water-marble of the telescope may be a sign of the use of a non-Japanese art tradition. Therefore, all the characteristics thus far explained about this telescope seem to indicate that it is surely neither a Western product nor of purely Japanese manufacture.

3.2 Measurements of the Magnifying Power

Since we could not obtain permission to attach a camera to the telescope in order to measure the magnification out in the open air, we had to measure the magnifying power in the narrow storage room at the Museum. Textbooks on optics (e.g. see Kingslake, 1983) teach us that the magnifying power of a telescope is equal to the ratio of the effective diameter of the objective lens (the entrance pupil) to the size of the Ramsden's circle (the exit pupil) observed at the eyepiece lens. With the telescope fully extended for distant viewing, a light plate for the inspection of slides was placed directly in front of the objective lens and the size of the exit pupil (namely, the image of the illuminated objective lens focused by the lenses) was measured using a magnifying glass of power 20 with a scale of 50-micron divisions. Using this technique, the magnifying power of Yoshinao's telescope was calculated to be 3.9, from the measured sizes of the entrance and exit pupils being 24.0mm and 6.2mm, respectively. We confirmed this magnification by viewing different items in the storage room through the telescope, which had a field of view of about 2.5° and showed erect images. We also checked the measuring error of this method by applying it to some modern telescopes and binoculars whose magnifying powers (from 5× to 15×) were precisely known, and found errors of ±0.2-0.3 for them. Hence we expect a similar error to apply to Yoshinao's telescope.

3.3 Lens Defects and Image Quality

In order to check production and erosion conditions of the lens material, we carefully examined the objective and some ocular lenses by back-illuminating them with white LED lights. This revealed plenty of fine cracks and bubbles. Some of these could be lens-surface scratches produced during the polishing process, while others were internal inclusions (see Figure 5a and 5b). We also noticed a ragged circumference (left) of the objective lens (Figure 5a), and a small fracture on the upper right edge (Figure 5b). Everything seemed to reinforce the impression that Yoshinao's telescope was really an old one.

In order to check the image quality seen through the telescope, we hung a calendar on the wall about 5m away in the storage room and photographed the numbers on it through the eyepiece (see Figure 6). Probably because of the low magnifying power of the eyepiece, we did not notice any particular image defects.

3.4 Optical Structure

In terms of the magnifying power mentioned in the previous Section, Yoshinao's telescope may look like no more than an opera-glass of Galilean type on sale at a toyshop. However, as shown schematically in Figure 7, this telescope was actually found to be of a more advanced form, namely a Schyrlean type consisting of four convex lenses (one objective and three eyepiece lenses).

It is well known that Anton Maria Schyrle (1597–1660) of Rheita, Bohemia, a friar of the Capuchins (a sect of the Franciscans), first invented practical telescopes with three- or four-lens configurations at Augsburg, assisted by two opticians, and publicized the fact in his book *Oculus Enoch et Eliae* in 1645 (Court and von Rohr, 1929). After Johann Zahn cited Schyrle's telescopic achievements in his book which was published in 1685 and included illustrations (see Figure 8), the fame of Schyrlean-type telescopes, with their erect and wide-field images, spread throughout Europe. Regarding this, Court and von Rohr (1929) present a story that Sir Charles Cavendish from Britain, hearing the rumor of the new telescope in 1644, went all the way to Augsburg to meet Schyrle and order one of his telescopes. Whether or not Cavendish actually succeeded in obtaining a telescope is not clear, but this anecdote does tell us that around 1645 it was still hard to acquire a Schyrlean telescope in Europe. According to Court and von Rohr (ibid.) and van Helden (1999), telescopes definitely attributable to Schyrle (or more precisely his artisan, Johann Wiesel) do not seem to have been widely known at this time.[5]

From what has been mentioned above, we conclude that Yoshinao's telescope is of the Schyrlean type. This means that a Schyrlean telescope was already in the Far East just a few years after the publication of Schyrle's book in 1645. If Yoshinao's telescope was a result of information on the Schyrlean telescope brought from the West, this represents a case of very swift cultural transmission. In regard to this hypothesis, another possibility is discussed below in Section 4.

4 IMPLICATIONS OF YOSHINAO'S TELESCOPE AND HISTORICAL BACKGROUND

Here we discuss what the existence of Yoshinao's telescope implies in the context of the historical development of the telescope. In the case of Galilean-type telescopes, it is likely that eyeglass polishing artisans without backgrounds in lens-making science could devise a telescope consisting of a convex lens and a concave lens by trial and error, if they were taught the concept of the Galilean telescope. In fact, Lipperhey is also believed to have invented his telescope through such a process (e.g. see King 1955; van Helden, 1977). Therefore, it is likely that in each country the early development of telescopes was intimately connected with the activities of eyeglass workers, whose history extends back about two centuries earlier than that of telescopes.

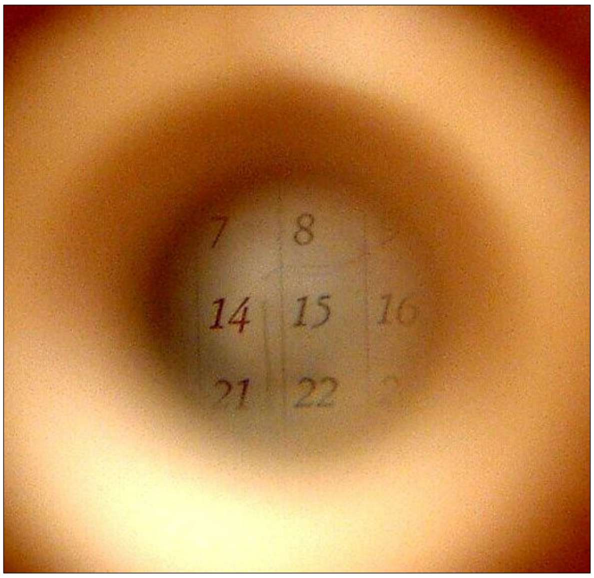

Figure 6: Day numbers of a calendar seen through Yoshinao's telescope. The defocused circular part is the ivory eyepiece ring.

4.1 The Japanese Background

First of all, let us consider the origin of eyeglass-making in Japan. In relation to this, there are a few legend-like anecdotes that date back to the 1620s and 1630s. The first story is recorded in *Nagasaki Yawaso* (*Night Stories of Nagasaki*), which Nishikawa Masayoshi, an astronomer at Nagasaki, published in 1720 after collecting the stories he heard from his father Joken, the highly-respected astronomer and geographer. It says:

> Hamada Yahyoye, the Nagasaki dweller, who used to sail to foreign countries during his peak-activity time, learnt there how to polish eyeglasses, and taught it to his disciple Ikushima Toshichi. This is the origin of lens-making in Japan, and thereafter Nagasaki became well known for its eyeglass production. The place where handiwork-skilled Hamada and his brother learnt lens-polishing was located in an adult country, to the east and south of Japan.

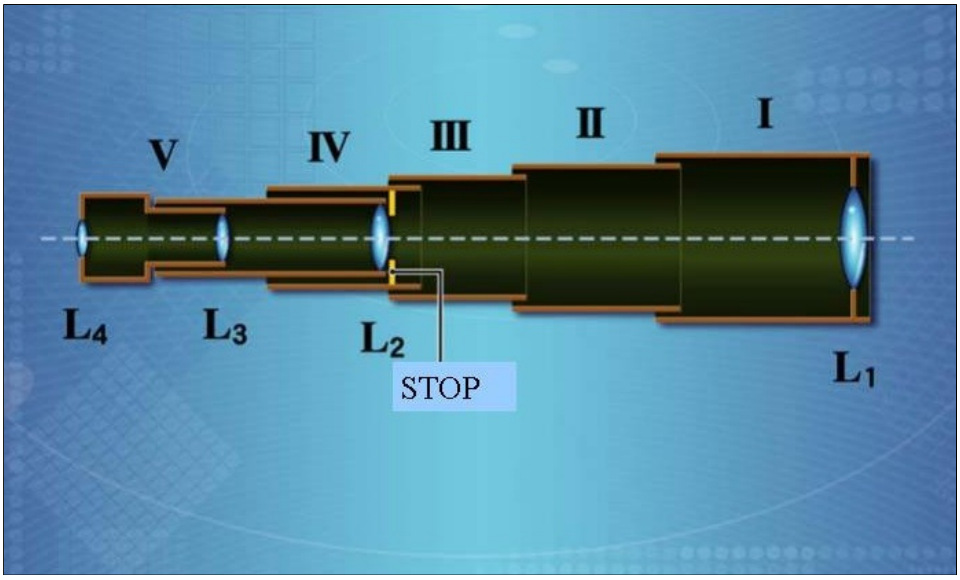

Figure 7: Schematic lens configuration of Yoshinao's telescope. The lengths of draw-tubes I, II, III, and IV are respectively about 400mm, 242mm, 246mm, and 200mm. The tube (267mm) connecting the eyepiece part and the IV-th draw-tube has a stop at the front end.

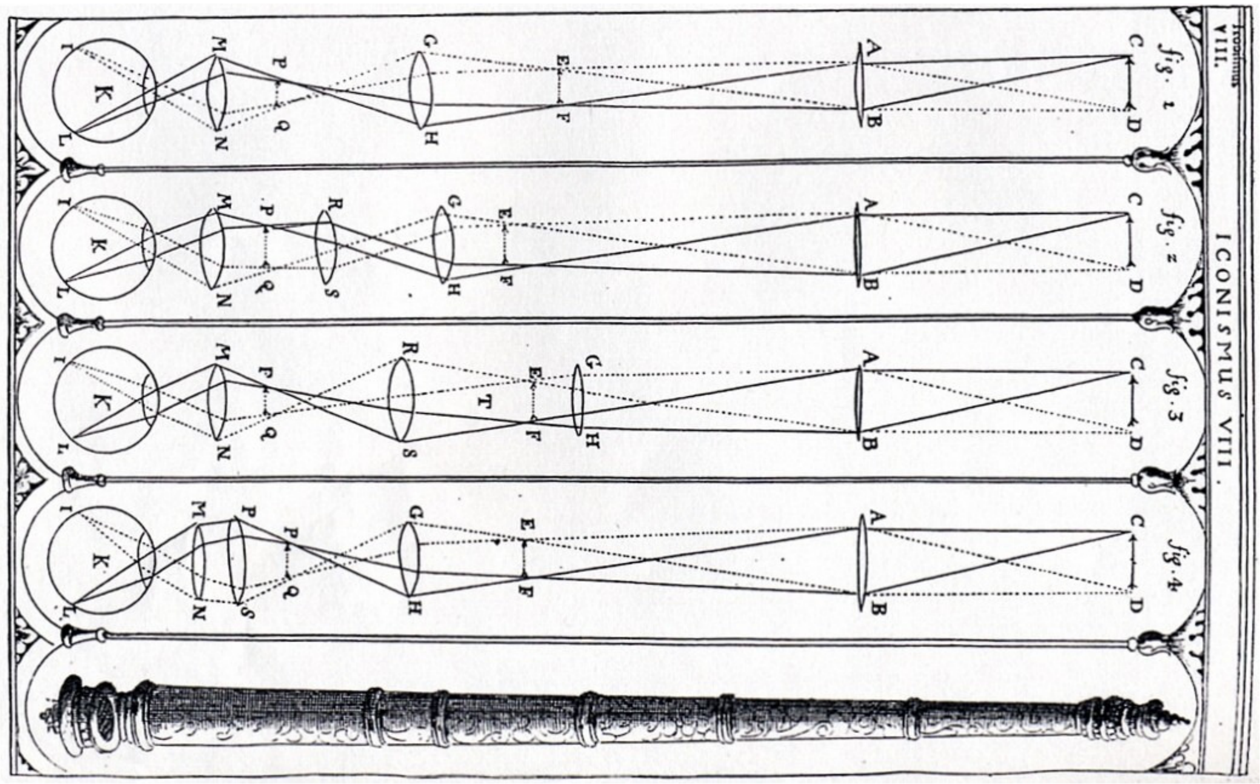

Figure 8: Johann Zahn's plate for optical configurations of Schyrlean telescopes (after Court and von Rohr, 1929).

Hamada Yahyoye was the captain of the *Shuin-sen* (a Government-licensed ship for foreign trade) based at the port of Nagasaki. No one knows for certain where the "adult country" actually was ("adult" usually meaning "civilized" at this time), but because Hamada's ship often went to Taiwan and Vietnam, and he and his crew came into conflict with the Dutch colonists in 1628 at a town in southern Taiwan, it would seem most likely to have been the southern coastal area of continental China. If this is the case, Hamada may have had a good chance to learn lens production there, possibly from Chinese experts.

Figure 9. *Natsume* (a small vessel for keeping tea-leaf) used in Japanese traditional tea ceremony.

There is another record relating to the origin of lens-polishing at Nagasaki. In 1632, a Chinese monk called Mokusu Nyojo (the Japanese pronunciation) from Jiangxi province came to Nagasaki, as an escapee from the political turmoil that followed the decline of the Ming Dynasty, and was nominated to be the chief priest of the Kohukuji Temple. He is famous for his construction of the first stone arch bridges in Japan. It is also said that, upon arriving in Nagasaki, he took some gemstone- and lens-polishers with him and taught the technique to people in Nagasaki.[6] Nyojo's alleged achievements are very likely to have been true, even though the relevant primary sources were stored at the Kohukuji Temple and were totally destroyed by the atomic bombing of Nagasaki in 1945.

In Section 3.1 we mentioned that the tube and caps of Yoshinao's telescope were produced using the *Ikkan-bari* technique, and that subsequent Japanese telescopes followed the same method. A cap skillfully made by *Ikkan-bari* fits the telescope tube very closely and smoothly. Here we should point out that the tube- and cap-making associated with Japanese traditional telescopes—including Yoshinao's—shows good resemblance to the technique used to produce *natsume* (Figure 9), a small tea-leaf vessel with a cap for protection from humidity, which has long been used in the Japanese tea ceremony.

During the period from 1624 into the 1630s, at about the same time that Nyojo came to Japan, a man called Hirai Ikkan (the Japanese pronunciation, his Chinese name is unknown) from the city Hangzhou in Zhejiang Province, China, arrived in Nagasaki and later became a naturalized Japanese (Ikeda, 1987; Sawada, 1966). His profession was to make various small containers by multiple layering of paper on a wooden base with glue and *urushi* lacquer. This technique was so favored by Sen Sotan, who was then the top master of the tea ceremony, that Ikkan was eventually appointed to be one of the ten important artisans to support the tea ceremonial world, and was given the honor of living in the capital, Kyoto, near the master. Japanese people at that time praised his technique by calling it the *Ikkan-bari*, but he kept his methods secret and never taught them to the Japanese during his lifetime (i.e. 1598–1657).

Since the *Ikkan-bari* technique had been used in the manufacture of Yoshinao's telescope in or before 1650 —while Ikkan was still alive—this suggests that the telescope probably was not produced by Japanese artisans. If Yoshinao's telescope was in fact fabricated in Nagasaki by a Chinese immigrant, it is likely that the maker of the telescope collaborated with Ikkan, because most Chinese were forced by the Shogunal Government to live in the small Chinese colonial district of Nagasaki. Or, it may be more likely that the *Ikkan-bari* technique had already existed in China before Ikkan's arrival in Japan, and that he merely applied it to *natsume*, while the maker of Yoshinao's telescope independently utilized the same method.

From what has been discussed thus far, no matter where Yoshinao's telescope was made, it seems clear that its outer appearance, design and production technique all point to a Chinese origin. At the same time, in the history of the Japanese telescope, one can see that Yoshinao's telescope is very important in the sense that its existence substantiates the above-mentioned anecdotes regarding Hamada Yahyoye, Mokusu Nyojo, and Hirai Ikkan, which have long been regarded only as legendary stories.

4.2 The Chinese Background

Given the importance of Yoshinao's telescope, a discussion of the history of telescopes in China would seem in order. Among the Japanese sources on astronomy written in the eighteenth century, some explain that the method of telescope-making in Japan was transferred from Europe, via China. For example, the book *Tenkei Wakumon Chukai Zukan* (*Annotated Illustrations of Tianjing Huowen*),[7] published by Irie Shukei in 1750, states that "… the telescope was invented in Holland in the Middle Ages and that instrument was brought to China and then to Japan."

Another example is in the book *Shusei Horyaku Kojutsu Genreki* (*Revised Compendium of the Horyaku Calendar*) written by the Shogunal astronomer Sasaki Nagahide in 1769. After confirming that he saw the same telescopic views of planets and stars as described by Yang Manuo, Sasaki says: "Both the splendid gadget due to Manuo and the one produced by the people of Qing Dynasty were certainly the instruments that made use of glasses for distant-viewing." There is no doubt that Sasaki is referring to the telescope. Here, Yang Manuo is the Chinese name of Emmanuel Diaz (1574–1659), the Portuguese Jesuit priest who came to China in 1610 and served the Qing Dynasty as an astronomer. He wrote the book *Tianwenlue* (*Concise Dialogue on Astronomy*) in 1615; the main part of the telescopic description in Sasaki's book is in fact no more than an abbreviated version of Yang's *Tianwenlue*. Sasaki's words seem to suggest that some Japanese understood that the telescope had been produced by the Chinese from a fairly early time.

Tianwenlue was actually the earliest book published in Chinese that introduced telescopic observations of celestial bodies. On the other hand, the first Chinese book that described the structure of the telescope and how it was made was *Yuanjingshuo* (*Explanations of the Telescope*), which was published in 1626 by the famous Jesuit astronomer Tang Ruowang (Johann Adam Schall von Bell, 1591–1666). The part of the book dealing with the making and use of the telescope simply states that telescope production can be achieved by combining a front convex lens and a rear concave one. However, even with such an elementary description, an eyeglass polisher with knowledge of the strength of the lenses and perhaps advice from a Missionary priest could have easily assembled a simple telescope of the Galilean type.

Bo Jue is the person whose name first appears in Chinese history as a telescope maker. He was a civil scholar from the city Suzhou, and was famous for his production of copper artillery. Bo owned his own workshop where he carried out experiments and manufactured various instruments. He also studied varied disciplines, including Yinyang divination, astronomy, iron manufacturing, military technology, agricultural irrigation, and so on. Bo began making telescopes in 1635 or a few years earlier, and even attached a telescope to an artillery gun for aiming purposes (Chen, 2003; Wang et al., 1997). According to Wang et al. (op. cit.), in the 1930s and later, in the 1950s, a certain professor examined one of Bo's telescopes and found that it gave *inverted* images, meaning that it was of a Keplerian type. If this is the case, it seems highly likely that Bo independently invented the two convex-lens telescope, since in Europe Christopher Scheiner produced the first Keplerian telescope in 1630 (King, 1955). But it is equally possible that Bo or some of the artisans who assisted him by trial and error came up with the idea of adding one or two lenses to obtain an erect image, since just such a process led Schyrle to succeed in inventing the Schyrlean telescope.

Incidentally, it is interesting to note that Bo worked as an independent scholar and had no contact with the Jesuit missionaries (Wang et al., 1997). A few decades later, an engineer from the city Suzhou named Sun Yunqiu (ca.1629–ca.1662) was engaged to produce various optical instruments, including telescopes which were said to have been almost the same shape, structure, and size as those made by Bo (Wang et al., 1997).

Given the foregoing accounts, it is natural to consider that telescopes themselves and their method of production were quickly transmitted to Japan, because in spite of Japan's strict national seclusion policy—which commenced in the 1620s—the Chinese visited Japan much more frequently and freely than the Europeans did. In this context, it is worth noting that Sun and Hirai Ikkan both came from Zhejiang Province.

5 SUMMARY AND DISCUSSION

In this paper we show that Tokugawa Yoshinao's telescope, which is preserved in the Tokugawa Art Museum, is of Schyrlean type (i.e. consisting of four convex lenses) and was made in or before 1650. In light of the anecdote by Sir Charles Cavendish mentioned in Section 3.4 (cited in Court and von Rohr, 1929) and the fact that Schyrle's *Oculus Enoch et Eliae* was only published in 1645, Yoshinao's telescope must be one of the oldest Schyrlean telescopes in existence.

In Sections 3 and 4 we demonstrated that it is likely that Yoshinao's telescope was produced in or near Zhejiang Province by Chinese artisans, or possibly in Nagasaki by a Chinese immigrant, without direct European influence. Having said that, however, one still cannot exclude the possibility that Jesuit missionaries in China taught the local Chinese how to produce the Schyrlean telescope, with its four convex lenses. The basis for this speculation is the fact that optical problems were widely studied by various Jesuit missionaries, as represented for example by the activities of Father C. Scheiner (see Shea, 1975).

In relation to this, Court and von Rohr (1929) emphasized the key role that the European Jesuit community played in the development of the telescope by including an interesting illustration (Figure 10), taken directly from Scheiner's *Rosa Ursina sive Sol* (1626-1630). Meanwhile, Baxandall (1922-1923) and King (1955) wrote that Scheiner added a second convex lens to the simple Keplerian telescope in order to get an erect image. Since Scheiner's invention and the telescopes made by Schyrle are both a natural development of Kepler's basic proposal consisting of two convex lenses, it is no surprise that the idea of the four-convex-lens telescope came to the minds of the Chinese-based Jesuits and to Schyrle quite independently. Then the concept was quickly communicated by the missionaries to Chinese artisans, eventually resulting in the manufacture of Yoshinao's telescope.

In any case, we emphasize the importance of Yoshinao's telescope in the history of the telescope, and we stress how vital it is for this telescope to be compared and contrasted with those made by Bo Jue and Sun Yunqiu (if they still exist), and with old telescopes preserved in such places as the Gugong Palace Museum (see Liu Lu, 1999).

Figure 10: Experimental activities in optics by the Jesuit fathers around 1630 (after *Rosa Ursina sive Sol* (1626–1630) by C. Scheiner).

6 NOTES

1. The original Japanese reference to Ieyasu's telescope is in *Sunpu-ki* (*Sunpu chronicle*), which describes Ieyasu's chronicle recorded by one of his vassals. The telescope was offered to Ieyasu on 17 September 1613 at Sunpu city by Captain John Saris who came to Japan in the ship *Clove* (Satow, 1900).
2. Regarding the history of Japanese telescopes, *Nihon Sokuryo-shi no Kenkyu* (*Studies on the History of Land-surveying in Japan*) by Y. Mikami and *Kinsei Nihon Tenmongaku-shi* (*Pre-modern History of Astronomy in Japan*), Volume 2, by T. Watanabe, give elaborate reviews and include bibliographical details and references. Also, Peter Abrahams, a past-President of the Antique Telescope Society, has produced an excellent and detailed chronology written in English, which is available at the following URL: http://home.europa. com/~telscope/ tsjapan.txt. Although it contains some unclear and/or incorrect descriptions, these are not Abrahams' fault as they were inherited from the original Japanese sources that he cites.
3. According to the Tokugawa Art Museum, the original inventory of the articles inherited from Yoshinao was called *Keian 4-nen Odogucho* (*The Articles Catalogue of 1651*).
4. *Kiyu Shoran* by I. Kitamura is now reproduced (2004) as one of Iwanami Bunko series, from Iwanami Shoten Ltd.
5. Willach (2002) reports two Schyrlean telescopes whose credit goes to J. Wiesel, which are preserved in Sweden, although both are in an incomplete state (i.e. some lenses are missing). These telescopes are characterized by five lenses, many-stage draw tubes, and a telescopic tube inversely tapered towards the objective lens. On the other hand, as discussed in Section 3, none of these is a characteristic of Yoshinao's telescope. Hence, if the telescopes preserved in Sweden are typical of those attributable to Schyrle and Wiesel, then we can conclude that European Schyrlean telescopes did not directly influence Yoshinao's telescope. According to Keil (2000: 375), Wiesel's new telescopes were sold from 1649 onward in many European countries.
6. *Encyclopedia of Nagasaki* (Nagasaki, 1984). This book includes an article about the Chinese priest Mokusu Nyujo, based on the *Nagasaki Shishi* (*Chronicle of City Nagasaki*) which was published by Nagasaki city in 1923-1925. Shirayama (1990) also mentions the connection between Nyojo and lens-polishing, but without supplying any references.
7. *Tianjing Huowen* (*Dialogue on Astronomy*, in Chinese), written by You Ziliu, was published in 1675. This book was imported into Japan soon after its publication and for a long time was very much welcomed as an introductory text on Western astronomy.

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Dr Tsuko Nakamura is Professor of Information Sciences at the Teikyo-Heisei University in Tokyo, and specialized in the minor bodies of the Solar System, with particular emphasis on size distributions of very small members of the main-belt and Trojan asteroids. He is also interested in the history of astronomical instruments and star maps in the Far East. In the latter field, he has written about 30 research papers, and is the author of the following books (all in Japanese): *General Catalog of the Japanese Astronomical and Land-surveying Books before 1870* (2005), *History of Cosmovision and Science* (2008), and *Astronomers of the Edo Period in Japan* (2008). Tsuko is a member of IAU Commission 41 (History of Astronomy) and also a member of the IAU Working Group on Historical Instruments.

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ETHNOGRAPHIC AND LITERARY REFLECTIONS ON ANCIENT GEORGIAN ASTRONOMICAL HERITAGE

I. Simonia
Department of Physics and Mathematics, Ilia Chavchavadze State University,
Cholokakashvili str. 3/5, 0162 Tbilisi, Georgia.
E-mail: simon_ir@access.sanet.ge

C. Ruggles
School of Archaeological Studies, University of Leicester, University Road,
Leicester LE1 7RH, England.
E-mail: rug@leicester.ac.uk

and

R. Chagunava
Pekin str. 2-23, 0171 Tbilisi, Georgia.

Abstract: Ethnographic records and Georgian literature contain useful information about ancient Georgian astronomical knowledge systems and beliefs, with specific reference to time-keeping, the months of the year, the planets, individual stars and specific asterisms, star clusters, the Milky Way, and various kinds of ancient astronomical instruments. In this paper we examine a selection of such material.

Keywords: Georgian astronomy, ethnoastronomy, Georgian literature.

1 INTRODUCTION

Lately, significant attention has been directed to the study of ancient Georgian astronomy (see Chagunava, 1990; Georgobiani, 1986; Kharadze and Cochlashvili, 1958; Simonia et al., 1994, 2000, 2001, 2003, 2004, 2005). General trends in the development of ancient Georgian astronomy have been outlined; problems connected with the functioning of old scientific centers—and particularly observatories—have been discussed; biographical sketches of a number of important Georgian astronomers and philosophers have been presented; and papers and books discussing calendar systems, old manuscripts and books, and artifacts having archaeoastronomical significance have been published. However, much remains to be done, particularly in relation to

1) ethnographical accounts containing information about the sky and celestial phenomena;
2) stone constructions, ruins, monuments, cult places, temples and churches dating from the Bronze Age to the Middle Ages, and their archaeoastronomical and ethno-cosmological significance; and
3) the complex of ancient Georgian astronomical and astrological manuscripts stored at various institutions, archives and museums both in Georgia and elsewhere.

In this paper we contribute to the first of these priority areas by reviewing the astronomical evidence preserved in a variety of ethnographic sources before briefly examining astronomical references contained in some Georgian literature.

2 ETHNOGRAPHIC RECORDS AS SOURCES OF GEORGIAN ASTRONOMICAL INFORMATION

For the purposes of this study, we subjected the following groups of records to comprehensive analyses: (1) ethnographical notes and investigations carried out by Georgian ethnographers and historians during the twentieth century; and (2) various books and dictionaries published at different times in Georgia. Of course, this material is not uniform, but it contains a variety of interesting facts that reflect the knowledge of ancient Georgians about the sky, celestial bodies and impressive astronomical phenomena. Much of this knowledge had practical applications in everyday life, either during agricultural practices, or in direction-finding while voyaging. While some astronomical knowledge was modified or lost with the passage of time, basic concepts about the Universe survived through to the present day. We believe that the motivating mechanisms that led to accumulation of this celestial knowledge were: (1) the need for orientation and to fix time in order to maintain a continuous agricultural cycle; (2) the need for local and global spatial orientation, in order to bring the land into cultivation (i.e. to be able to build roads, construct settlements, etc.), as well as for military purposes; and (3) the need to 'interact' with bright heavenly bodies for cultic, religious purposes. These were the main factors that served to stimulate the accumulation and adaptation of knowledge about heavenly phenomena, knowledge that was used by ancient people in everyday life. Among ancient Georgians, this eventually led to the formation of a rather harmonious ethno-cosmological system of beliefs. However, our knowledge of this system is far from complete, for we only have snippets of information drawn from scattered ethnographical sources. Let us now examine some of this fragmentary evidence.

2.1 Orbeliani's Dictionary of the Georgian Language

In the seventeenth and eighteenth centuries the Georgian philosopher Sulkhan Saba Orbeliani (1658–1725) composed an explanatory dictionary of the Georgian language (*Sitkvis kona* = Bunch of Words), which included various astronomical words and terms. This dictionary was based of early Georgian and foreign manuscripts, and the meanings of some of the astronomical terms were discussed by Simonia and Simonia (1994), who took into account the sources used when compiling the dictionary and the philosophical ideas of its author.

The final edition of Orbeliani's dictionary, published under the editorship of Abuladze in 1993, contains some terms relating to the calendar. For example, on pages 650-651 in the second volume the names of the months are given, not just in Georgian but also in Latin, Arabic, Turkish, Greek and Assyrian. Moreover, it is interesting that the author gives two different types of Georgian names for the months: the ancient Georgian literary names and the folk names. First, we shall list the folk names: gantskhadebis tve, phebervali, marti, aprili, maisi, ivanobis tve, kvirikobis tve, mariamobis tve, enkenis tve, gvinobis tve, giorgobis tve and kristeshobis tve (in that order). In English, these respectively mean: the month of Appearance, February, March, April, May, the month of Ivanoba, the month of Kvirikoba, the month of St. Mary, the month of Enkeni, the month of vine-making, the month of St. George and the month of Christmas. From the names it is obvious that the months were connected with important agricultural periods (e.g. the month of vine-making), as well as with the names of Christian Saints and with the birth of Christ. The dictionary demonstrates that these folk names for the months were used from the fourth century AD through to the eighteenth century.

For comparison we now give the ancient Georgian literary names for the months: apani, surtskunisi, mirkani, igrika, vardobis, marialis, tibis, kveltobisa, akhaltslisa, stvlisa, tirisknis and tirisdeni. The meanings of some of these names are clear, but others have still to be interpreted. For example, tibis tve (the seventh month), means the time of haymaking; akhaltslis tve (the ninth month), is the first month of a new year; and stvlis tve (the tenth month), means the month of counting—when they would count the harvest, and in particular the grape harvest.

It is clear that Orbeliani's dictionary contains valuable information about the astronomical and calendrical knowledge of the Georgian people, and that it warrants further study.

2.2 Javakhishvili's *Materials about the History of Georgian Local Manufactures and Small Handicrafts*

In 1983 a five-volume study titled *Materials about the History of Georgian Local Manufactures and Small Handcrafts* was published, based on ethnographic information gathered by I. Javakhishvili in villages throughout Georgia.[1] We found an interesting extract in the second part of the fourth volume, on page 154:

> When the time comes to let oxen off the plough, they (the drovers and ploughmen) eat. The day drover and ploughman go to sleep, while the night drovers send the cattle to pasture. When an ox is tired, it does not eat, but lies down to rest. A good drover does not allow it to lie and makes it eat. Otherwise, the following day a hungry ox cannot work. When the ox is sated, it lies down. A good drover does not lie down until the ox lies down. Then the drover lies down and puts his head on the ox. When the ox gets up, the drover wakes up. At dawn the ox usually runs away. It is tired of work. The drover needs to be very watchful and not allow the ox to run away. The ox goes far away to a pasture and the drover should know the time when to bring it back in order to yoke the ox in time. Night drovers compete with each other, and ploughmen compete in yoking the oxen. Drovers judged the dawn by the stars. When Mravalai was leveled, the drover sent the cattle to pasture. Little by little Mravalai declined and it was assumed that after Mravalai rose Sastsvrebi, and at the very end – Chkita or Tsiskari. Prior to Tsiskari, Khariparia rose. The drover looked up, saw Khariparia and used to say: "I can sleep a little more." He went to sleep and the ox ran away and was lost (an ox usually runs away at dawn). That is why this star is called "Khariparia". (Our translation).

First of all, it should be noted that "Khariparia" in English means "a runaway ox"; "Mravalai" means multiplicity; "Sastsvrebi" is Libra; "Chkita" is to peep out; and "Tsiskari" can mean "the door of the heaven" although it was also the name of the planet Venus. Ethnographers believe that the story of Khariparia has been part of Georgian folklore from time immemorial since farming has a long history in Georgia (see Assatiani et al., 1997; Braund, 1994).

We chose several stars as possible candidates for the 'role' of Khariparia on the basis of the following criteria, and estimated the changes in their coordinates over the last two thousand years as a result of precession.

1) The star had an apparent visual magnitude ≥1.5, in that it had to be bright enough to be conspicuous to an inexperienced observer.
2) Since it was visible following the spring period of sowing, we only chose stars with right ascensions between 18h and 05h 30m.
3) Given variations in Georgia's latitude, only stars with declinations north of –20° were considered.

We used the Simbad Astronomical Database for this analysis, and the results are presented in Table 1. This shows that the only viable candidates are α Lyr (Vega), α Aql (Altair) and α Cyg (Deneb). All of these bright stars could attract the attention of ancient drovers and ploughmen.

Table 1: Khariparia candidates.

Name	Magnitude	α 2000 (Simbad)	δ 2000 (Simbad)
α Lyr (Vega)	0.03	18 36 96.3	+38 47 01
α Aql (Altair)	0.76	19 50 46.9	+08 52 05
α Cyg (Deneb)	1.25	20 41 25.9	+45 16 49
α Tau (Aldebaran)	0.86	04 35 55.23	+16 30 33
β Ori (Rigel)	0.3	05 14 32.27	–08 12 05
α Aur (Capella)	0.03	05 16 41.35	+45 59 52

We then decided to extend the search to other objects and examined binary stars, wide pairs of double stars and selected variable stars, and so on, but none of these objects proved to be a suitable candidate on the basis of the aforementioned criteria.

2.3 Bochoridze's *Tusheti Ethnographic-folklore Material*

In his book on Tusheti ethnography and folklore, G. Bochoridze (1993; our translation) writes:

> Below I give brief information collected by me in the village of Omalo on celestial bodies. They are the stars: Khariparia, Tsiskari, Gutneuli, Jaraebi – a row of stars, Mravalai, Tsultokhebi – resembling a sickle. In spring they follow Mravalai at the distance of one sabeli (the unit of measurement in old Georgia), and in winter they are far from each other.
>
> Stars:
>
> 1. Tsultokhebi – rise in summer at supper-time, in the month of "Giorgobistve" they rise at midnight, they are 5 stars.
> 2. Mravalai follows Tsultokhebi as a cluster. Now it is called Jaraebi (Mravalai was its early name).
> 3. Tsiskari follows them, in summer – in the evening, in the month of Giorgobistve - at night, in supper time (in the morning it sets at dawn). It rises before sunrise, it is one star.
> 4. Mejoge, Jogis Tsiskari is one star. It rises after midnight and is a big star.
> 5. Irmebis Nakhtomi (Jump of deer). An ox and a deer competed with each other in serving a peasant, in adroitness and in ability. The ox won and the deer was torn into two parts when it jumped.

The ethnographers who collected this information probably did not have detailed astronomical knowledge or observing experience, and this is why they did not try to identify the different stars. What can we deduce?

It is interesting that in spring Tsiskari, Gutneuli, Jaraebi and Tsultokhebi follow Mravalai at a definite distance and then the distance between them increases. This indicates that some of these stars are 'wandering' stars, i.e. planets. Among them is Tsiskari, which we identify as Venus, and Mejoge—another planet which rises after midnight. However, our identification of Tsiskari and Mejoge as planets creates certain problems in that the positions of the planets change in the sky in the course of the year and from year to year, but this peculiarity is not reflected in the ethnographic record.

Also of interest in the above-mentioned quote is the "Jump of deer." In our opinion, this short legend shows how the ancient Georgian peasants described the faint strip of light that crossed the sky—namely the Milky Way. In modern Georgian, the Milky Way is translated as the 'jump of deer', whereas the *Georgian Encyclopaedia* (Volume 5, page 225) gives the following ancient Georgian synonyms for the Jump of Deer (Irmis nakhtomi): Trace of an Ox, The Way to Jerusalem, The Leg of a Bear and the Trace of a Bear's Knee.

S. Menteshashvili (1943) throws light on some of the other astronomical terms listed above. For instance, Gutneuli (which he terms Khargutani) is the constellation of Ursa Major, while Mravalai relates to Ursa Minor. The *Georgian Encyclopedia* (Volume 8, page 106) shows that the names Mravalai and Khomli (mentioned below in Section 2.4) are ancient Georgian synonyms for the Pleiades star cluster. The fact that the Pleiades lie within the constellation of Taurus is interesting from the viewpoint of the origin of the different names.

2.4 Khomli Stars in Oral Stories and Chronicles

Let us now consider the book by M. Makalatia (1972), in which he describes some ancient traditions associated with pasturing of sheep in different seasons. For example, on page 50 we read:

> The people living in the villages of Khizabavra and Zveli still remember the ancient traditions of determining the time of driving the cattle. After the week of Khomli they could drive the cattle over an upper mountain, as snow was not expected any more. The week Khomli comes in the month of Tibatve, when a group of Khomli stars appear. Khomli rises on the 6th day of Tibatve, but till 12 Tibatve it is not seen by eye. During this week great care is taken with the sheep being in the open air. The peasants … believe that Khomli is dangerous in the morning, when sheep still lie in sheep-pens. If Khomli rises above the lying sheep, it "strikes them and causes the falling-off of their hair and the ulceration of their heads and faces" (Khizabavra) ... In the morning they wake the sheep and drive them (Zveli). In the village of Zveli, during the Khomli week sheep are driven to the nearby fields at the edge of the forest, where there is a protected place Cholaka. (Our translation).

Georgian peasants knew of the heliacal rises of the stars, but they were afraid of this phenomenon. Such beliefs probably originated in pagan times (i.e. prior to the fourth century AD in Georgia). The above ethnographic fragment contains ancient data from a period when Georgians still used ancient terms—including Tibatve—for the names of the months.

The Georgian chronicle *Kartlis Tskhovreba* (*Description of the Kingdom of Georgia*) by Vakhushti Batonishvili (1973) also refers to Khomli. In Volume 4 on page 762 we find (our translation):

> … to the west of the Rioni [a river in western Georgia] at the base of the mountain is Khomli rock, which is very high. It deserves such a name on account of its height. It was identified with the star Khomli. In this rock a cave was cut, which was inaccessible to enemies, and this was used to store the Kings' treasures. (Vakhushti Batonishvili (1696–1784), Georgian historian and geographer, and the son of the Georgian King Vakhtang VI Bagrationi).

This Georgian chronicle accommodates a long period in Georgian history, from antiquity to the eighteenth century AD. Meanwhile, the brief above-mentioned quote indicates that a) knowledge of the Khomli star was widespread in Georgia in the past; and b) the exact spot where Khomli was seen to rise was observed by ancient astronomers from the high rock bearing the same name.

2.5 Bedukadze's *Popular System of Time Determination …*

In a monograph relating to systems of ancient Georgian time-determination, S. Bedukadze (1968; our translation) says:

> In Khevi [a region of Georgia] they have a cult-ceremony, the so called "Astvaglakhoba". On New Year's Eve, three archpriests ascend to the top of

> "Sameba" for the night. They sit in silence leaning against each other's backs and observe the sky until daybreak. In the morning they sacrifice a new-born calf, have a feast, and then predict the weather, the harvest, wars or diseases in the coming year.

In our opinion, this ethnographical account describes the ancient Georgian tradition of carefully-planned methodical observations of the positions of celestial bodies. This tradition was probably perfected over a long period of time. We believe that those living in the mountainous regions of Georgia divided the dome of the sky into three equal triangular sectors for better understanding of the phenomena taking place there. They realized that one observer could not adequately observe the whole sky and understand what he saw. Accordingly, on New Year's Eve three pairs of eyes carefully and simultaneously watched the sky. The division of the sky into two equal parts would have been insufficient and into six parts more than necessary, that this is why three (and not two or six) archpriests ascended to the top of mountain. Information about phenomenon seen by one of them was subsequently added to data obtained by the other two observers, and thus the whole picture was formed. Each of them was responsible for his sector of 120°. The fact that the archpriests predicted the future speaks in favor of the fact that there could be some empirical experience connected with atmospheric climatic phenomena determining the visibility of one or another celestial body. This mosaic triangular Universe impresses one with its thoughtfulness. The ancient priests knew how to observe, calculate time and orient themselves with respect to their environment. Here we speak from our own point of view, but this ethnographical material can be considered from other points of view as well.

2.6 Concluding Comments

The ethnographical examples containing astronomical information presented here in Sections 2.1 through 2.5 form only a small part of ancient Georgian folk heritage. It is to be regretted that the scholars who collected such celestial data lacked the knowledge to adequately investigate ancient Georgian astronomical systems, but this is thoroughly understandable given that the focus of their studies was the everyday life of Georgian peasants.

The landscape of Georgia is diverse and ranges from high mountains to low plains. Large and small villages are scattered throughout the country and the situation would have been the same in the past. Often when there were cold winters or hot summers ancient peasants from one village would have had little opportunity to meet their counterparts from other villages, as such rendezvous often would have involved trips of several hundred kilometers over high mountains and through thick forests. So it is quite possible that the same celestial objects went by totally different names in different regions of the country, or even in neighbouring villages. This interesting possibility clearly requires further investigation.

3 ASTRONOMICAL REFERENCES IN GEORGIAN LITERATURE

Let us now consider another source of ethno-astronomical information about ancient Georgians: classical and modern Georgian literature. Various examples—involving both prose and poetry—are discussed below.

3.1 Rustaveli's *The Knight in the Panther's Skin*

In the poem *The Knight in the Panther's Skin* by the well-known twelfth century Georgian scholar, Shota Rustaveli, one can find plenty of ethno-astronomical material. A full astronomical analysis of this ancient poem needs to be carried out separately, but for the purposes of this study we will only consider a sample of its contents. In 1968, Bedukadze also examined evidence of time-determination as reflected in Rustaveli's poem, and she particularly drew attention to strophae 184, 185, 770 and 1569.

Let us look at strophe 1569 (our translation):

> The star of dawn shines as bright as the moon when together in heaven,
> But if they part and withdraw from each other they fade and grow paler.
> They must alas withdraw from each other if heaven has willed it.
> One must be as high as a hill or a mountain to see them.

Bedukadze assumes that in this poem Rustaveli shows that the old way of determining time was through observations of the motion of specific celestial bodies from the top of a high hill. And the observer had to be able to observe in all four directions.

3.2 Astronomical Instruments Mentioned in Georgian Literature

On the basis of ethnographical documentation, Bedukadze (1968) also proceeded to describe the types of instruments used by the ancient Georgians to determine time:

> In Khevsureti [a region of Georgia] seasons were determined by means of a group of stone columns, the so-called "Sun nests" erected on peaks to the east of villages. According to the motion of the rising Sun from one nest to another, people determined: a month, a season, the end and the beginning of a year, the important dates of agricultural character.
>
> The important element of old houses in Svaneti [Svaneti is a region of Georgia] was a ritual east window (lakhvra), looking towards to the Sun. The head of the family – a man – used to read prayers by the window at each sunrise. Lakhvra was something like a calendar, or fixed tool relating to solar motion. In this calendar, the different places where the first sun-beam fell were marked … [and] the track of its motion during the days and months. In such a way the holy days of each season were determined, and the dates when agricultural work should start. (Our translation).

Bedukadze (ibid.) also describes various moondials and sundials of the simplest construction (circular or with a straight edge), used in different regions of Georgia (Kartli, Trialeti, Meskheti, Javakheti, etc.): a moon or sun beam reflecting a ray of light or a shadow and how these moved in the course of time around the family hearth in the center of the house. The head of the family (the father or the mother) used such dials in everyday life.

3.3 Concluding Comments

It can be seen from the foregoing material that in the twelfth century AD in Georgia a harmonious system existed to determine and make use of time. If Rustaveli used folklore in his poem then one can assume that this system of time-determination was developed in Georgia earlier than the twelfth century. Thus, Rustaveli's poem contains historical and ethnoastronomical information.

The system of time-determination by means of 'Sun nests' seems to be a very ancient one. In the high mountains of Khevsureti, processed stones and stone constructions served the ancient Georgians as farming implements, arms and simple instruments for time measuring. We think that stone columns on the tops of mountains were prehistoric Georgian sundials.

The Svan lakhvra was a fixed tool for demonstrating the motion of Sun, and was another type of ancient Georgian sundial. It was only used for domestic purposes. Taking into account the tower-like constructions of Svan houses and the mountainous terrain in Georgia, a small east window (lakhvra) seems to have served as a primitive type of sundial. It is obvious that in different regions of ancient Georgia various systems of time-determination were developed, and the simplest of instruments were made and used for measuring time. We think it would make good sense to organize scientific expeditions to mountainous areas of Georgia, such as Khevsureti, Svaneti, as well as some other regions, in order to search for the remains of ancient sundials. We also believe that a full ethnoastronomical analysis of Rustaveli's poem should be carried out.

4 CONCLUSION

Georgian ethnographic accounts contain an abundance of important astronomical information. Though this in-formation is diverse, it is scattered, and specialists working in the fields of history, ethnoastronomy and anthropology should make regular efforts to gather, optimize and analyze it. However, this is not a simple exercise as it requires great effort and time. In the villages in the mountainous and flat regions of Georgia one can still encounter many recorded legends, oral accounts and folk poems containing ancient Georgian information about the sky, the stars and the Universe and the place of a man in this boundless realm.

In this paper we have considered a number of records that contain information about ancient Georgian astronomical traditions and practices. While many such ethnographic records exist in Georgia, most have yet to be analyzed from an astronomical standpoint. Much research remains to be done, and we invite foreign scholars to join us in this endeavour.

5 NOTES

1. Professor Javakhishvili organized ethnographic expeditions to many Georgian villages between 1915 and 1935. He worked up his ethnographic notes and prepared them for publication, but died in 1940 before this could be arranged. The manuscript was kept in the museum, and was only published in 1983.

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Web site: simbad.u-starsbg.fr

Dr Irakli Simonia is an Associate Professor of Cultural Astronomy and Archaeoastronomy at the Ilia Chavchavadze State University in Tbilisi. His history of astronomy research interests are mainly directed towards Georgian ethnoastronomy and archaeoastronomy. He is the author of more than

40 research papers, a Committee member of the IAU Working Group on Archives and President of the International Scientific Interdisciplinary Association Astroarchaeocaucasus.

Professor Clive Ruggles is Emeritus Professor of Archaeoastronomy and Ancient History at the University of Leicester. His research interests are directed mainly towards archaeoastronomy and archaeology. He is the author of more than 100 research papers, and is currently the Acting President of IAU Commission 41 (History of Astronomy).

Dr Raul Chagunava is retired. His research interests lie in the history of the calendar and chronology. He is the author of about a dozen research papers, and is a member of International Scientific Interdisciplinary Association Astroarchaeocaucasus.

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LETTERS FROM AUGUSTIN HALLERSTEIN, AN EIGHTEENTH CENTURY JESUIT ASTRONOMER IN BEIJING

Stanislav Juznic
AMNIM-Ljubljana, Fara 2, 1336 Vas, *Slovenia.*
E-mail: stanislav.juznic-1@ou.edu

Abstract: Augustin Hallerstein (1703-1774) was the last astronomer sent to Beijing by the Society of Jesus. He left Europe for China in his mid-thirties, and continued to send letters back home until he died thirty-five years later. These letters and reports contained important information on Chinese astronomy, and were read in the courts of Europe; many were also published. Hallerstein was one of the most important European astronomers in Beijing, his European publications surpassing those of his predecessors.

Keywords: Hallerstein, Jesuits, Ljubljana, China, history of astronomy.

1 INTRODUCTION

The famous astronomer, Ferdinand Augustin (or Avguštin) Haller von Hallerstein was born in Ljubljana (now in Slovenia) on 27 August 1703, admitted to the priesthood in Vienna on 27 October 1721, and died in Beijing on 29 October 1774 (Dehergne, 1973: 122). He arrived in Beijing in March 1739, and while there was known by his Chinese names, Liu Sung-Ling and Lieou Song-Ling K'iao-Nien. Hallerstein was the last of the old Society of Jesus astronomers sent to Beijing, and without his contribution the story of the Jesuits' success in China would be incomplete (see Figure 1).

During his thirty-five years in Beijing, Hallerstein sent relatives back home a succession of letters and reports. These provide invaluable information on the life and times of the European astronomers based in the Chinese capital, as documented in this paper.

2 HALLERSTEIN DESCRIBES HIS BEIJING PREDECESSORS TO HIS BROTHER

Hallerstein regularly corresponded with his brother Janez Vajkard Baron Hallerstein (1706–1780), who was the confessor of the Empress Maria Theresia's brother-in-law, Prince Charles Alexander of Lorraine. Janez Vajkard lived in Brussels which was the capital of the Habsburg Netherlands and the source for modern science which the Empress needed in her southern lands (including Hallerstein's native Carniola). Augustin Hallerstein's letters to his brother were widely read in the European courts.

Hallerstein learned all about the works of his Jesuit predecessors in Beijing. In 1743 he reported to his brother about the writings of the Italian Jesuit, Matteo Ricci (1552–1610) (Dehergne, 1973: 219; Hallerstein, 1781: 5), who was Clavius' student at the *Collegio Romano* until 1577. From 1577 until 1582 Ricci was a missionary in India. According to Hallerstein's report mailed to Janez Vajkard, after 1601 Ricci was a missionary in China under the Emperor Wan Li (Chintsong), and he began the Jesuit mission. Before Ricci, the Muslims calculated the ephemerides according to Arabic tables in the first class of the Beijing Astronomical Bureau. The Muslim Bureau for Astronomy was established in 1268 and made use of trigonometry, which was not popular among the Chinese. In the second class (of the three classes, or departments) were the Mandarins who observed the sky (Montucla, 1799, 1: 474-475; Huff, 1993: 241).

Ricci translated into Chinese Euclid's first six books, Clavius' tractate and the shortened extract from Clavius' works. In 1607 he wrote the very first trigonometry text published in Chinese (Needham and Ling, 1959, 3: 110). With the introduction of the sign for equivalence and other algebraic elements, the Jesuits brought comparatively new European concepts to China (Needham and Ling, 1959, 3: 114).

Besides Ricci, Hallerstein also described to Janez Vajkard the work of the architect-astronomer Johann Adam Schall von Bell,[1] who studied mathematics with Christoph Grienberger (1564–1636), Clavius' successor at the *Collegio Romano*. In 1619 von Bell arrived in Macao. The Emperor Chongzhen[2] invited him and Giacomo Rho to continue to make changes to the inaccurate calendar which was used for the wrong prediction of an eclipse in 1630. The Jesuits Giacomo Rho (1592–1638) and von Bell had sailed for China in 1618 and the latter eventually succeeded Johannes Schreck (1576–1630) who was also known as Terrentius (Dehergne, 1973: 215, 241).

Figure 1: In 2003 this Slovenian stamp was printed to commemorate Hallerstein's important contribution to Beijing astronomy.

Von Bell and Rho were supposed to continue the reform of the calendar of the Chinese Catholic, Paul Xu Guangqi (1562–1633), who had collaborated with Ricci between 1604 and 1607. Von Bell also researched earthquakes (Hallerstein, 1781: 5), and in 1640 he developed a portable sundial with a compass (Needham and Ling, 1959, 3: 312). Sundials were one of the main research areas of the Jesuits, especially of Kircher in Rome in the following years.

Von Bell lived in China for forty-seven years, and with his collaborators he wrote 150 Chinese astronomical books about eclipses, telescopes, gnomonics, trigonometry, and the calendar. He also corrected the calendar and the ephemerides. In 1644, when the new Manchu Qing Dynasty came to power, von Bell became the (temporary) Director of the Astronomical Bureau, inheriting Schreck's position (Montucla, 1799, 1: 469-470). Von Bell became a teacher of the first Manchu Emperor of the Qing Dynasty, Shunzhi, who assumed power in 1651, after the regent's death, even though he was not old enough. In 1658 von Bell became a mandarin of the first rank, but the Emperor died just three years later, when he was only 23 years of age. The new Emperor, Kangxi, did not like von Bell as he had wrongly predicted the suitable marriage date of one of the Emperor's family members, which was considered high treason. At the trial, von Bell managed to avoid the worst possible fate as his group was able to over-shadow their Chinese competitor's prediction of the solar eclipse of 16 January 1665.

Von Bell's duties in Beijing were then assigned to his assistant, Ferdinand Verbiest.[3] On 15 May 1665, Verbiest and the Emperor's grandmother arranged for von Bell to return to Beijing, and he died there in the following year. Immediately after his death, A. Kircher (1667, 110-112) published the complete story, and it was included in modern histories, without making use of Chinese sources. Troubled times continued for the Jesuits until early 1669, and only ended when Verbiest's calculations of planetary positions and comparative shadow length predictions proved the superiority of Western astronomy.

The Jesuits brought about 7,000 printed scientific works with them when they came to China. For example, Ricci's collaborator in Beijing, Niklaas Trigault (1577–1628), brought Agricola's *De re metallica* to China (see Dehergne, 1973: 274), and in von Bell's time this book was translated into Chinese (1638-1640) and given to the Emperor. The Emperor wished to use it as the handbook for mining in China, but the Minister of Finance, Ni Yuanly, opposed the idea, fearing that the development of mining would damage the Chinese farmers. The quarrel only ended with the Manchu occupation of Beijing on 4 June 1644.

Hallerstein did not mention the Croatian missionary and astronomer Ivan Ureman (1583–1620) in the letters that he mailed to Europe. Ureman landed in China in 1615, and lived mainly in Macao. In Rome, Kircher published Ureman's letters about magnetic declination (see Dehergne, 1973: 277).

Before Hallerstein's time, Ferdinand Verbiest (1623–1688) worked at the Astronomical Bureau. He had arrived in Macao in 1658, and made his way to Beijing two years later. Verbiest published three theological and nine philosophical and natural history works in the Chinese language. He was the first in China to use a steam engine to drive a ship, many years before Robert Fulton. In 1670 he measured the expansion of air with a thermometer similar to Galileo's, which was also sensitive to changes in atmospheric pressure. Meanwhile, his hygrometer used the inner organs of animals, whereas the Chinese had earlier used carbon for this purpose (Needham and Ling, 1959: 466, 470). The Chinese astronomers accused Verbiest of getting rid of older Chinese astronomical instruments and replacing them with European ones, but Verbiest had left Europe too early to be aware of the achievements of astronomers like Gian Domenico Cassini, Edmund Halley, John Flamsteed or Jean Picard (Montucla, 1799, 1: 470).

Kilian Stumpf (1655–1720) made a quadrant from material derived from old Chinese astronomical instruments, and when the Chinese historian of mathematics, Mei Ku-Chhëng (1681–1763), complained about this (Jami and Qi, 2003; Sivin, 1965; Wong, 1763), Stumpf defended himself by saying that a Mandarin had bought a melted artifact of brass which he had just used, and he could prove it (see Dežman, 1881; Needham and Ling, 1959, 3: 380, 452). Mei Ku-Chhëng was the influential grandson of the famous mathematician, Mei Wending (1633–1721), whose second edition of mathematics, *Lisuan quanshu*, was published in 1723. Mei Ku-Chhëng collaborated with He Guozong, and refused to change the mathematical methods of his grandfather, because he believed in the strength of the Chinese astronomical tradition. He separated astronomy from astrology on the basis that astronomy was a Confucian discipline and astrology was not. Mei Ku-Chhëng's work was discussed in Chourenzhuan's *Biography of the Great Astronomers and Mathematicians*, issued in four volumes in 1810, which included an appendix containing von Bell's notes (recorded in 1645) about Western astronomers (Chu, 2003).

On 27 October 1765 Hallerstein reported to his brother how Ignatius Kögler (1680–1746) and Andrés Pereyra (1689–1743) faced an accusation similar to the one levelled at Stumpf, but successfully defended themselves (see Steska, 1918: 146). To get some peace, Kögler invited the Emperor into the Jesuit College and gave him some Brazilian bottled tobacco, which apparently was very well received (Hallerstein, 1781, 45)!

3 THE PORTUGUESE MISSION, AS DESCRIBED IN HALLERSTEIN'S LETTERS TO HIS BROTHER

On 1 March 1739 Hallerstein arrived in Beijing, and lived there for the last thirty-five years of his life as Court Astronomer and Mathematician. Among his friends were Florian Joseph Bahr and Anton Gogeisl, both of whom were trained astronomical observers. Gogeisl was appointed a mathematician, and Bahr was employed as a musician (Dežman, 1881, 10; Laimbeckhoven, 1740: 424). More than thirty Jesuit priests and some Russian Orthodox clergy were in Beijing at that time, and Verbiest stated that 105 Jesuits worked in China between 1551 and 1681 (Dežman, 1881: 1). We also know that 920 Jesuits went to China between 1580 and 1773 (Standaert, 2008). In 1701 a maximum of 96 Jesuits lived in China, with French Jesuits forming the largest group (ibid.), but they represented only a small percentage of the 22,000 Jesuits worldwide. Between 1731 and 1743, French Jesuits predominated in China, whereas between 1748 and 1767 Chinese Jesuits predominated, with French Jesuits only out-numbering them in 1755 (Koláček, 1999; Standaert, 1991). In the mid-eighteenth century, Chinese Jesuits formed a third of all the missionaries in China (Standaert, 2008). On average, Jesuits stayed in China for 20.5 years (Koláček, 1999; Standaert, 1991). A quarter of the Chinese Jesuits were of noble European origin (Duteil, 1994).

Hallerstein described his new collaborators in letters that he sent to his brother in 1739 and 1740. Among the most interesting Jesuits in Beijing at the time was Franciscus Stadelin (1658–1740) (Dehergne, 1973: 260; Hallerstein, 1781: 54; Hallerstein, 1737: AS 730, Manor Dol, fasc. 194: 844) who studied horology in Switzerland and in large European cities for eighteen years. Between 1689 and 1700 he was the 'director for watches' in Breslau (Wroclaw) and later in Brünn (Brno), Liegnitz (Legnica in Poland) and elsewhere. In 1707 he arrived in Beijing, where the Emperor and his court found Stadelin's instruments very amusing. Some Chinese liked to observe European mechanical watches, forgetting that they were a development of one of their own eighth century inventions. Before his first Chinese Christmas, Hallerstein moved to a residence near the church of Saint Joseph, where he lived with Bahr and several other Jesuits (while still others, including Pereyra, Felix de Rocha, Kögler and Gogeisl stayed in the Portuguese Jesuit College—see Dežman, 1881: 11; Šmitek, 1995: 101, 102).

Hallerstein's scientific supervisor, Kögler, continued Ricci's work in Beijing, producing accurate numerical tables that attracted the attention of the Emperor. As Laimbeckhoven (1740: 430) remarked: "Mathematics was besides astronomy highly praised in all the missions of China and in particular the astronomical calculations." (cf. Hallerstein, 1750: 894). Between 1712 and 1714, Kögler had been Professor of Mathematics at the University of Ingolstadt in Germany, before arriving in China on 30 August 1716.[4]

Andrés Pereyra was Kögler's assistant, and he was the only Jesuit in Beijing who was of English descent. He came from a family of wine-traders that moved to Porto (Oporto) and accepted Portuguese citizenship (Needham and Ling, 1959, 3: 448). Pereyra was a good friend of Kangxi's successor, the Emperor Yongzheng (Yung-cheng), who ended missionary activities in 1724 but allowed Pereyra to remain in his post (Šmitek, 1995: 133).

4 THE FRENCH JESUITS IN BEIJING ACCORDING TO HALLERSTEIN'S REPORT TO HIS BROTHER

According to a letter that Hallerstein sent to his brother on 12 February 1764, in 1739 there were thirteen people in the French Jesuit residence in Beijing, including Antoine Gaubil,[5] two Chinese priests (Šmitek, 1995: 102), and a court painter, Jean-Denis Attiret (1702–1768) (cf. Amiot, 1943: 472; Koláček, 1999: 27). Four years later, in 1743, the French residence housed just six Jesuit priests and four brothers (Hallerstein, 1781: 44).

In his letters, Antoine Gaubil (1748: 316-319) several times highly praised the measurements of his young friend Hallerstein. On 16 March 1730 Gaubil became a foreign member of the Petersburg Academy. He was a botanist, astronomer, and cartographer, and between 1742 and 1748 was the superior of the French residence. Gaubil had studied in Paris with G.D. Cassini and Cassini's nephew, Giacomo Filippo Maraldi, and he was the first to inform the Europeans of the existence of ancient Chinese astronomical records (see Ho, 1970: 261; Laplace, 1982: 280). Almost a century later, Laplace published Gaubil's manuscripts about ancient Chinese observations of the lengths of the Sun's shadow at the equinoxes, precession and other astronomical observations (Needham and Ling, 1959, 3: 173, 761).

One of the most important French Jesuit astronomers was Michel Benoist,[6] who served for thirty years under the Emperor Qianlong. Benoist had studied in Dijon and in Saint Sulpice in Paris, and after three years of repeatedly asking to be sent to the Chinese missions he was finally successful. Before departing, he completed his astronomical studies in Paris under Joseph Nicolas Delisle, The Abbe de Lacaille and Pierre Le Monnier, later exchanging many letters with his former teachers. In 1745 Benoist received the title of the Emperor's Mathematician (Aimé-Martin, 1843, 4: 122; Benoist, 1767). When Benoist arrived in China the missionaries were experiencing troubles in Beijing, but because of his superior knowledge Benoist made himself indispensible at the court. For example, he was hired to build a huge system of fountains in the Emperor's gardens, as Hallerstein reported to his brother in a letter dated 28 November 1749 (Hallerstein, 1781: 28-29; Šmitek, 1995: 113). Benoist worked successfully on that project for many years. He erected European-style houses in the gardens and installed an interesting water clock in front of an Italian-style house. In designing this he made use of local Chinese motifs: the Manchus marked the 24 hours of the day with 12 animals of different kinds, so on two sides of the tri-angular water reservoir Benoist put statues of three different animals. Guided by a mechanical tool, the water flowed every two hours from the mouth of a different animal. On 21 May 1766 Benoist and Attiret visited the court to obtain information about the paintings that would adorn the Emperor's palace (Amiot, 1943: 470). In order to comply with an Imperial request, Benoist invented new methods for paper-wetting and the use of ink. King Louis XV of France asked Benoist to make copies of the sixteen copperplates of the Emperor's battles for him.

In the company of such important scientists, Hallerstein was ready to make a meaningful scientific contribution in Beijing, but as a newcomer he first had to learn the Chinese language and script from his Chinese converts. He also received support from a new Chinese visitor, Giacomo Filippo Simonelli (Laimbeckhoven, 1740: 427; Needham and Ling, 1959, 3: 454; Šmitek, 1995: 109, 136; Steska, 1918: 147). The Emperor was pleased to see how quickly Hallerstein learned the Chinese language (Hallerstein, 1761: 851). On 6 November 1740 Hallerstein sent his computations of solar and lunar eclipses to his brother, and these were read with great care in Brussels. Hallerstein was soon recognized as an excellent organizer, and someone who knew how to choose the right collaborators from the Portuguese college. Initially, his travel companion, Gogeisl, was most helpful to him, but in 1751 Felix de Rocha and Jose d'Espinha arrived in Beijing from the Pyrenean peninsula. Then on 1 November 1754 the talented Jean-Joseph-Maria Amiot[7] arrived in China, and of the French Jesuits he soon became Hallerstein's closest collaborator. Amiot translated a book about Chinese wars and maps, and he improved on Thomas' 1702 measurement of the meridian of Beijing, according to a letter Hallerstein wrote to his brother on 29 October 1761 (see Hallerstein, 1761: 851, 852; Hallerstein, 1781: 37, 38, 42; Montucla, 1799, 1: 478; Šmitek, 1995: 114). In 1760 Amiot published Confucius' biography entitled

Vita Confucii. Although he did not publish theological papers, Hallerstein carefully reported on his own experiences with "… Mohamedians, political circumstances in China, and his work at the court." (Hallerstein, 1761: 852). As a high court official, Hallerstein certainly took every opportunity to obtain news from Europe. According to a letter he sent to his sister, there were twenty-eight European merchant vessels anchored at Canton on 15 October 1753, and Hallerstein (1756: 885-886) made a point of visiting all of the French and English ships.

There were too many problems with the authorities in Beijing during Hallerstein's time, and the Jesuits were not very successful in converting the local Chinese to the Catholic faith (Forgeot, 1747: 918). However, they were much more successful in teaching the Chinese about important discoveries made by European scientists, and especially astronomers. Yet this knowledge did not have much influence on the Chinese social system (see Huff, 1993: 361).

5 HALLERSTEIN'S DESCRIPTIONS OF CHINESE ASTRONOMY IN LETTERS TO HIS SISTER

Hallerstein's Chinese collaborators were listed in the register of the officials of the Emperor's Astronomical Observatory in 1754. The first President of the Observatory was Yun Lu (Yin-Lou), the second Prince Zhuang (Heshouzhuang, Tchoang, Chuang, Yün-lu, 1695–1767), the sixteenth son of the Emperor Kangxi. During the time of his father's reign he was not on good terms with his half brothers. Together with his half brother, the thirteenth son of the Kangxi Emperor named Yin-hsiang, he supported Yongzheng (Yungcheng) when he ascended to the Imperial Throne. Therefore, early in 1723 the new Emperor, Yongzheng, named Yun Lu as the successor of Boggomo, the first Prince Zhuang (Chuang). Boggomo was the grandson of Emperor Taizeng (T'ai-tsung), but he had no descendants. Yun Lu studied music and mathematics, and led the commission for the new edition of the encyclopedia *Lü-li yüan-yüan*, and probably also for the *Gujin tushu zhicheng* (*Ku-chin T'u-shu chi-cheng*) (Chu, 1994: 293; Hummel, 1944: 926).

The second President of the Emperor's Observatory was Ngo Eul-Tai (E Ertai), a Duke of the Third Range, the former Minister and the President of the Military Tribunal. The third President of the Observatory was Zhang Zhao (Tchang-Tchao, 1691–1745). He was a famous calligrapher, and between 1733 and 1742 served as the Vice-President and President of the Tribunal for Punishments. In 1736 he fell into disgrace and was sentenced to death but his especially-fine handwriting eventually saved him and the Emperor pardoned him, because nobody else in China was his equal in calligraphy. Zhang Zhao collaborated in the work of the Emperor's Observatory as the expert for music (Pirazzoli-t'Serstevens, 2007: 104).

Besides three Presidents, the Observatory also had two Vice-Presidents. The first was Kio-Lou-Le-Eul-Chen, the Vice-President of the Court Tribunal and substitute for the Marshal of the flags of the Red Coins among the Manchu. The second Vice-President was He Guozong (Ho Kuo Tsung), a mathematician and the editor of the *Lü-li yüan-yüan*, published for the first time in 1723. In 1739 He Guozong (Ho Kuo-Tsung, Ho Kouo-Tsung) became the head teacher at the Emperor's Academy in Beijing. In 1755 and 1756, he collaborated with Espinha in mapping the land of the Eleuts and Tartars. At the beginning of 1757 Guozong became President of the Ministry (tribunal) for the Rites, and between 1757 and 1759 he taught mathematics in the Palace for Princes (Nan-shu-fang, Shang-shu-fang) (Amiot, 1943: 436, 438; Chu, 1994: 287; Hummel, 1944: 286; Jami, 1994: 241).

The astronomers of the Emperor's Observatory were Kögler (President of the Bureau and the candidate for the Vice-President's title in the Ministry for Rites), his successor President Hallerstein, and Vice-President Gogeisl. In 1755, Rocha joined them, after Hallerstein intervened in his favour when accusations were made against him, as Hallerstein (1750: 894) proudly stated in a letter to his sister.

In addition to the afore-mentioned personnel, nine people had the status of 'experts' for calculations (Tsuchihashi, Chevalier, 1914, II). Foremost among them was Ming'antu (Ming Antu, 1712–1764), who was in charge of the seasons of the year at the Astronomical Bureau. Already in 1721 he worked in the Calendar Department. Between 1756 and 1760 he collaborated with Rocha and Espinha in mapping Xinjang (Xinjiang Uygur, Sinkiang), the province that the Emperor just invaded. Later, between 1759 and 1762, he and Hallerstein served jointly as the second (Manchu-Mongolian) President (Jianzheng) of the Astronomical Bureau. Later, in 1774, he wrote a book about the rapid computation of trigonometric functions and squaring the circle which proved popular and was reprinted as late as 1839. In this book, he used the infinite series for the first time in China (see Jami, 1990: 39, 156).

In 1754 Ming'antu headed the group of three specialists in charge of the seasons of the year: spring, summer, and winter. In addition, they employed five computing experts, each of which used a title equivalent to a European Ph.D. (Boshi, Bo Shi) (Zhang, 2002).

Finally, the Observatory had five students (Tian Wen Sheng). Among them was the famous painter Changgong (Tchang Kong, Chang keng, 1685–1760), who after his studies with Chen Shu published several books about the history of painting (ibid.). Another painter, and three other students, also worked in the Observatory (ibid.).

6 HALLERSTEIN'S ADVICE TO KOREAN SCIENTISTS AS RELATED IN A LETTER TO HIS BROTHER

During Hallerstein's time in Beijing, Europeans were not able to visit Japan or Korea (Park, 2004). Japan was largely isolated between 1616 and 1720, then in 1725 the first modern astronomical observatory was opened under the Directorship of Nakane Genkei (1661–1733) who completely accepted Copernican ideas (Nakayama, 1969: 171).

Koreans learned about European science, technology and religion from the books published in the classical Chinese language, but Western mathematics, astronomy and technology interested them much more than cosmology (Grayson, 2002: 132).

Due to incorrect computations, errors in the Korean calendar were numerous and predictions of astronomical events in ephemerides were no longer accurate. As every type of calendar had its errors, the Koreans changed their calendar several times. They also had to redraw their star maps, as the evening and the morning stars were no longer in their computed places. Han Hungil (1587–1651) obtained a book in Beijing that explained the Western calendar, but even after studying it for a decade the Koreans were still unable to generate a completely accurate calendar (Needham et al., 1998: 178).

Between 370 BC and AD 1742, the Chinese made one hundred calendars or editions of astronomic tables with constants for the measurement of the solstices, the movements of planets, and the length of the day, month and year. By 1637 they had come to accept Western ways for computing the calendar. Later, the new Chinese calendar influenced neighboring Korea (Moon, 2008). As foreign citizens, Koreans were not allowed to examine the computation of the Chinese calendar. As a way around this, the Koreans obtained copies of new ephemerides from the translators at the Bureau of Astronomical Observations, and they were then able to study the methods used for calendar computation (ibid.).

Kim Yuk (1580–1658), one of the highest-ranking administrators in Korea (Choson), supported the use of the new Western technology for the computation of the calendar. In 1645, as the Director of the Bureau of the Astronomy and Meteorology in Korea, he successfully convinced the ruler to accept Western calendar science, and from 1653 Koreans were able to compute the calendar according to the new Western methods (see Needham et al., 1998: 178). However, admiration of the Western calendar did not necessarily imply admiration of all Western culture: Koreans were willing to accept Western technology, but not European philosophy or theology since technical experts were answerable to Confucian teachers (Moon, 2008).

Ricci, however, was successful in convincing some Confucian scientists to accept some European concepts. Accurate geography first reached Korea with the work titled *World Geography* (Zgifang waiji, Chih-fang wai-chi), written by the Italian Jesuit, Giulio Aleni (1582–1649) in 1623 (Needham and Ling, 1959, 3: 584). Along with an accurate description of peoples and cultures of the world he included Renaissance maps. The Korean philosopher Yi Ik (1681–1763) was glad to have the new information, and he prepared the Koreans to accept it. Just like Kim Manjung before him, Yi also introduced Aleni's work as an improvement on old Confucian geographical traditions.

According to Korean ideas, the Earth was in the center of the spherical cosmos. The Earth did not move, but the cosmos made one turn every day. Because the cosmos was huge, a great centripetal force was needed to keep all the stars in their positions (Moon, 2008). Therefore the old Korean concept of cosmology was closer to Ptolemy's ideas than to those promoted by Copernicus.

Koreans were only able to meet Europeans when they visited China. During a diplomatic visit to China in 1631, Chong Tuwon met Joao Rodrigues (1561–1633), a Catholic missionary from the Japanese Jesuit province who presented him with several European books and other gifts. Eventually Chong Tuwon brought these back to Korea, and among them was a telescope—but Tuwon was more interested in its military potential than in its astronomical use. In 1632, before the Manchu invasion, Rodrigues moved to Macao (Needham et al., 1998: 159, 176).

Early in the eighteenth century, the Korean Yi Imyong (Yun Inyong) visited Kögler and Joseph Suárez (1656–1736) in the Beijing Portuguese Mission, and discussed Western astronomy and religion with them.

In 1708 a Korean named Tyentung Sanguiko published an important book which contained descriptions of unusual events seen in the eastern sky (Needham, Ling, 1959, 3: 683), then in 1741 the Korean astronomer An Kuk-pin (Kuk-bin) broadened his astronomical knowledge by visiting the Beijing Jesuit College along with Pereyra. Kuk-pin and Pyon Chunghwa were Korean ambassadors in China, and Kögler gave them ephemerides of the Sun, the Moon and the planets; tables of logarithms; a list of solar and lunar eclipses; several papers about mathematics; and a copy of Kögler's planisphere (Needham et al., 1998: 178-179; Šmitek, 1995: 117).

Hallerstein had a particularly favorable opinion of the clever Koreans, who supposedly asked questions all the time but never answered if they were asked, as he waggishly reported to his brother on 12 October 1757 (Hallerstein, 1781: 36; Juznic, 2007: 9). Hallerstein did not mention any Koreans in particular, but he was probably thinking to Kuk-pin and Pyon Chunghwa.

In 1766 a Korean named Hong Taeyong (1731–1783) visited Hallerstein and Gogeisl in Beijing where they discussed astronomy—which interested the Koreans— and theology—which interested the Jesuits. Hong Taeyong liked to carry out scientific research, but he also served in minor government posts and eventually became a county judge. His uncle was the Korean ambassador to China so Hong Taeyong was in an unusual position in that he was able to visit China. During his 1766 visit he researched the foundations of Korean cosmology and philosophy, presenting his questions as a dialogue between traditional neo-Confucians and a comparatively free-thinking man from the mountain Iwulu in the province of Liao-ning near the Chinese-Korean border (Needham et al., 1998: 113, 171). Neo-Confucianists believed that the Earth was not only spherical, but that it also rotated each day around its polar axis. This supposition was not completely Copernican in that it did not move the Earth from the center of the Universe. Hong Tae-yong did not propose any scientific reasons for the rotation of the Earth—he simply put forward the philosophical supposition without introducing any observational evidence, thereby showed the new way of Korean thinking as a result of European influence.

Between 1759 and 1761 Taeyong erected a private observatory in Korea, where he used gravity to drive his *sphaera armillaris* and clock. His observatory was eventually repaired after Hallerstein's death in 1777 (Needham et al., 1998: 98, 113-114, 168; Qi, 2007).

7 CONCLUDING REMARKS

Ferdinand Augustin Haller von Hallerstein was the last of the important European astronomers sent by the Society of Jesus to serve in their Portuguese mission in

Beijing (Juznic, 2008), and he surpassed all his predecessors with his numerous and well-received publiccations. Hallerstein (1750: 894) proudly told his sister that the Chinese Emperor urgently needed him! After he had a stroke on 29 July 1774 Hallerstein offered his resignation, but the Emperor gracefully ordered that he should continue his work as much as possible (as Hallerstein's brother reported to his cousin; see Hallerstein, 1775: 574-575). Hallerstein's death on 29 October 1774 ended almost two centuries of Jesuit astronomy in Beijing.

Even now, more than two hundred years after his death, Hallerstein's success is well remembered in his native Slovenia where the author of this paper helped arrange the issue of an official commemorative stamp on 23 January 2003 (see Figure 1 on page 219).

8 NOTES

1. Von Bell was also known by his Chinese names, Tang Ruowang, T'ang Jo-wang and Tao-Wei. He was born in Colonge on 1 May 1592, admitted to the priesthood in Rome on 21 October 1611 and died in Beijing on 15 August 1666 (Dehergne, 1973: 241).
2. In a letter to Janez Vajkard dated 6 October 1743, Hallerstein gave this Emperor's name as 'Chuntsci'.
3. Verbiest was also known by his Chinese names, Nang-hoai-gin, Nan Huai-Jen and Nan Houai-Jen Touen-Pei. He was born in Pitthem near Bruges in Belgian Flanders on 9 October 1623, admitted to the priesthood in Malines (Mechelen), Belgium, on 29 September 1641, and died in Beijing on 28 January 1688 Beijing (Dehergne, 1973: 288-289).
4. Kögler's student, Nicasius Grammaticus (1684–1736), although just four years his junior, replaced him in Ingolstadt. Grammaticus studied in both Ingolstadt and Freiburg, and subsequently taught grammar and poetry high school classes at Trient College and theology at the Lyceum of Amberg. In 1720 he became Professor of Hebrew and Mathematics at the University of Ingolstadt, and taught his own version of Newton's and Copernicus' ideas. King Philip V then invited him to the new seminary for nobles in Madrid, where he taught mathematics. After three years there, Grammaticus returned to Ingolstadt. From 1730 to 1732 he was Professor of Moral Theology at the Lyceum in Amberg, and he then went to Regensburg. Kögler translated Grammaticus' tables of the eclipses of the Moon into Chinese (Dehergne, 1973: 136-137).
5. Gaubil was also known as Gobil, Gaubille, Song Kiun-Yong K'i-Ying, Song Junrong Qi Ying and Sun Kiun-yung. He was born in Langedoc on 14 July 1689, admitted to the priesthood in Toulouse on 13 September 1704, and died in Beijing on 24 July 1759 (Dehergne, 1973: 106).
6. Benoist was also known as Benoît and Tsiang Yeou-Jen Tö-Yi. He was born in Dijon on 8 October 1715, admitted to the priesthood in Nancy on 19 March 1737, arrived in Beijing on 12 July 1744, and died there on 23 October 1774 (Dehergne, 1973: 30).
7. Amiot was also known by his Chinese name, Ts'ien té-ming jo-ché. He was born in Toulon on 8 February 1718, admitted to the priesthood in Lyon on 27 September 1737, and died in Beijing on 8 or 9 October 1793 (Dehergne, 1973: 12).

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Dr Stanislav Juznic was born in San Francisco, but was educated mainly in Europe. He is Assistant Director of a Ljubljana enterprise for scientific visualization in Slovenia. His main interests cover eighteenth century European astronomy and other mathematical sciences, especially in relation to the Jesuits. His recent books include *Hallerstein, kitajski astronom iz Mengsa* (2003) and *Jesuit General Gabrijel Gruber* (2006), and he currently has a book on the history of Slovenian astronomy in press. At the Third European History of Science Conference which was held in Vienna on 8-10 September 2008, he was head of the Hallerstein panel.

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THE PHANTOM MOON OF VENUS, 1645-1768

Kurt Møller Pedersen and Helge Kragh
Department of Science Studies, University of Aarhus, 8000 Aarhus, Denmark
E-mails: kmp@ivs.au.dk; Helge.Kragh@ivs.au.dk

Abstract: With the invention of the telescope around 1600 astronomers saw a new world in the sky. They saw mountains on the Moon, moons around Jupiter and Saturn, and a few astronomers believed they saw a moon orbiting Venus. That moon became a problem for astronomers because they only saw it occasionally, separated by many years. The moon was reportedly seen in Italy, France, England, Germany and Denmark between 1645 and 1768. Thereafter it disappeared from the sky. The most obvious explanation was, of course, that the moon never existed. In this paper we detail the observations and how they were assessed. The last reports about this phantom moon of Venus came from the observatory in Copenhagen between 1761 and 1768. In this paper we focus especially on these observations. Observations elsewhere are treated in Kragh (2008). We shall argue that the alleged Venus moon detections were not constructions in the brain, influenced by astronomers' expectations that Venus, like the Earth, Jupiter and Saturn, ought to have a companion. Most astronomers who thought they saw the moon had no preconceived ideas about a Venusian moon. We shall show that from the late 1760s it became generally accepted that the so-called 'moon of Venus' was a ghost image in the telescope, a reflection of Venus in the lens' surfaces.

Keywords: moon of Venus, Lalande, Horrebow, Copenhagen Observatory.

"I have never before seen a spectacle in the heavens which has captivated me more; I thought that I truly saw the satellite of Venus." (Christian Horrebow, Copenhagen, 1775).

1 INTRODUCTION

On 6 June 1761 astronomers observed the transit of Venus across the Sun's disk. Such transits are rather rare phenomena, having only been seen once before, in December 1639, by Jeremiah Horrocks (1618–1641) and William Crabtree (1610–1644). These transits could only be seen through a telescope, and in 1761 not only were the lenses of a much better quality than in 1639, but also micrometers and clocks were much more accurate. Astronomers therefore hoped to measure very precisely the time of the transit of Venus, thus enabling them to calculate the solar parallax, and hence the distance from the Earth to the Sun. The history of the transits of Venus has been told many times and it is not the story of this paper. Instead we offer another story: the attempts to discover a moon around Venus. Since the beginning of the seventeenth century a handful of astronomers had claimed to have observed such a moon. Now, during the 1761 transit of Venus a new opportunity emerged for astronomers to see that moon accompanying Venus across the solar disk.

2 ALLEGED DETECTIONS OF VENUS' MOON PRIOR TO 1761

The first to report having seen a moon of Venus was the Neapolitan astronomer Francesco Fontana (ca. 1585–1656), the most renowned Italian telescope-maker of his time. On 11 November 1645 he saw two small dots or globes that followed Venus, but on Christmas Day 1645 he saw only one at the top of the convex side of Venus and on 22 January 1646 he saw it again now facing the concave edge of Venus (Fontana, 1646, and 2001). Most astronomers at the time had little faith in Fontana's observations. Evangelista Torricelli (1608–1647) called them "… stupidities observed, or rather dreamed up, by Fontana in the heavens." (Fontana, 2001: iii). Giambattista Riccioli (1598–1671), Francesco Maria Grimaldi (1618–1663), and Pierre Gassendi (1592–1655) never acknowledged Fontana's claim to have discovered a Venus moon, simply because as Gassendi (1997: 106) put it: "We have not been able to this day to seize anything about this with our telescope although it was a Galilean one." A more favourable opinion, however, was forwarded by Andreas Tacquet (1612–1660), a Flemish mathematician, who suggested that the failures of Riccioli, Grimaldi and Gassendi to confirm Fontana's observations might be due to their telescopes being of an inferior quality than the one used by Fontana (Tacquet, 1669: 310).

Much more credibility was given to the alleged detections of a Venusian moon by Jean-Dominique Cassini (1625–1712). In 1669 he was called to Paris to become Director of the new observatory. This position, his membership of the French Academy of Sciences, and the fact that his Paris Observatory telescope was one of the best in Europe led to a personal prestige that implied that his observations and discoveries should be taken very seriously. That, indeed, was the case when in October 1671 Cassini discovered a satellite around Saturn, a discovery that was accepted immediately by astronomers.

In 1672 and 1686 he claimed to have observed Venus' moon, in both cases as a faint object showing phases similar to those of Venus. Cassini, however, was not absolutely sure that he had seen a real moon and was rather vague in his written statement:

> But in spite of some research I have done from time to time after these two observations, in order to complete a discovery of such great importance, I have never succeeded in seeing it except these two times; and this is why I suspend my judgement. (Cassini, 1730: 245).

In the following almost sixty years astronomers argued for or against the existence of a Venus moon. Thus David Gregory (1659–1708) wrote in 1702 approvingly about Cassini's discoveries telling us that they gave "… more than a bare Suspicion to incline us to believe that Venus has a Satellite." (Gregory 1702: 472 and 1736: 834-35). Neither Cassini nor Gregory mentioned Fontana. Francesco Bianchini (1662–1729) argued in 1726 (Bianchini, 1996: 158-159) that the observations of Fontana and Cassini were due to a certain thickening of "… the heavenly fluid substance …" which according to the Cartesian vortex theory occupied the space between the observer and the planet. That was also the opinion of Cassini's son Jacques Cassini (1677–1756) when in 1732 he claimed that the moon was nothing but a temporary condensation of the celestial fluid matter.

A new observation of Venus' moon was announced by James Short, an expert manufacturer of reflectors and other optical instruments and since 1737 a member of the Royal Society. In a paper titled "An observation on the planet Venus (with regard to her having a satellite)" he reported having observed on 3 November 1740 with an instrument magnifying 240 times a "Star put on the same Phasis with Venus. I tried another magnifying Power of 140 times, and even then found the Star under the same Phasis." (Short, 1744: 646). During the following mornings he continued to look for it, "… but never had the good fortune to see it again." Neither in the main text of his paper nor later in any other papers did he refer to the phenomenon as a Venusian satellite. Only in the actual title of his paper is a satellite mentioned.

Did Short at the time of his observation believe that he saw a moon? We do not know. However, when in 1763 the French astronomer Joseph-Jérôme Lefrançais de Lalande (1732–1807) paid him a visit in London, Short admitted that now he no longer believed that he had seen a satellite (Lalande, 1792, 3: 210).

Lalande's fellow academician and for a time Secretary of the French Academy of Science, Jean Jacques d'Ortous de Mairan (1678–1771), was in favour of the existence of a moon of Venus, primarily because it was given, as he believed, observational support by Cassini and Short (Mairan, 1744). He rejected other more anthropomorphic arguments for the existence of a Venusian moon, that the outer planets must have moons to enlighten their inhabitants, whereas planets closer to the Sun were in that respect not in need of moons. That argument was contradicted by the fact that Mars did not have a moon, whereas Venus being closer to the Sun than Mars did in fact, as he believed, have one.

On 20 May 1759 the German Professor of Mathematics, Physics and Astronomy, Andreas Mayer, wrote in his observation diary: "In the evening about $8^h\ 45'\ 50''$ I saw above Venus a little globe of far inferior brightness, about 1½ diameter of Venus from herself. Future observations will show whether this little globe was an optical appearance or the satellite of Venus." (Lambert, 1776: 186). Mayer's observation was not known to his fellow astronomers until it was mentioned by him in 1762 in a report on the transit of Venus (see Mayer, 1762: 16-17).

3 VENUS' MOON AND THE TRANSIT OF 1761

In 1716 Edmund Halley (1656–1743) published an *Admonition* to astronomers all over the world to follow Venus when it passed over the Sun, as he then believed, on 26 May 1761. He was wrong on the date, but right in recommending that astronomers should measure the time of the transit because that would enable them to calculate the Sun's parallax (provided that observations were taken at many different places on Earth). In good time before the event on 6 June 1761 international astronomical activities were launched to measure the time of the transit (see Woolf, 1959). More than 100 astronomers at many different places throughout the world were engaged. Jean Le Rond d'Alembert (1717–1783), who together with Diderot edited the French *Encyclopédie*, wrote in his article on Venus: "The following year, 1761 [this was written in July 1760], she [Venus] will pass across the Sun's disk, and M. Halley has shown that by means of this observation we will have the Sun's parallax." (Diderot and d'Alembert, 1765: 34, and 1781: 245).

Many astronomers who observed the transit also tried to see if there was a little moon leading or following Venus across the Sun's disk. Before we detail that story let us summarize in Table 1 what had reportedly been seen prior to 1761. Altogether we have eight observations, but many of them were questioned even by their observers as genuine testimonies of the existence of a moon of Venus. Cassini, Short and Mayer all were not too sure that they had observed a moon.

Table 1: Alleged detections of Venus' moon prior to 1761.

Year	Published	Observer	Place	No of Detections
1645	1646	Fontana	Naples	3
1646	1646	Fontana	Naples	1
1672	1730	Cassini	Paris	1
1686	1730	Cassini	Paris	1
1740	1741	Short	London	1
1759	1762	Mayer	Greifswald	1

Astronomers realized that the 1761 transit provided a unique opportunity to either confirm or refute the existence of a Venusian moon. James Ferguson, a Scottish astronomy writer and designer of astronomical instruments, wrote in his widely-read popular book on astronomy from 1756:

> But if she [Venus] has a Moon, it may certainly be seen with her upon the Sun, in the year 1761, unless its Orbit be considerably inclined to the Ecliptic; for it should be in conjunction or opposition at the time, we can hardly imagine that it moves so slow as to be hid by Venus all the six hours that she will appear on the Sun's Disc. (Ferguson, 1778: 18).

Altogether there were 19 reported observations of a Venusian moon in 1761 (more than twice as many as in the preceding years), but it is, however, quite strange that only two of these took place on 6 June during the transit. Abraham Scheuten, an amateur astronomer who was largely unknown to the astronomical community, reported in letters to Johann Lambert in 1776 that on 6 June 1761 he saw "Venus and its small moon in the middle of the solar disc." (Lambert, 1776: 186-188). However, since he was totally convinced that he had seen a moon of Venus, it is strange that he did not report his discovery until fifteen years later. An anonymous Englishman also saw the moon, as he told a London journal in a letter dated 6 June 1761. While occupied with the transit, he saw "… a phenomenon which seemed to describe on the Sun's disk a path different from the spots that is seen now and then." (Diderot and d'Alembert, 1781: 259). It is quite remarkable that the phenomenon was seen on the solar disk, but the details of the observations were so mediocre that no astronomer paid much attention to them.

Nobody else observed a moon following Venus on its transit across the Sun's disk, so why is it that astronomers saw a moon in 1761 but not on the day of the transit? One possible answer is that prior to the transit astronomers began preparing their observatories for the event and setting up their telescopes and equipment to observe Venus. Then after the transit, the equipment was there, so they continued observing Venus.

Louis Lagrange (1711–1783), a French-Italian astronomer, made three observations of a Venusian moon between 10 and 12 February 1761 at the well-equipped observatory in Marseille. However, he soon abandoned the idea that he had seen a moon because it followed a path perpendicular to the ecliptic, which to him seemed so strange that, according to Lalande, he did not find it "… difficult to abandon all the consequences which he had drawn from these observations." (Diderot and d'Alembert, 1781: 259).

Jacques Montaigne (b. 1716) was asked to look for the moon during the transit of Venus, having already seen it (or so he believed) four times between 3 and 11 May 1761, while observing from Limoges in central France. His findings were read to the French Academy of Sciences in May 1761 by Armand Henri Baudouin de Guémadeuc (1734–1817) who later that year published two memoirs on the subject, which included Montaigne's observations (Baudouin, 1761a; 1761b; 1761c). We do not know if Montaigne observed the transit, but Baudouin did so in Paris without seeing the moon.

Although he did not see the moon himself, Baudouin (1761c: 31) was very impressed by Montaigne's observations and was in no doubt: "It is certain that Venus has a moon, and we hope unceasingly to see it." From Montaigne's four observations he proceeded to calculate its period of rotation, its distance from Venus, the mass of Venus and its density relative to the Earth. The observations also showed, as did those of Lagrange, that the moon's orbit was nearly perpendicular to the ecliptic. The astronomers in Paris were quite impressed, as can be seen from an official report by two members of the Academy of Sciences, Nicolas-Louis de Lacaille (1713–1762) and Lalande:

> We have examined, by order of the Academy, the remarks of M. Baudouin on a new observation of the satellite of Venus, made at Limoges the 11th of May by M. Montaigne. This fourth observation, of great importance for the theory of the satellite, has shown that its revolution must be longer than appeared by the first three observations. M. Baudouin believes it may be fixed at 12 days; as to its distance, it appears to him to be 50 semi diameters of Venus; whence he infers that the mass of Venus is equal to that of the Earth. This mass of Venus is a very essential element in astronomy, as it enters into many computations, and produces different phenomena. But although M. Baudouin holds back in order to report many more observations about what is mentioned above, we consider this second memoir as an essential continuation of the first, and we believe it worthy of being printed. (Baudouin, 1761b: 15-16).

This indicates that the two distinguished astronomers took the observations and calculations seriously. This is not to say, however, that they took it for granted that now there was a proof of the moon's reality.

From what we have seen until now, no professional astronomer saw a companion of Venus during the transit although we know that many astronomers looked for it. A few astronomers choose to report their negative results. Thus Lacaille (1763: 78), the chief organiser of the French transit project, wrote: "We did not see the appearance of the satellite on the Sun." But most astronomers, having seen nothing did not feel it necessary to report this.

Figure 1: The Round Tower in Copenhagen.

4 ALLEGED DETECTIONS IN COPENHAGEN IN IN 1761

The Round Tower, Copenhagen's Observatory (Figure 1), was built by King Christian IV in 1637-1642 to replace Tycho Brahe's observatory *Stellæburgum* on the island of Hven.[1] In 1761 its Director was Peder Nielsen Horrebow (1679–1764), but from 1753 the *de facto* Director was his son, Christian Horrebow (1718–1776) who was assisted by his brother Peder Horrebow (1728–1812), Peder Roedkiær (d. 1767) and a dozen assistants. Christian Horrebow participated in the international endeavour to measure the contact times during the transit, and for months before the event he and his staff worked hard to ready the instruments and to hone their observational procedures.

Horrebow was very much taken by the idea of observing the Venusian moon in transit on the Sun's disk. Some years later Christian Horrebow (1764) gave a detailed history of the observations that purportedly identified a moon of Venus. He mentioned Cassini's observations (but not Fontana's), how Bianchini had looked in vain for it, Christian Wolff's (1679–1754) belief that the moon should not be included as an element of our Solar System, and that Gregory gave its existence a more lenient verdict, saying that some physical causes were responsible for the fact that only a few observers had seen it. Horrebow also mentioned how the whole issue was reconsidered in 1740 when Short reported seeing the moon, and how Mairan thoroughly examined Short's observations and came up with the verdict that nothing was yet settled: it was not possible, using Short's data, to tell with certainty whether Venus had a moon or not (Horrebow, 1765b: 396-397).

We have already pointed out that astronomers in France and England believed that the transit would settle the question of the existence of a Venusian moon, and this was also Horrebow's opinion:

> When in 1761 Venus made a transit through the Sun, all astronomers prepared themselves to observe this important phenomenon, and they believed not without reason as they were reminded by M. Delisle in his memoir that on this occasion the question about Venus' satellite could be settled, because when Venus transits through the Sun, its satellite, which they believed was close to it, should also make a transit and thus be seen by many observers. (Horrebow, 1765b: 397).

On 4 June 1761 Horrebow published a memoir on the transit of Venus in which he detailed the history of the transit and the benefits and use of measuring the contact times of the transit. Observations made in Copenhagen when combined with those made at other places on the Earth would enable astronomers to determine the distance to the Sun. He also briefly pointed out that astronomers might be able to see the satellite accompanying Venus in its transit across the Sun's disk. Such a moon, Horrebow (1761a) wrote, had already been seen by Cassini and Short. The transit that would take place only two days later was therefore a very important event, and Horrebow and his staff were well prepared.

On 6 June only the professional astronomers of the Observatory and two trustworthy dignitaries were allowed into the Observatory. The observations had been rehearsed for a long time "… so that any single person is not being confused by the others when the real observations take place." (Horrebow, 1765a: 377). The clocks were checked and the instruments were readied, but because of clouds it was not possible to follow the entire transit, and only the times of the ingress and egress contacts were recorded. While it may have been quite disappointing to miss seeing Venus transit the Sun's disk, from a scientific perspective these transit times were all-important and they were duly passed on to Lalande in Paris (who was collecting data from all parts of the world).

The Copenhagen astronomers did not see Venus' moon during the transit, but since the instruments were operational Roedkiær continued to look at Venus during the summer of 1761, and he reported seeing its moon on several occasions and recorded his findings in the observation diaries (that are now kept in the archives of the Department of Science Studies at the University of Aarhus). Initially he did not publish his observations—for reasons to be specified later—and they only became known many years later, in 1882, when excerpts from the diaries were published by the Danish astronomer Hans Schjellerup.

The entry in the observation diary for 28 June 1761 reveals that Roedkiær saw the moon that day:

> While observing Venus with the quadrant, Roedkiær saw some whiteness which followed Venus. He found the distance between it and the upper rim of Venus to be 0.66, and he observed a transit of 11″ between it and Venus. After that he saw it again by means of a telescope of 17′, and because its appearance was sickle-shaped, not as pronounced as that of Venus but shining with almost half its face, the observer surmised that he had seen the satellite of Venus. The others of us could not see this whiteness even though we observed Venus often, with the quadrant, the meridian circle and the 17 feet telescope. (Observation diary 1761, reproduced in Schjellerup, 1882: 165).

Roedkiær saw the moon again on 29 and 30 June, and on 19 July he saw it at a distance of almost 40 semidiameters from Venus, and he could see it even if Venus herself were not in the field of view. On 5 August Roedkiær and Boserup determined very accurately the distance between Venus and its moon. Roedkiær saw it again on 8, 12 and 13 August (Schjellerup, 1882, and the observation ledger at the archives of the Department of Science Studies). In the middle of all these activities, on 28 and 29 July, Horrebow (1761b) published two dissertations detailing the time measurements made during the transit. In the main text he did not mention anything about the moon or Roedkiær's new observations, but in the Introduction he briefly stated that

> We do not dare deny that Venus has a satellite. This real satellite, very different in nature from the other satellites in our known planetary system, is probably truly seen. (ibid.).

Roedkiær's new observations were not mentioned at all, and from this we suggest that Horrebow did not consider them to constitute proof of the existence of a Venusian moon. We think it is fair to conclude that Horrebow had looked forward with great expectation to seeing a moon on 6 June, and when he saw nothing this convinced him that more solid observations were needed to turn the small and faint spot seen by Roedkiær into a real moon.

When Schjellerup published the observations in 1882 he pointed out that it was a puzzle why Horrebow chose to ignore Roedkiær's observations. We believe that they were not published because Horrebow did not want to do so. He knew all too well that the international astronomical community would come up with a very harsh verdict if what Roedkiær saw were optical illusions. He felt that he had to be very careful, now that the moon had failed to reveal itself on the disk of the Sun.

5 ALLEGED DETECTIONS IN COPENHAGEN IN 1764 AND 1768

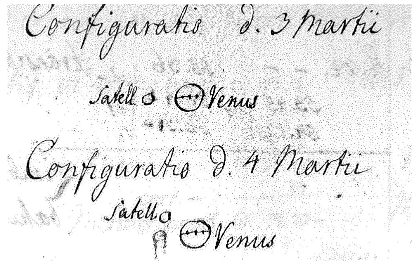
Things changed dramatically in 1764 when Roedkiær saw the moon again, as he wrote in a report to the Royal Danish Academy:

> On 3 March [1764] in the evening when investigating a new double sided convex objective glass of an extraordinary quality I saw at Venus a star which had a weak light and a recognizable diameter. Its shape was perfectly like that of Venus … Being therefore very much inclined to believe that it might be the so long searched for satellite I decided the best I could without a micrometer to determine its position relative to Venus. (Roedkiær, 1765: 394).

The focal length of his telescope was 9½ feet and he saw the moon on the left-hand side of Venus at a distance of three-quarters of its diameter.

The next day Roedkiær wrote in the observation diary:

> 1764, March 4. This evening at the same time at 6 Roedkiær again saw the satellite of Venus. Its distance to the left of Venus was ½ of Venus' diameter. Its centre made with Venus' centre an angle of about half a right angle: it appeared higher than Venus' centre in the telescope. He could also very well distinguish its phase which conformed to Venus' phase. He used partly the same glass objective as yesterday, and partly a meniscus objective of 14 feet with an ocular of 3 inches. The configurations of 3 and 4 March were:



Configuratio d. 3 Martii

Satell. Venus

Configuratio d. 4 Martii

Satell

Venus

> N.B. This is how the satellite and Venus appeared in the telescope. That it was a satellite was clear primarily because both the diameters of Venus and the satellite were enlarged noticeably (by the telescope of 14 feet as compared with the telescope of 9 ½ feet), which applied to none of the fixed stars. (Schjellerup, 1882: 166-167).

For Roedkiær, an important argument for the existence of the moon was that the spot he saw had a recognizable diameter. He first observed with the 9½ feet telescope that enlarged 38 times, and thereafter with a 24 feet telescope that enlarged 56 times—and he then saw that the object was enlarged. At 6.30 pm it could no longer be seen, although he still saw two stars close to Venus.

In the observation diary Roedkiær clearly marked the position of the moon on 3 and 4 March 1764. He concluded that he had seen a Venusian moon for the following reasons:

1. Its light was faint and weak.
2. Its shape was like that of Venus.
3. It was enlarged when viewed through the larger telescope.
4. It disappeared while other stars were still visible.
5. Both Venus and the object were seen distinctly and clearly.

Furthermore, Roedkiær saw his observations as proof of the truth of Montaigne's earlier observations, and he compiled a report on the event hoping that his Director would communicate this to the Royal Danish Academy. At the end of this report he wrote:

> I hope that my humble account shall not be unpleasant to this esteemed Academy; if I achieve this, it will be my greatest award and the most powerful encouragement to apply with more assiduity my poor strength on new and useful discoveries. (Roedkiær, 1765: 395).

Christian Horrebow in fact found Roedkiær's arguments so convincing that he decided to read his paper to the Academy, and he did so on 9 March. By a remarkable coincidence, on the very night that Horrebow was making his presentation at the Academy, Roedkiær, Peder Horrebow and an assistant, M. Boserup, again saw the moon, this time, however, not as distinctly as previously, and it was also smaller. The moon was on the right-hand side of the planet, at a distance of 1.25 or 1.5 Venus diameters. They used the 9½ and 6½ feet telescopes and also a quadrant with a 3 feet telescope. They were all so excited that they initially decided to bring a telescope to the Royal Academy so that the assembled members could witness the phenomenon, but changed their minds after remembering their former experiences when the moon disappeared as Venus approached the horizon (Horrebow, 1765c: 400).

On 10 March between 6 and 7 p.m. Roedkiær, Peder Horrebow and Boserup saw the moon again with the 9½ feet telescope which was now provided with a micrometer, but the light from the moon was so weak that they could not use it. They also used telescopes of 6½ and 18 feet but with these they saw no moon. On 11 March they continued their search with all four telescopes, but their observations from the previous night did not endow them with too much optimism. To their great surprise, however, they saw with the 9½ feet telescope a faint light on the right-hand side of Venus. This was the first time that Christian Horrebow actually saw the moon:

> I have never before seen a spectacle in the heavens which has captivated me more; I thought that I truly saw the satellite of Venus and felt happy in my heart that I now saw that the Lord had provided the inhabitants of Venus with a satellite, just as ours. I sought to establish in many ways whether this weakly shining body might be a deceptive reflection in the telescope, but … [reached the conclusion] that the light must really be the Venus satellite … To describe this observation more closely I know of no better way than to refer to precisely the expressions that Mr. Cassini uses when he describes his observations of 25 January 1672 and 28 August 1686. All of these fit closely with the ones here observed, and thus our observation might be considered a perfect repetition of the ones reported by Cassini. (Horrebow, 1765c: 401-402).

Christian Horrebow was an experienced astronomer and knew very well that he could have been deceived by reflections in the lenses or other optical illusions. In his paper he argued that this was not the case when he saw the moon:

> To be more certain, on the same evening when I saw Venus' satellite I turned the telescope towards Jupiter and Saturn, and I saw them both very distinctly and precisely. … without any indication at all of a false light in the telescope. What is more, during the observations I turned the telescope in a variety of ways, and yet the position of the satellite relative to Venus always remained fixed. In addition, a couple of times I let Venus pass through the tube, from beginning to end, and the satellite followed its primary planet all the time, just as it should; had it been a reflection, it would sometimes have disappeared. In the case where I arranged the telescope in such a way that Venus was just outside it, I could still see the weak light of the lone satellite. (Horrebow, 1765c: 403).

One last objection to having found a moon was that all they saw was a star. Against this Horrebow assembled four arguments:

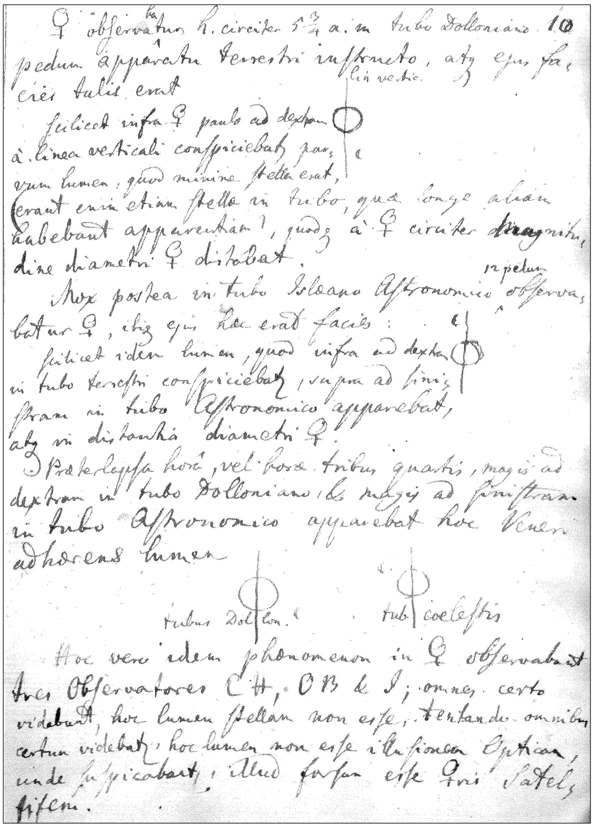
1. There was a noticeable difference between the light and distinctiveness of fixed stars and the observed object.
2. The satellite had described a half circle around Venus, and that would not be possible for a fixed star.
3. He often saw the satellite and fixed stars at the same time in the telescope and could assure himself that there was a marked difference in their appear-

ances.
4. No fixed star was at the time of observation in conjunction to Venus, and therefore close enough to be mistaken for a satellite.

Horrebow ended his paper by urging astronomers to free themselves from the "… fear and modesty …" that would prevent them from presenting corroborative data.

Nearly four years later, on the evening of 4 January 1768, Horrebow saw the moon one last time (Figure 2), now in company with his assistants Ole Nicolai Bützov and Ejolvor Johnsen (Roedkiær having died the previous year). Using a Dollond telescope, the three astronomers observed below Venus

> … a small light, certainly not a star (for there were stars in the telescope, which had a fully different appearance), and it stood at a distance from Venus of about one Venus diameter. Soon afterwards Venus was observed in the Islaean telescope [a telescope named after the French astronomer Joseph Nicolas Delisle] of 12 feet.



♀ observatur h. circiter 5¾ a. in tubo Dolloniano 10
pedum apparatu terrestri instructo, atq ejus fa-
cies talis erat
lin. vertic.
scilicet infra ♀ paulo ad dextram
à linea verticali conspiciebatur par-
vum lumen, quod minime stella erat,
(erant enim etiam stellæ in tubo, quæ longe aliam
habebant apparentiam), quodq à ♀ circiter magnitu-
dine diametri ♀ distabat.
Mox postea in tubo Isleano Astronomico 12 pedum observa-
batur ♀, atq ejus hæc erat facies:
scilicet idem lumen, quod infra ad dextram
in tubo terrestri conspiciebatur, supra ad sini-
stram in tubo Astronomico apparebat,
atq in distantia diametri ♀.
Præterlapsa horâ, vel hora tribus quartis, magis ad
dextram in tubo Dolloniano, et magis ad sinistram
in tubo Astronomico apparebat hoc Veneri
adhærens lumen

Hoc vero idem phænomenon in ♀ observabant
tres Observatores C.H., O.B. et J.; omnes certo
videbant, hoc lumen stellam non esse, tentando omnibus
certum videbatur, hoc lumen non esse illusionem Opticam,
unde suspicabantur, illud forsan esse ♀ris satel-
litem.

Figure 2: Drawings in the observation diary in 4 January 1768, where the moon of Venus is clearly depicted.

Christian Horrebow believed that the new observation confirmed the hypothesis of a Venusian satellite:

> After an hour or three quarters of an hour that light which adhered to Venus appeared more to the right in the Dollian and more to the left in the astronomical telescope. Three observers observed this same phenomenon at Venus, C.H., O.B., and J.; all saw with certainty that this light was not a star, and were certain that the light was not an optical illusion, and they therefore surmised that perhaps it was a satellite of Venus. (Schjellerup, 1882: 167-168).

This observation was never published. On 18 February 1768 and again on 6 April 1770 Horrebow read papers to the Royal Danish Academy on the transit of Venus, but they were oral presentations and we do not know if he mentioned the local observations of the satellite of Venus. In spite of what he enthusiastically wrote in the observation diary, had he only a few days later already abandoned his belief in the existence of the moon? Whatever the case, this 4 January 1768 observation is the last one recorded, and after this the Venusian satellite disappeared from the astronomical sky—if not from the astronomical literature.

6 EVALUATION TWENTY YEARS LATER

Lalande acted as the coordinator of all the observations of the 1761 transit of Venus, and Horrebow duly sent observations to him. Although Lalande included the Copenhagen observations in the 1761 *Mémoires* of the French Academy, apart from this he decided to ignore them, for the following reasons:

1. Bad weather prevented astronomers in Copenhagen from following the entire transit.
2. Only simple non-achromatic telescopes were used.
3. The observations were not properly reduced because Horrebow forgot to include in his letter to Lalande the corrections that should be applied to the times indicated by his clocks.

According to Pedersen (1992, 103) this left the Paris astronomers with the impression that the Copenhagen astronomers were incompetent and unable to handle even simple observational programmes requiring only a clock and a telescope.

When in 1781 Lalande summarized the observational evidence for a moon of Venus he did not include the observations that were made in Copenhagen in 1761, 1764 and 1768. The 1764 observations were the only ones published (but in Danish), so unless he had been notified by letter Lalande would have been unaware of them—and of the 1761 and 1768 results.

Lalande's report was included in the second edition of the widely-distributed *Encyclopédie*, and can be seen as a general statement of the state of affairs concerning the Venusian moon (Diderot and d'Alembert, 1781: 256-260). He reported the observations of Cassini, and he was very impressed with Short's observation:

> This observation, being one of those that best establishes the existence of the satellite of Venus by the impossibility of supposing that the observer was deceived by optical illusions, deserves particular attention … [but] still it seems that one ought to be uncertain about the existence of this satellite.

Lalande also considered the contributions of Montaigne and Baudouin and found two supportive arguments for the existence of the moon, namely that both astronomers saw the moon whether Venus was in the field of the telescope or not, and that they were able to deduce elements of the orbit of the satellite. Lalande continued his assessment, writing that "In spite of so many testimonies which establish the existence of the satellite of Venus it seems that we are still in a situation to doubt its reality."

Lalande did not totally reject the possibility of a satellite of Venus, but wrote that there were reasons to believe that what astronomers saw were optical illusions. This was also the opinion of the new Director of the Round Tower, Thomas Bugge, who succeeded

Christian Horrebow in 1776. In a 1783 report to the Royal Danish Academy on Herschel's discovery of Uranus he also mentioned the satellite of Venus:

> There has been much discussion about the satellite of Venus, Cassini, Short, Montaigne, Horrebow and Montbarron believe to have seen it; but it is strange that it was never seen at other times, when the sky was just as clear, using the same instruments and by the same persons. This inclines us to believe, that it was due to an optical illusion in the telescope, and the esteemed Vienna astronomer, Mr. Hell, has shown that in any telescope and at any planet, when the eye is in a certain position, there appears close to the planet a dioptrical ghost or a small imitation of the main planet. (Bugge, 1783: 219).

In this quote Bugge mentions Montbarron, a councillor in Auxerre, south of Paris, who observed the moon on 15 March 1764 with a 32-foot Gregorian telescope. This observation, and observations made by other amateur astronomers, were reported by Lalande in his 1781 account published in the *Encyclopédie*. The last reported observation of a Venusian moon was the one made in Copenhagen in 1768.

The search for the moon during the transit of Venus in 1769 was not on the agenda of astronomical activities. Of course astronomers looked for it, but it was not officially put forward as a scientific project. That was due to the fact that astronomers no longer believed that such a moon existed. The death blow was not that the moon failed to reveal itself on the Sun's disk in 1761, but rather that the image of the moon was due to an optical illusion. As mentioned by Bugge, this idea came from Maximilian Hell who in 1766 published a dissertation in which he concluded that the image of the strongly-luminous Venus was reflected both in the lenses of the telescope and in the eye's cornea, and that this gave the impression of a satellite with the same phase as that of Venus (see Figure 3). Careful experiments carried out from 1757 showed that this image only occurred if the eye was held in a certain position relative to the eyepiece of the telescope (Hell, 1766).

Some of Hell's conclusions were independently reached by his fellow Jesuit, Roger Boscovich, in 1767. Hell's views were known to the Round Tower astronomers, if not before then at least in 1768 when he arrived in Copenhagen to take part in the observations of the 1769 transit of Venus. He met with Horrebow and probably also with Bugge. We do not know if Hell was successful in convincing Horrebow that the moon was an illusion, but the fact that Horrebow did not publish his final observation of the moon points in this direction. Bugge was in favour of the ghost explanation, as we saw above, and he specifically mentions Hell, so was directly inspired by him.

7 CONCLUDING REMARKS

Hell's and Boscovich's views quickly became known in astronomical circles, and with them Venus' moon disappeared from the sky. Yet in his 1781 report Lalande wrote that Short was such a professional astronomer that he would never have been deceived by an optical illusion. In so doing, Lalande opened up the possibility of an alternative to Hell's and Boscovich's ghost explanation, so would a new search for the moon begin? In fact this did not happen: there are no letters, programmes, reports or memoirs from astronomers telling about a specific search for this moon. The search for Venus' enigmatic satellite effectively ended in 1768.

8 NOTES

1. The first Director of the Round Tower was Longomontanus, one of Tycho Brahe's assistants. The Round Tower has survived and is still located in the centre of Copenhagen. It functioned as an observatory until 1862 and is now a museum.

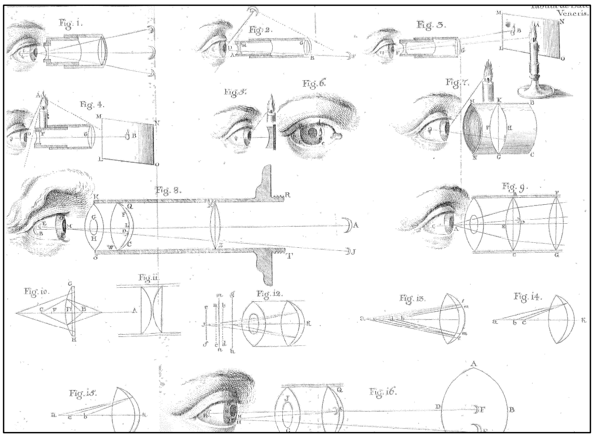


Figure 3: Hell's (1766) illustrations of how a bright planet produces an illusion of a satellite in the eye of the observer.

9 ACKNOWLEDGMENTS

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Kurt Møller Pedersen is a Lecturer in the Department of Science Studies at Aarhus University, Denmark. His research interest is the history of science in the seventeenth and eighteenth centuries. He is currently working on Leonhard Euler's optical theories and Ole Roemer's discovery of the finite velocity of light. Together with Peter de Clercq he is arranging for a diary of Professor Thomas Bugge to be published in which Bugge describes what he saw when he visited German, Dutch and English observatories in 1777.

Helge Kragh is Professor of History of Science at the University of Aarhus, Denmark. His main work is in the history of modern physical sciences, including astronomy and chemistry, where he has done research on the development of quantum physics, astrophysics, geochemistry and cosmology. Among his books are *Conceptions of Cosmos* (2007), a history of cosmology from myths to the twenty-first century. Kragh is a member of the Royal Danish Academy of Science and serves as President of the European Society for History of Science.

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WILLIAM HERSCHEL'S FIFTY-TWO FIELDS OF EXTENSIVE DIFFUSED NEBULOSITY – A REVISION

Arndt Latusseck
Lützowstrasse 5, 31141 Hildesheim, Germany
E-mail: arndt@latusseck.com

Abstract: Since its publication in 1811, William Herschel's list of fifty-two fields of extensive nebulosity has been largely disregarded, or even discredited, by the astronomical community. Neither he nor his successors decided to include the observations of large structureless fields of background nebulosity in their major catalogues. It was only during a short period in the early twentieth century that astronomers like I. Roberts, E.E. Barnard, and M. Wolf started more serious investigations into the nature and reality of Herschel's nebulosities, but without deriving conclusive results. Those few who tried to understand Herschel's elusive observations were often puzzled by his ambiguous descriptions and frequently tended to reject the nebulosities as being optical illusions, because only a small number of them could be proven by celestial photography. The only unconditional supporter of the reality of the nebulosities was Johann Georg Hagen, who in the 1920s used them as evidence for his hypothesis that nebulous matter covered almost the entire celestial sphere. He claimed to have succeeded in visually observing nebulous matter in every single one of Herschel's fields, which raised sharp opposition from his numerous critics. The questionable quality of Herschel's original descriptions, the weak supporting arguments, and the lack of photographic evidence, finally led historians to conclude that Herschel's fifty-two fields of extensive nebulosity were illusions. But it would seem astonishing that this gifted observer could have been fooled to such an extent. As a first approach to investigate this apparent anomaly, a complete analysis of Herschel's observing books was carried out, and the raw observations of the various catalogued nebulous fields were extracted. Some important stylistic uncertainties in the descriptions of the visual appearance of the nebulosities were cleared up, leading to a better understanding of what Herschel actually saw. Possible sources of error were excluded, or at least qualitatively estimated, for certain regions. One outcome of this project is a completely revised list of fields of largely extended nebulosity observed by Herschel, which certainly does not prove the correctness of all of his observations but does at least clarify the context in which they should be regarded. As a useful by-product, some poorly-known first-time observations of nebulous fields that are well known today by means of photography can now be assigned to William Herschel.

Keywords: W. Herschel, I. Roberts, D. Klumpke Roberts, J.G. Hagen, fifty-two fields of nebulosity, extensive nebulosity, dark clouds.

1 INTRODUCTION

William Herschel's catalogue of more than 2500 non-stellar celestial objects is without doubt one of the great astronomical achievements of this exceptional astronomer. Largely unknown, however, is a list containing fifty-two fields of extensive nebulosity, which Herschel published in 1811 as a supporting argument to his nebular hypothesis (Herschel, 1811: 275-276), all of which were observed as a by-product of his sweeps between 1783 and 1802 (see Figure 1). For the purpose of a final revision of Herschel's objects, his sister Caroline's copies of the eight observing books containing the results of his decade-long sweeps (Herschel, Herschel and Herschel, 2004) were analyzed. As a result, a number of errors and inaccuracies were found and corrected. Furthermore, the terminology used to describe the observed nebulosity—which differed widely from that used by Herschel to describe non-stellar objects in his better-known catalogues of nebulae—was investigated in order to obtain a clearer impression of the appearance of Herschel's objects. The accompanying revised list summarizes all of the noticed peculiarities.

2 BACKGROUND HISTORY

2.1 The Original Observations

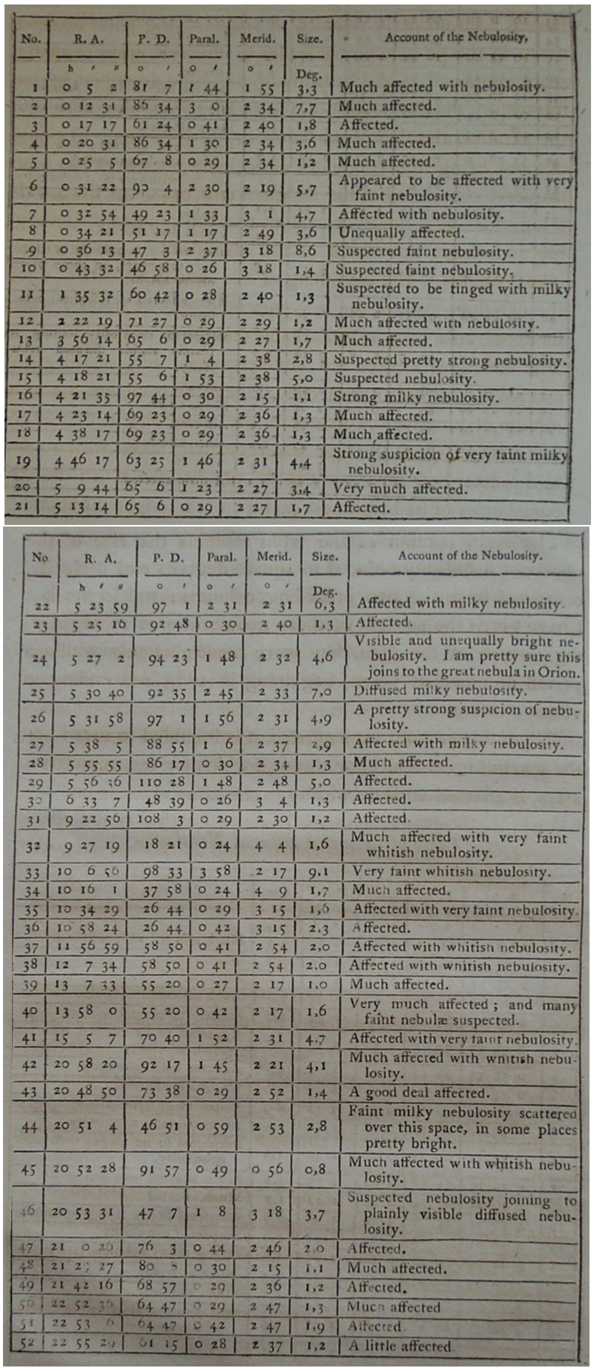
From the beginning in 1783, throughout his observing sessions William Herschel casually noticed large areas of sky extending over many square degrees which seemed to be affected by very faint veils of nebulosity, a phenomenon which was completely different from the mostly well-defined spots of nebulosity he came across every clear night. Obviously these observations were always made near the absolute physiological limit of the human eye: in Herschel's (1811: 277) own words "… [the nebulosities] can only be seen when the air is perfectly clear, and when the observer had been in the dark long enough for the eye to recover from the impression of having been in the light." Showing his talent as an extraordinarily careful observer, Herschel logged every such case of an apparent large-scale brightening of the sky background—'bottom' or 'ground', as he called it (see Figure 2). However, this method proved to be quite inexact in terms of gauging the total extension of such areas, and Herschel knew about its limitations when he wrote in his 1811 paper that "… the nebulous state of the heavens could only be noticed when its appearance became remarkable enough to attract attention."

2.2 Cosmological Significance

The first published mentioned this particular type of object occurred in 1791 when Herschel stated that after observing the region of southern Orion he found evidence of

> … a telescopic milky way, which I have traced out in the heavens in many sweeps made from the year 1783 to 1789. It takes up a space of more than 60 square degrees of the heavens, and there are thousands of stars scattered over it. (Herschel 1791: 77).

While this may be the first published account of extensive diffused nebulosity, many years before Herschel was convinced of the stellar nature of all nebulous objects, whether well-defined or large and extended. But it was only in 1811 that he published his opinion that nebular matter must exist in great abundance throughout the Universe, even though the idea of its general existence was foreshadowed in his 1791 paper. Prior to this Herschel held the opinion that 'real' nebulosity did not exist but could be explained by a clustering of stars too weak to be resolved in the telescope, just as faint stars form the band of the naked-eye Milky Way. Consequently, the term *resolvable* occurs quite often in Herschel's early records when describing the appearance of nebulous objects (cf. Hoskin 1983: 135), indicating that they would presumably be resolved into individual stars if the telescope were powerful enough.



No.	R. A.	P. D.	Paral.	Merid.	Size.	Account of the Nebulosity.
	h ′ ″	° ′	° ′	° ′	Deg.	
1	0 5 2	81 7	1 44	1 55	3,3	Much affected with nebulosity.
2	0 12 31	86 34	3 0	2 34	7,7	Much affected.
3	0 17 17	61 24	0 41	2 40	1,8	Affected.
4	0 20 31	86 34	1 30	2 34	3,6	Much affected.
5	0 25 5	67 8	0 29	2 34	1,2	Much affected.
6	0 31 22	90 4	2 30	2 19	5,7	Appeared to be affected with very faint nebulosity.
7	0 32 54	49 23	1 33	3 1	4,7	Affected with nebulosity.
8	0 34 21	51 17	1 17	2 49	3,6	Unequally affected.
9	0 36 13	47 3	2 37	3 18	8,6	Suspected faint nebulosity.
10	0 43 32	46 58	0 26	3 18	1,4	Suspected faint nebulosity.
11	1 35 32	60 42	0 28	2 40	1,3	Suspected to be tinged with milky nebulosity.
12	2 22 19	71 27	0 29	2 29	1,2	Much affected with nebulosity.
13	3 56 14	65 6	0 29	2 27	1,7	Much affected.
14	4 17 21	55 7	1 4	2 38	2,8	Suspected pretty strong nebulosity.
15	4 18 21	55 6	1 53	2 38	5,0	Suspected nebulosity.
16	4 21 35	97 44	0 30	2 15	1,1	Strong milky nebulosity.
17	4 23 14	69 23	0 29	2 36	1,3	Much affected.
18	4 38 17	69 23	0 29	2 36	1,3	Much affected.
19	4 46 17	63 25	1 46	2 31	4,4	Strong suspicion of very faint milky nebulosity.
20	5 9 44	65 6	1 23	2 27	3,4	Very much affected.
21	5 13 14	65 6	0 29	2 27	1,7	Affected.
22	5 23 59	97 1	2 31	2 31	6,3	Affected with milky nebulosity.
23	5 25 16	92 48	0 30	2 40	1,3	Affected.
24	5 27 2	94 23	1 48	2 32	4,6	Visible and unequally bright nebulosity. I am pretty sure this joins to the great nebula in Orion.
25	5 30 40	92 35	2 45	2 33	7,0	Diffused milky nebulosity.
26	5 31 58	97 1	1 56	2 31	4,9	A pretty strong suspicion of nebulosity.
27	5 38 5	88 55	1 6	2 37	2,9	Affected with milky nebulosity.
28	5 55 55	86 17	0 30	2 34	1,3	Much affected.
29	5 56 36	110 28	1 48	2 48	5,0	Affected.
30	6 33 7	48 39	0 26	3 4	1,3	Affected.
31	9 22 56	108 3	0 29	2 30	1,2	Affected.
32	9 27 19	18 21	0 24	4 4	1,6	Much affected with very faint whitish nebulosity.
33	10 6 56	98 33	3 58	2 17	9.1	Very faint whitish nebulosity.
34	10 16 1	37 58	0 24	4 9	1,7	Much affected.
35	10 34 29	26 44	0 29	3 15	1,6	Affected with very faint nebulosity.
36	10 58 24	26 44	0 42	3 15	2.3	Affected.
37	11 56 59	58 50	0 41	2 54	2,0	Affected with whitish nebulosity.
38	12 7 34	58 50	0 41	2 54	2.0	Affected with whitish nebulosity.
39	13 7 33	55 20	0 27	2 17	1.0	Much affected.
40	13 58 0	55 20	0 42	2 17	1,6	Very much affected; and many faint nebulæ suspected.
41	15 5 7	70 40	1 52	2 31	4.7	Affected with very faint nebulosity.
42	20 58 20	92 17	1 45	2 21	4,1	Much affected with whitish nebulosity.
43	20 48 50	73 38	0 29	2 52	1,4	A good deal affected.
44	20 51 4	46 51	0 59	2 53	2,8	Faint milky nebulosity scattered over this space, in some places pretty bright.
45	20 52 28	91 57	0 49	0 56	0,8	Much affected with whitish nebulosity.
46	20 53 31	47 7	1 8	3 18	3,7	Suspected nebulosity joining to plainly visible diffused nebulosity.
47	21 0 26	76 3	0 44	2 46	2,0	Affected.
48	21 23 27	80 8	0 30	2 15	1.1	Much affected.
49	21 42 16	68 57	0 29	2 36	1,2	Affected.
50	22 52 30	64 47	0 29	2 47	1,3	Much affected
51	22 53 6	64 47	0 42	2 47	1,9	Affected.
52	22 55 29	61 15	0 28	2 37	1,2	A little affected.

Figure 1: Herschel's table of extensive diffuse nebulosity (1800.0) (after Herschel, 1811).

Thus the 1791 account of the "… telescopic milky way …" in Orion should be regarded in accordance with the state of Herschel's cosmology at that time. Once more it indicates Herschel's conviction that large nebulous fields were clearly stellar. From this perspective it is obvious why most observations of extended fields of diffuse nebulosity remained untreated for so many years: Herschel simply did not judge those fields important for his research into the structure of the Universe as there were no stars to be counted and the relevant regions of the sky did not contain any other physical objects of interest. So we have an explanation as to why the fifty-two fields of extensive diffused nebulosity—which Herschel finally published in 1811—never made it into any of his earlier well-known catalogues: it was only in that year that he classified them as a specific class of objects.

However, opinion had changed significantly by the time Herschel published his 1811 paper. At last he had changed his own mind about nebulosity, stating that "… in this new arrangement I am not entirely consistent with what I have in former papers said on the nature of some objects that have come under my observation." He now believed that nebulous matter was very common throughout the Universe, being the material from which stars formed.

In order to support this hypothesis further, Herschel put his observations of nebulae-related objects into a new order: starting from the most extended nebulosities he thought of an evolution up to 'stellar nebulae nearly approaching to the appearance of stars' in order to demonstrate the increasing condensation of nebular matter into stars. As one starting point for this argument Herschel then introduced his thus-far unpublished list of fifty-two "… extensive diffused nebulosities."

2.3 Early Treatment

From the day of its publication not much attention was paid to these areas of nebulosity, which was probably as much a consequence of the exceptional observational equipment that Herschel used for the observations as it was of the missed publicity through not having been included in his three catalogues of nebulae. Possibly he foresaw the difficulties that might arise in trying to verify his observations: "... we find that extreme faintness is predominant in most of [the fields]; which renders it probable that our best instruments will not reach so far into the profundity of space, as to see more distant diffusions of it." (Herschel 1811: 277-278). In fact, during the early nineteenth century there was almost no telescope which matched Herschel's 20 feet reflector, and the few comparable instruments in the hands of professional astronomers (such as J.H. Schröter in Lilienthal) were mostly used for planetary observations. Thus, Herschel's fifty-two fields of extended diffuse nebulosity quickly fell into oblivion and attracted little attention for the next eighty years.

Even William Herschel's son, John, decided to omit these fields of nebulosity from his general catalogue of 1864 (Herschel 1864: 7), even though he had Arthur Auwers' (1862: 42) reduced list of his father's observations and knew about them. His reasons must have been the same as J.L.E. Dreyer's forty-four years later, when, in the Foreword to his second Index Catalogue, published in 1908, Dreyer stated that

> … of the very extensive and diffused nebulosities ... I have only inserted a few fairly well-defined objects of limited size. An object like No. 27 in W. Herschel's list of regions 'affected with nebulosity', filling the whole constellation of Orion, could obviously not find a place

> here. (Dreyer 1971: 286).

Thus—just as John Herschel had done previously—Dreyer consciously refrained from including the areas of diffuse nebulosity in his catalogues. Both astronomers must have shared the same arguments: objects of this size and covering such large areas of the sky tend to mask other objects in the same region, thus leading to confusion in identifying more distinct nebulae.

2.4 Observational Attempts Around the Turn of the Century

Whether for these reasons or others, we know of no attempt to re-observe Herschel's areas of diffuse nebulosity until 1891 when the British amateur astronomer, Thomas Backhouse (1891: 1), wrote:

> I have examined with my field-glass [a pair of binoculars of 2.05 inches aperture and 3.8 times magnification] the places of several of these nebulosities, and find that his objects do not agree with those seen with this smaller instrument. It is true there are wisps occupying part, or the whole, of some of the nebulous regions quoted by Sir W. Herschel, but in other cases there is nothing special visible. Also, in the neighbourhood of Herschel's nebulosities, there are numerous faint wisps far more conspicuous with the field-glass than those in the areas he enumerates.
>
> I do not fully understand his list, for some of the regions of nebulosity overlap ... One may conclude from these observations that a large part of the wisps visible with my field-glass were resolved by Herschel's telescope of 20 inches diameter; and that what he saw were fainter nebulosities, or it may be, in some cases, unresolved portions of those seen by me.

Backhouse did observe Herschel's nebulosities in Taurus in the course of examining the extent and detailed structure of some parts of the northern Milky Way, but his judgement about them was rather devastating, even if he was not very influential (for later observers did not refer to his paper).

But the time was favourable for the study of elusive celestial objects. With the application of photography to astronomy large nebulosities raised the interest of many astronomers. Just one year after Backhouse's observations, E.E. Barnard (1892) published Herschel's list anew as of "… extremely great value …" to those interested in photographing such objects. One of the better-known nebulae from the list, Herschel's no. 27, had already been photographed three years before Barnard's paper by W.H. Pickering, and in 1894 by Barnard himself (the so-called 'Barnard's Loop'; see Barnard, 1894), thus giving rise to hope that the other nebulosities might also exist. The high value Barnard attached to Herschel's nebulosities may be estimated from his 1903 statement that

> … this question of large areas of diffused nebulosity in the sky is a very important one, not yet fully appreciated, but which must sooner or later have the highest bearing on a proper understanding of the physical condition of the universe.

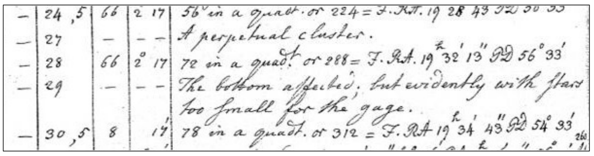
In 1896, Isaac Roberts began a photographic survey of the fifty-two nebulosities, the results of which were reported in three different papers (Roberts, 1902; 1903a; 1903b), together with Herschel's table reduced to epoch 1900.0. Roberts' motivation was of course investigative, knowing that

> … no systematic efforts were made to verify Herschel's observations of these 52 regions until six years ago, when the work of photographing them was commenced at my Observatory, using for the purpose the 20-inch reflector and the 5-inch Cooke lens. (Roberts 1903c).

The exposure time was 90 minutes, a standard for his photographic works. Thus Roberts' expectation to unveil the real nature of Herschel's objects was high:

> My long previous experience in photographing the heavens enabled me to judge that under these conditions nebulosity of at least the degree of faintness that could be seen by Herschel with his two- and four-feet reflectors would be shown on the photographic plates.

Nevertheless, as with Backhouse, the result was almost negative: "Of the fifty-two nebulous regions described by Herschel, the photographs showed diffused nebulosity on four of them only; there is no visible trace of diffused nebulosity on forty-eight of the areas". The four positive detections were nos. 7, 25, 44 and 46, with nos. 44 and 46 obviously related to the then well-known nebular complex NGC 7000 (in Roberts' eyes more or less representing the same physical object). In addition, no. 7 was regarded by Roberts as part of the outer areas of M 110, a companion of the Great Andromeda Nebula, thus definitely leading him to the conclusion that not much new was contained in Herschel's list, to the mind of the twentieth century astronomer.



—	24,5	66	2 17	56 in a quadt. or 224 = F. RA. 19 28 43 PD 50 33
—	27	—	— —	A perpetual cluster.
—	28	66	2° 17	72 in a quadt. or 288 = F. RA. 19h 32′ 13″ PD 56° 33′
—	29	—	— —	The bottom affected, but evidently with ſtars too ſmall for the gage.
—	30,5	8	17′	78 in a quadt. or 312 = F. RA 19h 34′ 43″ PD 54° 33′

Figure 2: Example of a logbook entry showing an account of the sky background apparently affected by nebulosity. This observation was made during sweep no. 269 on 13 September 1784 and demonstrates Herschel's early view that this type of nebulosity is just a summation effect of faint background stars.

But now that an authority like Roberts had written off Herschel's observations as essentially being deceptions, reactions from the astronomical community quickly occurred—and they were crushing. In the very issues of *Monthly Notices* and the *Astrophysical Journal* where Roberts published his results, Heidelberg astronomer Max Wolf (1903) and Barnard (1903) respectively, published harsh criticisms of his conclusions. Barnard opposed Roberts' opinion on two grounds: first, he claimed that a 90 minute exposure was not sufficient to trace nebulae as faint as Herschel's, and as an example he mentioned Herschel's nebulosity no. 27, its position being in perfect agreement with the earlier-mentioned 'Barnard's Loop' that Barnard had discovered photographically in 1894, but which was totally invisible on Roberts's plates. Second, he thought it

> … a little unreasonable to suppose that Herschel, who made so few blunders compared with the wonderful and varied work that he accomplished, should be so palpably mistaken in forty-eight out of fifty-two observations of this kind. (Barnard, 1903: 77-78).

This second remark clearly shows the great respect that William Herschel still received even by some of the most eminent observers of the time, in not wanting to let his observational work fall into disrepute.

More factual criticisms came from Max Wolf, who considered it strange that Roberts had not been able to detect the extensive nebulosity of southern Orion, with its dimensions filling dozens of square degrees around the Great Orion Nebula, thus covering Herschel's objects no. 22, 23, and 24. Wolf and Barnard were both able to do so (e.g. see Figure 3), but used much longer exposure times than Roberts had.

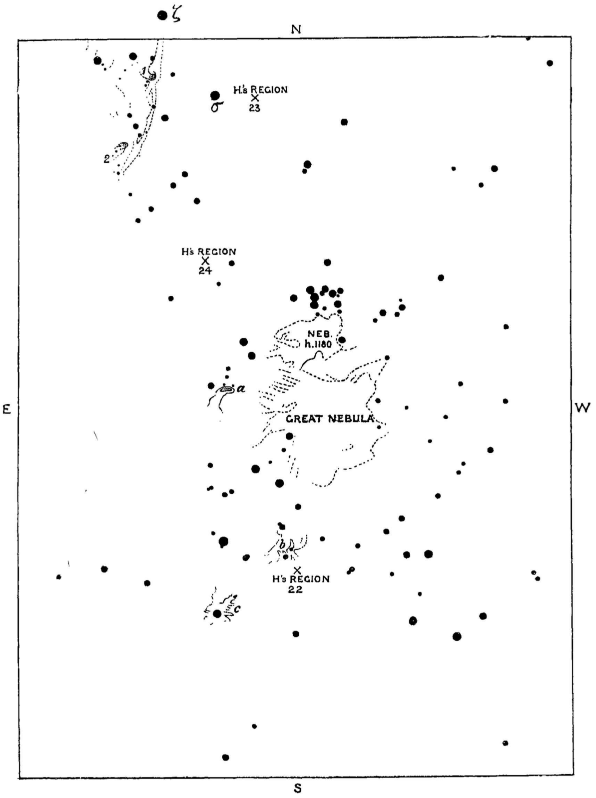


Figure 3: Drawing by Max Wolf to illustrate the situation of Herschel's nebulosities no. 22, 23, 24 in relation to the faint photographic nebulosities about the Great Orion Nebula (after Wolf, 1903: f. 302).

In the end, this dispute proved to be of short duration, probably because Roberts' death in June 1904 left little room for further debate.

Now that only the two advocates of Herschel's nebulosities were left, the general opinion was that Herschel was indeed right, and around 1904 his nebulosities were regarded simply as fainter examples of the well-known diffuse emission and reflection nebulae. But the final word about their reality had yet to be written, since only eight entries were considered confirmed (see Table 1).

2.5 Father Hagen's Observations

Almost twenty more years passed before Herschel's list was analyzed once again. In 1920 the Austrian-American Jesuit astronomer Johann Georg Hagen (1847–1930), then Director of the Specola Vaticana in Rome, had started a program to visually detect what he called 'cosmic clouds' throughout the celestial sphere, using an f/15 16-inch refractor. His results, first presented in 1921 to the Royal Astronomical Society (Hagen 1921a; cf. 1921b), soon met with criticism and refusal because these supposed clouds—which Hagen described as 'obscure'—were not detectable by photography, nor did they influence the light of stars in any measurable way (as Barnard's Milky Way 'dark markings' supposedly did). Nevertheless, up until his death Hagen continued to compile a catalogue of these 'obscure clouds', which he saw as faintly luminous objects covering much of the night sky, and becoming gradually 'denser' (more luminous) towards the Galactic Poles, while towards the Milky Way they seemed to thin out, leaving nothing but black background sky.[1]

In the course of this much-criticized work, Hagen began to search for supporting arguments for his 'cosmic clouds'. One strategy was to find supporters among earlier observational astronomers, and it was not long before Hagen promoted William Herschel as the real discoverer of his clouds by referring to Herschel's 1811 paper. Certainly Hagen (1916) quoted Herschel on the large nebulosities from his very first publication concerning the discovery of large nebulous fields in comparatively high galactic latitudes, but it is revealing to see his growing efforts to relate his own observations to the eminent William Herschel after 1923, when Hagen published Herschel's list anew, together with some historical remarks (Hagen, 1923). At the height of the debate, in 1926, Hagen (1926a-1926f) published his own visual re-observations of all of Herschel's nebulosities in a series of six papers in *Monthly Notices of the Royal Astronomical Society*. The result was nothing short of a sensation in that Hagen confirmed every single one of Herschel's fifty-two extended diffuse nebulosities (see Figure 4)!

Throughout his lifetime, Hagen, always trusted his own eyes more than the photographic plate, and he expressed his satisfaction in the following words: "While it took six hours and more to photograph some of [the nebulosities], six minutes would have sufficed to see them." (Hagen, 1923: 202). Consequently, "… there is no doubt that Herschel's table contains [nebulosities] some of which are known as dark nebulae."

Table 1: A list of Herschel nebulosities that were regarded as confirmed by 1904.

No.	Observer	Object or Region	Area (sq°)	Herschel's Description
07	Roberts	The outer areas of M110	4.7	Affected with nebulosity
22	Wolf	Southwest of Orion Nebula	6.3	Affected with milky nebulosity
23	Wolf	Between Orion's belt and Orion Nebula	1.3	Affected
24	Wolf	In the immediate vicinity (north) of the Orion Nebula (near NGC 1981)	4.6	Visible and unequally bright nebulosity. I am pretty sure this joins to the great nebula in Orion
25	Roberts	40' east of IC 434	7.0	Diffused milky nebulosity
27	Pickering & Barnard	The central part of Barnard's Loop	2.9	Affected with milky nebulosity
44	Roberts	NGC 7000 ('Florida')	2.8	Faint milky nebulosity scattered over this space, in some places pretty bright
46	Roberts	NGC 7000 ('Panama')	3.7	Suspected nebulosity joining to plainly visible diffused nebulosity

Hagen's critics had always focussed on the common conviction that everything visually recognizable must also be photographable, which he passionately denied. He thus advised sceptics to put aside their cameras and look through the telescope, although he seemed not to have any illusions about his appeal. "Should Herschel's skill have been lost by our photographic training of astronomers?" he would ask Robert Aitken some years later (Hagen, 1927).

Basically, Hagen's results were regarded as extremely doubtful, and his confirmation of Herschel's nebulosities was not taken seriously. This is vividly demonstrated by J.L.E. Dreyer (1926), whose response to Hagen's first published note on his observations of the first four of Herschel's nebulosities is telling:

> Before acknowledging that W. Herschel was the discoverer of dark cosmic clouds, it will be well to bear in mind that he does not anywhere make any distinction between the general appearance of the objects examined by Father Hagen and the rest of the fifty-two objects. He certainly saw, or believed that he saw, in all the fifty-two places recorded by him, luminous objects. A few of them are well-known nebulae, such as NGC 7000. Considering his vast experience it is difficult to believe that he saw something totally different in the four places examined by Father Hagen, without realising it and drawing special attention to him.

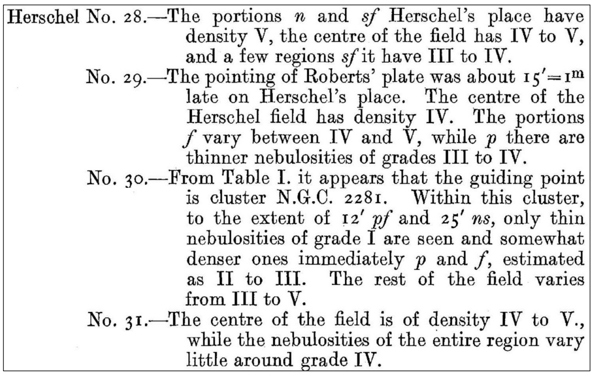
It is interesting that it was Dreyer who responded to Hagen's paper, the very same Dreyer who had earlier decided not to include Herschel's list in his NGC and IC catalogues. Hagen did not care. In a letter dated 21 January 1926 to fellow astronomer Johann Stein of Valkenburg, he wrote:

> In the January issue of M.N. you will find an article, in which I call W. Herschel the discoverer of the Cosmic Clouds. The 'Council' asked me by Prof. Turner, if I would consider a critical remark by the editors an offence. I answered: no. They don't want Herschel to become involved into my 'deceptions'. I have, however, proved ... that our Cosmic Clouds match exactly with the 52 nebulosities. (Hagen, 1926g).

This statement excellently expresses Hagen's general attitude: he was certainly aware of the prevailing opinion, but any opposition only led him to double his efforts to provide further evidence of the correctness of his observations in a bid to alter that opinion.

In order to have his views prevail, Hagen started to activate other observers, however, he could only interest amateurs and second-rate astronomers. What links almost all later observers is the fact that Hagen had cultivated friendly relations with them over many years, which is evident from their correspondence. In any case it can be stated that almost every publication concerning Herschel's nebulosities (and even Hagen's cosmic clouds) after 1926 was in some way directly related to Hagen's initiative. Whether or not these circumstances caused an observational bias because of the preoccupation of the observers remains to be investigated.

Soon W.S. Franks, the former observing assistant of Isaac Roberts, started his own observing project on Herschel's nebulosities at the Brockhurst Observatory. The idea that Franks would be a suitable observer was put forward by Dorothea Klumpke Roberts (1926), the widow of Isaac Roberts and a long-time friend of Hagen. Franks' good relations with Hagen are also reflected in their correspondence, and finally, both astronomers had even published a joint paper some years before Klumpke Roberts' suggestion (see Franks and Hagen 1923). In one of his publications, Franks (1928) even admits that he had been "… urged by Father Hagen to undertake some visual observations of these neglected and much disputed nebulous regions"—which yet again emphasizes Hagen's persuasiveness. Franks' observing results were generally positive, and by using Hagen's published notes on the nebulosities he was able to trace the brighter objects on up to three occasions but could not detect the six faintest regions.



Herschel No. 28.—The portions *n* and *sf* Herschel's place have density V, the centre of the field has IV to V, and a few regions *sf* it have III to IV.

No. 29.—The pointing of Roberts' plate was about $15'=1^m$ late on Herschel's place. The centre of the Herschel field has density IV. The portions *f* vary between IV and V, while *p* there are thinner nebulosities of grades III to IV.

No. 30.—From Table I. it appears that the guiding point is cluster N.G.C. 2281. Within this cluster, to the extent of 12′ *pf* and 25′ *ns*, only thin nebulosities of grade I are seen and somewhat denser ones immediately *p* and *f*, estimated as II to III. The rest of the field varies from III to V.

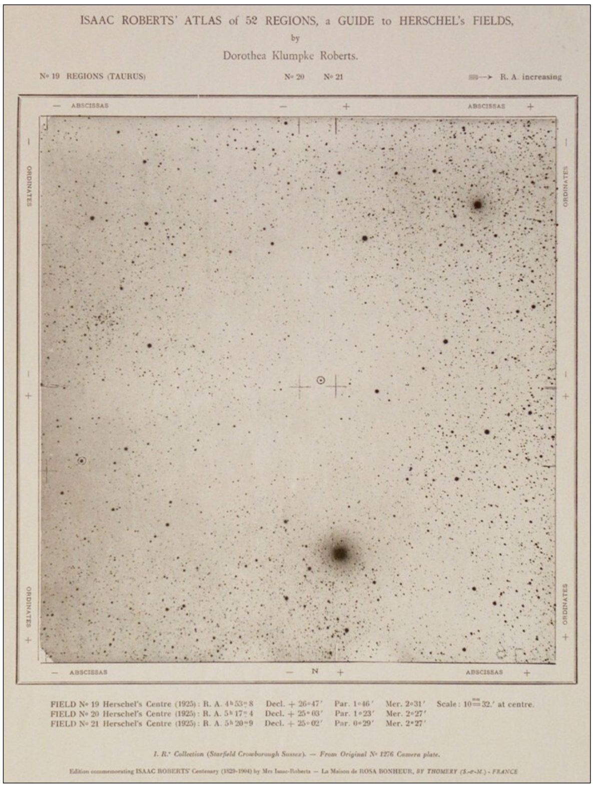
No. 31.—The centre of the field is of density IV to V., while the nebulosities of the entire region vary little around grade IV.

Figure 4: Part of Hagen's observations of Herschel's list of nebulosities. Hagen estimated the visual brightness of the nebulosities using a five-step scale (I-V), where 'I' represented the faintest and 'V' the strongest light impression.

In 1928 Paul McNally (a Jesuit like Hagen) was appointed Director of the Georgetown Observatory, but before taking up this post he spent part of October at Mt. Wilson Observatory where Hagen taught him how to carry out visual and photographic observations of nebulosities (McNally 1929). Although he could not find the time to carry out any further observations once he was settled in Georgetown, after the IAU General Assembly at Leiden he did publish a useful paper containing an historical overview of Herschel's nebulosities (ibid.). McNally's main research preoccupation was also variable stars, and this 1929 paper can be traced back directly to Hagen's influence.

Concerning amateur astronomers, in 1930 and 1931 G. Lehner from Erfurt in Germany was encouraged by Josef Hopmann and Heinrich Osthoff to observe Herschel's nebulosities and some of the more generally-distributed cosmic clouds which Hagen thought he saw near the North Galactic Pole (Lehner 1930/1931). Another amateur who became involved with Herschel's nebulosities was Marcel de Kérolyr. This avid French astrophotographer, who had built the first stationary observatory in Haute-Provence (the Station Astrophotographique de Haute Provence, at Forcalquier), was well known for his excellent wide-field photographs of nebulae which he published in the *Bulletin de la Societé Astronomique de France* during the 1920s and 1930s. What probably made de Kérolyr interesting to Hagen was the fact that he had perfected the technique of photographing celestial objects for many hours, which enabled him to expose his photographs for up to 24 hours, split over several days. In September 1929 Hagen wrote to de Kérolyr, encouraging him to use his skills for the purpose of solving the "… problème international …" of Herschel's nebulosities. Although both were French citizens, no direct relation between Klumpke Roberts and de Kérolyr can be traced which might lead to the conclusion that she recommended her compatriot to Hagen.

In his paper published only in 1931, one year after Hagen's death, Kérolyr confirmed the successful photography of Herschel's nebulosities 22, 23, 24, 25 and 26 in southern Orion (which was nothing new really), but in addition he published a complete list of visual observations of Herschel's nebulosities, which again served to confirm most of results obtained earlier by Herschel and Hagen.


ISAAC ROBERTS' ATLAS of 52 REGIONS, a GUIDE to HERSCHEL's FIELDS,
by
Dorothea Klumpke Roberts.

N° 19 REGIONS (TAURUS) N° 20 N° 21 → R. A. increasing

FIELD N° 19 Herschel's Centre (1925): R. A. $4^{h}53^{m}8$ Decl. + 26°47′ Par. 1°46′ Mer. 2°31′ Scale: 10^{mm} = 32.′ at centre.
FIELD N° 20 Herschel's Centre (1925): R. A. $5^{h}17^{m}4$ Decl. + 25°03′ Par. 1°23′ Mer. 2°27′
FIELD N° 21 Herschel's Centre (1925): R. A. $5^{h}20^{m}9$ Decl. + 25°02′ Par. 0°29′ Mer. 2°27′

I. R.' Collection (Starfield Crowborough Sussex). — From Original N° 1276 Camera plate.

Edition commemorating ISAAC ROBERTS' Centenary (1829-1904) by Mrs Isaac-Roberts — La Maison de ROSA BONHEUR, *BY THOMERY (S.-et-M.) - FRANCE*

Figure 5: A page from *Isaac Roberts' Atlas of 52 Regions, a Guide to Herschel's Fields*. This page shows nebulosities nos. 20 and 21, and their centres are marked by crosses on the photograph.

2.6 Isaac Roberts' Atlas of Fifty-two Regions

None of the efforts mentioned above was of any avail, and the resonance Hagen had hoped for was negligible. Luckily there was one person who was willing to push his case forward and this was Dorothea Klumpke Roberts,[2] whose husband's photographic experiments on Herschel's nebulosities had started the initial debate soon after 1900. In 1925 Hagen contacted Klumpke Roberts (whom he had known for decades) to ask for contact prints of some of Isaac Roberts' original plates, namely of Herschel objects 27, and 50-51 (Klumpke Roberts, 1925a). From then on, a vivid correspondence developed between the two astronomers, and it quickly showed that Klumpke Roberts was a dedicated supporter of Herschel's (and certainly of Hagen's) cosmic clouds. Before long, Klumpke Roberts (1925b) raised the idea of publishing Isaac's photographs, and over the next three years this plan was put into action—as shown by her correspondence with Hagen, who turned into a mentor. Both astronomers knew that their symbiosis had good prospects to serve their own personal aims: Hagen finally received support from an eminent, influential colleague, while Klumpke Roberts, in her turn, was certainly keen to rehabilitate Isaac, given the criticism he had received as a result of his far-reaching conclusions about the non-existence of Herschel's nebulosities in 1903.[3] As it happened, Isaac Roberts' centenary was to occur in 1929, giving Klumpke Roberts another reason to publish the atlas.

The result of Hagen's and Klumpke Roberts' cooperation was titled *Isaac Roberts' Atlas of 52 Regions, a Guide to Herschel's Fields* (Klumpke Roberts 1928a; see, also, Figure. 5), which was published in July 1928. Klumpke Roberts prepared all the plates and designed the layout while Hagen provided scientific support and advice, and the Foreword (a straightforward text in which Hagen took the chance to get even with his critics).

Immediately after its publication, Klumpke Roberts started to promote the atlas through astronomical societies, taking advantage of meetings of the Astronomische Gesellschaft in Heidelberg (Klumpke Roberts, 1928b) and the IAU Meeting in Leiden (Meetings of Commissions …, 1928); she also advertised the atlas at the 1928 meeting of the Comité National Français d'Astronomie.[4] According to Klumpke Roberts (1928b), the atlas was accepted "… with appreciation and applause …" at the IAU meeting, and even Hubble, who was known to be one of the sharpest critics of Hagen's cosmic clouds, suggested that photographic experiments using yellow filters might be worthwhile. This is a clear indication of the strong impact that Klumpke Roberts' attendance had on the audience (see Klumpke Roberts, 1935).

2.7 Later Activities

With Hagen's death on 5 September 1930 the most determined supporter of Herschel's nebulosities disappeared from the scene, still leaving the matter unsettled. But Klumpke Roberts carried on propagating the accuracy of Herschel's and Hagen's observations. Having learned about the power of skilful political manoeuvring, she used her contacts with astronomers like the late Max Wolf as well as assemblies of the Astronomische Gesellschaft to try to convince the still-numerous critics of her late husband's theories. Klumpke Roberts continued to receive support by continued delivery of photographs from de Kérolyr, which she presented to audiences as slides or as small exhibitions (Klumpke Roberts, 1928b). De Kérolyr finally claimed to have successfully photographed Herschel's nebulosities 12, 14-15, 20-21, 33 and 41, and Klumpke Roberts confirmed that all the photographs of these fields indeed showed traces of nebulosity. However, the reliability of these claims must be questioned given that de Kérolyr also presented a photograph showing the illusive 'Baxendell Nebula' (NGC 7088), which in those days was known to be nonexistent.[5]

In 1932 a supplement volume to the 1928 *Isaac Roberts' Atlas of 52 Regions* … was published, showing additional photographic plates of Herschel's fields of nebulosity that Roberts had exposed around 1900 (Klumpke Roberts, 1932). Klumpke Roberts presented this supplement to the Astronomische Gesellschaft at the 1933 Meeting in Göttingen, for which she was accepted as a member of the Gesellschaft.

Around 1929, Klumpke Roberts donated a prize to the Société Astronomique de France, "… for observing Herschel's diffused nebulosities …" (Klumpke Roberts, 1929). It was "… a modest little prize; nevertheless, I trust it will encourage the members of the S.A.F. in observing diffused nebulosities." She also thought of a prize for the British Astronomical Society, but this idea was not realized. Finally, in 1930 another prize of $100 was donated to the Astronomische Gesellschaft to be granted to an astronomer who had published "… an important work about obscure clouds." (Schmeidler, 1988). Up to 1937 this prize was repeatedly granted to different astronomers, such as Friedrich Becker (Hagen's former assistant in Rome between 1925 and 1926) in 1930 and 1931, and Marcel de Kérolyr in 1935 (he had already received the French prize in 1929). But the list of prize winners threw a sobering light on the status of researching the cosmic clouds, for even the incentive of winning a reasonable amount of money seemingly did not encourage astronomers to strengthen their research in this field—or was it that there was nothing to gain because there was nothing to find?

Whatever research interest there was at this time tended to be concentrated in the German astronomical community, and the outbreak of World War II brought a sudden halt to any interest in cosmic clouds, then with Klumpke Roberts' death in 1942 the last supporter left the stage. Since that date, nothing of substance has been published on Herschel's list of fifty-two fields of diffuse nebulosity.

To the present day, the existence of the majority of Herschel's catalogued nebulosities remains a subject of speculation. Throughout all of his observations Herschel certainly worked at the absolute physiological limit of the human eye, which is to be regarded as one of the reasons why the existence of extensive nebulosities was always met with doubt. Nevertheless, these days successful visual observations of definitely-existing objects with extremely low surface brightnesses are regularly carried out by avid amateur astronomers with optical instruments that are comparable to those used by Herschel.[6] Thus, the existence of Herschel's extensive nebulous fields should not be automatically ruled out. Indeed, besides the few regions which were verified photographically around 1900 by Roberts, Wolf, Barnard and others, there are other fields discovered in recent years which have been shown to contain vast, faint emission and reflection nebulosity (see Section 3.9, below).

It seems that there is still more to be discovered from Herschel's list. What, precisely, was it that Herschel observed? The list does not quite tell the whole story, which makes it seem practical to re-analyze the list in as much detail as possible.

3 THE RE-ANALYSIS OF HERSCHEL'S LIST

As a basis for a final revision of Herschel's objects, his sister Caroline's copies of the eight observing books containing the results of his decade-long sweeps were analyzed. As a result, a number of errors and inaccuracies were detected and corrected. Furthermore, the terminology used to describe the observed phenomena—which differs widely from that used by Herschel to describe non-stellar objects during his other deep sky observations—was analyzed in order to obtain a clear image concerning the appearance of the nebulosities. The resulting revised list summarizes all of the noticed peculiarities in a separate column. Table 3 lists the first four objects in Herschel's list of fifty-two.

3.1 Terminology

So what do these fifty-two nebulosities look like? In general, their appearance is described in Herschel's 1811 publication as being extremely extended throughout the sky and largely structureless, and it is surely not a coincidence that those fifty-two objects were listed directly after a brief description of well-catalogued objects of 'extensive diffused Nebulosity' (i.e. the fifth class according to Herschel's system).

In fact, Herschel often became aware of their existence only through noticing a brightening of the sky background, which he termed as "ground" or "bottom". In order to mark an area as being influenced by nebular matter, the term "affected" was assigned to these regions. Certainly Herschel distinguished, especially throughout his early sweeps, regions "… affected with nebulous ground …" from regions "… affected with milky nebulosity." Nevertheless, the term "affected" always related to the sky background, and Herschel leaves no doubt about the specific meaning of his words: "When this account says affected, it is intended to mean that the ground upon which, or through which we see, or may see stars, is affected with nebulosity."[7] My re-analysis shows that even in cases when in the published list the sky background was marked only as "affected", the original record is always accompanied by the remark "bottom" (only in very early records is "ground" used), which independently proves the direct relation of this term to the sky background.

3.2 Positions and Dimensions

Every position in the revised list was reduced according to the correction values in right ascension and declination given by Caroline Herschel, which she determined by aligning the raw telescopic positional readings of known objects (mostly stars) with their catalogued positions.

Assigning the listed fields to distinct observing records was unambiguously possible (except for nos. 22 and 26), although the values of the published coordinates could not be reproduced precisely, neither in declination, nor in right ascension. Astonishingly, Herschel's published positions are often not very close to the records' geometrical centre, although both sources (the observing books and the published list) were reduced to the same epoch of 1800.0.

The extension of each nebulous field in declination could only be determined from the width of the according sweep zone, since Herschel did not record declinations independently for them. For determining the extension in right ascension, only the preceding and succeeding records were available; no right ascension values were directly recorded for the fields. The objects' dimensions are roughly multiples of 15 minutes of arc, which is the true field of view of his large 20 foot reflector that was used for the sweeps. The listed extensions in declination correspond well to the relevant recorded sweep borders; this is not generally the case with the right ascension extensions, though. As a general rule of thumb, this extension can be calculated as the difference between the western and eastern neighbouring records, minus 15 minutes of arc. There are, however, a number of objects to which this rule is not applicable.

3.3 Plausibility of the Observations and Possible Sources of Error

Assuming a very low general surface brightness, any observation of Herschel's extensive nebulosities must have been extremely sensitive to interfering light sources like the zodiacal light, aurora borealis or loss of eye adaption due to exposure to artificial light sources (see Herschel, 1811: 270). Throughout his sweeps, Herschel was attentive to such sources of error and therefore tried to distinguish actual object identifications from apparently increased sky brightness. Furthermore, he recorded meteorological peculiarities at the time of observation, such as passing clouds, upcoming mist or high winds. We can imagine the inner conflict between the discoverer and the critical scientist when we read Herschel's thoughts during sweep 340 on 13 December 1784:

> In this sweep I found the same kind of suspected nebulosity again as before ... but removing the telescope sideways to a part 10 or 12 degrees preceding where I had found.

Nevertheless, a number of fields observed under less favourable weather conditions were included in the list, which clearly compromises their credibility.

The influence of zodiacal light can be excluded as a possible source of error for most objects, because even those near the ecliptic were never observed closer than 90° distance from the Sun. Only three fields were observed near 180° ecliptical longitude distant from the Sun, thus offering the actual observation of the gegenschein as a possible explanation. Nevertheless, nos. 17, 18, 20 and 21 (no. 19 also belong to this group, but an influence of the zodiacal light can be excluded) coincide in large part with the extensive dark nebula complexes in eastern Taurus which are known to have a higher surface brightness than the surrounding sky background.[8] Presently it cannot be stated with certainty which of these explanations fits the actual observations better. Finally, the influences of moonlight can be excluded for all but two fields (i.e. nos. 24, 49).

In summary, ~30% of Herschel's extensive nebulous fields that were not described as 'suspected' were observed under questionable circumstances.

3.4 Multiple Observations

In order to strengthen the credibility of his observations, Herschel (1811: 277) wrote:

> I have almost without exception found, in a second review, that the entertained suspicion [of nebulosity] was either fully confirmed or that, without having had any previous notice of the former observation, the same suspicion was renewed when I came to the same place again.

Nevertheless, assuming that the observing log is complete, most of the nebular fields were observed only once. Still, a number of objects were recorded more often; most of these are, however, records made during one single observing sweep. In the case of fields nos. 22 and 26 (but not 23-25), no clear demarcation was possible from the log entries; thus additional datasets in the revised list record all those observations which fell into the region of sky covered by these nebulosities. Both regions cover part of the Hα emission region extending over large sky areas of southern and eastern Orion, which explains the fields' uncertain boundaries.

3.5 Doubtful Observations, Erroneous and Non-existing Records

Nebulosity no. 36 could only be assigned to observational records through comparing their celestial coordinates, while the descriptions of their visual impressions logged in Herschel's observing books differ widely or even contradict the list entries.

No. 38 represents a completely erroneous record. Except for the right ascension value, the complete dataset of the preceding nebulosity no. 37 was copied by mistake and taken as no. 38, including the description. However, nebulosity no. 38 does exist, but it has a completely different dataset.

Positional errors and non-existent objects could be determined directly from the observational data. Essentially, the reasons for such erroneous entries could be cleared up, such as confused numbers or incorrect position calculations. Thus, nos. 42 and 44 contained erroneous positional data, which could be corrected, while nebulosities nos. 9/10 and 50/51 proved to be identical: for each pair of fields—which show almost identical mutual datasets—only one appropriate record could be found in the observing logs.

3.6 Correlations with Better-known Objects

As described above, as early as 1904 astronomers involved in the debate (Isaac Roberts being the most avid promoter) sought to correlate the nebulosities with some of the brighter objects contained in the catalogues of nebulae produced by Herschel and others. Indeed, fields no. 7 and 8 (Messier 31), 22 to 26 (nebulae in southern and eastern Orion), as well as nos. 44 and 46 (NGC 7000), coincide at least in part with better-known and elsewhere-catalogued objects, so one might conclude that Herschel had taken those objects for the corresponding nebular fields, which surely seems an obvious explanation. Nevertheless, the re-analysis of his records shows a clear distinction between both object classes: one finds both classes observed next to each other, and obviously separated as different records.

The most instructive case in this context is certainly the NGC 7000 nebula complex, which covers much of the regions of nebulosities no. 44 and 46; thus a closer look seems worthwhile. Strangely, Herschel describes the appearance of nos. 44 and 46 as

> … in No. 44 of the table, we have an instance of faint milky nebulosity, which, though pretty bright in some places, was completely lost from faintness in others; and no. 46 confirms the same remark. (Herschel, 1811: 278).

This is indeed an odd case: the description leaves no doubt that Herschel really observed the region of the North America Nebula, but strangely enough, among his notes we find in total four different entries for the region of sky around that object: V-37 (Herschel's catalogue entry no. 37 in the fifth class of nebulae), which finally led to the entry NGC 7000 in Dreyer's catalogue, and the two additional fields nos. 44 and 46 from his list of 52 nebulosities (see Table 2).

The position of nebulosity no. 44 fits NGC 7000 very well, which might suggest that Herschel identified the former nebulosity with the latter nebula. However, the entry for no. 46 shows that he definitely distinguished NGC 7000 from both nebulosities. Whatever Herschel might have thought about this special sky region, he definitely saw three different objects.

3.7 Observations of Nebulosities not Recorded the Original List

Six additional records of sky regions with characteristics similar to the fields previously published were found, which do not have counterparts in Herschel's original list. These fields have been added to the revised list.

3.8 Records of "Pure Ground"

Besides sky regions with brighter background, Herschel recorded a large number of areas (~100) with a remarkably *dark* appearance of the background, and he described these as "clear" or "pure". The re-analysis of his records showed that by these terms Herschel actually noted the apparent absence of nebulosity and not just a lack of faint field stars. For a final verification, these records might represent an important completion of the observational database, since they now allow correlations with both bright and dark areas in modern catalogues. My revised list does not include these records.

3.9 Possible Discoveries Contained in the List of Nebulosities

Aside from those nebulosities that were part of well-known objects like NGC 7000 or the Great Orion Nebula, there are other objects whose discovery should possibly be assigned to William Herschel. Although no systematic correlation of the fifty-two nebulosities with known objects seems to have been performed to date, a preliminary analysis already shows some surprising coincidences. It cannot, though, be stated with absolute certainty that the objects mentioned below were all actually observed successfully by Herschel. Nevertheless, the following listing shows that a mere denial of the reality of most of Herschel's fifty-two nebulosities is premature.

First, as already stated, the large nebulous arc of Hα emission encircling the eastern parts of Orion which around 1900 was named 'Barnard's Loop' coincides in part with nebulosity 27, and represents the brightest part of the Loop. To call Herschel the discoverer of Barnard's Loop would therefore not be too presumptuous, although the overall shape of that nebula remained unknown to him.

Next to be mentioned are no fewer than eight nebulosities (13, 14, 15, 17, 18, 19, 20 and 21), which were catalogued in the region of the Auriga-Taurus dark cloud complex. Today these absorbing clouds are known from photography to have a slightly higher luminosity than the general sky background. The whole complex was also successfully observed visually by other observers,[10] which makes the dark clouds in Taurus and Auriga good candidates for the objects behind Herschel's observations in this area. The connection between the named nebulosities and the dark cloud complex was assumed as early as 1904 when H.C. Wilson (1904) postulated their possible identity.

Table 2: Observing book entries concerning NGC 7000.

Object	Description	Date
V-37	vL. diffused nebulosity plainly visible. bM 7' or 8' l: 6' b. and losing itself gradually.	Sweep 620 24 October 1786
44	B. considerably affected.	Sweep 959 11 September 1790
46	All this time suspected diffused nebulosity throughout the whole breadth of the sweep. RA From 20h52′9″ to 20h55′46″ PD from 45°35′ to 48°38′	Sweep 620 24 October 1786
	Faint milky nebulosity scattered over this space, in some places pretty bright. The brightest part of it about the place of my V.37.	Sweep 959 11 September 1790

Another interesting case is nebulosity 32, which correlates well with a brighter feature in the galactic cirrus near M81/M82 (see Figure 6). This discovery by Allan Sandage (1976) might point to a promising approach to explain some of Herschel's observations of extensive nebulosity, at least those at higher galactic latitudes. Sandage measured the surface brightness as ~24.5 magnitudes per square arc second, thus these nebulosities should indeed be detectable by the human eye under favourable conditions.

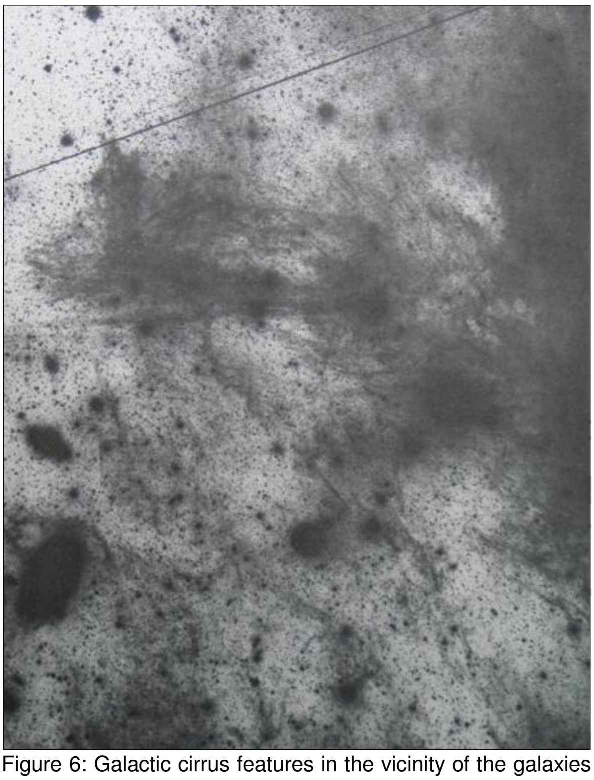


Figure 6: Galactic cirrus features in the vicinity of the galaxies M81/M82 (near the left margin). Herschel's nebulosity 32 was catalogued to the upper right from the centre (after Sandage, 1976).

Last, the whole nebulous region of southern Orion around the Great Orion Nebula, covering nebulosities 22 to 26, was catalogued as number 35 of the fifth class in Herschel's catalogue of nebulae and clusters of stars, thus to some extent Herschel nebulosities 22 to 26 were catalogued twice.[9]

A discovery only indirectly correlated with the nebulosities is the following case. While observing the region of σ Orionis, Herschel noticed the bright streak catalogued today as IC 434 and included it in his own catalogue of nebulae and clusters of stars as number 35 of the fifth class. Attached to this region, to the west, lies nebulosity 23. What is most interesting is Herschel's note from sweep 518 of 1 February 1786 of a special feature in the bright streak: "Wonderful black space included in Nebulosities. 48 (σ) Orionis f. 2′ 46″ n 0° 44′ RA 5h 31′ 27″ PD 92° 0′ (1280)." The precise positional data leave no doubt, for on this night Herschel discovered the Horsehead Nebula, which was much later catalogued as number 33 in E.E. Barnard's catalogue of dark nebulae. Till now, this discovery is often credited to Williamina Fleming, who noticed it when measuring a plate of the region taken by E.C. Pickering in 1888.

3.10 Structure of the Revised List

This list, being one principal result of the review of Herschel's list of fifty-two nebulosities, contains all the information found about each object in the observing books, which in case of multiple entries partially resulted in more than one row per object. Also, six notes on objects with characteristics that were similar as the nebulosities but excluded from Herschel's list have been added. Table 3 shows just the first four entries in the revised list. The complete table will be published elsewhere (Latusseck, 2008).

The first seven rows show the original datasets of Herschel's list of extensive fields of nebulosity as published in 1811,[11] with the epoch of the coordinates 1800.0. The abbreviation 'RA' stands for 'right ascension' and 'Decl' for 'declination', but Herschel used 'polar distance' ('PD'; PD = 90° – declination) instead of declination. The next ten columns catalogue the extracted information from Herschel's observational logs. The column 'Recorded description' contains the unchanged records of Herschel's visual impressions during his observation(s) of each field.

4 CONCLUSION

Herschel used the list of fifty-two extensive nebulosities as a supporting argument for his thesis of the existence of real nebulous matter in space. He used his own observational results in a largely uncritical way, though. However, he pointed out that fields containing an uncertain amount of nebulous material—and therefore viewed only as 'suspected'—were intentionally included in his list.

It would seem that Herschel 'sifted' his observing logs somewhat superficially in order to quickly gather material for his list of nebulosities. This view is supported by his opinion that "… the abundance of nebulous matter diffused through such an expansion of the heavens must exceed all imagination." (Herschel, 1811: 277); even if Herschel had overlooked some log entries, this minor flaw would not have affected the general argument that these nebulosities were present in "… great abundance". In addition, the errors that we have identified in this study could be conveniently explained by assuming a rather 'relaxed attitude' concerning the gathering of observational data.

Considering the uncertain circumstances of the observations, Herschel's list is open to attack, and even his own remarks sometimes place their validity into doubt. As mentioned above, since the early twentieth century his nebulous fields have been greeted with widespread suspicion, and today are, in general, viewed as nonexistent, and thus as deceptions.

The matter is not so straightforward, though. Even a cursory analysis shows a number of correlations between Herschel's fields and existing celestial objects which cannot be explained as purely coincidental. The large faint emission nebulosities in Orion (including IC434 and Barnard's Loop) and the galactic cirrus structures near M81/M82 show that a significant percentage of his extensive nebulosities might indeed have physical counterparts. However, it is likely that a larger percentage of Herschel's nebulosities will be proven to be non-existent. A thorough comparison with modern catalogues will surely throw further light on Herschel's elusive objects.

5 NOTES

1. The catalogue was published in the year following Hagen's death, after Friedrich Becker had completed the work of his former master (see Hagen, 1931).
2. Dorothea Klumpke Roberts was an accomplished astronomer in her own right. Born in San Francisco, she was educated in France and was the first woman to obtain a Ph.D. in astronomy in France (from the Sorbonne). She was Director of the Bureau of Measurements at the Paris Observatory and had a substantial list of publications before marrying the much older Isaac Roberts when she was 40 years of age. Already a prize-winner from the French Academy of Sciences, she was elected a Chevalier de la Légion d'Honneur before leaving Paris in 1934 and returning to San Francisco.
3. As an example of how actively Klumpke Roberts tried to rehabilitate her husband's reputation see Klumpke Roberts, 1930.
4. Note by Dorothea Klumpke Roberts to Vesto M. Slipher in his capacity as Chairman of IAU Commission 28 (Nebulae), July 1928.
5. http://www.klima-luft.de/steinicke/ngcic/persons/baxendell.htm
6. For discussions on observations of low surface brightness objects, see for example the following discussion group: http://groups.yahoo.com/group/amastro/
7. For example see Sweep 244 on 27 July 1784:

   > The bottom or ground (if I may so call it) of the heaven is not clear but contains faint patches produced by stars not bright enough to come to a focus in passing the field of view.

8. For example see Sweep 266 on 11 September 1784:

   > The whitish nebulosity from having been in the light is very different from the resolvable nebulous appearance of affected ground of the milky way.

9. Hagen (1921b) claimed to have observed this region successfully in 1920, although his results were met with skepticism.

	Herschel's 1811 description						recorded datasets (from logbook)									
No.	RA 1800.0	Decl. 1800.0	RA extension	Decl. extension	Size	Account of the Nebulosity	Sweep no.	Date of observation	Recorded description	RA 1800.0 Most probable eastern border	Object RA 1800.0	RA 1800.0 Most probable western border	Breadth of Sweep	Decl. top 1800.0	Decl. bottom 1800.0	Remarks
1	00h 05m 02s	+08° 53'	1° 44'	1° 55'	3,3°	Much affected with nebulosity	52	1783 Dec 19	Much nebulosity .	23h 41m 02s	00h 02m 02s	00h 08m 02s	01° 40'	+09° 43'	+08° 03'	
							52	1783 Dec 19	Much nebulosity many L.	00h 02m 02s	00h 08m 02s	00h 18m 02s	01° 40'	+09° 43'	+08° 03'	
2	00h 12m 31s	+04° 26'	3° 00'	2° 34'	7,7°	Much affected	338	1784 Dec 13	B much affected.	00h 05m 01s	00h 07m 01s	00h 18m 01s	02° 19'	+04° 36'	+02° 17'	Succeeding sweep record is nebulosity #4.
3	00h 17m 17s	+28° 36'	0° 41'	2° 40'	1,8°	Affected	266	1784 Sept 11	The ground affected.	00h 15m 17s	00h 16m 17s	00h 19m 29s	02° 25'	+29° 50'	+27° 25'	Possible weather interference: preceding: "Very clear"; following: "A faint haziness, very clear, flying haziness, clear again"
4	00h 20m 31s	+03° 26'	1° 30'	2° 34'	3,6°	Much affected	338	1784 Dec 13	B very much affected, rather more so than I have seen it before. I wish it were possible to compare it with some very distant situation.	00h 07m 01s	00h 18m 01s	00h 28m 01s	02° 19'	+04° 36'	+02° 17'	Continuation of nebulosity #2 (from same sweep). Deception? Succeeding record: "In this sweep I found the faint kind of suspected nebulosity again as before at 0h 10'; but removing the telescope sideways to a part 10 or 12 degrees preceding where I had found B very pure I could perceive no difference and believe this kind of deception is owing to the snow which covers the ground, northern lights that illuminate the snow, and a pretty high wind that agitates the air."

Table 3: The initial section of the revised list of Herschel's nebulosities, showing the first four entries.

10. The catalogue record relates even more objects to V-35:

> Diffused m. nebulosity, extending over no less than 10 degrees of PD. and many degrees of RA. It is of very different brightness, and in general extremely F. and difficult to be perceived. Most probably the nebulosities of the 28th, 30, 31, 33, 34, and 38th of this class are connected together, and form an immense stratum of far distant stars, to which must also belong the nebula in Orion.

11. According to his own words, the nebulous fields cover such large sky areas that Herschel was not able to explore their true dimensions. Thus he cut every field by a parallelogram, limited by declination and right ascension.

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Arndt Latusseck teaches mathematics, physics, astronomy and computer science at a high school in Hildesheim, Germany. He is a graduate of the University of Hildesheim and has a degree in Computer Sciences. His main interests lie in the history of observation and research of interstellar matter, as well as the development of astrophysics in the early twentieth century. Presently he is undertaking a Ph.D. at the University of Hamburg, and his thesis is about the observational history of the elusive 'cosmic clouds' discovered visually by Father Hagen at the Vatican Observatory in the early years of the twentieth century. Arndt is also interested in the life and work of William Herschel and his sister Caroline, having in recent years assisted in gathering and verifying biographical data on the Herschel family.

BOOK REVIEWS

***Astronom in zwei Welten*, by Dieter B. Herrmann (Halle, Mitteldeutscher Verlag, 2008), pp. 255, index, many b&w illustrations, ISBN 978-3-89812-557-4 (hardcover), €19.90, 205 x 140 mm.**

An Astronomer in Two Worlds is the translated title of the autobiography by Dieter B. Herrmann (Figure 1). The author has been long-time Director of Archenhold Observatory, a popular observatory in Berlin-Treptow, that was founded by Simon Archenhold in 1896, and run by him until 1931. The Observatory houses the longest existing refracting telescope (f = 21 m, d = 0.68m). Archenhold's son, the second Director, had to emigrate when the Nazis came to power, and the Observatory was taken over by the Berlin school administration. After WW II, in 1948, Diedrich Wattenberg was appointed Director. Our author succeeded him in 1976, and retired in 2005. During his career, German re-unification took place, and he is one of the few prominent East Germans (an 'Ossie') in an executive position who survived in his job, i.e. was not replaced by a so-called 'Wessie'. Thus, he is certainly qualified to write about his experiences in these "two worlds".

The author unfolds his life in the broad panorama of post-wartime (Eastern) Germany, when there was—at least in the beginning—a spirit of optimism and progress. He describes his interest in music (with his later acquaintance of the composer Hanns Eisler), in acting (he was a member of a student cabaret and theatre), and in physics and astronomy (from his early lecture of the popular astronomy writer B.H. Bürgel to his activities at Archenhold Observatory, from his physics studies at the Berlin Humboldt University, via newspaper articles on science, to his job at a state authority of radiation protection). In the early 1960s, he got an offer to work towards a Ph.D. in the history of science, which in 1969 resulted in a thesis on "The Emergence of Astronomical Professional Journals in Germany". When he became Head of the History of Astronomy Section at the Archenhold Observatory with a chance to become the new Director in a few years, he decided to join the ruling party (the Socialist Unity Party of Germany). A 'party group' had been installed in the Observatory, and instead of interfering with its decisions (and as a non-party member being always on the weaker side), he found it more efficient for the Observatory to take its lead.

In this way, his years at the Observatory were successful: an increase in visitor numbers, the publication of books, trips to conferences, research visits abroad and appearances on the television programme *Aha.* Finally, he became founding Director of the major Zeiss Planetarium in East Berlin which opened in 1987.

After German reunification, a major readjustment of public life in Eastern Germany took place, when an evaluation of institutions and persons took place, where not only the competence, but also the 'proximity to the former ruling system' played a decisive role in re-employment. In addition, in the formerly divided city of Berlin, cultural institutions existed both in the East and the West, and if something had to be closed, it was of course the Eastern one ... It is a splendid testimony to the qualifications, ability, openness, and flexibility of Dieter B. Herrmann that the two institutions entrusted to him survived and are now part of the German Technikmuseum Berlin.

This elegantly-written book is not only a valuable autobiography of an important popularizer and historian of astronomy, but also an extraordinary document of public life in post-war Germany before and after re-unification.

Hilmar W. Duerbeck, Brussels

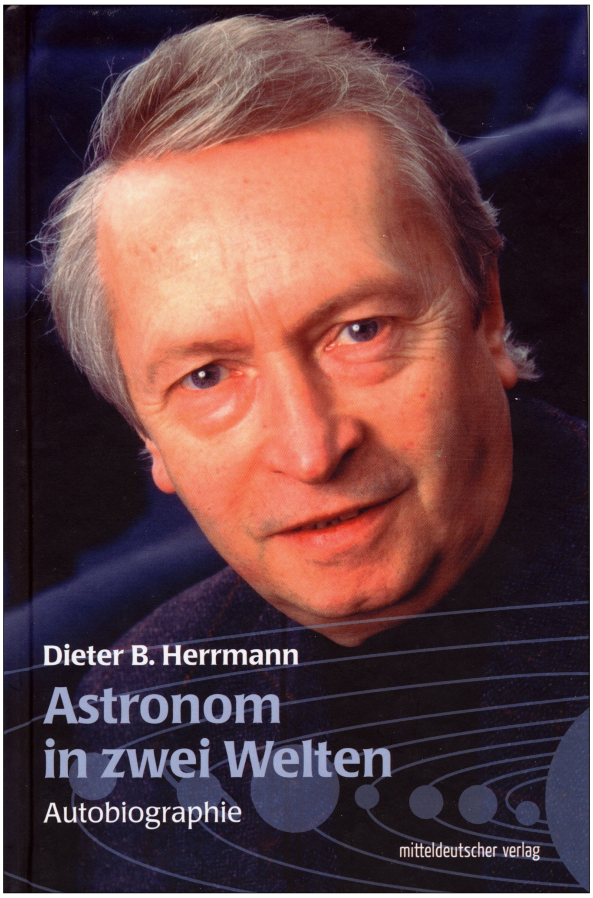

Figure 1: The cover of Dieter B. Hermann's autobiography.

***Der Wissenschaftsmacher: Reimar Lüst im Gespräch mit Paul Nolte*, by Reimar Lüst (Munich, C.H. Beck, 2008), pp. 288, index, 23 illustrations, ISBN 978-3-406-56892-3 (hardcover), €24.90, 220 x 150 mm.**

This book (Figure 2) is sort of an autobiography of Reimar Lüst, a one-time astronomer who was born in 1923. His early research on magnetic fields led to rocket experiments with Barium clouds ('artificial cometary tails'), and he then became an accomplished science manager (or 'science maker', as the title implies).

Instead of providing an autobiography, this book presents a series of discussions (or interviews) between Lüst and Paul Nolte. Born in 1963, Nolte is a Professor of Modern and Contemporary History at the Freie Universität Berlin.

Before giving a brief summary of the contents of this book, I would like to state that this series of questions and answers, through which Reimar Lüst's life unfolds, makes for quite enjoyable reading. Sometimes I have the impression that during the editing of the interviews, part of the answer was moved into the question—I simply cannot believe that the interviewer was so well informed about the life of the interviewee that he could ask such specific questions. While some personal, scientific and science management facts are discussed in great detail, other personal matters remain deliberately vague (e.g. when Lüst's first wife, the astronomer-physicist Rhea Lüst, was replaced by his second wife, the science journalist Nina Grunenberg).

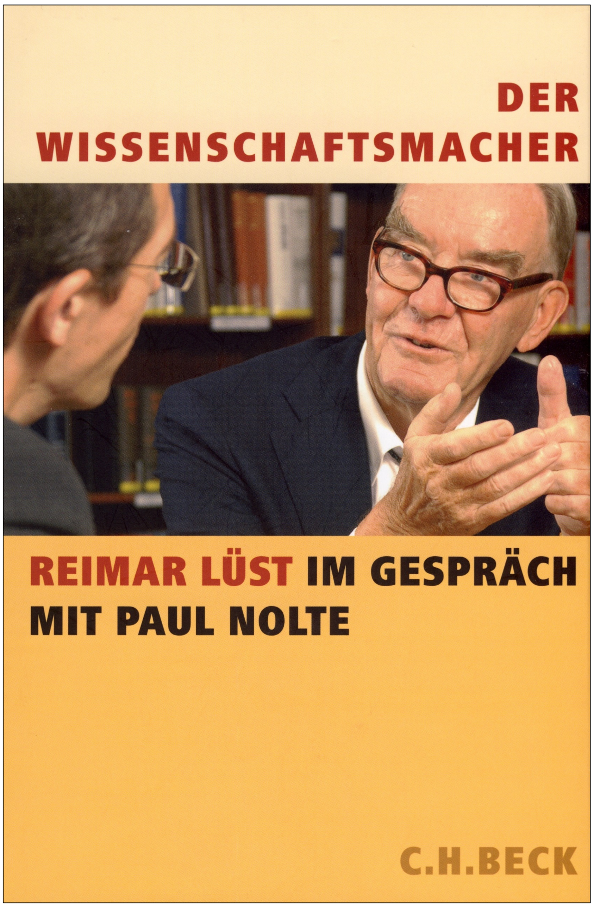

Figure 2: The cover of Reimar Lüst's pseudo-autobiography.

Lüst begins by describing his youth, his interest in engineering that led to his activity (and sinking) as chief engineer on a German U-boat in WW II and subsequent P.O.W. times in the USA, where he started his studies in physics and mathematics. After the war, he continued these studies in Frankfurt and Göttingen, where in 1951 he wrote a Ph.D. thesis on "The evolution of a gaseous mass rotating around a central body" at the Max Planck Institute for Physics under the supervision of C.F. von Weizsäcker. Lüst then became a member of the Max Planck Institute for Physics and Astrophysics after its move to Garching near Munich, and he carried out rocket experiments on the magnetic field of the Earth ('artificial comets'). From 1963 to 1972 he was Director of the Max Planck Institute for Extraterrestrial Physics. Afterwards, he was President of the Max Planck Society for the Advancement of Sciences (also social sciences and humanities), and from 1984 to 1990 Director General of the European Space Agency. Finally, from 1989 to 1999 he was President of the Alexander von Humboldt Foundation, which arranges exchange visits of scientists from abroad at German universities. In recent years, Lüst has been active in establishing the privately-endowed International University Bremen (now Jacobs University). For most readers of *JAH*^{2} I suspect that those parts of Lüst's autobiography that relate to his involvement in astronomy and space flight will be of most interest.

Besides Lüst's personal reminiscences of important scientists like Werner Heisenberg and Carl-Friedrich von Weizsäcker, this book offers interesting insights into science organizations in Germany and Europe.

Hilmar W. Duerbeck, Brussels

***Sternwarten: 95 astronomische Observatorien in aller Welt*, by Stefan Binnewies, Wolfgang Steinicke, and Jens Moser (Erlangen, Oculum Verlag, 2008), pp. 280, glossary, index, about 230 color illustrations. ISBN 978-3-938469-20-0 (hardcover), €49,90, 325 x 250 mm.**

95 astronomical observatories around the world is, first of all, a profusely illustrated book. Many of the very appealing colour photographs—showing the outside appearance of observatories and also many new and old telescopes—were taken by two of the authors, S. Binnewies and J. Moser, while historian of astronomy, W. Steinicke, added detailed notes on the history, present state and activities of these observatories.

The distribution of the optical and radio observatories described in this book was certainly dictated by their 'accessibility': famous US and Chilean observatories, as well as most of the (central) European ones are covered, among them also are two historical ones (Greenwich and Birr Castle) and four popular or private ones. South America is reduced to the string of large observatories in Chile, although there are also remarkable ones in Venezuela, Brazil, and Argentina. Africa is only represented by the South African stations at the Cape and Sutherland, as well as the recently-inaugurated HESS Gamma-ray detector in Namibia. As for Asia, the three Russian stations, Zelentschuk, RATAN600 and Pik Terskol are presented, as well as Xingling in China and Nobeyama in Japan. Observatories in Armenia, Georgia, India, Indonesia, Iran and Israel are lacking. Of course, this book is mainly a 'travel document' to major astronomical places, not a systematic overview. For a complementary study of observatories built before 1950 (with especial emphasis on their architecture), Peter Müller's *Sternwarten in Bildern* (Berlin, Springer, 1992) is recommended.

The informative, detailed, and well-written text contains a number of inaccuracies that each visitor/user of the respective observatory/telescope will quickly discover (e.g. the Hoher List 0.75m reflector was not built by Zeiss Jena—only its mounting was, and the ESO La Silla 1.52m reflector has a closed tube, not an open one). Someone writing from a distance unavoidably is prone to such minor errors, but these do not interfere with the general usefulness of this book (although they will hopefully be remedied in a planned English edition). But as it stands, *Sternwarten* is an attractive, up-to-date guide to major observatories across the world.

Hilmar W. Duerbeck, Brussels

***Universe in a Mirror: The Saga of the Hubble Space Telescope and the Visionaries Who Built It,* by Robert Zimmerman (Princeton, Princeton University Press, 2008), pp. 320, index, 251 color and b&w illustrations. ISBN 978-0-369-113297-6 87-71668-8 (hardcover), US$29.95, 234 x 157 mm.**

Robert Zimmerman, a science writer, whose books focus on space flight, presents here a very readable and up-to-date account of the planning and performance of the Hubble Space Telescope (henceforth HST). The book (Figure 3) starts with the post-WW II ideas of space astronomy, put forward by Lyman Spitzer, up to the (still pending) HST fifth servicing mission (SM4) now scheduled for 2009.

Robert W. Smith's book on *The Space Telescope* (1989) presented a more official view of the same history, from the beginning up to launch (and, in its 1993 edition, the first critical years), while Eric Chaisson's *The Hubble Wars* (1994) describes these critical years as viewed through the eyes of a somewhat partial participant. Zimmerman succeeds in presenting a more balanced, but also very personal, view of the planning, construction and operation of the HST and of the work of many of the 'dramatis personae' whose fate during long periods of time was intimately connected with it. Besides accessible published material, Zimmerman has used a plethora of archival material, has interviewed people involved in the HST, and has evaluated interviews from the Space Astronomy/Space Telescope/NASA and AIP History Projects (about a hundred in total).

I find the author's tales of the activities at the various NASA centers and manufacturing companies, the actions of the scientific proponents and opponents of the HST (one out of the latter group even became the Director of the Space Telescope Science Institute), the activities of the politicians and NASA bureaucrats very interesting. Cursory readers might find themselves lost at a first reading, when confronted with the plethora of persons involved in this enterprise. Nevertheless, I think that the author manages to tell this fascinating story in such a way that no one will drop the book before finishing it.

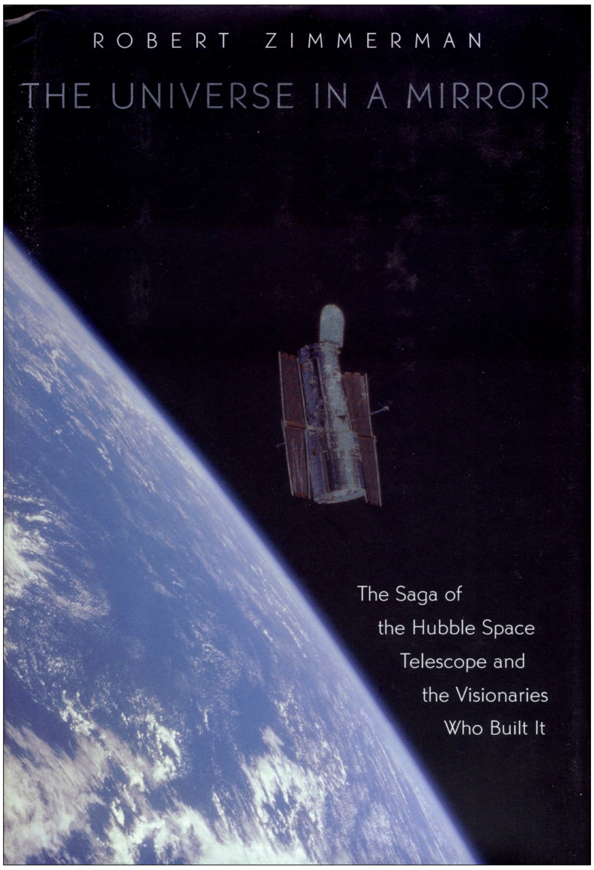

Figure 3: Cover of Zimmerman's *The Universe in a Mirror*.

Concerning the scientific achievements of the HST, the author chooses, as a continuous leitmotif of the book, observations of the star Eta Carinae from the early photographic attempts of the mid-1940s via SIT-Vidicon observations from Chile to the fascinating HST frames. Other projects are more superficially described and hardly rise above the public outreach descriptions of NASA. But this is not the main task of the book. The author presents us with an informative and very readable case study of modern 'big science', which traces the lives and works of many people between the poles of science, politics, funding, bureaucracy and (military) industry.

Hilmar W. Duerbeck, Brussels

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