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COVER PHOTOGRAPH

Bruce Slee (1924–) at the control desk of the 64-metre Parkes Radio Telescope in the late 1960s. After this radio telescope was opened, in November 1961, Slee used it to carry out a variety of Solar System, Galactic and extra-galactic research projects. This is the third in the series of *JAH*^{2} issues celebrating Bruce Slee's sixty years in astronomy. For an overview of his research see pages 3-10 in the June 2005 issue of this journal, while two different aspects of his research portfolio are discussed on pages 97-106 in the December 2005 issue and on pages 35-56 in the current issue.

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THE BEGINNINGS OF RADIO ASTRONOMY IN THE NETHERLANDS

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Abstract: The birth of Dutch radio astronomy can be rather precisely dated to 15 April 1944, when H.C. van de Hulst presented the results of his theoretical research into the origin of radio waves from space. We have investigated the events leading up to the momentous suggestion that hydrogen emission at 21 cm ought to be detectable. Both published material and letters from the Oort Archive have been consulted. Not having direct access to either radar technology or trained engineers, as was the case in countries like England and Australia, Jan Oort had to turn to a diversity of organizations: Philips Electronics Company, the Post Office, and academic colleagues in other disciplines. It was the Post Office's head of radio, A.H. de Voogt, who provided a 7.5 m Würzburg radar reflector and technical support at the Kootwijk station, starting in 1948. We trace the events leading up to the 21 cm line's detection in 1951, and discuss the early results. After a year spent rebuilding and thereby improving the receiver, C.A. Muller, together with Oort, Van de Hulst and others, was able to initiate an extensive HI survey of the Galaxy. The results fully justified the year's wait: a map of the Galaxy, spiral arms, the first rotation curve, and a much improved system of Galactic coordinates. We also present a discussion of Würzburg antennas used for research in the Netherlands, and a brief biography of A.H. de Voogt.

Keywords: radio astronomy, 21 cm hydrogen line, Oort, Van de Hulst, Muller, Netherlands, Kootwijk, NERA

1 INTRODUCTION

At its General Assembly in Sydney in 2003, the International Astronomical Union (IAU) formed a Working Group on Historical Radio Astronomy, with the task to document and, where possible, preserve radio-astronomical instruments of historical significance. Our goal in the present paper is to document the first radio telescopes used in The Netherlands. Our paper roughly covers the period 1944-1956; we intend to discuss the 25-meter reflector, inaugurated at Dwingeloo in 1956, in a later paper. The present paper thus concentrates on the 7.5-meter Würzburg reflector used between 1951 and 1955 for studies of 21-cm line radiation from atomic hydrogen in the Milky Way Galaxy (Section 4), and on the several other Würzburgs used mainly for solar radio astronomy (Section 5).

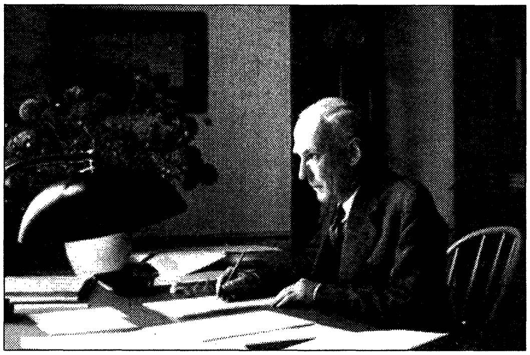
Figure 1: J.H. Oort behind his desk at Leiden Observatory, 1953 (photo by H. Kleibrink).

As compared with developments in some other countries, the beginnings of Dutch radio astronomy had a special character in several respects. In Australia and England radio-astronomical research was started in 1945 by radio engineers and physicists who had acquired extensive experience with radar receivers during World War Two; the Australians, moreover, had access to large amounts of war surplus equipment (Bowen, 1984; cf. also Sullivan, 2005). In The Netherlands, such experience and equipment were essentially lacking. The country had been occupied during the War; the universities had been closed for two years; the Institute of Technology at Delft, the only source of engineers at academic level, had even been closed for four years. On the other hand, in The Netherlands the drive towards radio astronomy came from leading individuals at classical astronomical observatories, notably Jan H. Oort at Leiden, who saw new, major opportunities for studies of Galactic structure.

These differences in circumstance were of such importance that we consider it appropriate to sketch the background of Dutch radio astronomy in two separate Sections (2 and 3).

2 A SPECTRAL LINE AT RADIO WAVELENGTHS!

During World War II the paper on "Cosmic Static" published by Grote Reber (1940) in the *Astrophysical Journal* came to Oort's attention. Reber was a remarkable radio astronomy pioneer (e.g. see Kellermann, 2005) who, using a paraboloid of 9 meters diameter which he had built in his backyard, had found strong radio radiation at wavelengths of order 2 meter, distributed over the sky and peaking near the position of the Galactic Centre. Oort (Figure 1) realized that radio waves would suffer no extinction by interstellar dust particles, and hence might allow an unhindered, complete view of the Galactic System—a major leap forward. In 1927, from a careful analysis of stellar motions, Oort had found evidence for differential Galactic rotation, and derived rough estimates of the rotation speed and mass of the Galaxy. However, interstellar extinction limited the reach of optical observations to a few kiloparsecs at best, while the distribution and distances of globular clusters indicated a distance to the Galactic Centre of order 10 kpc. Hence, the structure of the Galaxy remained essentially unknown. Oort sensed that radio astronomy might, in principle, change this situation drastically.

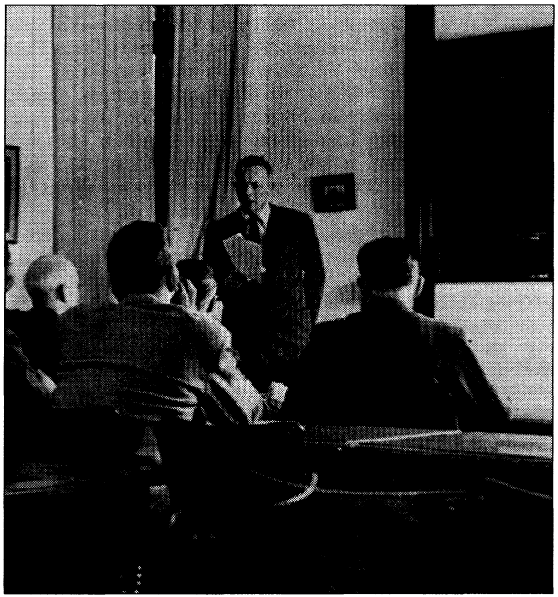

Figure 2: H.C. van de Hulst in 1955, during a restaging of the 15 April 1944 meeting of the Netherlands Astronomers' Club at Leiden Observatory, for the Kleibrink film (1957). The fully-visible heads (left to right) are: J.H. Oort, J.J. Raimond and J. Houtgast.

In 1941 Leiden University had announced a prize competition on "The formation of solid particles in the interstellar gas",[1] and in 1942 essays were submitted by three graduate students: D. ter Haar from the Lorentz Institute for Theoretical Physics in Leiden, H.C. van de Hulst from the Sonnenborgh Observatory in Utrecht and A.J.J. van Woerkom from the Observatory in Leiden. Together with Oort, these three young astrophysicists discussed their results at an Inter-university Colloquium of the Netherlands Astronomers' Club (NAC) on 9 January 1943 (Ter Haar et al., 1943). It may have been on this occasion that Oort first met Van de Hulst. Sometime early in 1943, Oort invited Van de Hulst for an extended visit to Leiden, but the War circumstances led to a delay (Van de Hulst, 1943a). M.G.J. Minnaert, Director of the Utrecht Observatory, had been held hostage in a detention camp since 1942. In October 1943, during a short leave from his camp, Minnaert was able to discuss the planned visit with Van de Hulst; he gave full approval, provided the stay at Leiden would be *temporary* (Van de Hulst, 1943b). Early in January 1944, Van de Hulst came to Leiden for a period of three months; his program was still open.

Oort was then planning another NAC Colloquium, this time about "Radiogolven uit het Wereldruim" (Radio Waves from Space), to discuss Reber's findings. Dr C.J. Bakker from the Philips Physical Laboratory would speak about the reception of radio waves, and Van de Hulst was invited to discuss their origin. Oort mentioned to him:

> We should have a colloquium on the paper by Reber; would you like to study it? And, by the way, radio astronomy can really become very important if there were at least one line in the radio spectrum. Then we can use the method of differential galactic rotation as we do in optical astronomy. (Van de Hulst, 1957b: 3).

In a brilliant paper at the NAC Colloquium (Figure 2) on 15 April 1944, Van de Hulst (1945) presented his results. Modifying earlier work by Henyey and Keenan (1940), he calculated the continuous spectrum expected from a layer of ionized hydrogen, and suggested that this might explain the radiation reported by Reber. He further considered various possible spectral lines. For the transitions between high-excitation levels of the hydrogen atom, now known as 'recombination lines', he overestimated the broadening by the Stark effect and concluded that the lines would be fully effaced (this mistake was caused by a substitution error; see Van de Hulst, 1998: 4). But a hyperfine transition in the ground state of the hydrogen atom appeared promising. This 'spin-flip' transition, in which the electron spin changes from parallel to the proton spin into anti-parallel, would correspond to a wavelength of 21 cm. Van de Hulst found that the line would stand out from the background continuum, provided that the transition probability exceeds 10^{-16} sec^{-1}, corresponding to an average lifetime for the upper level of less than 300 million years. (The lifetime was later shown to amount to 11 million years.) Although at the time receivers were not sensitive enough, Van de Hulst (1945: 219) stated: "The matter does not look hopeless, although the existence of the line remains speculative."

In fact, the situation looked quite favourable indeed: line emission expected from the most abundant atom, present essentially everywhere in interstellar space! And that at a wavelength of 21 cm, where even a reflector of 10 meter diameter would provide an angular resolution of ~1.5°! The War delayed publication of the papers by Bakker (1945) and Van de Hulst (1945) until December 1945. An independent investigation by Shklovsky (1949) confirmed the findings of Van de Hulst. However, it would be several years before the 21-cm line was actually detected.

3 THE LONG ROAD TOWARDS A RADIO TELESCOPE FOR 21 CM WAVELENGTH

After the NAC colloquium on 15 April 1944, Oort immediately made plans to obtain equipment for observations at radio wavelengths.[2] On 19 April 1944, Oort wrote to C.J. Bakker, asking him whether Philips could provide a receiver for wavelengths of order 50 cm; Oort thought that the mechanical workshop at Leiden Observatory could construct a reflector of 20 m diameter; in fact, he wanted an angular resolution of about 0.5° (Katgert-Merkelijn, 1997: xx). Bakker answered that a receiver might become available after the War; he further pointed out that the desired angular resolution would require a larger telescope, or going to shorter wavelengths.

Soon after the War, Oort wrote to a variety of persons and institutions, enquiring about possibilities for construction of a radio telescope and for obtaining receivers (Oort, 1944-48). These enquiries led to a 'pre-project' by Werkspoor, the company which had built several railway bridges, and even to a tentative design in 1945. Apparently, Oort had dropped the idea to have a large reflector built by the Observatory Workshop.

In November 1945 Oort presented a plan for a radio reflector of 25 m aperture to the Board of the Royal Netherlands Academy of Sciences, which supported a request for funding of this project (Oort et al., 1951: 53). The request probably was submitted to the Department of Education, Arts and Sciences. On 8 November 1945 Oort wrote to the Prime Minister, Ir. W. Schermerhorn, and a few weeks later they discussed the plan (Oort, 1945). Schermerhorn, who had been Professor of Geodesy at the Delft Institute of Technology, and was anxious to restore Dutch science after the setbacks suffered during the War, was strongly interested in the 'kippegaas-telescoop' (chicken-wire telescope), as he called it (Oort, 1970), but in the immediate post-war period no funds of the required magnitude were available (Katgert-Merkelijn, 1997: xxii).

The lack of funds and of suitable engineers, together with worries about the required sensitivity, at times made Oort wonder whether the plan should be completely dropped (Oort, 1947). However, a Netherlands Organization for the Advancement of Pure Scientific Research (ZWO) was founded in 1949, following ideas conceived by Prime Minister Schermerhorn and the Minister of Education and Sciences, G. van der Leeuw, as early as 1945/1946 (Oort et al., 1951: 53). And even before the official start of ZWO, its Director-to-be, J.H. Bannier, strongly supported Oort's plans. On 23 April 1949, the *Stichting Radiostraling van Zon en Melkweg* (Netherlands Foundation for Radio Astronomy, SRZM) was officially founded (it actually had already started work in 1948); its goal was "to investigate the radio radiation coming from outside the Earth." The Board of SRZM was formed by representatives of the astronomical institutes at Leiden and Utrecht, later also Groningen, plus the Post, Telephone and Telegraph Service (PTT), the Physical Laboratories of Philips at Eindhoven, and the Royal Netherlands Meteorological Institute (KNMI) at De Bilt. In fact, in 1946 the solar physicists M.G.J. Minnaert and J. Houtgast from Utrecht Observatory had already joined forces with the KNMI and PTT in studies of the ionosphere, and its relationship with radio propagation and solar activity (Houtgast, 1946; 1949). The new Foundation, chaired by Oort, provided a broad base of knowledge and interest, suitable to administer the large amounts of money required for the construction of the big radio telescope first conceived in 1944. The strong support given by ZWO, PTT and Philips partly compensated the handicaps imposed by the lack of experience and equipment mentioned above (Oort, 1952).

Clearly, finance, design and construction of the envisaged 25-meter dish would still require several years. Meanwhile, however, Ir. A.H. de Voogt, Head of the PTT Central Department for Radio, who was involved in radio studies of the Sun and ionosphere, had salvaged several Würzburg radar reflectors used by the German forces along the coast during the War, and brought these to the Radio Transmitting Station at Kootwijk (cf. Section 5.3.1, below). One of these (Figure 3) was made available to SRZM for studies of Galactic radio radiation. In 1948 Professor C.J. Gorter from the Kamerlingh Onnes Laboratory at Leiden University had provided a war-surplus radio receiver; a student of electronics, H. Hoo, started attempts to measure the 21-cm line using it with the Kootwijk dish. Progress was slow, and on 10 March 1950 a fire destroyed the receiver; many months of development work were lost.

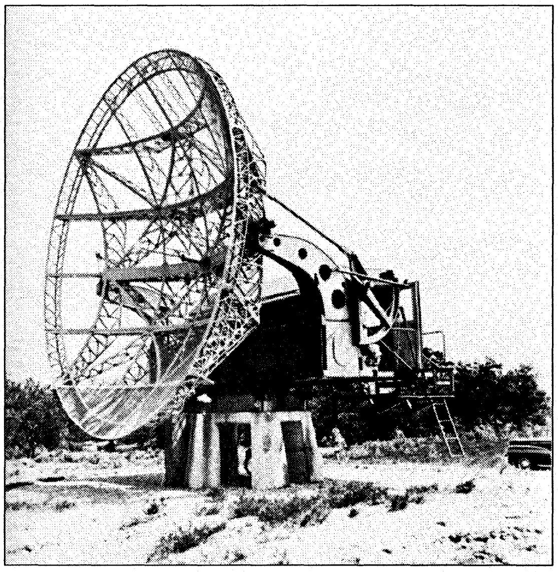

Figure 3: The Würzburg at Kootwijk used for observing the 21 cm HI line between 1951 and 1955.

In December 1950, C.A. Muller, a young engineer fresh from Delft, started development of a new receiver, using equipment made available after the fire by Dr F.L. Stumpers of Philips. Muller (1980: 65-67) recalls:

> On a snowy day in December 1950 I bicycled through the woods from Apeldoorn to Kootwijk-Radio, the central transmitting station of the Dutch Post Office, to start my work for the Netherlands Foundation for Radio Astronomy. Some months before I had finished my studies in physical engineering at the Delft Technical University and this was my first job. I was to continue the work started in 1948 by Mr. Hoo towards the discovery of the 21.2 cm line of neutral hydrogen. This line had been predicted … at a colloquium on "Radio Waves from Space" held in 1944, during the war, at Leiden Observatory.
>
> This colloquium marks the beginning of Dutch galactic radio astronomy. It was probably the first time that professional astronomers discussed the possibilities of radio astronomy and it is obvious that Oort had stimulated this meeting … At the Post Office it was Mr. A.H. de Voogt … who had an interest in solar radio astronomy … He had rescued a few of the 7½-meter Würzburg antennas, which had been part of a German radar chain along the coast during the war, from destruction and had them repaired for radio-astronomical purposes. One of these antennas was made available to the new Foundation for its hydrogen-line experiments. It

> had been placed on the southern slope of a small hill at the Kootwijk-Radio transmitting station, overlooking a beautiful area with heath and woods, but uncomfortably close to high-power transmitting antennas!

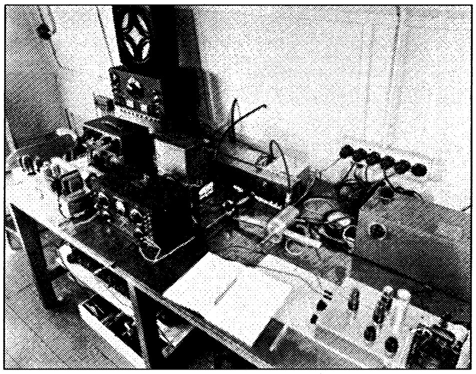

Figure 4: The HI receiver assembled by Lex Muller in 1951 (after Muller, 1980: 68, Figure 2; copyright 1980 by D. Reidel Publishing Company; with kind permission of Springer Science & Business Media).

> Here I started my work with one assistant. Though the experiments had been going on for two years, I had to start almost from scratch because all receiver equipment had been destroyed in a small fire earlier that year. However some parts for a new receiver were already under construction at the Philips laboratories under the supervision of Mr. F.L. Stumpers [see Figure 4]. I knew almost nothing of astronomy or radio astronomy at the time, and looking back it seems a small miracle that some five months later we observed the 21-cm line [Figure 5]. I think this miracle was possible because all the circumstances were favourable for it. The discovery of the line was primarily a technical problem of constructing a suitable receiver, and it was a great help that I was working in the almost ideal surroundings of the small transmitter-construction division at Kootwijk with its group of enthusiastic collaborators, a well-equipped laboratory and a large workshop, which worked quickly and efficiently. With my experience as a fervent radio amateur I fitted quite well into this group. The same group also had experience with equipment for radio astronomy because it had constructed the receivers for solar observations.

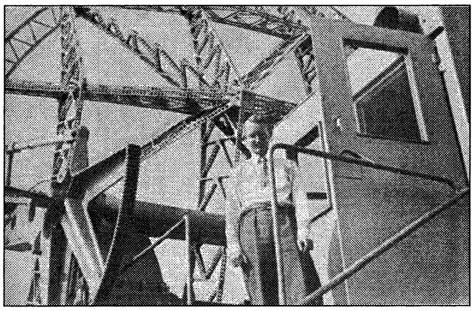

Figure 5: Lex Muller shown on the platform of the Kootwijk Würzburg in 1952 (Raimond, 1952: 141).

> Our discovery of the line was hastened by the fact that Van de Hulst was visiting the United States at the time. At Harvard he met H.I. Ewen, who a few weeks later would make the first observations of the 21-cm line. In a letter which I received early in March, he told me that Ewen was working for his thesis on a hydrogen-line receiver with frequency switching in the second local oscillator. This information came just at the right moment. Our first experiments with part of the receiver had clearly shown that the stability of a simple non-switching receiver would be insufficient, and from the literature I had available, it was clear that it would be necessary to use some form of Dicke receiver in which the input was switched periodically between the antenna signal and the signal from a noise source. A synchronous detector would then measure the difference between the two signals. Then the letter arrived with the proper solution to our problem: the noise-source signal could be replaced by an antenna signal at a different frequency. The construction of the frequency-switched receiver did not take too much time. We modified the Harvard concept somewhat by using frequency-switching in the first local oscillator, by adding a reactance frequency modulator to the 6.4-MHz oscillator of the crystal-controlled Philips-built frequency multiplier. We also added a tunable second local oscillator and a narrow-band second i.f. amplifier as well as a 30-Hz amplifier-synchronous detector section and some calibration facilities, and then we were ready for our first attempts to observe the line. I think it was on the second night, on May 11, 1951, that the line was found.
>
> I remember very little of that night. There is a vague recollection of sitting in the telescope cabin on a nice spring evening, switching the second local oscillator every few minutes between the two frequencies, which in the presence of the line should give alternately a positive and a negative deflection on the recording meter, while a region near the galactic plane drifted through the antenna beam, but that is all. I think that at that time I hardly realized the importance of what I was doing and perhaps I had enough confidence in our equipment to expect the line to show up. At that time I must have known that it was there, because Ewen had already observed it some six weeks earlier.

At Harvard, H.I. Ewen and E.M. Purcell had first detected the line on 25 March 1951, using a horn antenna with a 12° beam. After that detection, Purcell asked H.C. van de Hulst from Leiden and F.J. Kerr from Sydney, who both happened to be at Harvard at that time, to report the detection to their home institutes, and to enquire whether the detection could be confirmed (Kerr, 1984: 137). As a result, reports by Ewen and Purcell (1951) and by Muller and Oort (1951) were published side-by-side in *Nature*, under the heading "Observation of a line in the Galactic radio spectrum", together with a confirming telegram from J.L. Pawsey of the CSIRO's Division of Radiophysics in Sydney, where "W.N. Christiansen and J.V. Hindman had started on a crash program and were able to assemble the necessary equipment and make a detection after a short period of six weeks ..." (Kerr, 1984: 138). As noted by Kerr (ibid.): "This whole episode was a fine example of international cooperation, which has always been the hallmark of the relationships in radio astronomy."

The successful observations at Kootwijk (for details see Section 4) obviously were a strong boost to the plans for a 25-meter radio telescope. ZWO now firmly approved these plans, and made funds available. In November 1951 Werkspoor undertook design studies; the design was completed early in 1954, and construction started the same year, with supervision by Ben G. Hooghoudt (another young Delft engineer) on behalf of SRZM. The telescope was completed, and erected in Dwingeloo, in the summer of 1955. Meanwhile, the Kootwijk Würzburg had mapped the interstellar hydrogen in the Galaxy (see Section 4.2.4).

4 THE KOOTWIJK HI 7.5-METER WÜRZBURG

4.1 Description of the Instrument

The paraboloid employed at Kootwijk for Galactic HI studies was one of several Würzburg reflectors used in the post-war period in The Netherlands (Section 5). The telescope was located at the Kootwijk Radio Transmitting Station of the Netherlands Post and Telegraph Service (PTT), about 15 km west of Apeldoorn, at geographical co-ordinates $\varphi = +52°\ 10'.2$ and $\lambda = -5°\ 50'.7$ (Van de Hulst et al., 1954: 119). In 1948, the dish was made available to SRZM for its research program, and the 1951 detection of the 21-cm line by Muller followed.

From July 1952 to August 1955 the dish was used almost exclusively for hydrogen-line studies of the Galaxy; its main achievements are summarized in Section 4.2. Two brief studies of continuum sources at 1390 MHz (wavelength 21.6 cm) were reported by Westerhout (1956a; 1956b). When, in August 1955, SRZM moved its equipment to the new Radio Observatory at Dwingeloo (Muller and Westerhout, 1957: 151), the 'HI Würzburg' was left behind at the Kootwijk Radio Station; at Dwingeloo, two other Würzburgs were installed (Section 5). In April 1956, at Dwingeloo, Van Woerden was told that the 'Kootwijk HI Würzburg' had broken down; the telescope was probably scrapped (cf. Section 5).

The properties of the HI Würzburg and the receivers used with it are well documented in various research papers (Kwee et al., 1954; Muller, 1956a, 1956b; Muller and Oort, 1951; Muller and Westerhout, 1957; Van de Hulst et al., 1954). The focal length was 1.7 meter. The beamwidth between half-power points was originally reported as 2.8° (Muller and Oort, 1951). The later papers listed above mention widths (FWHM) of 1.9° (or 1.85°) in the horizontal and 2.7° (or 2.78°) in the vertical plane. The telescope was fixed to an altitude-azimuth mount. In March-May 1952, before the first major HI-line survey started, Van Woerden had calibrated the axes and coordinate scales; as a result, the telescope could be pointed to an accuracy of 0.1° (Van de Hulst et al., 1954: 119). Telescope settings had to be done by hand, and no automatic guiding was available. Since the recording of a full line profile (intensity versus frequency) at a fixed position in the sky took a few hours, this required many manual setting corrections (at 2.5-minute intervals!); Westerhout (2002: 27) gives a vivid, witty description of the operations, in which dozens of students were involved.

The receiver developed by Muller was improved several times throughout the years 1951-1955; Figure 6 compares line profiles obtained in the same direction, but at different stages of improvement. Muller and Westerhout (1957) give an extensive description and illustration (see Figure 7) of the final three-stage superheterodyne receiver. The detection paper (Muller and Oort, 1951) mentions a 'noise factor' of about 25, which is probably equivalent to a system temperature of about 7,000 K. The first major survey (Van de Hulst et al., 1954: 120) was done with a noise figure N of 10 ($T_{\rm sys}$ about 2,600 K); the second major survey ended with N = 6.0 ($T_{\rm sys}$ ~ 1,500 K) (Muller and Westerhout, 1957: 155). In order to obtain maximum stability, and following (but modifying) the principle developed by Dicke (1946), switching between two frequencies (in the first local oscillator, see Section 3) was employed, and the difference signal recorded. The roles of the two frequencies, serving as signal frequency and comparison frequency, were interchanged every few minutes in the second local oscillator. For the first measurements, the switch rate was 30 times per second, and the two frequencies were only 110 kHz (i.e. 23 km/sec) apart. The major surveys were done with a switch rate of 430 or 400 Hz, and the frequency difference was raised first to 648 kHz = 137 km/sec, and later to 1,080 kHz = 228 km/sec. The frequency resolution of the profiles was about 40 kHz, or 8 km/s; in 1951, it was 25 kHz. Both Van de Hulst et al. (1954) and Muller and Westerhout (1957) give extensive discussions of the frequency and intensity scales of the observations.

The story of Kootwijk has been told in many places. The film about the construction of the Dwingeloo Telescope made by Herman Kleibrink (1957) also deserves mention in this connection.

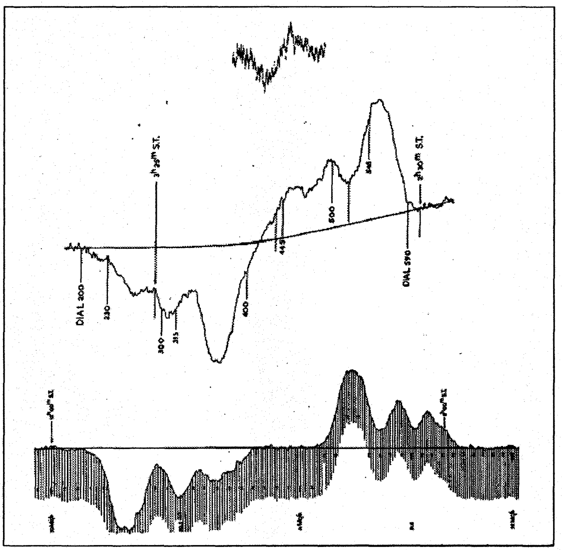

Figure 6: HI profiles of a point in the Galactic Plane at 50° longitude (old system) obtained with the Kootwijk Würzburg in (from top to bottom) 1951, 1952/1953, and after November 1953. The frequency scale is the same, but the band width for Dicke switching was increased twice (see text). As a result of the switching in frequency, each profile appears twice, positive and negative. The frequency of the lowest profile increases to the right, opposite to the one above it (after Van de Hulst et al., 1954: 123)

4.2 Scientific Achievements of the Kootwijk HI Würzburg

This section intends to summarize the main scientific results obtained with the Kootwijk HI Würzburg. It briefly mentions related work done elsewhere (at Harvard and Sydney), without any attempt at completeness. During its few years of scientific use, the Kootwijk HI Würzburg was extremely productive. Its studies of Galactic 21-cm line radiation were ground-breaking indeed; they included the first maps of the distribution of Galactic neutral hydrogen, and the first rotation curve of the Milky Way Galaxy.

4.2.1 First Results, and a New Receiver

Even the very first paper (Muller and Oort, 1951), based on only a few weeks of preliminary measurements, yielded results of fundamental importance. Drift scans across the Galactic Equator, at various constant declinations, indicated latitude distributions of about 8° width, and upon discussing the same material Van de Hulst (1951) stated that intensities fell to half the maximum value at latitudes of ±4°–8°. Since the random motions in the gas appeared to be of order 5 km/s, allowing distances from the Galactic Plane no greater than about 50 pc, the hydrogen radiation had to come from distances less than 500-1000 pc, suggesting that the 21-cm line becomes opaque within this short distance. Ewen and Purcell (1951) had come to the same conclusion, on the basis of their measurements of spin temperature and line width, combined with an estimate of the average hydrogen density. From a measurement of line width at a longitude 30° away from the Galactic Centre, Muller and Oort estimated a rotation speed of 190 km/s. At the same position, hydrogen with a radial velocity of + 55 km/s, with an estimated distance of 8 kpc, was found to have a very narrow latitude distribution. At positions like these, clearly the differential Galactic rotation prevented the line from becoming optically thick.

After the first few weeks of exploratory observations, Muller felt that a much better receiver was required. He dismantled his receiver and completely rebuilt it. After a year the first major survey could be started. Muller (1980: 68-69) recalls:

> The year between the discovery of the hydrogen line and the beginning of the first survey I remember as a most productive period in my life. It meant understanding and solving all problems of a new technique. We learned how to build suitable components for a radio astronomy receiver and we learned to cope with strong interference from the nearby transmitters. It laid the foundation for further receiver developments in later years, in which a steady improvement in sensitivity and stability took place as well as a gradual development towards multi-channel receivers. It was not until many years later that I realized how difficult the long delay between the discovery and our first systematic observations must have been for Jan Oort. I still appreciate very much the fact that he never showed any impatience or annoyance, but just had confidence in us and gave his full support. This attitude towards us in those early days and in fact during all the years I worked with him in radio astronomy is, I think, typical of his attitude towards the instrumentalist in his field of science.

4.2.2 The First Survey: Spiral Structure

In June 1952 the first major survey was started, consisting of profiles (intensity versus frequency) taken at fifty-four positions, at 5° intervals in Galactic longitude along the Galactic Equator (Van de Hulst, 1953). A quick analysis of the first results allowed Oort to derive locations of hydrogen concentrations in the Galactic Plane; these gave hints of spiral arms—a major success for Oort in September 1952, which he was able to report at the IAU General Assembly in Rome.

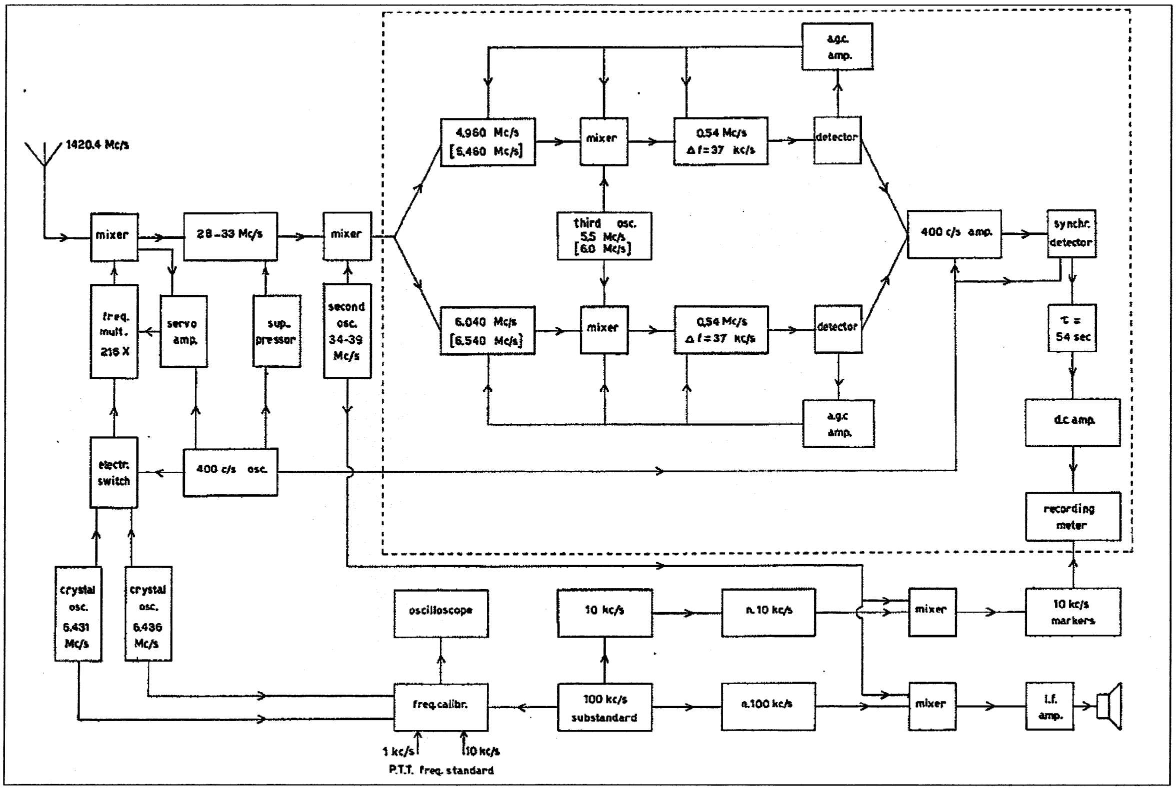

Figure 7: Block diagram of the HI receiver used in 1953-1955 (after Muller and Westerhout, 1957: 153).

A hint of spiral arms had also been found by Christiansen and Hindman (1952), from a three-month survey (1951 June-September) immediately following the observation confirming the first detections (Section 3). The aerial used for this survey was a 25-m^2 section of a paraboloid, with a beam of 2.3° FWHM. The receiver was switched 25 times per second between two frequencies 160 kHz apart and had a passband of 50 kHz width. The survey covered a wide strip of 270° length along the Galactic Equator. Line doubling at (old) longitudes between 170° and 240° suggested the presence of two spiral arms. Line intensities were found to vary along the Galactic Equator, reaching a maximum $T \sim 100$ K near the Galactic Anticentre. Enhanced intensities were found away from the equator, in the Taurus and Ophiuchus dark clouds. A thickness of 250 pc was found for the hydrogen layer.

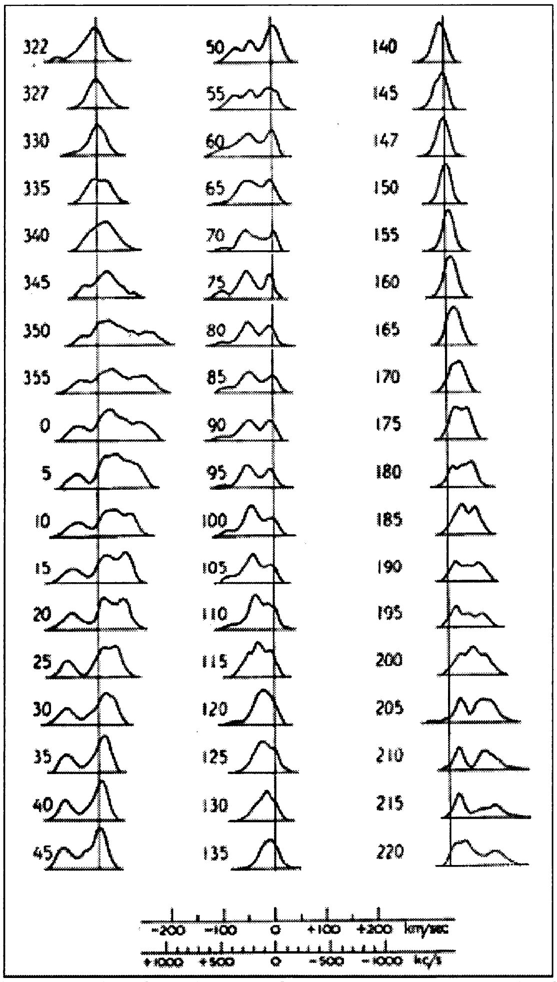

Figure 8: HI profiles along the Galactic Equator, at intervals of 5° in (old) longitude as indicated. The frequency and velocity scales are shown at the bottom (after Van de Hulst et al., 1954: 125).

In his Halley Lecture in Oxford on 13 May 1953, Van de Hulst (1953) presented a first complete set of results from the Kootwijk survey along the Galactic Equator. After a crisp outline of the advantages and disadvantages of radio as compared to optical astronomy, he discussed the various causes of Doppler shifts in the 21-cm line: the orbital motion of the Earth around the Sun, the motion of the Sun relative to its surroundings, the thermal and turbulent motions in the gas, and the differential Galactic rotation. Before applying the differential rotation to derive locations of gas from the measured radial velocities, corrections for the various shifts were required. Also, measurements had to be brought to common intensity and frequency scales, and averaged: the fifty-four profiles shown were the result of four hundred noisy tracings. Meanwhile, the set of profiles in Figure 8 was the result of a crash effort of about a dozen students and Observatory personnel, sitting together in the Observatory's lecture room and working hard for a week in April under Van de Hulst's guidance. Using preliminary values for the rotation constants, the intensity maxima and minima in the profiles could be interpreted as maxima and minima in the density distribution of hydrogen in the Galactic Plane; the resulting map clearly showed two spiral arms, and Van de Hulst noted that their location agreed well with the sections of spiral arm found by Morgan et al. (1952) from HII-regions and bright OB-stars.

The full results of the Galactic Equator survey were published by Van de Hulst, Muller and Oort (1954). The paper discusses in detail the receiver properties and observing procedures, the frequency and intensity scales, the correction for standard solar motion, and the use of differential Galactic rotation to locate positions in the Galactic Plane corresponding to maxima in the line profiles. In deriving densities at these locations, the profiles were corrected for the smoothing effects caused by random cloud motions; these motions were assumed to follow an exponential distribution, with an average velocity (in one coordinate) of 8.5 km/s. Figure 9 shows the density distribution of atomic hydrogen in the Galactic Plane so derived. This map contains only the parts of the Plane accessible to observation from The Netherlands, and lying outside the 'solar circle', i.e. the circle through the Sun around the Galactic Centre; inside the solar circle a distance ambiguity still had to be resolved. Nevertheless, for a few years this diagram was considered *the* map of spiral structure in the Galaxy.

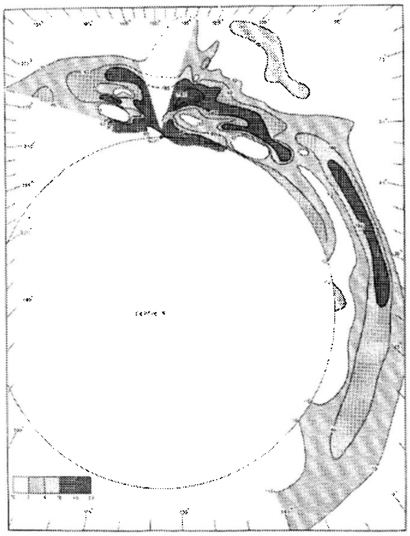

Figure 9: The first HI map of the Galaxy outside of the solar circle, showing spiral structure, with contour shading to indicate "points of equal density" (after Van de Hulst et al., 1954: 146).

After completion of the Galactic Equator survey in June 1953, Muller again made many major improvements to the receiver. In fact, the new measurements in late 1953 were so much better that Oort said to Van de Hulst (as recalled by one of us [HvW]): “We cannot publish the old stuff; the new material is so much better!” But Van de Hulst answered: “Are you mad? It will be years before we have the new results complete.” Fortunately, Van de Hulst won this brief debate.

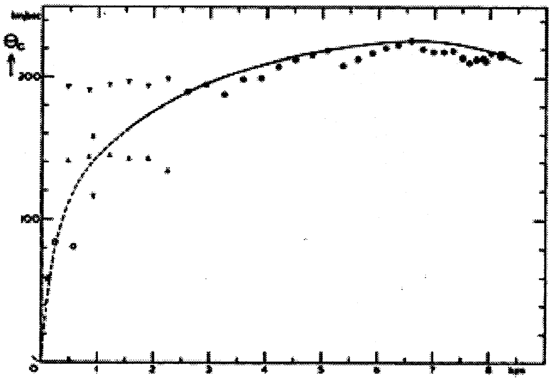

Figure 10: First rotation curve of the Galaxy, which is nearly flat from 3-8 kpc (after Kwee et al., 1954: 219).

4.2.3 The Galactic Rotation Curve

The first major project with the new receiver was a solid determination of the rotation curve of the Galaxy (Kwee et al., 1954). At various fixed declinations and frequencies, drift curves were obtained, giving the position and the width of the layer of neutral hydrogen. Since in the first Galactic quadrant (that is, at longitudes between 0° and 90° on the current system) the maxima of these drift curves generally fell close to latitude −1.5°, a correction of that order to the Galactic Pole assumed by Ohlsson (1932) was indicated, and the derivation of a rotation curve was based on a set of profiles taken at (old) latitude −1.5°, at 2.5° intervals in longitude between old longitudes l = 320° and 45° (with the Galactic Centre lying near longitude 328°). Assuming hydrogen to be present everywhere in the disk, and moving in circular orbits, with angular velocities decreasing monotonically towards greater Galactocentric distances, the highest radial velocity in each profile would come from the point on the line of sight lying closest to the Centre (the ‘subcentral point’), and in combination with the rotation speed at the Sun’s position, this highest velocity in the profile would yield the rotation speed at that subcentral point. Before this method could be applied, the profiles had to be corrected for beam smearing, for extinction and background continuum radiation, for optical-depth effects, and for smoothing by random cloud motions. Figure 10 shows the rotation curve obtained: almost flat at 200-220 km/s between 3 and 8 kpc from the Centre; the minor dips were ascribed to a lack of hydrogen at the subcentral points. At longitudes less than 20° from the Centre, long profile wings were found, and interpreted as turbulent motions (of order 50 km/s and more) at distances less than 3 kpc from the Centre.

From the rotation curve derived by Kwee et al. (1954), Schmidt (1956) constructed a model for the mass distribution in the Galaxy. This model has a total mass of (only) 7 × 10^{10} solar masses; the escape velocity at the Sun exceeds the local rotation speed by (only) 70 km/s. The major difference with later mass models lies in the fact that Schmidt had no data on rotation outside the solar circle, and assumed that the mass distribution declined strongly there.

4.2.4 The Second Survey: Three-dimensional Mapping

The second major Galactic 21-cm line survey started in November 1953 and was finished in August 1955. It covered a strip along the Galactic Equator from (old) longitude 320° to zero and on to longitude 220°. Over most of this strip, the longitude spacing was 2.5°, and it reached out to latitudes +10° and −10°, in steps of 2.5°. The catalogue of 21-cm line profiles resulting from this survey and published by Muller and Westerhout (1957) contains profiles for 694 positions. With the survey going on twenty-four hours per day, and manned by dozens of volunteer students, the total observing time for this programme was 7,500 hours; the reductions took more than 23,000 man-hours; in total, about forty-five people were involved in observations or reductions. Muller and Westerhout give extensive discussions of the antenna and receiver (see Section 4.1), of the calibration of frequencies and intensities, of extinction and sky radiation, of observing procedures and of velocity corrections.

The survey just discussed required intensity scanning in three coordinates: Galactic longitude and latitude, and frequency. The image is blurred in *l* and *b* by the limited angular resolving power of the telescope (‘beam smearing’), and in frequency by the finite bandwidth of the receiver. Transformation of frequency into distance from the Sun, through differential Galactic rotation, involves further distortion by the deviations from circular motion, for instance by random cloud motions. Ollongren and Van de Hulst (1957) developed methods to correct for these various smearing effects, which were used in the analysis of the profiles by Westerhout (1957) and Schmidt (1957).

Westerhout (1957) derived the distribution of atomic hydrogen in the outer parts of the Galactic System from 620 profiles between (old) longitudes 340° → 0° → 220°, latitudes −10° and +10°. In correcting for profile smoothing by random cloud motions, he assumed a Gaussian distribution with dispersion 6 km/s. From the measured intensities, optical depths were calculated assuming a temperature of 125 K; no evidence was found for important deviations from this value. In calculating distances from (corrected) radial velocities, a rotation law was used, based on the final mass model adopted by Schmidt (1956). The results (density as a function of distance from the Sun and from the Galactic Plane) are displayed in a series of seventy-one cuts perpendicular to the Plane. (Since the mass model in the outer parts is open to question, so are these cuts.) The maximum densities found in each vertical column were projected on one plane; the resulting colour plate (Westerhout, 1957, Plate B; cf. Figure 12 below) displays these and represents a partial ‘face-on view’ of atomic hydrogen in our Galaxy (but without integration along the vertical lines!). Within 8 kpc from the Centre, the mean plane of the hydrogen turned out to be very well-defined, and flat to within 100 pc, but the outer parts showed deviations exceeding 300 pc. The Pole defined by the hydrogen plane in the inner parts was found to lie at longitude 322°, latitude +88.56° on the old (Ohlsson) system.

The distribution of hydrogen in the inner parts of the Galactic System was determined by Schmidt (1957). He used a complete grid of line profiles spaced by 2.5° between (old) longitudes 340° and 40°, and by 2.0° between latitudes −5.5° and +2.5°, plus 215 drift curves at constant declination, spaced by 40 kHz in frequency, and crossing the Galactic Equator at longitudes spaced 5° between 340° and 35°. Lines of sight through the inner parts of the Galaxy, inside the solar circle, contain pairs of points at the same distance from the Centre. Hence, gas with a certain observed radial velocity may lie at either or both of these points. Separation of the contributions from these two points can in principle be obtained from the latitude distribution of the radiation, if the linear distribution perpendicular to the Plane is known. This *z*-distribution was measured at the tangent points (or 'subcentral points'), where the radial velocity in a profile reaches a maximum and the distance ambiguity vanishes. At Galactocentric distances exceeding 3 kpc, Schmidt found layer thicknesses averaging 220 pc, and showing surprisingly little variation. Applying similar corrections as Westerhout (1957) for beam and band smearing, continuum radiation, optical-depth effects and random cloud velocities, Schmidt derived maximum hydrogen densities and mean *z*-values for vertical columns through 808 points in the Galactic Plane. These results were incorporated in the colour plate mentioned above. Schmidt also discussed the spiral arms found in the inner parts of the Galaxy.

The above results had a major impact on two symposia held in August and September 1955 on "Radio Astronomy" (Van de Hulst, 1957a) and on "Comparison of the Large-Scale Structure of the Galactic System with that of other Stellar Systems" (Roman 1957), in the new series of IAU Symposia that started in 1953. Another prominent publication was that by Oort (1956) in *Scientific American.*

4.2.5 North and South Combined: The Galactic System as a Spiral Nebula

The surveys mentioned and results obtained applied, of course, only to the parts of the Galaxy visible from Kootwijk. Following the early work by Christiansen and Hindman (1952) discussed above, new surveys of the southern parts of the Milky Way were undertaken by Kerr and collaborators from the Division of Radiophysics, CSIRO, using an 11-meter reflector, movable in the meridian only, at Potts Hill near Sydney; its beamwidth was 1.5° at 21 cm. In a discussion of the large-scale structure of the Galaxy, Kerr et al. (1957) published a composite spiral diagram, combining Leiden and Sydney data. In the regions of overlap the profiles agreed quite well, although differences in approach were found to lead to differences in the hydrogen distribution in the two halves of the map. In the outer parts of the Galaxy, the gas layer showed a systematic distortion, twisting downward on the southern and upwards on the northern side. Kerr (1957) considered whether this twist might be due to a gravitational tide caused by the Magellanic Clouds, but noted that the observed effect was much too large for a simple gravitational explanation. The full results of the survey were published by Kerr et al. in 1959.

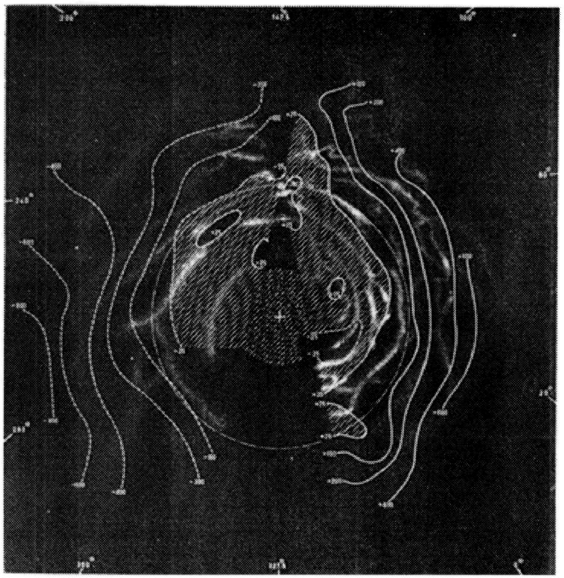

Figure 11: Contours showing deviations in the HI layer from a plane, the first evidence for a Milky Way warp. The HI distribution is shown as gray shading. The Galactic Centre is shown by +, and the location of the Sun by ⊙ (Oort et al., 1958: Plate 5).

In 1957, Kerr spent several months at Leiden working on a map that combined the northern and southern results. In a joint publication, with the title "The Galactic System as a Spiral Nebula", Oort, Kerr and Westerhout (1958) compared the rotation curves derived from both sets of data and found satisfactory agreement; the differences were less than 10 km/s, on average. They further emphasized the great flatness of the inner parts of the hydrogen disk. Figure 11 shows the systematic deformation of the disk in its outer parts: an upwards warp on one side, downwards on the other side. Figure 12 gives the combined hydrogen distribution projected on the Plane. Comparison with the results of Westerhout (1957) and Schmidt (1957) shows that the structures found from the Australian and from the Dutch observations differ in character. Oort et al. (1958: 382) have written about this:

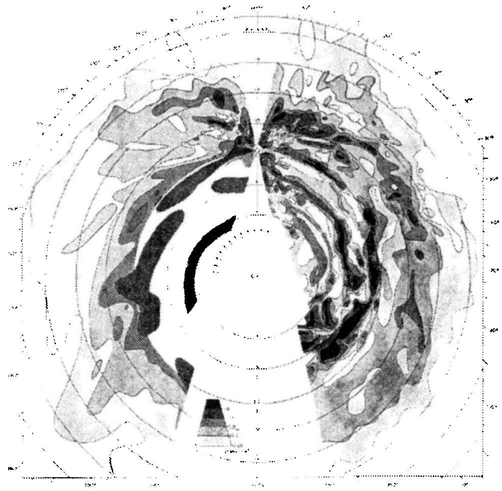

Figure 12: The complete map of HI distribution in the Galaxy, combining the northern (Kootwijk) and southern (Potts Hill) data. The maximum densities in the *z*-direction have been projected onto the Galactic Plane. The projection and symbols as in Figure 11 (after Oort et al., 1958: Figure 4).

> It should be emphasized that the distribution obtained depends considerably on the resolving power and on the particular assumption regarding the velocity dispersion, temperature of the gas, circularity of the average motion, etc. We believe that the diagram gives the general pattern fairly well, but the densities must be considered very uncertain, in some cases by a factor of two or more. The relatively wide beams efface the detail structure of the interstellar medium and are suitable for observing the large-scale features with which the present report is concerned.

However, there appears to be more to the difference. At an after-dinner speech in Penticton in 2001, Westerhout (2002) said:

> … the drastic correction for random cloud velocities really sharpened up the line profiles, and provided the considerable details in the final map. The difference is clear when you compare the Northern and Southern spiral structure in the Oort, Kerr and Westerhout review … There is much less detail in the Southern part. Kerr refused to correct for random cloud velocities, calling that "arbitrary". Of course it *was* a very rough treatment, but it certainly helped highlighting all the minute details in the line profiles.

Hence, the difference between North and South might have been largely due to different reduction procedures. Nevertheless, Kerr (1962), in a later detailed comparison of the Leiden and Sydney surveys, noted that the Leiden velocity model led to an implausible spiral structure diagram on the southern side, and considered alternative models in which the structure and motions of the Galaxy would be symmetrical on a large scale. He found that an outward velocity component of 7 km/s for the Sun and the Local Standard of Rest would reconcile the results on the two sides of the Galaxy.

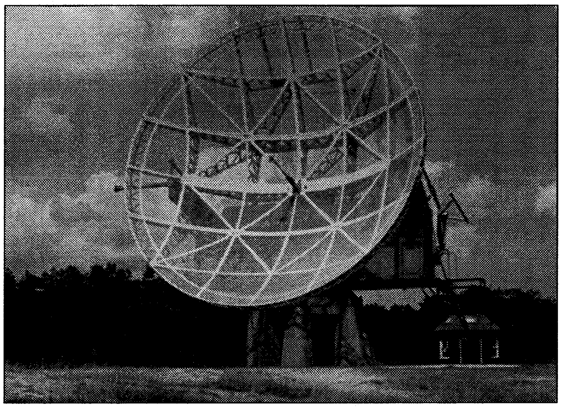

Figure 13: The Würzburg used for the HI observations at Kootwijk, seen from the front of the dish (photo by A.C. Hin).

4.2.6 New Galactic Coordinates

The systematic deviations of the hydrogen layer from the Galactic Equator defined by Ohlsson (1932) prompted the IAU to define a new Galactic Coordinate System. A Sub-Commission 33b, appointed by the Dublin General Assembly in 1955, investigated "… the desirability of a revision of the galactic pole and of the zero point of galactic longitude …", and reported to the Moscow General Assembly in 1958. This Assembly endorsed a resolution reproduced in the final report of the Sub-Commission (Blaauw et al., 1960). The main clauses of this resolution were: (a) adoption of a standard system of Galactic coordinates for which the Pole is based primarily on the distribution of neutral hydrogen in the inner parts of the Galactic System; (b) the zero of longitude to be chosen near the longitude of the Galactic nucleus; (c) authorization for Commission 33b to define the exact values of the coordinates of the Pole and of the zero of longitude. The paper by Blaauw et al. (1960) gives the precise definitions of the Galactic Pole and of the zero of longitude. The final report of the Sub-Commission included four papers (Gum et al., 1960; Gum and Pawsey, 1960; Blaauw, 1960; and Oort and Rougoor, 1960) which discussed various details relating to the main issue. It is clear that the definition of the Galactic Pole (and hence, of the Galactic Equator) was essentially based on the 21-cm line observations carried out at Kootwijk and Potts Hill.

4.2.7 Other Developments

One more paper based on Kootwijk HI observations deserves mention. Lindblad (1966) presented isophote maps in velocity-latitude planes, based on the observations published earlier by Muller and Westerhout (1957) and by Westerhout (1957). These isophote maps show the velocity distribution of atomic hydrogen in the outer parts of the Galactic System, without assuming a velocity-distance relation based on an uncertain model of Galactic rotation in the outer parts of the Galaxy.

At Harvard, after the initial detection of the 21-cm line by Ewen and Purcell, Ewen obtained his doctorate, and no further hydrogen-line work was done for a few years. Starting in 1953, Bok and his students undertook various studies of special regions such as dark clouds, stellar associations and star clusters. These studies, however, fall outside the scope of the present review.

5 WÜRZBURG ANTENNAS USED IN DUTCH RESEARCH AFTER 1945

5.1 The Situation in Europe just after the War

When the war ended in Europe in May 1945, there was considerable interest among radio engineers in German radar technology. Martin Ryle, for example, was sent to Germany in a Major's uniform to appropriate any equipment he thought might be useful (Robinson, 1999: 66). Among the items he collected was a pair of Würzburg antennas which were later used at Cambridge to determine source positions (Ryle and Smith, 1948). The Würzburg 7.5 m parabolic antenna, with its alt-azimuth mounting, was a legacy of the German war machine which would be employed in the nascent radio astronomy effort throughout Europe and even overseas. Used during hostilities as a 54 cm wavelength radar antenna for aircraft location, the reflecting surface and pointing accuracy were sufficient for 20 cm use. The 'Würzburg-Riese' (or '-giant' [FuMG65]; there was also a 3 m diameter version, the 'Dora' [FuMG39/62]) became a valuable instrument for radio astronomers in countries such as England, France, the Netherlands, even Sweden (which had been neutral in the war) and America (Robinson, 1999: 66).

The Dutch study of the 21 cm neutral hydrogen line in the first half of the 1950s amounted to what was arguably the greatest research achievement of any Würzburg antenna. (In this paper, 'Würzburg' is used to refer to the 7.5 m version of the reflector.) Its accomplishments would rightly earn the 'Kootwijk HI Würzburg' a unique place in astronomical history, and one might be unaware that it was not the only such instrument used in the Netherlands after the war. There may have been as many as eight (and probably at least six) 'Dutch' Würzburgs pressed into scientific service by a variety of research organizations. All came from the Zeppelin Factory in Friedrichshafen, where an estimated 2,000 or more were produced between 1941 and 1945, and they had seen service in the *Atlantikwall*, the German defence line which stretched along continental Europe's west coast from France to Norway. All but two of the appropriated 7.5 m reflectors had been abandoned in the Netherlands as the occupiers surrendered. Although most of the forty or so Würzburgs which were used in Holland during the war were scrapped after 1945, a handful was rescued. The rest of this paper will attempt to recontruct their history. A very useful source of information is an article which endeavours to locate the Kootwijk HI Würzburg (Beekman, 1999); it will be regularly cited below and referred to simply as 'B99'.

5.2 The TNO Würzburg near The Hague

The Würzburg with the best-documented history was acquired and used by the Netherlands Organisation for Applied Scientific Research (TNO) (B99). During the war it was part of the radar installation on the (Dutch) island of Rozenburg (German radar stations would have had a number of antennas for locating targets and guiding their own fighter aircraft, usually including two Würzburgs). In 1947 it was moved to the Vlakte van Waalsdorp on the northern outskirts of The Hague (and near the present TNO headquarters) for experimental use by the Physics Laboratory of the Netherlands State Defence Organisation (*Rijksverdedigingsorganisatie*, which later became a TNO division). At first the radar was used to study the propagation of radio waves in collaboration with SRZM (*Stichting Radiostraling van Zon en Melkweg*, the Netherlands Foundation for Radio Astronomy).

It seems to have only once been used for astronomical research, in June 1954, when a partial solar eclipse was observable from the Netherlands. The measurements were carried out and published by Seeger (1955), who observed the entire eclipse (three-quarters of the photosphere was occulted) at 400 MHz. The resulting occultation curve was smooth, and an observed asymmetry was discussed and interpreted. The antenna was later used for receiving signals from weather and communication satellites. In 1965, the Moon was used as a reflector to transmit telegraph signals between the TNO Würzburg and Puerto Rico (B99).

What finally happened to it has also been documented (B99). In 1977 the antenna was donated to the German Air Force (*Luftwaffen-*) Museum, then in Appen (north of Hamburg) where it was on display at the Marsielle-Kaserne. In 1994 it moved again when the Museum was rehoused at the General-Steinhoff-Kaserne near Berlin, though it first had to be restored at an airbase near Hannover. It should have gone on display in 1999.

5.3 The Würzburgs in Kootwijk, Nederhorst den Berg and Dwingeloo

The clear and well-documented story of the TNO Würzburg contrasts with the complex and in some respects uncertain saga of the remaining antennas operated by SRZM and the PTT. This is largely because most were moved at least once between the three observing stations, and records were either not kept, or later disappeared. About one fact there is general agreement: the initiative to acquire the PTT Würzburgs came from Ir A.H. de Voogt; see, for example, Muller (1980: 65-6). In his PTT function, De Voogt was in charge of the transmitting and receiving stations for communication with the Dutch colonies in particular. After the war he decided to launch a research programme to understand how the ionosphere influences radio propagation, and the effect which solar activity has on it (De Voogt, 1952; Muller, 1980: 65). This appears to have been his motivation for acquiring a number of Würzburgs in about 1947 and installing them at the PTT stations. A brief sketch of De Voogt's life is given in Appendix 10.2.

5.3.1 The Transmitting Station at Kootwijk

PTT radio transmissions to Indonesia and Surinam were broadcast from near Kootwijk (a village west of Apeldoorn) starting in the early 1920s. After 1945, wartime damage was repaired (several of the antenna masts had been blown up), and the transmissions resumed. It was about this time that, according to most former Kootwijk employees contacted (B99; based on interviews made in 1998), four Würzburgs arrived at the station. Three were complete, but the surface of one was damaged. It was replaced with a new reflector of 10 m diameter constructed by the PTT. The three unmodified Würzburgs were placed on concrete foundations at a location on the southern side of the original transmitter terrain. The easternmost of the trio was made available to the new astronomical organisation SRZM by De Voogt in 1948, and was used for the HI work (Figure 13). The antenna with the new 10 m dish was installed some 1,500 m to the southeast (B99). While this seems to be the most likely scenario, there are some former employees who can only remember two 7.5 m Würzburgs (in addition to the 10 m reconstructed one).

Whatever their exact number, the antennas were all used for research until at least 1955. In August of that year, the HI group (belonging to SRZM) moved to Dwingeloo, where the 25 m dish was nearing completion. Sometime in the following years (and probably before 1958; see next section), two 7.5 m Würzburgs and the 10 m one were moved from Kootwijk to the PTT receiving station at Nederhorst den Berg, where solar radio research had been actively pursued since 1951. The main reason for the move was almost certainly the hostile radio environment of a high-power transmitting station. The HI team had been more than happy with the transfer to the benign forest near Dwingeloo, Kootwijk being "... uncomfortably close to high-power transmitting antennas ..." (Muller, 1980: 66), and their departure may have triggered the more substantial removal of antennas. Certainly by 1963, the Kootwijk 'HI-Würzburg' had disappeared (B99: 155; a photograph taken by A.C. Hin shows a vacant concrete foundation).

In light of the interference climate, one might wonder why the Würzburgs were installed at Kootwijk in the first place. One reason may have simply been the large terrain available there, over which De Voogt had complete control. In addition, there was an electronics development workshop where antennas and receivers could be constructed and repaired. This infrastructure was also extremely useful to the SRZM team (Muller, 1980: 66). Finally, the severity of the problem on the one hand, and the exact nature of the research to be carried out on the other, may not have been clear when the original location was decided upon; the HI research would probably not have been anticipated, for example.

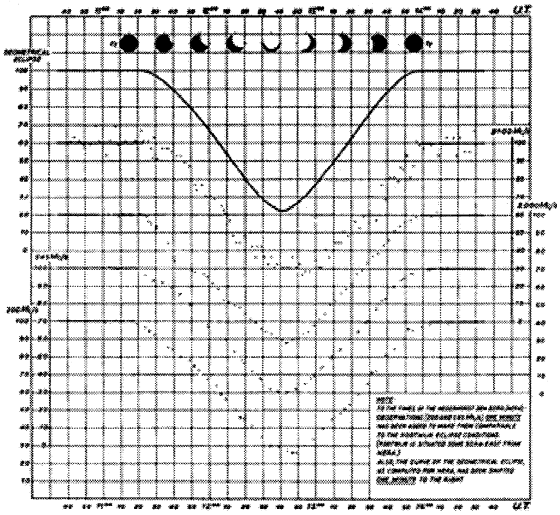

Figure 14: The solar eclipse of 30 June 1954, as observed at NERA (200 and 545 MHz) and Kootwijk (3 and 9.1 GHz) by Fokker et al. (1955). The four curves (dots) show the change in solar radiation as a function of time.

The results of the HI work at Kootwijk, published in a series of articles, have been discussed above (in Section 4.2). Very little seems to have come out of observations with the other Würzburgs there, and nothing appears to have been published. De Voogt (1952) does mention Kootwijk, but the only result shown, the intensity of background radiation as a function of RA at Dec = 41.5° for the wavelength 6 m, came from an entirely different antenna there. The solar results presented nearly all came from Nederhorst den Berg. It seems likely that experiments were done to monitor the Sun, and investigate ionospheric effects, but presumably the results were either compromised by the interference situation, or not of sufficient scientific interest.

5.3.2 The PTT Station at Nederhorst den Berg

Until 1950 the PTT operated a receiving station for overseas radio communication at Noordwijkerhout, near the coast north of the town of Leiden. It then moved to Nederhorst den Berg (southeast of Amsterdam), a facility also under the direction of De Voogt. This station would, like Kootwijk, be host to a number of Würzburg antennas, certainly with one from the start of operations. If it too was procured before 1948, as seems probable, it must have been stored somewhere for a couple of years. Perhaps it was simply moved from Kootwijk around 1950 (and this might explain the confusion of former employees as to the number of 7.5 m Würzburgs in Kootwijk). Nederhorst den Berg became the main radio observatory for solar research in Holland in the 1950s, and operated under the acronym NERA (NEderhorst den Berg RAdio). Among the references to the instruments at NERA, De Voogt (1952: 211) himself describes the 'Würzburger' of 7.5 m diameter, "… just as this is used at Nederhorst den Berg and Radio-Kootwijk."

There was certainly one Würzburg operating at NERA from as early as 1951. De Voogt (1952: 209) himself, describing observations with a parabola, writes, "… at Nederhorst den Berg there are two receivers in service, one at 200 megaherz and one at 140 megaherz." This most likely refers to two receiver systems mounted in a single Würzburg (and also used from 1952 by Fokker and De Feiter, 1954b). An independent source of information on the instruments used at NERA in the early 1950s is Table 1 in the doctoral thesis of Fokker (1960: 3), which lists a "7.5 m parabola" used for 200 MHz solar monitoring from 1952 until May 1957, and a "7.5/10 m parabola" used at 545 MHz from 1953 until December 1958 ("7.5/10 m" presumably means that separate antennas were used for monitoring at different times). This most likely refers to the same 7.5 m Würzburg rather than two separate ones. Another source of information comes from the radio monitoring published in the Quarterly Bulletin on Solar Activity (QB), which is also consistent with one Würzburg at NERA between 1951 and 1954. A more detailed discussion of all these facts, and reproduction of the relevant parts of Fokker's Table 1, can be found in Appendix 10.3.

As noted in the previous section, after 1955 most (if not all) of the parabolic antennas in Kootwijk were moved to NERA. Fokker (1960: 3) records the presence of two 7.5 m parabolas and a 10 m one for 200 MHz interferometry from February 1958. They were almost certainly the Kootwijk antennas, and they must have moved by 1957. By the late 1950s then, NERA was probably making use of three 7.5 m Würzburgs, three 10 m parabolas (partially) constructed by the PTT, as well as a number of smaller instruments for solar radio observations.

Most of the research done with the Würzburgs at NERA involved the Sun, and it was published in a variety of ways. Solar monitoring data appeared monthly in the QB, starting in July 1951 at 140 MHz (QB95). By August and September, bursts were also being reported at 73, 200, 255 and 545 MHz; being much stronger, they could be monitored with simple Yagi antennas. Daily monitoring continued throughout the 1950s and 60s, with peak activity during the International Geophysical Year (1957-1958). There were also research articles published in standard astronomical journals and conference proceedings.

De Voogt's department, the IRA, also issued its own reports in the years 1954 to 1958. The work published there included a 2-year study of the quiet Sun (Fokker and De Feiter, 1954b), a classification of solar storms (Fokker, 1954) and observations of polarized emission (Neubauer and Fokker, 1958). The June 1954 eclipse observed by Seeger (see Section 5.2) was also monitored with the NERA Würzburg at 200 and 545 MHz, and at Kootwijk with 1.2 m (3 GHz) and 60 cm (9.1 GHz) dishes (Fokker et al., 1955). The resulting eclipse curves (Figure 14) were all smooth, symmetrical and rather similar. There was also non-solar research, like a study of the scintillation of Cygnus A (Van 't Veer, 1956). Fokker and De Feiter (1954a) mapped the background radiation at 140 MHz (see Figure 15).

5.3.3 The Dwingeloo Radio Observatory

In addition to the 25 m parabolic dish, two Würzburgs were also used at Dwingeloo from its inception, but they were different from the others described above in several ways (B99; Hooghoudt, 1957). They were originally part of the *Atlantikwall* in Norway, located on one of the coastal islands. In 1952 the Norwegian Government sold them to the Netherlands for the symbolic price of 1 guilder each. They were transported in 1954, and the alt-azimuth mountings were replaced by equatorial ones. In 1955 they were installed at Dwingeloo, one to the east and the other to the west of the nearly-completed 25 m dish, where they were mainly used for solar research, notably a spectral study of solar bursts by De Groot (1966). In 1962 the eastern dish moved to NERA to strengthen the solar effort there, and in 1973 it was returned to Dwingeloo as the PTT receiving station was being wound down.

The Dwingeloo Würzburgs were mainly used for solar research, as a two-element interferometer, but one of them was also used as a single dish in early polarization studies. A photograph from the 1980s is reproduced in Figure 16.

5.4 What Finally Became of the 'Dutch' Würzburgs?

The fate of the TNO-Würzburg has already been noted above; it should now be on display at a museum near Berlin. Some of the remaining antennas which served at the PTT stations and Dwingeloo are also on display, but the others can no longer be located. Much of the information has been assembled by Beekman (B99), although his account has to be corrected in one respect: he was apparently unaware that NERA already had a number of paraboloids before the post-1955 move from Kootwijk.

Let us first consider the 10-m dishes, which were only partially Würzburg constructions. Besides the one which came from Kootwijk and was described above, Fokker (1960; and see Appendix 10.3) refers to two others. This agrees with an account in De Voogt (1952: 211) who says, after describing the Würzburgs in Nederhorst den Berg and Kootwijk, "Two other reflectors are coming to Nederhorst den Berg, constructed by the PTT with 10 m aperture and 2.5 m focal distance and destined for interferometer tests." There is no indication whether they also used a Würzburg mounting, or whether the entire construction was of PTT origin, although the former seems more likely. One of these 10 m PTT dishes (according to B99 this was the one originally in Kootwijk, but as Beekman was not aware of the others in NERA, it could have presumably been any of the three) was sold in 1981 to the *Volkssterrenwacht Drenthe* (an amateur observatory). It was set up in Emmercompascum in 1983, but when it was moved to a new planetarium (the Planetron) in Dwingeloo, the mounting was damaged. It was replaced with the equatorial system from one of the Würzburgs in Dwingeloo (see below).

Of the three (7.5 m) Würzburgs in NERA around 1960, the fate of only two is known. According to Beekman (B99: 157), these were the two which flanked the 10 m dish and were used as an interferometer, hence the two which had come from Kootwijk. The eastern one went to the National War and Resistance Museum in Overloon at the end of the 1970s, while the western one was donated to the *Volkssterrenwacht Simon Stevin* in Hoeven early in the seventies. Both are still on display, though they are not in the best of condition.

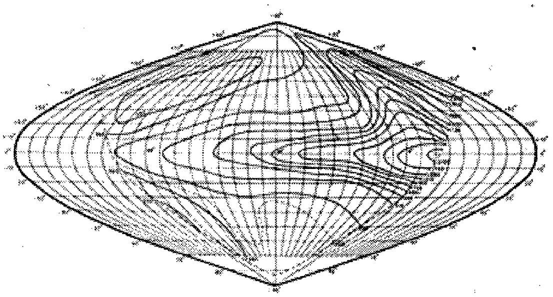

Figure 15: Map of the 140 MHz background radiation made with the NERA Würzburg by Fokker and De Feiter (1954a). The contours were 'corrected for the antenna pattern', and the bright sources Cas A and Cyg A were subtracted off.

Of the two Würzburgs used in Dwingeloo, one ended up in a German museum. It originally stood to the west of the 25 m telescope, and in 1991 was donated to the *Deutsches Museum für Naturwissenschaften und Technik* in Munich. After being restored by the Bundeswehr, it has been on display since 1997. The Technical University of Munich has provided it with a receiver, and it is still used for demonstrations and teaching (B99). The eastern Würzburg, after returning from NERA in 1973 (above), was dismantled in the late 1980s. The mounting was donated to the Planetron to replace the one damaged in moving the 10 m dish from Emmercompascum (above) in 1989 (B99). It can to this day be seen at the entrance to the Planetron.

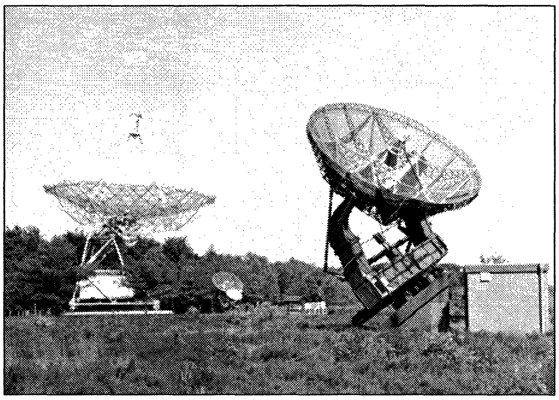

Figure 16: The telescopes at Dwingeloo Observatory, with the western Würzburg in the right foreground. The equatorial mounting can be seen clearly. The 25-m telescope is on the left, and the eastern Würzburg can be seen in the distance (photo by H. Schneider).

We are left with the question of what happened to the most famous Würzburg of all, the one used for the

HI studies at Kootwijk (Figures 3 and 13). If it should be any of those still in museums, then it would have to be either in Overloon or Hoeven, since neither of the other ones in Germany (near Berlin or Munich) was ever in Kootwijk. One of the few people still alive who worked regularly with the HI Würzburg, A.C. Hin, has examined the two candidates and is quite certain that they are not the missing one. As mentioned above, one of us (HvW) recalls hearing that it may have broken down in 1956. If this is correct, then it seems likely that it was scrapped about the time that the other Würzburgs moved to NERA.

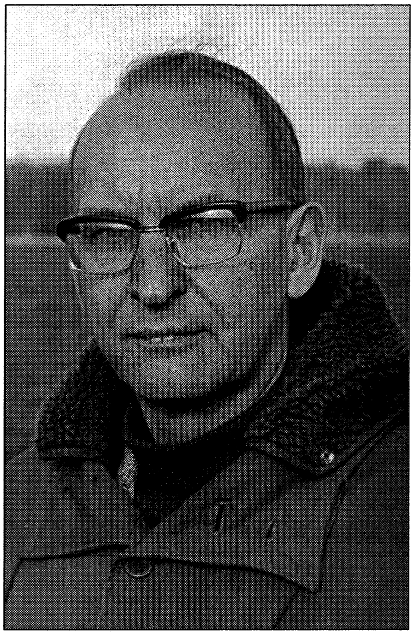

Figure 17: C.A. Muller (photo from the Muller family).

6 CONCLUSION

It is noteworthy that when Van de Hulst arrived in Leiden early in January 1944, the research he was to do had not yet been settled. He had known Oort for a year or so, and their collaboration up to that point concerned the formation and destruction of interstellar dust (or as they then called it, "smoke"). The suggestion from Oort to look into the unexplored potential of radio emission must have come early during his stay in Leiden; Van de Hulst completed the investigation (and prepared his talk) in just three months. An interesting, but unresolved, question is: when did the idea that there might be a radio line come to Oort? A few years had elapsed since he first saw Reber's paper, but it had been a difficult time and it must have often been filled with more pressing concerns. Moreover, Oort had also been busy with other astronomical research, such as the work on the progenitor of the Crab Nebula, SN1054.

The speed with which Oort decided to pursue the radio potential in a serious way was typical of him, as was his resolution in the face of adversity, be it financial or technical. The early prominent radio astronomers in Britain (like Lovell and Ryle) and Australia (Bolton and Pawsey) had been involved in radar; their Dutch counterparts—Oort and Van de Hulst—were astronomers with a strong astrophysical bent. Muller (Figure 17) was, to be sure, an engineer, but even he had not come from the radar fraternity. We do not know the exact sequence of events which motivated De Voogt (also an engineer with no radar connections) to initiate his group's radio investigations, and in particular to appropriate the abandoned Würzburgs. His amateur interest in both radio and astronomy (Section 10.2) probably played a role, but it is certainly possible that he had contacts with professional astronomers. The collaboration involving both Utrecht and the PTT reported at the end of Houtgast's (1946) paper suggests an early liaison with Utrecht astronomers.

The approach adopted in the HI effort, once the 1951 detection had been made, would make sense to most researchers: first get a global impression by sampling the Galactic Plane at a variety of locations. But it soon became apparent to Muller that the receiver could and should be improved. The year's delay this precipitated must have been frustrating to Oort, but he clearly had confidence in his young colleague, and that this was well-placed is demonstrated by the results of the survey which began in 1952. To finally be able to examine the kinematics of the entire Galaxy would have pleased no one more than Oort: the topic had fascinated him since his doctoral research, and had only been pushed to the 'back-burner' by optically impenetrable dust. The success of the HI detection, followed by the survey, provided the ammunition Oort needed to get funding for the large, 25 m radio telescope. Ironically, Kootwijk completed the survey which had been the main reason for building the new dish; fortunately, no one suggested that the project be terminated. However, the achievements of the Dwingeloo Telescope are a different story, one we plan to discuss in a separate paper.

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8 NOTES

1. The chronology of events described here was carefully checked by us in the original sources quoted. Some of the stories about it are not quite accurate, including that by Sullivan (1982: 299). Raimond (1996: 12) mistakenly places the line prediction by Van de Hulst later in 1944, after the correspondence between Oort and Bakker (in April-June 1944) about a large radio telescope and receiver. An excellent, detailed story is given by Sullivan (2000: 237-261), although the *Nederlandse Astronomen Club* was (and

still is) not a "… gathering of amateur and professional astronomers ..." (Sullivan, 2000: 237), but a society of professionals. In addition, Van de Hulst did not "… shift his studies informally to Leiden …" (Sullivan, 2000: 238); rather, he remained Minnaert's student and an Assistant at Utrecht Observatory, visiting Leiden for only three months (cf. Section 2, above).

2. Oort was not only anxious to observe the 21-cm line; he was also interested in the radio continuum as a source of information about Galactic structure: witness his "Comparison of the intensity distribution of radio-frequency radiation with a model of the Galactic System" (Westerhout and Oort, 1951).

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10 APPENDICES

10.1 Obituaries of the pioneers

We briefly refer to a few obituaries of the leading persons mentioned in this paper. Obituaries of Henk van de Hulst have been written by Habing (2001) and by Welther (2000). Many obituaries of Jan Oort have been published; we mention an extensive one (in Dutch) by Blaauw (1993), and one in English by Blaauw and Schmidt (1993). For Lex Muller the only obituary we know is by Van Woerden et al. (2005), in Dutch.

10.2 Brief Sketch of the Life of Ir A.H. de Voogt

Anthonet Hugo de Voogt (Strom, 2005) was born on 1 May, 1892. While still a teenager he became one of the first radio amateurs in the Netherlands, having constructed his own station in 1909. As a student he earned the radio-telegrapher's diploma (and studied electrical engineering at the Delft Institute of Technology). He completed his studies, earning the engineer's (Ir) title, in 1916, and was almost immediately mobilized. Until the 1920s he was active in amateur radio, meteorological and astronomical clubs, giving talks and contributing to various specialist magazines. In 1919, after his discharge from the army, he became a telegraph engineer with the PTT, soon moving from wireless to cable-linked communication. He had several inventions in this period which were patented, and also served in a number of Governmental advisory committees. During the war-time occupation (1940-1945), he was head of the telephone district of Breda.

Immediately after the war, De Voogt (Figure 18) became Head of the PTT's Radio Service, and undertook initiatives to study the ionosphere, and the effects of solar activity upon it. The acquisition of a number of abandoned German radar antennas formed part of this activity. In 1948 he loaned one of the Würzburgs at Kootwijk to the newly-established SRZM to search for and study the 21 cm neutral hydrogen line, and also joined the board of the organization. He established a department (IRA) within the PTT to study the Ionosphere and Radio Astronomy, and helped set up and coordinate a world-wide survey of solar radio emission at the behest of URSI. He is credited with having pointed out the need to protect the 21 cm band from harmful transmissions, and was active in the international effort of radio astronomers to secure frequency allocations for passive radio services. In 1953 the PTT promoted him to Deputy Chief-Director of General Affairs and Radio, and in 1957 he was awarded a Dutch knighthood. He continued his work with the IRA past the usual retirement age to see out the International Geophysical Year, and retired in 1960. A.H. de Voogt died in 1969 at the age of 77.

10.3 Parabolic Antennas at NERA in the 1950s

The best source of information comes from Fokker's (1960) doctoral thesis, in fact from a single table. For the reader's reference, relevant parts of this table are reproduced below in Table 1. (What have been left out are entries for smaller antennas, and columns relating to recording speed and normal observing times.) The first column—"Label"—has been added in an attempt to identify the different antennas, and for ease of the discussion. It should perhaps be noted from the outset that there are errors—or in any event omissions—in the Table. Reference was made above to solar monitoring at 140 and 200 MHz in 1952 (De Voogt, 1952: 209), but the former frequency is not mentioned anywhere in the Table. That 140 MHz was regularly used is confirmed by entries in the QB (see below), and Fokker himself published a map (Figure 15) of the background emission made with a NERA Würzburg at 140 MHz (Fokker and De Feiter, 1954a). In addition, regular monitoring began before 1952, as reported in the QB: at 140 MHz, in July 1951 (QB95); and at 200 MHz, in October 1951 (QB96). In a discussion which one of us (RGS) had with Aad Fokker, he was unable to provide additional information concerning the Table.

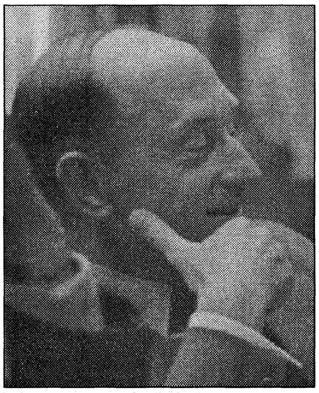

Figure 18: A.H. de Voogt in about 1947 (courtesy of the Museum voor Communicatie, Den Haag).

Let us first consider the three elements used for interferometry, labeled C, D and E. The fact that they are labeled by location means that we can be pretty certain that the following two entries were indeed C and E, as indicated. As noted above, the trio C, D, E almost certainly arrived from Kootwijk after 1955. In addition to D there should be two more 10 m reflectors if we accept De Voogt's (1952: 211) account that they were then on their way to NERA. They are labeled B and F. Although we cannot be certain that the two instruments in the third line ("A/B") were the same as A and B, this is the most likely identification. The frequency used is the same, polarization could be added to total flux determinations, and Fokker uses a similar format in repeating C and E for the total flux measurements. For the last entry, it is clear from other sources that a 7.5 m Würzburg was outfitted with dual 545/200 MHz receivers, so identification with A is the most likely. As for the 10 m parabola, it could be identified with B or D, but F has been chosen since other evidence suggests that there should have been three of the larger dishes. If this analysis is correct, then a total of three 7.5 m and three 10 m parabolas passed through NERA before 1960. There could have been more 7.5 m dishes – if the identification with 'A' in line 3 and/or 9 of the Table is incorrect – but it is unlikely that there were more than three of the 10 m type.

Table 1: Parabolic reflectors at NERA to 1960 (after Fokker, 1960: 3).

Label	Instrument	Frequency	Use	Period of observation
A	7.5 m parabola	200 MHz	total flux	1952–May 1957
B	10 m parabola	200	total flux	May 1957–now
A/B	7.5/10 m parabola	200	polarimetry	December 1955–now
C	7.5 m parabola, east	200	large – spacing – interferometry	February 1958–now
D	10 m parabola, centre			
E	7.5 m parabola, west			
C	7.5 m parabola, east	198.5	total flux	October 1958–now
E	7.5 m parabola, west	203	total flux	October 1958–now
A/F	7.5/10 m parabola	545	total flux	1953–December 1958

Hugo van Woerden was born in 1926, and studied at Leiden with J.H. Oort and H.C. van de Hulst. After his graduation in 1955 he did extensive research with the Dwingeloo Radio Telescope, was appointed at the Kapteyn Institute in Groningen, and obtained his doctorate there in 1962 from A. Blaauw. He was Professor of Radio Astronomy at Groningen, and was Department Chairman from 1985 to 1991. After his retirement in 1991 he continued his research on the properties of interstellar clouds, and on the structure and kinematics of galaxies, and he did much work on popularizing astronomy. He has edited several books, including *Oort and the Universe* (Reidel, 1980) and *High-Velocity Clouds* (Kluwer, 2004). He served on the Board of the Netherlands Foundation for Radio Astronomy from 1965 to 1991, and as President of IAU Commission 34 (Interstellar Matter) from 1973 to 1976. He is currently a Committee Member of the IAU Working Group on Historic Radio Astronomy.

Richard Strom was born in New York City, USA, in 1944. While living in New York, he gained entrance to and attended the Bronx High School of Science. He earned a B.A. degree in Physics from Tufts University, and M.Sc. and Ph.D. degrees in Radio Astronomy from the University of Manchester (Jodrell Bank), UK. He is currently a Senior Research Astronomer with ASTRON (the Netherlands Foundation for Research in Astronomy) in Dwingeloo, and Adjunct Professor at the University of Amsterdam. Richard is a past Secretary and member of the SOC of IAU Commission 40 and has been on the SOC of several conferences, including 'Woodfest' and IAU Colloquium 182 (of which he was co-Chair). He is a member of IAU Commissions 28, 34, 40 and 41, and has been on a number of time allocation panels for radio, infra-red and X-ray telescopes. His research interests include supernova remnants (especially those associated with historical supernovae), pulsars, large radio galaxies, radio polarimetry and interferometry, new telescopes, Chinese historical records and the history of radio astronomy in the Netherlands.

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FROM POTTS HILL (AUSTRALIA) TO PUNE (INDIA): THE JOURNEY OF A RADIO ASTRONOMER

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Abstract: In this paper I recapitulate my initiation into the field of radio astronomy during 1953-1955 at CSIRO, Australia; the transfer of thirty-two parabolic dishes of six-feet (1.8-m) diameter from Potts Hill, Sydney, to India in 1958; and their erection at Kalyan, near Bombay (Mumbai), in 1963-1965. The Kalyan Radio Telescope was the first modern radio telescope built in India. This led to the establishment of a very active radio astronomy group at the Tata Institute of Fundamental Research, which subsequently built two world-class radio telescopes during the last forty years and also contributed to the development of an indigenous microwave antenna industry in India. The Ooty Radio Telescope, built during 1965-1970, has an ingenious design which takes advantage of India's location near the Earth's Equator. The long axis of this 530 m x 30 m parabolic cylinder was made parallel to the Equator, by placing it on a hill with the same slope as the geographic latitude (11°), thus allowing it to track celestial sources continuously for 9.5 hours every day. By utilizing lunar occultations, the telescope was able to measure the angular sizes of a large number of faint radio galaxies and quasars with arc-second resolution for the first time. Subsequently, during the 1990s, the group set up the Giant Metrewave Radio Telescope (GMRT) near Pune in western India, in order to investigate certain astrophysical phenomena which are best studied at decimetre and metre wavelengths. The GMRT is an array of thirty fully-steerable parabolic dishes of 45 m diameter, which operates at several frequencies below 1.43 GHz. These efforts have also contributed to the recent international proposal to construct the Square Kilometre Array (SKA).

Keywords: History of radio astronomy, history of science in India, Sun, solar radio bursts, cosmology, radio telescopes, GMRT

1 INTRODUCTION

There are many instances in the field of astronomy whence initial pioneering work at field stations has led to the development of major instruments for the investigation of the mysteries of the Universe. Although Karl Jansky serendipitously discovered radio emission from our Galaxy in 1931 while working at the Bell Labs in USA, active research in the field of radio astronomy only started after 1945, following developments in electronics and radar engineering during World War II (see Sullivan, 1984). The discoveries made between 1945 and 1955 at Sydney, Cambridge, Harvard, Jodrell Bank and Leiden laid a firm foundation for the new field of radio astronomy. In this paper, I describe my initiation into the field of radio astronomy in Australia during 1953-1955; my contributions during 1956-1963 in the USA; early attempts by Sir K.S. Krishnan to form a radio astronomy group at the National Physical Laboratory in New Delhi (for Indian localities mentioned in the text see Figure 1); and the subsequent development of radio astronomy at the Tata Institute of Fundamental Research (TIFR) in Mumbai, as a result of initial support given by Dr Homi J. Bhabha (who was one of the main architects of the growth of modern science in India).

Radio astronomical research is also being carried out at other institutions in India, mainly at the Raman Research Institute (Bangalore), the Indian Institute of Astrophysics (Bangalore) and the Physical Research Institute (Ahmedabad). The facilities developed by these institutes are described on their websites. There has been a close collaboration between the TIFR and the Raman Research Institute, where Professor V. Radhakrishnan established a radio astronomy group in 1971, after spending nearly twenty years abroad (mostly at Caltech in the USA, and at the Commonwealth Scientific and Industrial Research Organization (CSIRO) in Australia).

Figure 1: Indian localities mentioned in the text.

2 THE INITIAL YEARS

After obtaining an M.Sc. degree in Physics from Allahabad University (India) in 1950, I joined the National Physical Laboratory (NPL) of the Council of Scientific and Industrial Research (CSIR) in New Delhi, and worked in the field of paramagnetic resonance under the guidance of K.S. Krishnan (Figure 2), who was the Director of the Laboratory. During 1946-1947 he had taught me Electricity and Magnetism in the first year of the B.Sc. degree at Allahabad University before moving to the NPL. Krishnan was the co-discoverer of the Raman Effect which won C.V.

Raman the Noble Prize in Physics. Later Krishnan shifted his research interests to the field of magnetism, and he asked me to develop equipment that could be used to investigate the phenomena of electronic paramagnetic resonance at a wavelength of 3 cm. Over the next eighteen months, I was able to set up equipment by cannibalizing surplus radar sets procured by the NPL, and by studying parts of the remarkable set of twenty eight volumes of the Radiation Laboratory Series that described almost all the radar techniques that were developed during World War II.

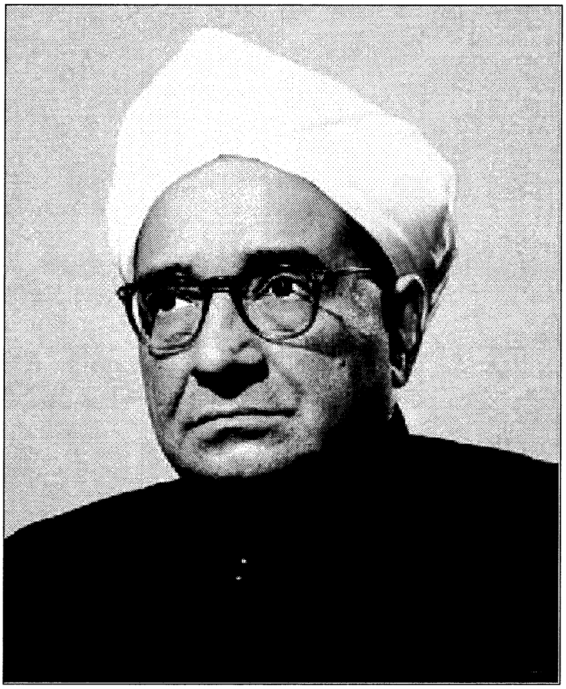

Figure 2: Dr K.S. Krishnan, Director of the National Physical Laboratory, New Delhi (courtesy National Physical Laboratory).

In August 1952 Krishnan attended the General Assembly of the International Radio Scientific Union (URSI) in Sydney, and he was struck by the dramatic and remarkable discoveries being made in the field of radio astronomy by staff from the CSIRO's Division of Radiophysics (RP). Under the inspired leadership of J.L. Pawsey (Figure 3), several ingenious radio telescopes had been developed by the Australian scientists to investigate radio emission from the Sun and distant cosmic sources in our Galaxy (see Davies, 2005; Orchiston and Slee, 2005; Sullivan, 2005). On his return to India, Krishnan described these developments in a colloquium at the NPL, and these caught my imagination. I then visited the NPL library, where I studied some of the thirty papers that had been published by the RP scientists in the *Australian Journal of Scientific Research* and in *Nature* describing these discoveries. I was told that these were almost half of the papers on radio astronomy that had been published worldwide up to that time. I, too, was fascinated by this new field. Krishnan was also interested in initiating radio astronomical research at the NPL, and he put my name forward for a two year Fellowship under the Colombo Plan to work at RP in Sydney.

Figure 3: Dr J.L. Pawsey, leader of the radio astronomy group within the CSIRO's Division of Radiophysics in Sydney (ATNF Historic Photographic Archive: 7454-2).

3 MY INTRODUCTION TO RADIO ASTRONOMY, AND THE SOLAR GRATING INTERFEROMETER AT POTTS HILL

The Colombo Plan application was successful, and in March 1953 R. Parthasarathy from the Kodaikanal Observatory (in South India) and I joined RP to work under Pawsey's guidance (Figure 4).

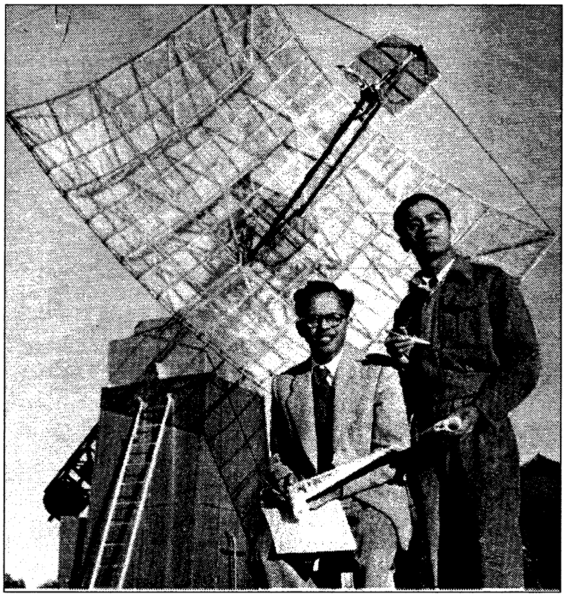

Figure 4: Govind Swarup (left) and R. Parthasarathy (right) in at Potts Hill field station in 1954. At this time, searches were being made for hydrogen clouds in the Milky Way with this 16 x 18 ft ex-radar antenna (after *Illustrated Weekly Times of India*, 14 September 1954).

Australian-born Joseph Lade Pawsey (Lovell, 1964) was a scientific leader *par excellence*. In 1931 he obtained a Cambridge Ph.D. under J.A.Ratcliffe in the field of ionospheric research, and then spent several years working on antennas and transmission lines in the television industry in UK before returning to Australia and joining the newly-formed Division of Radiophysics, which was involved in radar research.

In October 1945, as the War ended, he initiated a study of the Sun using a radar installation in suburban Sydney (Orchiston, 2005). This produced immediate results which led to further successful studies, and the small RP radio astronomy group never looked back! In a pioneering paper published in *Nature* in 1946, Pawsey announced that radio emission from the Sun arises from a hot corona at a temperature of about one million degrees. Soon, several different research groups were formed at RP (Orchiston and Slee, 2005; Sullivan, 2005), and under Pawsey's guidance they conducted detailed investigations of radio emission from the Sun, our Galaxy and distant extragalactic radio sources, with new discoveries being made every few months!

After finding that my interest was more in experimental rather than theoretical work,[1] Pawsey suggested that I work for three months each in the groups led by W.N. Christiansen, J.P. Wild, B.Y. Mills and J.G. Bolton (Figure 5). Each of these scientists had made important discoveries, and they were already acknowledged world leaders in their respective fields. I was to report back to Pawsey every two weeks. S.F. Smerd, a very pleasant man but a tough task master, was asked to coordinate my activities and to provide me with guidance on the rapidly-growing literature in radio astronomy. For his part, Parthasarathy was to develop a 10.7 cm solar radio telescope, as this was needed by the Kodaikanal Observatory (which had a long history of solar observations at optical wavelengths—see Kochhar, 1991—and now wanted to expand into radio astronomy). Then, after the first year, Parthasarathy and I would select a joint project. What a great opportunity for initiation into the new field of radio astronomy!

For the first three months, I assisted W.N. Christiansen and J.A. Warburton to make a two dimensional map of the quiet Sun at a wavelength of 21cm, using strip scans obtained with the east-west and a north-south grating interferometers at the Potts Hill field station (Figures 6 and 7). We first Fourier transformed each scan manually using an electrical calculator, plotted the outputs on large graph paper along respective angles of the scans, scanned the resulting 2-dimensional Fourier-transformed map at various angles and again reversed the process thereby obtaining a map of the quiet Sun at 21 cm. The final result, after Christiansen and Warburton (1953), is shown in Figure 8. I highlight this work here in some detail because a decade later that painstaking experience gave me an idea of a simpler scheme to make maps from one-dimensional scans without taking Fourier transforms. The new concept was described by me to R.N Bracewell in late 1962, just before I returned to India from Stanford. In this method, a 2-dimensional map can be readily obtained by multiplying amplitudes of each of the one-dimensional strip scans by appropriate weights and then plotting the resulting modified scans along corresponding scan-angles, in order to obtain a 2-dimensional map (Bracewell and Riddle, 1967). This technique is widely used today in X-ray imaging and has revolutionized medical tomography.

Figure 5: Some of the distinguished radio astronomers who attended the 1952 URSI Congress in Sydney. Chris Christiansen, Paul Wild and Bernie Mills (in the dark suit) are first, third and fifth from the left respectively, and Steve Smerd is in the front row immediate to the right of Mills. John Bolton is the man on the extreme right of the group photograph (ATNF Historic Photographic Archive: 2842-43).

Figure 6: View looking southwest across the two Potts Hill water reservoirs in 1953, showing Christiansen's solar grating arrays along the banks of the eastern reservoir. The E-W array consisted of thirty-two elements and the nearer N-S array just sixteen elements (ATNF Historic Photographic Archive: 3475-1).

Figure 7: Close-up, looking east, showing the E-W grating array (ATNF Historic Photographic Archive: B2976-1).

During the next three months, under the guidance of Paul Wild, J.A. Roberts and I developed a 45 MHz receiver that was then used at the Dapto field station to determine the velocity of ionospheric turbulence. After this, I spent three months to develop a phase shifter for the prototype Mills Cross antenna that Bernie Mills and Alec Little were building at Potts Hill field station, and for the final three months of the first year I worked in the group led by John Bolton, and made a highly stable D.C. power supply.

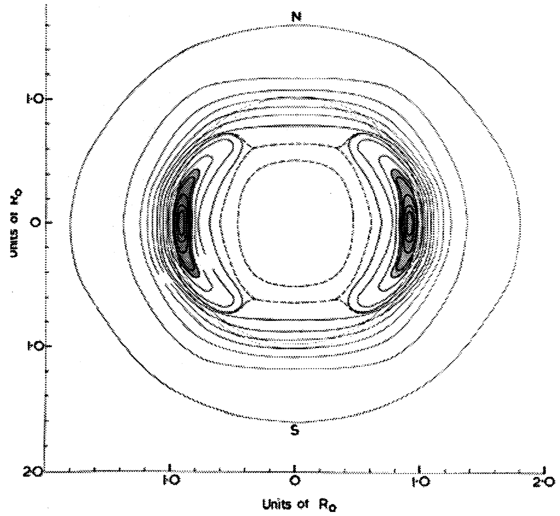

Figure 8: Isophote map of the quiet Sun at 21 cm, showing equatorial limb-brightening (ATNF Historic Photographic Archive: B3400-3).

In 1954, Christiansen went to work at Meudon Observatory in France for a year. After discussions with Pawsey, Parthasarathy and I decided to convert the Potts Hill EW grating array (Figure 7) from 21cm to 60 cm (500 MHz), in order to investigate whether the quiet Sun exhibited limb brightening at that frequency. This was predicted by Smerd (1950), but was in conflict with measurements made at Cambridge by Stanier (1950). Our results (Swarup and Parthasarathy, 1955; 1958) agreed with Smerd's prediction. For us, this was a great experience: building dipoles, a transmission line network and a receiver system; making the observations; and finally, carrying out data reductions—not to mention saving my dear friend Parthasarathy from drowning in the Potts Hill reservoir! At the time he was using a bucket to draw some water from the Reservoir so that we could make a cup of tea and wash our faces (after a day of hard work), and he accidentally fell into the water.

Upon his return from France in early 1955, Christiansen decided to build a new cross-type antenna array at RP's Fleurs field station near Sydney. Known as the Chris Cross, this consisted of two orthogonal grating interferometers, which were used to make daily solar maps at 21cm (see Orchiston, 2004). As a result, all thirty-two of the 6 feet diameter dishes making up the E-W grating array at Potts Hill, along with associated equipment, became surplus to requirements and were to be scrapped. Pawsey liked to visit all the RP field stations unannounced to see what his staff were doing (Sullivan, 2005), and during one of his surprise visits to Potts Hill I asked whether these dishes could be gifted to India. He readily agreed to this suggestion, as did E.G. (Taffy) Bowen, Chief of the Division of Radiophysics.[2] On 23 January 1955, I wrote to K.S. Krishnan about the possibility of transferring the thirty-two dishes from Sydney to the NPL in New Delhi (Swarup, 1955). I proposed simultaneous dual frequency observations with a 2,100-feet long grating interferometer using the thirty-two dishes at 60 cm and 1.8m. On 22 February Krishnan (1955) replied: "I agree with you that we should be able to do some radio astronomy work even with the meager resources available." Pawsey obtained approval from the CSIRO authorities for the donation of the dishes to India under the Colombo Plan scheme, but with the proviso that India must bear the cost of their transportations (which amounted to about 700 Australian Pounds, as I recall).

4 RADIO ASTRONOMY AT THE NATIONAL PHYSICAL LABORATORY

Upon my return to New Delhi in August 1956, Krishnan gave approval to start a radio astronomy program at the NPL. I then began building a sensitive receiver system for operation at 500 MHz. However, Krishnan could not get approval from the CSIR authorities in New Delhi for transfer of the dishes from Sydney. Instead, the CSIR had suggested that the Australian authorities should bear the cost of transportation, considering the shortage of foreign exchange in India at the time, but this request was turned down. As there seemed to be no early resolution to the tangle, later in 1956 I decided to go to the USA for a year or two. Meanwhile, Parthasarathy had also joined the NPL in 1956, and he went on to build a 10.7 cm receiver, but left the NPL in the following year and joined C.G. Little's group in Alaska. T. Krishnan also joined the NPL in 1956, after completing the physics tripos at Cambridge and spending a year working with Martin Ryle. In late 1958 he joined RP to work with Pawsey and Christiansen (Figure 9). Dr M.R. Kundu joined the NPL in 1958 after completing his Ph.D. in solar radio astronomy in France, but also went to the USA soon afterwards. M.N. Joshi and N.V.G. Sarma joined the NPL soon after finishing their M. Sc. degrees in India in 1956, and they built a 500 MHz receiver for the proposed grating array. Later, Joshi went to France for a Ph.D. degree, which he obtained in 1962. For his part, Sarma spent two years at Leiden Observatory, where he built radio astronomy receivers. Both he and Joshi subsequently returned to India and joined the NPL. In the meantime, CSIRO eventually paid for the transport of the thirty-two Potts Hill dishes to New Delhi. Thus, it may be said that the NPL acted as a foster mother for the subsequent development of radio astronomy in India by the above persons, who were trained across the world.[3]

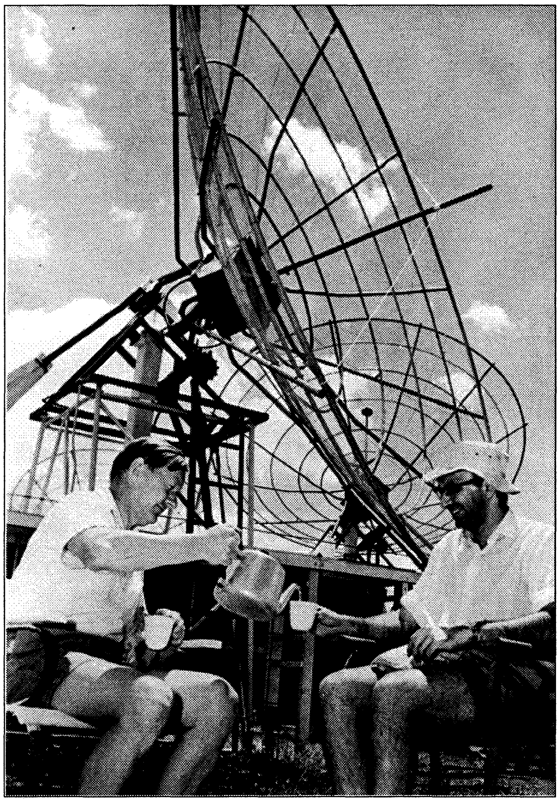

Figure 9: W.N. Christiansen (left) and T Krishnan (right) at Fleurs field station in front of one of the Chris Cross antennas (Krishnan Collection).

5 INDIAN RADIO ASTRONOMERS IN THE USA IN THE EARLY YEARS

I joined the Fort Davis Radio Astronomy Station of the Harvard Observatory in August 1956. This Texas field station was set up by Dr Alan Maxwell, a New Zealander with a Ph.D. from Manchester, in order to record the dynamic spectra of solar radio bursts over the frequency range 100-600 MHz using a swept-frequency receiver connected to a 28-ft dish (Figure 10). In December 1956 I discovered the Type U burst while Maxwell was on a holiday in New Zealand (Maxwell and Swarup, 1957). In early 1957, I decided to work for a Ph.D. degree in the USA and received favourable responses from Harvard, Caltech and Stanford, all of which were already active in radio astronomy (e.g. see Bracewell, 2005; Cohen 1994; Kellermann et al. 2005). Pawsey (1957) wrote: "Stanford is famous for radio engineering, Caltech for its physics and, of course, its astronomy research, and Harvard for its training in astronomy… If you are returning to India, I should recommend to you to place great emphasis in electronics. It is a key to open many doors."

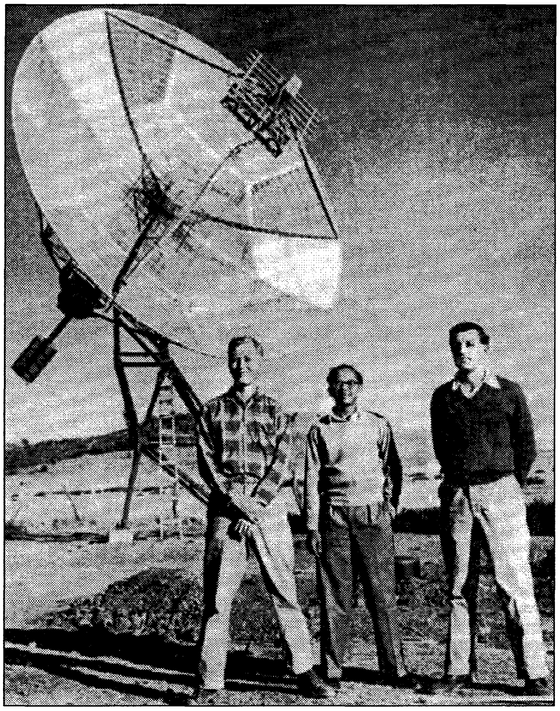

Figure 10: Alan Maxwell, Govind Swarup and Sam Goldstein (left to right) posing in front of the 28-ft dish at Harvard Observatory's Fort Davis field station in Texas.

I decided to join Stanford University, and in September 1957 began Ph.D. research under the guidance of R.N. Bracewell, who was in the process of building a cross-antenna interferometer (Figure 11) that would be used to generate daily solar maps at 9.2 cm (Bracewell and Swarup 1961). On 1 January 1961, soon after obtaining my Ph.D. degree, I joined the University as an Assistant Professor.

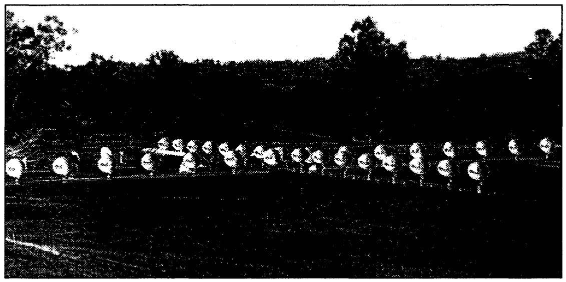

Figure 11: The completed Stanford 9.2 cm cross-antenna interferometer (after Bracewell, 2005: 75).

After graduating from the Indian Institute of Science in Bangalore in 1950, T.K. Menon (Figure 12) went to Harvard University in 1952, where he completed M.S. and Ph.D. degrees. He was on the faculty of the Astronomy Department from 1956 to 1958, before joining the (U.S.) National Radio Astronomy Observatory in 1959 as one of the senior scientists. He went on to make pioneering contributions to the studies of HII regions and HI clouds in our Galaxy.

In 1958, M.R. Kundu (Figure 12) joined the radio astronomy group led by Fred Haddock at the University of Michigan, and he soon became an internationally-recognized leader in the field of solar radio astronomy.

During meetings of the American Astronomical Society and of the URSI chapter in the USA, on several occasions the three of us working in USA discussed the possibility of returning to India and forming a major radio astronomy group. In 1960 and 1961, I also corresponded with Christiansen, Frank Kerr and Pawsey in this regard. They recommended to us T. Krishnan (Figure 13), who was then at the University of Sydney. On 22 September 1960, Christiansen wrote "… you two and Menon and Kundu should get together for a united attack on the monolith of Indian bureaucracy….", and on 26 October Pawsey (1960) wrote: "But keep off the fashionable ideas. Be original."

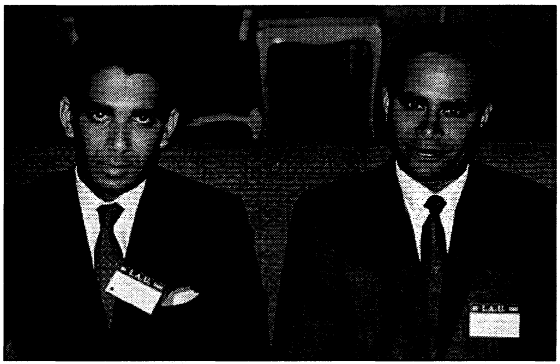

Figure 12: T.K. Menon (left) and M.R. Kundu (right) at the Berkeley IAU General Assembly in 1961 (Menon Collection).

In August 1961, Krishnan, Kundu, Menon and I met at Berkeley during the General Assembly of the International Astronomical Union (Figure 12), and we discussed our interest in returning to India to form a radio astronomy group. We wrote a detailed proposal indicating our initial plans to start solar radio astronomical observations using the thirty-two dishes already donated to the NPL (which had still not been used), and thereafter to set up "… a very high resolution radio telescope of a novel design would be the next step in our programme ... certain types of radio telescopes would be cheaper to build in India due to lower labour cost … such as a Mills Cross operating at low frequencies ..." (Krishnan et al., 1961). In September 1961, the proposal was sent to five major scientific organizations and agencies in India, indicating our desire and willingness to return to India and form a radio astronomy group and also to attract others in due course. Copies of our proposal were also sent to five distinguished astronomers, Bart Bok, D.J. Denisse, Jan Oort, Joe Pawsey and Harlow Shapley, and they were asked to send their confidential assessments to the authorities in India. Copies of the letters of recommendation from Bok, Oort and Pawsey to Bhabha are available in the TIFR archives. Bok's (1961) recommendation was very generous: "… it seems to me that their offer to return to India as a group is a unique one, and that should by all means be accepted and acted upon promptly. An offer like the present one comes only rarely in the history of a nation, which scientifically, is obviously coming of age". We got replies from all the concerned authorities from India, but the most encouraging and highly supportive was from the great visionary scientist and a dynamic organizer, Dr Homi J. Bhabha (Figure 14), Director of the Tata Institute of Fundamental Research (TIFR) in Mumbai. He sent a cable to all four of us on 20 January 1962: "We have decided to form a radio astronomy group stop letter follows with offer…." (Bhabha, 1962a). He wrote to me on 3 April 1962: "If your group fulfills the expectations we have of it, this could lead to some very much bigger equipment and work in radio astronomy in India than we see foresee at present." (Bhabha, 1962b). The above-mentioned correspondence and several other related letters from the period 1955-1963 in my files have been scanned, and are now available at the NCRA Library in Pune.

6 RADIO ASTRONOMY AT THE TIFR; BEGINNING WITH THE KALYAN RADIO TELESCOPE

I resigned from Stanford, and returned to India on 31 March 1963. On a request made by Homi Bhabha to the NPL and CSIR authorities, the thirty-two Potts Hill dishes were transferred to the TIFR by the middle of 1963. In the meantime, I developed a design involving 20-ft dishes to complement the thirty-two smaller dishes for operation at a longer wavelength. Soon after, in June 1963, I came across a paper by Cyril Hazard in a recent issue of *Nature* describing observations of a lunar occultation of the radio source 3C273 made with the 64 m Parkes Radio Telescope, as well as a companion paper by Marteen Schmidt, concluding that the enigmatic spectrum of the blue stellar object identified with 3C273—which had been a great puzzle for several years—was easily explained for an object with a redshift of 0.17. This marked the discovery of quasars, and has revolutionized our understanding of the Universe. While reading the two papers, a thought flashed through my mind: that the lunar occultation method could provide accurate positions and angular size measurements of a large number of radio sources, much weaker than those in the 3C catalogue, and thus distinguish between competing cosmological models. At that time there was a raging controversy between the Steady State and Big Bang cosmologies. A quick calculation showed that in order to obtain occultation observations of a sizable sample of distant weak radio sources, say ~200 per year, one would need a telescope with a collecting area of more than four times that of the 64 m Parkes or the 76 m Jodrell Bank Radio Telescopes, which was not practical to build, even in advanced countries. It occurred to me that the solution would be to construct a large cylindrical radio telescope on a suitably-inclined hill in southern India so as to make its axis parallel to the Earth's axis, and thus taking advantage of India's close proximity to the Equator. I discussed this idea with Professor M.G.K. Menon, Dean of the Physics Faculty, who responded enthusiastically. In August 1963 I had a long discussion with Bhabha, who grilled me for over two hours. I asked him whether I should write a detailed Project Document and he replied: "Young man, do not waste your time writing a project report; your main problem would be to collect a team; when you have managed that, you can submit a project report and proceed with its design and construction."

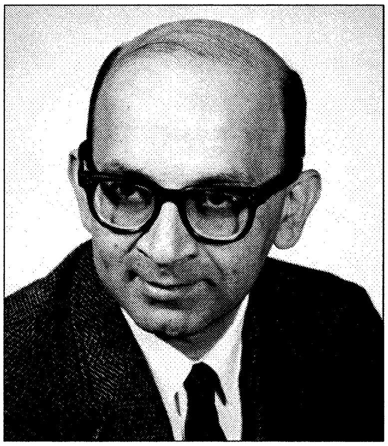

Figure 13: T Krishnan in India in 1970 (Krishnan Collection).

In August 1963, V.K. Kapahi and J.D. Isloor, fresh graduates from the Atomic Energy Establishment Training School (AEET) and two scientific assistants, joined the young radio astronomy group of the TIFR. R.P. Sinha, who was also from the AEET, joined in August 1964. As a first step, a grating type of radio interferometer was set up at Kalyan near Bombay to observe the Sun at 610 MHz (Figure 15). The array consisted of thirty-two ex-Potts Hill parabolas; twenty-four of them were placed along a 630 m East-West baseline and the remaining eight along a 256 m North-South baseline. As we used a simple and novel transmission line system to connect the antennas, we were able to complete the Kalyan Radio Telescope by April 1965 (*Nature*, 1966). This radio telescope was used to investigate properties of the quiet and active radio Sun at 610 MHz during 1965-1968. It was found that the Sun showed considerable limb brightening, and that the solar corona had a temperature of around one million degrees.

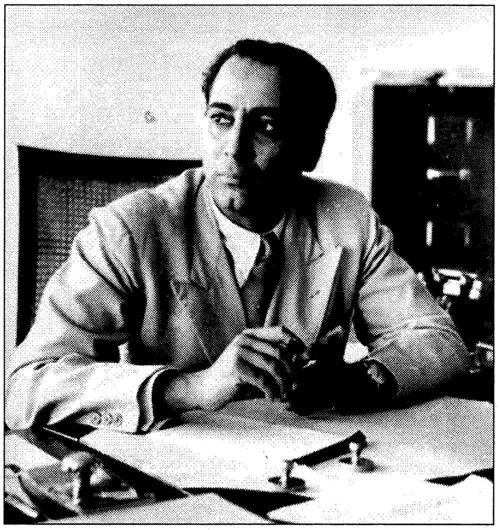

Figure 14: Dr Homi Bhabha (1911–1966), founding Director of the Tata Institute of Fundamental Research, Mumbai (courtesy: TIFR Archives).

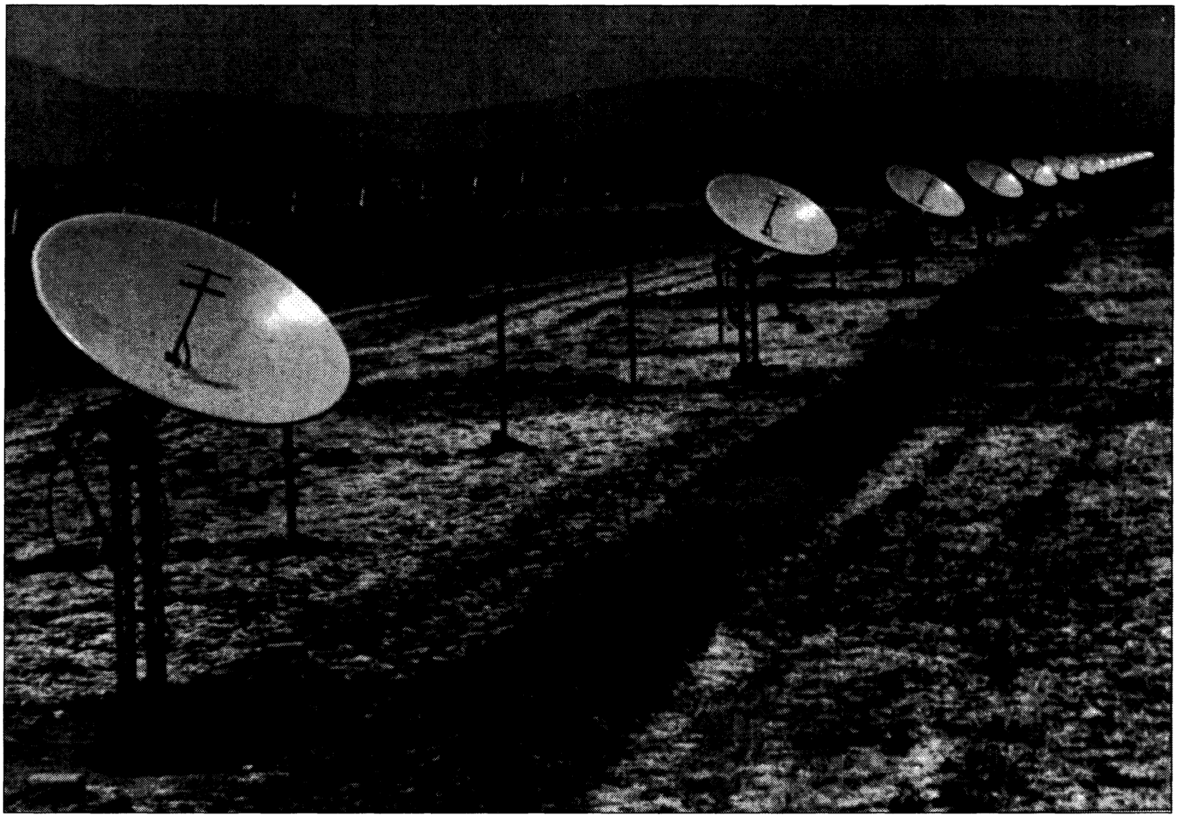

Figure 15: View of the east-west grating array consisting of twenty-four 6-ft diameter dishes built at Kalyan, near Mumbai, in 1965.

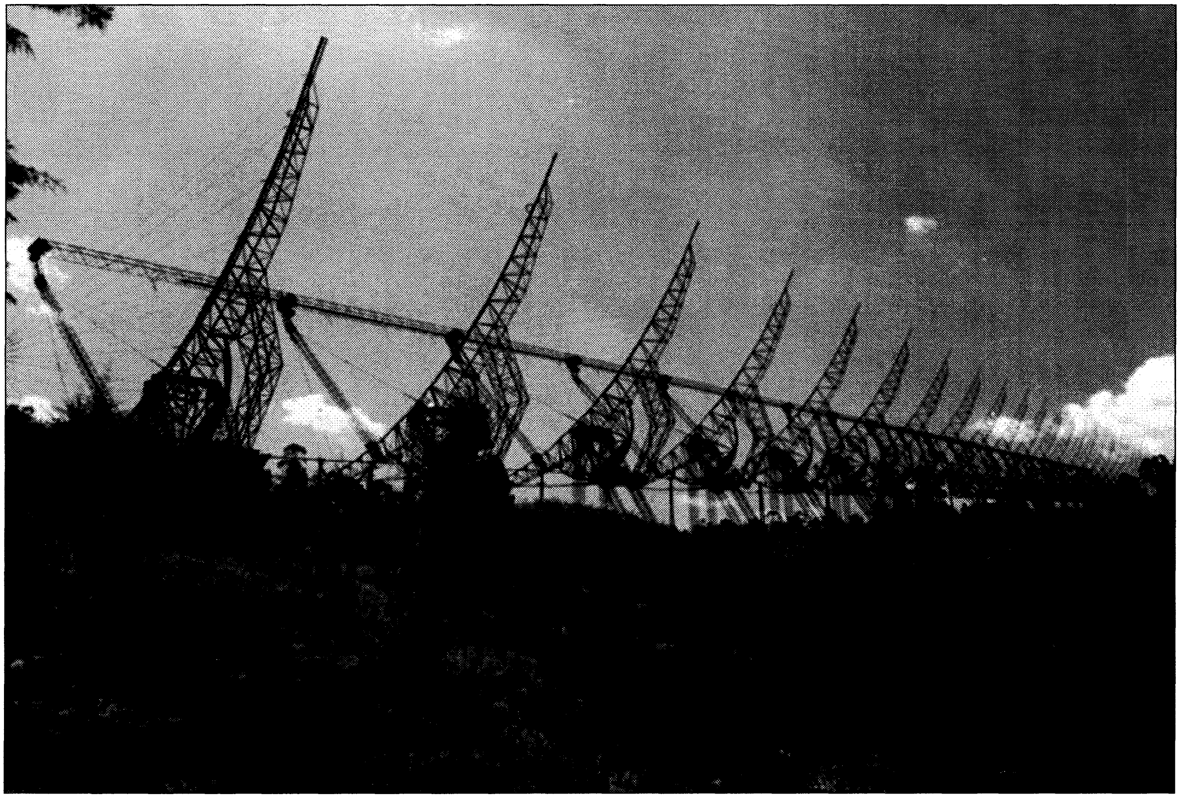

Figure 16: The Ooty Radio Telescope, consisting of the 530 m long and 30 m wide cylindrical parabolic antenna placed along a north-south sloping hillside at an angle of 11.3° so that its axis of rotation is parallel to that of the Earth.

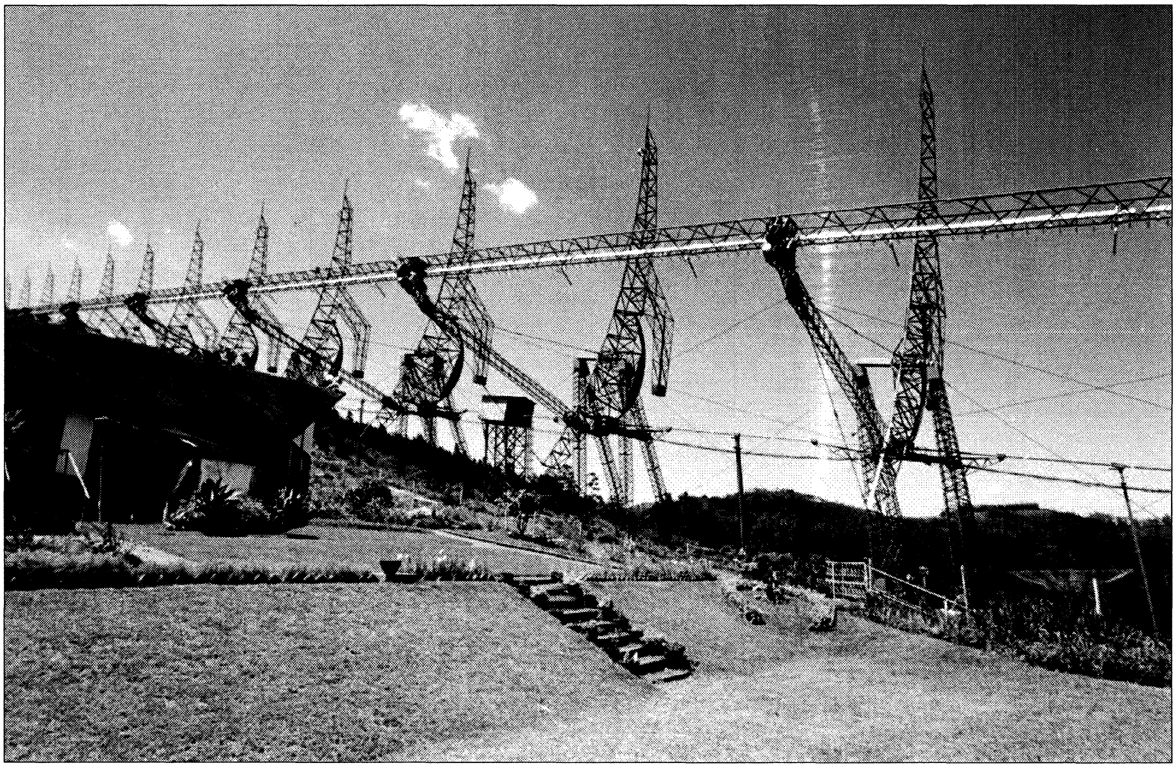

Figure 17: Another view of the Ooty Radio Telescope; reflections of sunlight by 1,100 stainless steel wires are seen on the right.

N.V.G Sarma and M.N. Joshi resigned from the NPL and joined the TIFR in 1964. D.S. Bagri, with a fresh M.Tech degree, joined the group in August 1964. Mukul Kundu returned from USA in early 1965 and contributed a great deal to the growth of the group during its critical formative years. Later, in 1968, he returned to the USA and joined Cornell University.

7 THE OOTY RADIO TELESCOPE

In early 1965, after an extensive search Sinha and I located a suitable hill at Ooty for the proposed equatorial radio telescope. This site is situated in the picturesque Nilgiri Hills in southern India, at an altitude of about 2,100 m. In late 1965, Bhabha approved the setting up of the Ooty Radio Telescope at that site, as part of an Inter University Centre (IUC). After corresponding with Jawahar Lal Nehru, India's Prime Minister, funding designed to give a boost to science education in India was obtained by Bhabha. A 600 acre plot of land was earmarked by the Tamil Nadu State Government for the IUC (Bhabha 1966). Soon after, in January 1966, Bhabha met a tragic end in a plane crash in The Alps at a relatively young age of 55 years, and the IUC did not materialize. However, the radio astronomy group continued to receive support from the TIFR, due the close interest and guidance of Professor Menon, an eminent cosmic-ray physicist who succeeded Bhabha as Director of the TIFR.

The Ooty Radio Telescope (ORT) was completed in early 1970, and it is still in operation. It consists of a 530m long and 30 m wide parabolic cylindrical antenna (Figures 16-17), located along a North-South hill slope equal to the latitude of the station (+11.35°). It is, therefore, possible to track celestial radio sources continuously every day up to 9.5 hours by a simple mechanical rotation of the telescope along its long axis. Along its 500-m long focal length is placed a phased array consisting of 1,024 dipoles operating in the RF band of 322-328.6 MHz, an internationally-protected band for radio astronomical observations. The structural and mechanical design was done by M/s Tata Ebasco, later named as Tata Consulting Engineers (TCE). The ORT was completed in December 1969, and the first occultation observations were made on 18 February 1970 (see Swarup et al., 1971). By early 1971, the radio astronomy group consisted of sixteen research workers: S. Ananthakrishnan, D.S. Bagri, V. Balasubramanian, Gopal Krishna, J.D. Isloor, M.N. Joshi, V.K. Kapahi, S. Krishna Mohan, V.K Kulkarni, D.K. Mohanty, T.K. Menon, A. Pramesh Rao, N.V.G. Sarma, C.R. Subrahmanya, T. Velusamy and myself. Dr. V.R. Venugopal joined in 1971. In addition, the group included several engineers and technical staff members. S.V. Damle, who had built the novel trombone-type phase shifters for ORT in collaboration with Kapahi, shifted to the microwave group at the TIFR. S.M. Bhandari from the Physical Research Laboratory also worked at Ooty for a Ph.D. degree. The design and construction of the ORT was a great challenge to the above team, as the development of technology in India was still in its infancy in those years, and foreign exchange for importing components was very limited (particularly after the India-China conflict in 1962, and later during the war between India and Pakistan). We were fortunate to receive a grant of US$70,000 from the National Science Foundation of the USA, which was used mainly to import a Varian Computer and test equipment. For the required electronic components, we ended up arranging for coaxial cables, type N and UHF connectors, and many other critical components to be developed by various firms for the first time in India. It must be noted that our success was solely due to a close teamwork of all the staff, whose median age in 1971 was about 27 years. The above scientists made many pioneering contributions and gained world-wide recognition for themselves and for Indian radio astronomy, thus paving the way for the future growth of radio astronomy in India.

T.K. Menon joined the TIFR in 1970 and guided several of the young research workers in the radio astronomy group. He returned to USA in 1974.

During the 1970s, lunar occultation observations of more than 1,000 radio sources were made at a frequency of 327 MHz using the ORT. The median flux density of these sources is about 0.6 Jy at 327 MHz, being about ten times lower than that of the 3C catalogue. The occultation survey was able to provide accurate positions of the source, and to reveal their angular structure with arc-second resolution. The data provided independent support to the Big Bang model (Kapahi, 1976; Swarup, 1976). Detailed physical properties of many Galactic and extragalactic were also derived. In addition, interplanetary scintillation (IPS) observations of selected samples of radio galaxies and quasars provided information on their compact structure with a resolution of 0.05 to 0.5 arc-second at 327 MHz. Valuable contributions were also made in the new field of pulsar astronomy. By 1984, the Ooty Synthesis Radio Telescope (OSRT) of 4 km extent was set up. It consisted of seven small parabolic cylindrical antennas measuring 23 m × 7.5 m and the large ORT itself, all combined with rather cumbersome radio links. The OSRT provided a resolution of ~45 × 50 arc second at 327 MHz. Scientific contributions made by the above group during the first twenty-five years are described elsewhere (Swarup, 1991).

8 THE GIANT EQUATORIAL RADIO TELESCOPE (GERT)

Following the success of the equatorially-mounted ORT, a proposal was mooted, first in 1976 and later more formally in 1978, to construct a Giant Equatorial Radio Telescope (GERT) consisting of a 2 km long and 50 m wide cylindrical radio telescope. This would be placed at a suitable site at the Earth's Equator in either Kenya or Indonesia. It was envisaged as the focal point of an associated International Centre for Space Sciences and Electronics (INISSE), a collaborative effort between several developing countries (Swarup et al., 1979). There was much talk at that time by world leaders stressing the need for South-South cooperation and India was very supportive of this concept. UNESCO provided a grant of US$14,000 for a feasibility study of the GERT and also arranged visits by Professor A.R. Hewish, Nobel Laureate, and myself to Kenya, Nigeria and Senegal, and later by an Indian team to Indonesia. The TIFR provided funds for the design and cost estimates of the proposed telescope, and also of a proposed 10 km synthesis radio telescope consisting of ten 100 m × 50 m parabolic cylinders in conjunction with the 2 km × 50 m main telescope. India indicated support for half of the all-up cost of the project, which was estimated as US$20 million. With the help of the local authorities, a suitable site was located close to the Equator in Kenya. However, Kenyan scientists were not able to follow up on the project after the demise of President Kenyatta. Later, two suitable sites were identified in West Sumatra (Indonesia) very close to the Equator, but progress was slow because of a lack of astronomical interest in most of the developing countries. In 1983 President Suharto of Indonesia pledged support for half the cost of the GERT. However, concerns were expressed about the high levels of seismic activity in West Sumatra, even though our engineers indicated that a suitable antenna could be built there without much cost penalty. Meanwhile, other major developments were taking place in international radio astronomy instrumentation and image processing techniques, as summarized below.

9 THE GIANT METREWAVE RADIO TELESCOPE: THE OFFSHOOT

By early 1982, revolutionary methods of phase and amplitude closures and self-calibration allowed radio astronomers to obtain radio maps of celestial sources of high quality even in the presence of phase and amplitude variations caused by electronics, the ionosphere or the atmosphere. It also seemed feasible to connect the antennas of a radio interferometer of a relatively large separation by using lasers and optical fibres. Further, after Ravi Subrahmanyan joined our radio astronomy group in 1983, we started calculating whether the ORT or the Very large Array (VLA) in the USA or the GERT would be suitable for studies of proto-clusters, the postulated condensates of neutral hydrogen existing at very high redshifts prior to the formation of galaxies in the Universe. To pursue this interesting problem, which is still a major challenge for radio astronomy, it became clear to us that a major new instrument was needed in order to fill the existing gap in radio-astronomical facilities at metre wavelengths. This goal, experience gained in designing and building the ORT, and the dynamism of the younger members of our group propelled me to propose the Giant Metrewave Radio Telescope on 1 January 1984.

Initially, in a flash, I divided the 2 km long and 50 m wide GERT into 34 smaller parabolic cylindrical antennas, joined by optical fibres, to form a synthesis radio telescope of about 25 km in extent. Since the operation over a wide frequency range seemed problematic using parabolic cylinders, we finally invented the concept of SMART (Stretched Mesh Attached to Rope Trusses) in order to build parabolic dishes of 45 m diameter economically and affordably: in this case necessity was the mother of invention![4] The GMRT project was approved by the Government of India in March 1987.

The GMRT consists of thirty parabolic dishes of 45 m diameter each, located across a region of about 25 km (Figure 18). Fourteen antennas are placed somewhat randomly in a central array of about 1 km × 1 km in extent, while the other sixteen dishes are situated along three 14 km long arms, making a Y-shaped array (Swarup et al., 1991). The GMRT operates at five radio frequency bands between ~ 110 and 1,430 MHz. Because of recent developments in electronics, it seems feasible to be able to operate the GMRT in any band free of radio frequency interference between ~ 40 MHz to 1,700 MHz. The GMRT became fully operational in 2000. Since January 2001 Indian and international astronomers are invited to apply for observing time, which is subsequently assigned to those who submit the best proposals. The GMRT has become the world's largest radio telescope operating in the above frequency range, and it complements existing large telescopes elsewhere. It has been used by more than three hundred astronomers from over twenty countries, and many interesting results have been obtained.[5]

I may note here that the untimely demise of M.N. Joshi in 1988 and of V.K. Kapahi, who were amongst the main architects of the group, was a great loss to the India and international radio astronomical communities.

10 THE SQUARE KILOMETRE ARRAY PROJECT (SKA): TO THE FUTURE

In 1960 Jan Oort highlighted the importance of building a radio telescope with a collecting area of about one million square metres in order to investigate the distribution of neutral hydrogen gas and its evolution in our Galaxy and across the Universe. As a result, Belgian and Dutch astronomers proposed a large Cross-type antenna with an area of ~1 million square metres, which they called the Benelux Cross, but this did not materialize. After noting the success of the one mile telescope in UK and developments in interferometry in Australia and elsewhere, the Dutch astronomers decided to build the Earth's Synthetic Radio Telescope at Westerbork, consisting of twelve dishes each 25 metres in diameter (two more were added later).

Oort's vision of a radio telescope with a large collecting area also led to a proposal for an array of 'Venetian blind' type configuration with a large number of parabolic cylindrical antennas (see Bracewell, Swarup and Seeger, 1963). In 1980, Barney Oliver proposed construction of 1,000 microwave antennas each of 100 m diameter in order to search for evidence of extra-terrestrial intelligence (SETI). In 1988, a proposal was put forward by Swarup at the International Astronautical Congress at Bangalore, for the construction of 1,000 dishes of 45 m diameter (similar to the GMRT antennas) for SETI, as well as for radio astronomical studies. In a symposium held at Socorro on the occasion of 10th anniversary of the operation of the VLA, Peter Wilkinson (1981) proposed construction of 100 antennas of 100m diameter, called 'The Hydrogen Array'. Swarup (1991) examined the cost of a large number of 45 m dishes for such a telescope, and called it the International Telescope for Radio Astronomy (ITRA). Earlier, Jan Nordaam in Netherlands had informally discussed a large telescope with similar objectives, called the SKAI. All of these ideas led to the setting up of a 'Large Telescope Working Group' at the Kyoto URSI meeting in 1993, and the IAU endorsed this group in 1994.

These historical developments paved the way for a more definite proposal for a billion dollar radio telescope project, called the Square Kilometre Array (SKA), which will be built between 2010 and 2020 by an international consortium consisting of institutions in Argentina, Australia, Brazil, Canada, China, seventeen countries in Europe, India, South Africa and the USA. The SKA will be mankind's most ambitious step in the exploration of the Universe through the radio window of the electromagnetic spectrum.

Figure 18: A close view of one of the thirty 45 m diameter fully steerable parabolic dishes of the Giant Metre Wavelength Telescope located at Khodad, near Pune, in western India. A few dishes of the central array of the GMRT are also visible in this picture.

11 EPILOGUE

Ever since its independence, India has made steady progress in scientific endeavours, thanks largely to the substantial support of the great visionary Prime Minister, Jawahar Lal Nehru, who viewed the newly-established scientific laboratories as the temples of modern India. Although pioneering work in international radio astronomy started at metre wavelengths, the emphasis in advanced countries soon shifted to much shorter wavelengths, in order to obtain high angular resolution (even though it required more expensive equipment). However, there are many exciting and challenging astrophysical problems that can be studied better, or exclusively, at metre wavelengths. At such long wavelengths the required tolerances of antennas are much lower, and wire mesh suffices as the reflecting surface of parabolic antennas, and minimizes the wind loading. Further, such antennas are labour-intensive, and because of low labour costs they can be built economically in India. The above factors have contributed to the success of radio astronomy endeavours in India.

Currently, several countries are building order-of-magnitude larger facilities at decimeter and metre wavelengths, such as LOFAR in Netherlands, Milleura in Australia (a joint venture of US and Australian scientists), and the seeding of the very ambitious SKA project. India can make substantial contributions to these efforts by using expertise developed in the field of radio astronomy and its proven expertise in the area of computer software development.

12 NOTES

1. Pawsey remarked that I was unlike most Indians, who preferred theoretical work.
2. Dr Bowen had played an important role in the development of radar in the UK and the USA during the War years. After joining RP, rain-making and upper atmospheric physics became his personal fields of research, but throughout his directorship he continued to provide vital support for the growth of radio astronomy within the Division. His principal legacy must surely be the 64 m Parkes Radio Telescope (see Robertson, 1992).
3. In recent years, tens of radio astronomers and engineers have migrated from India to observatories around the world, thus proving that the Earth is round!
4. We understand that Chinese radio astronomers are adopting certain aspects of the above concept for their proposed 500 m diameter antennas for the SKA, and that they have recently constructed a 30 m diameter prototype antenna.
5. For examples of some of the research results see the web site: www.ncra.tifr.res.in).

13 ACKNOWLEDGEMENTS

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Our success with the Ooty Radio Telescope and the GMRT has been due to the close teamwork of the NCRA staff, and I am grateful to all of them.

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THE GENESIS OF SOLAR RADIO ASTRONOMY IN AUSTRALIA

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Abstract: In late 1945, O.B. Slee at RAAF Radar Station 59 near Darwin and staff from the CSIRO's Division of Radiophysics in Sydney were involved in Australia's first investigation of radio emission from the Sun. After WWII, the Sydney radio astronomers were joined by small independent groups based at the Commonwealth Observatory, Mt Stromlo, and in the Physics Department at the University of Western Australia, in Perth. Between 1946 and 1948, these young scientists made an important contribution to international astronomy, heightening our understanding of solar physics and the relationship between sunspots and solar radio emission.

Keywords: Australia, solar radio astronomy, O.B. Slee, CSIRO Division of Radiophysics, Commonwealth Observatory, University of Western Australia, W.N. Christiansen, L.L. McCready, J.L. Pawsey, R. Payne-Scott

1 INTRODUCTION

Radio astronomy enjoys a history that extends back a little over seventy years, but it only blossomed following WWII, largely as a result of technological advances made during the war years. These years of turmoil also marked the independent detection of solar emission in the U.S.A., Denmark, England, Australia and New Zealand (see Orchiston and Slee, 2002a; Sullivan, 1984), and it was the British and New Zealand discoveries that precipitated the solar radio astronomy program at the CSIRO's Division of Radiophysics (RP) in Sydney. In the immediate post-war years the RP group quickly gained world supremacy in this field of radio astronomy (Sullivan, 1988), a status that they were able to maintain with distinction through into the mid-1980s when the closing of the Culgoora Radioheliograph and staff promotions and retirements saw the demise of the Solar Group.

In this paper we describe the independent discovery of solar radio emission at Darwin in 1945, and document the solar radio astronomy research programs pursued by RP, the Commonwealth Observatory and the University of Western Australia during the formative years, 1945-1948.

2 THE DIVISION OF RADIOPHYSICS

According to Payne-Scott (1945: 1), the initial solar observations carried out by the RP group "… were inspired by the almost simultaneous arrival of three reports in the laboratory …" in mid-1945. One was Reber's (1944) paper in the *Astrophysical Journal*, and the other two were 'secret' war-time accounts of the independent detection of solar radio emission by New Zealand and British radar operators (Alexander, 1945; Army Operational Research Group, 1945).

Of most significance were the New Zealand results, which were coordinated by Dr Elizabeth Alexander (Figure 1), a British-born Cambridge geology graduate who unwittingly became the world's first female radio astronomer (see Orchiston, 2005a). The observations were made with five British-built 200 MHz COL units (Figure 2) sited at Royal New Zealand Air Force radar stations on the northern part of the North Island of New Zealand and on Norfolk Island (Figure 3).

Figure 1: Dr Alexander (1908–1958) the world's first female radio astronomer (courtesy Mary Harris).

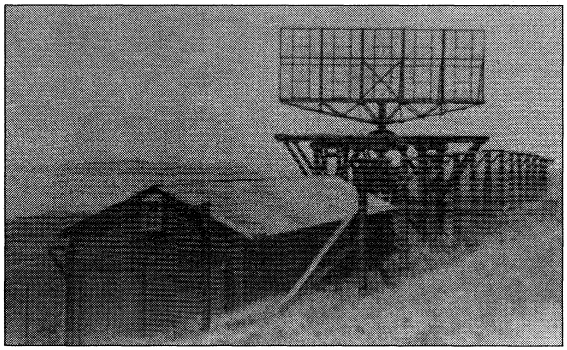

Figure 2: The RNZAF Whangaroa Radar Station (courtesy Gordon Burns).

Figure 3: Locations of the five Royal New Zealand Air Force radar stations where solar observations were made during March-April 1945. Key: 1 = Norfolk Island, 2 = North Cape, 3 = Whangaroa, 4 = Maunganui Bluff, and 5 = Piha (adapted from Orchiston and Slee, 2002a).

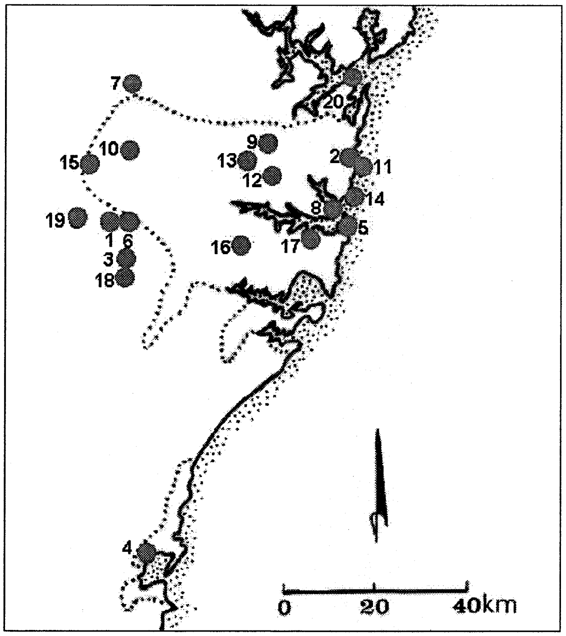

Figure 4: The twenty different sites in the Greater Sydney-Wollongong regions associated with radio astronomy, 1945-1963. Those mentioned in this paper are Collaroy (2), Dapto (4), Dover Heights (5), Fleurs (6), Georges Heights (8), Hornsby Valley (9), North Head (14), Penrith (15), Potts Hill (16) and the Radiophysics Laboratory in the grounds of the University of Sydney (17). (after Orchiston and Slee, 2005a).

2.1 The Initial Observations at Collaroy

Since similar COL radar units were deployed in Australia at Royal Australian Air Force radar stations the RP radio astronomy group leader, Dr Joe Pawsey (1908–1960; see Figure 3 on page 22), commandeered the antenna at Collaroy, a coastal suburb of Sydney (location 2 in Figure 4), in an attempt to replicate the New Zealand results. Radar 54 at Collaroy was situated ~107 m above sea level, and comprised a vertical "… broadside array of four horizontal rows each of ten half-wave dipoles with a reflector, having a gain of 80 relative to a half-wave dipole (i.e. g = 130) and a horizontal beamwidth (to the half power points) of 10°." (Payne-Scott, 1945: 6-7). The antenna was mounted so as to rotate in azimuth only. In this configuration it functioned as a sea interferometer (see Bolton and Slee, 1953), and because of the location of the site could only observe the rising and setting Sun.

Despite this shortcoming, from 3 October 1945 RAAF personnel at Station 54 carried out regular solar monitoring on behalf of RP for about an hour after sunrise and before sunset. At these times,

> The aerial (whose elevation remains zero) is swept from a bearing of about 15° south of the position of the sun at rising (or setting) to a position 25° on the other side of the bearing of rising (or setting); it is stopped at intervals of about 2° and a reading taken of the meter. The zero and general noise level are checked at the beginning and end of each sweep. (Payne-Scott, 1945: 7).

From the very first observations there were promising results:

> The first readings were taken on the morning of October 3rd of this year [1945]. The time of sunrise was 0530 hours and the bearing 95°. A few sweeps were taken before sunrise and showed only a slight random variation, due probably to a combination of changes in aerial impedance with bearing, man-made noise on certain bearings and changes in the receiver itself. At 0531 an increase in noise power of about 27% over the general level was observed at a bearing of about 94°. In successive sweeps this increase in noise became more marked, until at 0540 the noise power on a bearing of 93° was 4½ times the normal noise power. Over the next twenty minutes it declined, rose again to a smaller peak at 0610 and then declined again, the effect being just detectable at 0730.
>
> Since this date a fairly continuous series of observations have been made each sunrise and sunset, and on each occasion radiation has been observed ... One feature not shown [in Figure 5, overleaf] … is the short period fluctuations; the noise from the sun causes a fairly steady meter deflection on which are superimposed at intervals of perhaps a few seconds kicks which may be of the same order as the steady deflection; the relative magnitude and frequency of occurrence of these kicks seems to be independent of the elevation of the sun over the hour or so during which it is observed. (Payne-Scott, 1945: 7-8)

The accumulated evidence from October 1945 left no doubt that the anomalous 'noise' was of solar origin. It was there at sunrise and sunset and came from ±3° of the bearing of the Sun; the rate of change of peak intensity reflected that of the Sun; the 'noise' was only detectable when the Sun was in the aerial beam (i.e. for about an hour after sunrise and before sunset); and the horizontal field pattern was similar to the aerial field pattern, "… indicating radiation from a source of small angular width." (Payne-Scott, 1945: 8).

However, a major limitation of the Collaroy COL radar system was that the antennae could only monitor solar emission at sunrise and sunset. To determine the level of solar noise between sunrise and sunset it was necessary to employ a different antenna system, one that could track the Sun throughout the day. Accordingly,

> … some observations were made with an Army S.L.C. radar at North Head [location 14 in Figure 4] having 4 Yagi aerials with a total gain of about 24 and a receiver of worse noise factor than that used at Collaroy. The radiation from the sun produced a change in meter reading of only a few percent on this set. On 31st October the radiation from the sun was observed from 1400 hours till sunset, during which time there was no observable change in its value except for a gradual increase towards sunset to about twice the original level, probably due to reinforcement from ground reflections. The radiation ceased within a few minutes of sunset. (Payne-Scott, 1945: 11).

One of the features of the solar emission, the short-period fluctuations (or 'kicks') mentioned above by Payne-Scott, provided an intriguing challenge:

> The meter deflections observed over a period of a few seconds … may be due either to absorption or scattering of the radiation in the earth's atmosphere or to genuine fluctuations in the solar radiation. There is so far little evidence one way or the other, but *this will be one of the first points to be investigated in future work, as it is critical in deciding the origin of the radiation.* (Payne-Scott, 1945: 9; our italics).

Payne-Scott's (1945: 10) detailed unpublished report reveals that on 4 and 5 October solar observations also were made at sunrise "… on two other laboratory systems, one operating on 600 Mc/s. and the other on 1200 Mc/s."[1] While no solar emission was noted at 600 MHz, detections were recorded on both days at 1,200 MHz.

In their war-time 'secret' reports, Alexander and Hey both commented on the association between sunspots and solar emission, and this was immediately apparent when the mean daily 200 MHz Collaroy noise levels between 3 and 23 October 1945 were compared with total sunspot area (see Figure 5).

On 23 October 1945, Ruby Payne-Scott (Figure 6) joined with Joe Pawsey and Lindsay McCready in penning a brief report on their work for *Nature*, and after unexpected and totally unacceptable delays[2] this was published in the 9 February 1946 issue of the journal—shortly after Hey's (1946) belated announcement of his war-time observations.[3] In their paper, "Radio-frequency Energy from the Sun", Pawsey et al. (1946: 158) announced that they had

> … observed, from the direction of the sun, a considerable amount of radiation having the apparent characteristics of fluctuation 'noise' when observed on a cathode-ray oscillograph or head-phones. However, the output meter reading fluctuated considerably, a characteristic which is not typical of normal thermal agitation 'noise'. The variation of apparent azimuth of arrival and of intensity with horizontal rotation of the aerial and the sun's elevation was qualitatively consistent with the assumption of radiation from the body of the sun modified by the known directional characteristics of the aerial.

In commenting on the Figure 5 data, Pawsey et al. (ibid.) stated that:

> It is apparent that the peaks of 1.5-metre radiation coincide with the peaks of the sunspot area curve and with the passage of large sunspot groups across the meridian. This strongly indicates a physical relationship between the two phenomena …

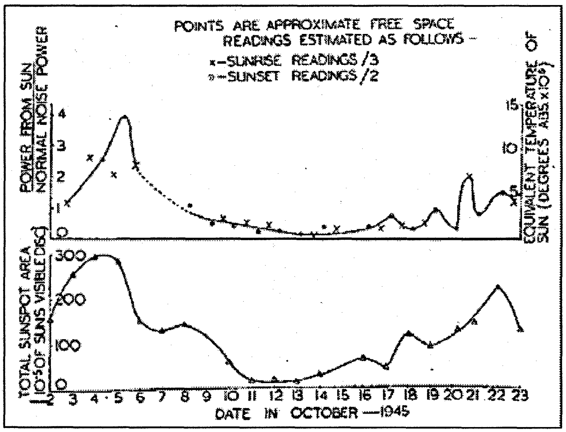

Figure 5: Plots of solar radio emission at 200 MHz and total sunspot area, in 1945 October (after Pawsey et al., 1946).

These initial RP excursions into solar 'noise' produced promising results but they also raised many interesting questions. They also served to justify the continuation of radio astronomy (as it would later become known) as a valid field of investigation in RP's quest to identify a viable post-war research portfolio.

Figure 6: Ruby Payne-Scott (1912–1981), Australia's first female radio astronomer (courtesy Miller Goss).

2.2 Parallel Observations at Dover Heights

Following the successes of October 1945, solar monitoring at Collaroy continued through to March 1946, and it was soon arranged for parallel observations to be made with the SHD antenna at the Dover Heights radar station (Figure 7). This was situated in the eastern suburbs of Sydney, and ~17 km to the south of Collaroy (location 5 in Figure 4). In order to obtain a visual record of solar radio emission, chart recorders were included in the receiving systems, and in February 1946 (when a massive sunspot group dominated the photosphere) these provided a clear permanent record of the Sun's passage through the sea interferometer fringes, not only revealing temporal variations in the background level of solar radiation but also intense bursts of short duration (see Figure 8). Obviously these bursts were the 'kicks' that Payne-Scott had earlier alluded to.

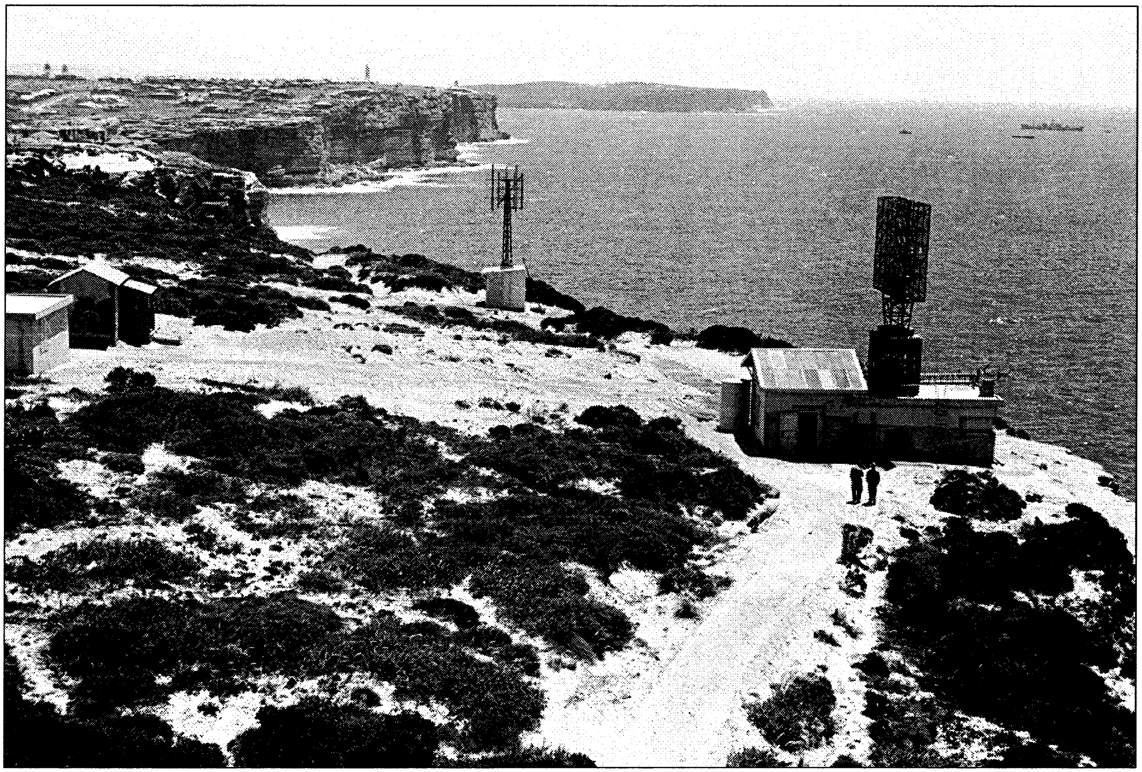

Figure 7: The Dover Heights Radar Station, showing the 200 MHz Shore Home Defence (SHD) radar antenna centre right. An identical unit was sited at the Collaroy Radar Station (ATNF Historic Photographic Archive: B81-1).

In addition to this burst emission, Pawsey was interested in the quiescent level of solar radiation at 200 MHz, and by isolating and quantifying the non-burst component he was able to demonstrate the existence of a 'hot corona' that far exceeded the temperature of the photosphere, something that had been hinted at previously by optical astronomers (see Sullivan, 1982: 208). Pawsey's (1946b) announcement in *Nature* of a temperature of 10^6 K immediately followed a theoretical paper by D.F. Martyn (1946b) predicting a coronal temperature of that order. Originally an RP employee, Martyn was at that time based at the Commonwealth Observatory, Mt Stromlo, and had good working relations with some of his former colleagues at RP. Notwithstanding the order in which the two *Nature* papers were published and Martyn's inclusion in Pawsey's acknowledgements (but not *vice versa*), there has been considerable debate about who first came up with the million degree coronal temperature. Did Pawsey discover this through his Collaroy observations and pass on this result to Martyn (who subsequently developed the appropriate theoretical framework), or was it Martyn's theoretical work that prompted Pawsey to look at the radio data? Sullivan (2005: 19) recently reviewed this interesting issue and although he assigns Pawsey chronological priority, he concludes that

> … Martyn was indeed the one who brought in the previous astronomical evidence of a million-degree corona and who pointed out that that the million-degree 'effective' or 'apparent' temperatures cited by the RP group could actually represent *thermal* emission from the solar atmosphere. Pawsey and his colleagues had calculated these temperatures, but thought of them only in a formal sense. In fact, to them these incredibly high values were at first prima facie evidence of *non*-thermal phenomena.

One of the major problems with the Collaroy and Dover Heights radar antennas was that they could not track the Sun for extended periods, so in early 1946 this prompted the RP staff to install simple 200 MHz 4-Yagi arrays on the roof of the blockhouse at Dover Heights (Figure 9), and at the North Head radar station. A third Yagi array was also set up at the Commonwealth Solar Observatory, Mt Stromlo, but more on this later.

Parallel daily monitoring with these steerable antennas quickly revealed almost identical patterns of solar emission, with a changing level of background radiation upon which were superimposed bursts of varying duration and intensity. The precise correspondence in the case of bursts, and the fact that burst activity did not vary systematically as the Sun rose from the horizon towards the zenith, proved conclusively that the emission was of solar origin and not caused by ionospheric scintillations

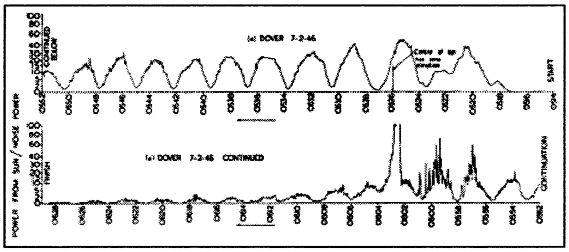

Figure 8: Chart record of the Sun passing through the sea interferometer fringes on 7 February 1946 at Dover Heights. Time, in minutes, runs from left to right, and the chronological sequence is continued in the lower strip where intense burst emission is apparent immediately before the Sun rises above the beam of the antenna (after McCready et al., 1947: 366).

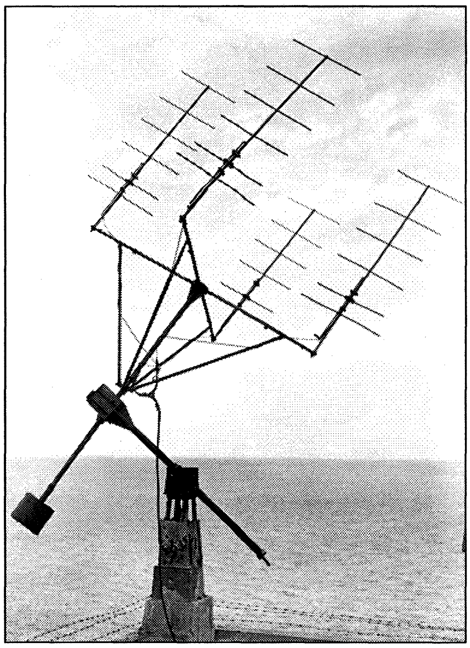

Figure 9: 200 MHz 4-Yagi antenna on the roof of the blockhouse at Dover Heights. Similar antennas were also installed at North Head and Mt Stromlo in early 1946 (ATNF Historic Photographic Archive: B1165-2).

By plotting the general background level of solar emission during October 1945-February 1946 against sunspot number and sunspot areas, the correlation noted earlier for October 1945 was confirmed, but in this instance McCready et al. (1947: 363) observed that "… the correlation with areas is somewhat closer than with sunspot number but neither is exact." The Sun was particularly active during this period, and as Sullivan (1982: 183) has pointed out, "… these novice astronomers were somewhat lucky in being able to observe the great sunspot group of February, 1946, one whose main sunspot is amongst the largest ever photographed." This group is shown in Figure 10.

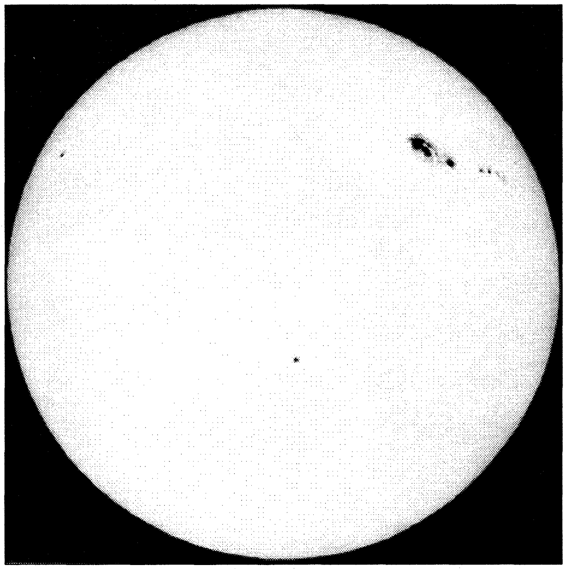

Figure 10: U.S. Naval Observatory photograph of the Sun taken on 8 February 1946, showing what at that time was the largest sunspot group on record (after Stetson, 1947: Plate I).

McCready et al. also investigated the location of the source of emission on the Sun by analyzing the interference fringes, as shown in Figure 8, and during the presence of the great sunspot group of February 1946 they were able to demonstrate conclusively that the solar radiation originated from a strip that in each case included this sunspot group. Examples deriving from Collaroy and Dover Heights are shown in Figure 11, and

> In each case the radiating strip has a width considerably less than that of the sun's disk, being of the order of the size of the sunspot group, and passes through the group. It moves across the sun with the spots as the sun rotates … There seems no reasonable doubt that the source was localized in a small region in the vicinity of the spots. [However] The observations do not provide any information as to the detailed structure of the source within this region. (McCready et al., 1947: 368).

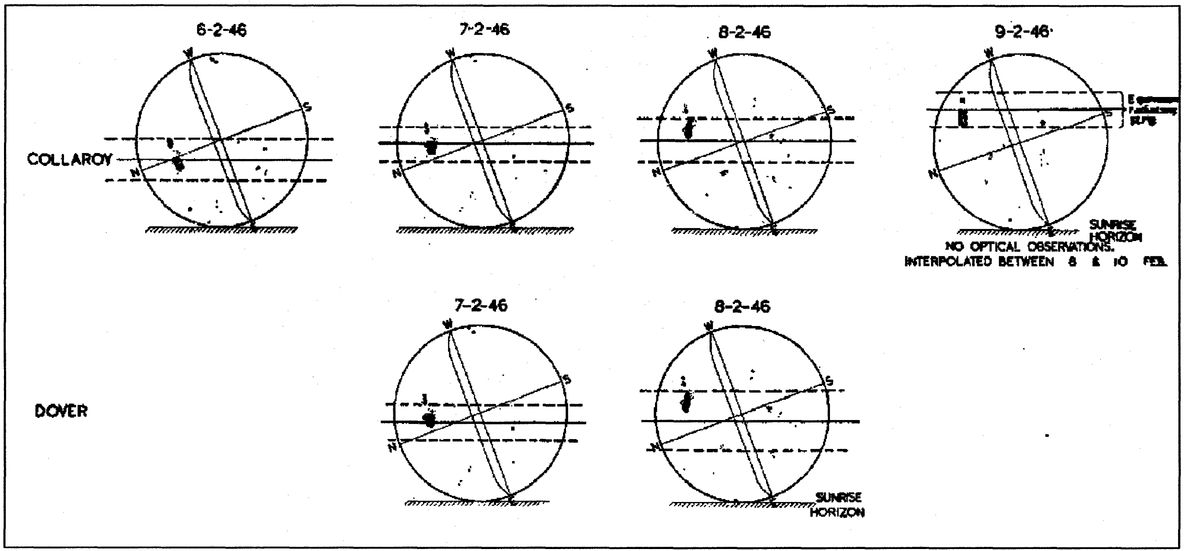

Figure 11: Drawings of the Sun between 6 and 8 February 1946 showing the position and width of the 'equivalent radiating strips' (dashed lines) from which the solar radiation originated (after McCready et al., 1947: 369).

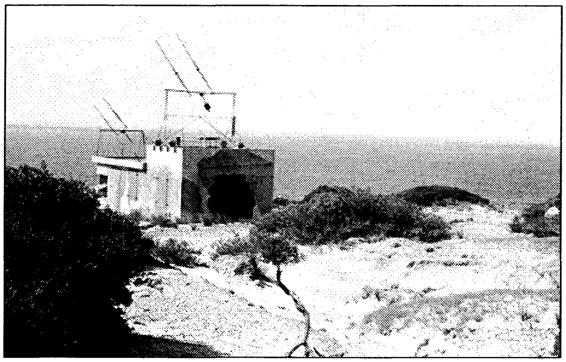

Figure 12: The Dover Heights blockhouse on 1 May 1947, showing the 60 MHz (left) and 100 MHz twin Yagis (right) (ATNF Historic Photographic Archive: B1031-3).

Although McCready et al. (1947) conducted most of these pioneering investigations at 200 MHz, they also made a few observations at 75 and 3,000 MHz. At the higher frequency they detected a low level of solar radiation, consistent with Southworth's (1945) published results, whereas at 75 MHz the solar emission was comparable to that recorded at 200 MHz. They concluded that the intensity of the 200 MHz was at times too great to be accounted for in terms of thermal radiation, and suggested the solar noise derived from "… gross electrical disturbances analogous to our thunderstorms." (McCready et al., 1947).

The next challenge was to expand these multifrequency observations, with particular emphasis on burst emission, and in mid-1946 a simple 60 MHz twin Yagi was constructed and joined the 75 MHz twin Yagi and 200 MHz 4-Yagi array already on the roof of the blockhouse at Dover Heights. At the time, Pawsey was pleased with progress, and in a letter to the British radio physicist Jack Ratcliffe, dated 2 August 1946, he wrote:

> I have been principally interested over the last six months in the problem of radio frequency noise from the sun … At the moment we are doing a bit of exploring, taking measurements of intensities at a number of different frequencies, some during the day and others at dawn. We have found that the variation of solar noise on different frequencies is dissimilar and that the dawn effect on 60 Mcs. is much more complicated than it is on 200 Mcs. (Pawsey, 1946a).

In November 1946, Payne-Scott and her colleagues were able to improve the range of observations made at Dover Heights when a 100 MHz twin Yagi (Bolton, 1982) replaced the 75 MHz antenna on the roof of the blockhouse. The general appearance of the blockhouse in mid-1947 is shown in Figure 12, although the 200 MHz antenna is hidden from view.

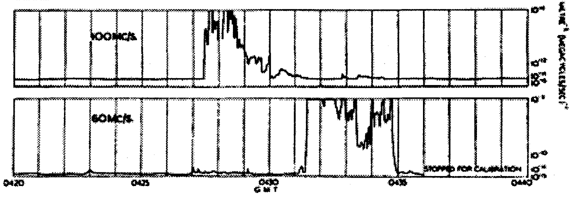

Figure 13: Large outburst on 8 March 1947 (after Payne-Scott et al., 1947).

From July 1946 the relative arrival times of bursts recorded at three frequencies were noted, and on 21 May 1947 Payne-Scott, Don Yabsley and John Bolton (1947) completed a paper reporting the results of this research. They found that small bursts often were not correlated at the three frequencies. In contrast, many of the larger bursts detected during July-August 1946 were present at all three frequencies, but did not occur simultaneously. They arrived in the sequence 200, 75 and 60 MHz, with a typical delay of ~2 seconds between bursts at 200 and 75 MHz, and a similar interval between bursts at 75 and 60 MHz. A few observations were also made at 30 MHz, and the delay in arrival times of bursts at 60 and 30 MHz was also a few seconds. When it came to outbursts, the delays in the respective arrival times at the different frequencies were of at the order of several minutes, and an excellent example, recorded at 200, 100 and 60 MHz on 8 March 1947, is shown in Figure 13 (see, also, Bolton, 1982: 350 for a dramatic account of this event). This outburst was associated with a solar flare and short-wave radio fadeout, and an aurora was visible from some areas of Australia on the evening of 9 March.

In interpreting their observations, Payne-Scott et al. (ibid.) concluded that "The successive delays between the onset of the outburst on 200, 100 and 60 Mc/s. suggest that the outburst was related to some physical agency passing from high-frequency to lower-frequency levels [in the solar corona]." Later this 8 March 1947 event would be classified as a Type II outburst in Paul Wild's spectral classification of solar bursts, while those bursts with short inter-frequency time delays referred to above are examples of Type III and the isolated non-correlated bursts belong to Type I.

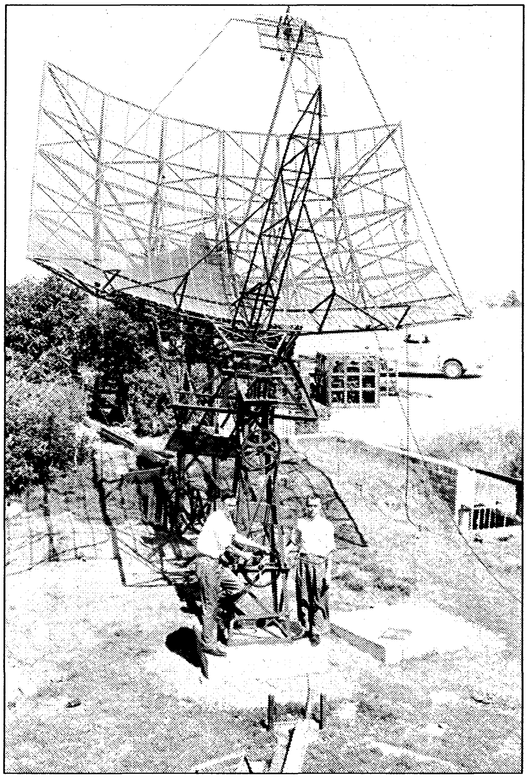

Figure 14: The radar antenna that was used for solar radio astronomy at Georges Heights and later at the Potts Hill field station. Joe Pawsey is on the left and Don Yabsley on the right (ATNF Historical Photographic Archive: B1031-1).

2.3 Expanding the Frequency Coverage

Pawsey was keen to expand the range of frequencies used in the RP solar monitoring program, and in 1947 and 1948 the work at Dover Heights was complemented by data supplied from two new observing sites, Georges Heights and the 'Eagles Nest'.

The WWII Georges Height radar station (Orchiston, 2004b; Orchiston and Slee, 2005a) occupied an attractive site at Middle Head over-looking the entrance to Sydney Harbour (location 8 in Figure 4), and Pawsey wanted to take advantage of a novel experimental radar antenna comprising a 14 × 18 ft (i.e. 4.3 × 4.8 metre) section of a parabola (Figure 14) which was located there. In June 1947 Fred Lehany and Don Yabsley were assigned to this instrument, and many years later Lehany (1978) recalled that their involvement "… came about in a typical 'Pawseyian way', before I knew what was happening … there was an observing program and … Yabsley and I were a suitable pair to share not only the week days but also the weekend duty …" Between June and August 1947 Bruce Slee assisted Lehany and Yabsley in developing the receivers and feed systems and operational testing of the antenna, before returning to Dover Heights.

Yabsley (1978) built a triple-feed system that allowed the ex-radar antenna to operate simultaneously at 200, 600 and 1200 MHz (see Figure 15), but a problem was the cumbersome altazimuth mounting that was never designed for radio astronomy. The fledgling radio astronomers found that the only way the antenna could be used effectively was to position it ahead of the Sun, let the Sun drift through the beam, hand-crank it ahead of the Sun again, and repeat the process throughout the day. This procedure produced a distinctive 'picket fence' chart record (see Figure 16).

By August 1947 the equipment was fully operational, and solar monitoring was carried out for about two hours daily, from 18 August until 30 November, resulting in the detection of many bursts at 200 MHz. In contrast, bursts were rare at 600 and 1200 MHz (see Figure 16), where the general flux variations with time were correlated with sunspot area (Figure 17). This distinctive pattern was discussed by Lehany and Yabsley in papers published in *Nature* and the *Australian Journal of Scientific Research* in 1948 and 1949 respectively. The 600 and 1,200 MHz emission was "… compatible with the thermal radiation expected from the solar atmosphere at these frequencies. It is considered that the variations in intensity … are at least partly due to the magnetic fields of the sunspots raising parts of the effective radiating shells into the corona …" (Lehany and Yabsley, 1949: 60).

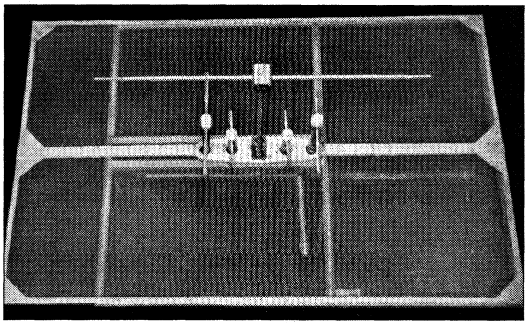

Figure 15: Close-up of the 200, 600 and 1,200 MHz dipoles (after Lehany and Yabsley, 1949: Plate 2).

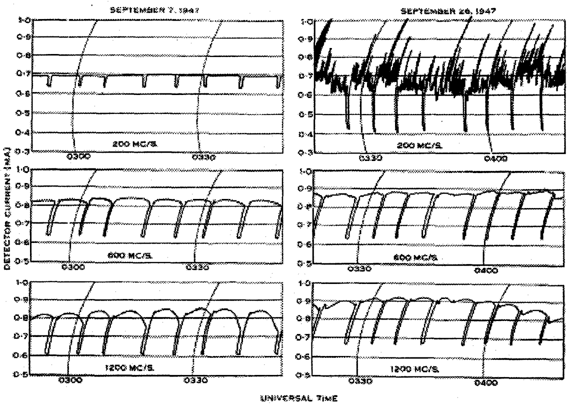

Figure 16: Chart records for 200, 600 and 1,200 MHz obtained at Georges Heights on 7 and 26 September 1947. Note the extensive 200 MHz burst emission on 26 September. The distinctive 'picket fence' nature of the chart record is clearly illustrated in the 600 and 1,200 MHz plots (after Lehany and Yabsley, 1949: 55).

Although burst emission at 600 and 1,200 MHz was rare, Lehany and Yabsley occasionally recorded

> Isolated disturbances … mainly of low intensity and fairly short duration. In some instances they were definitely associated with chromospheric flares and sudden daylight radio fadeouts …
>
> The largest 600 Mc/s. and 1200 Mc/s. disturbance in the series occurred on October 4, when the intensity levels at both frequencies increased to thirty times their normal value and remained high for approximately ten minutes … (Lehany and Yabsley, 1949: 58; cf. Lehany and Yabsley, 1948).

The most conspicuous 'disturbances'—or outbursts—mirrrored the event recorded at Dover Heights on 8 March 1947 (cf. Figure 13).

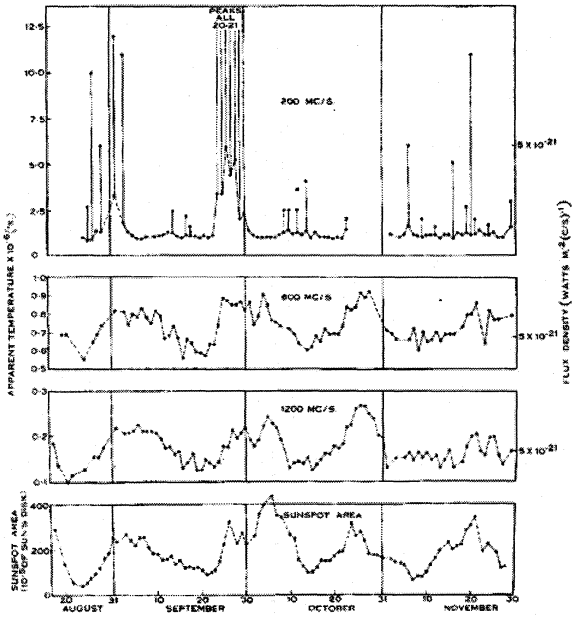

Figure 17: Georges Heights 200, 600 and 1,200 MHz observations during August-November 1947. At the two higher frequencies the correlation with variations in sunspot numbers is obvious. The sunspot data were provided by the Commonwealth Solar Observatory at Mt Stromlo (after Lehany and Yabsley, 1949: 56).

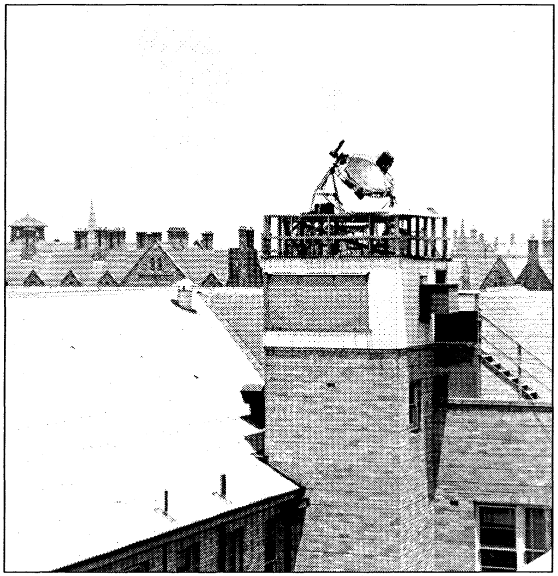

Figure 18: The 'Eagle's Nest' at the top of the Radiophysics Laboratory, showing the small antenna used Piddington and Minnett (ATNF Historic Photographic Archive: B1641).

These promising results from Georges Heights inspired Pawsey to expand the solar program far beyond 1,200 MHz, and in early 1948 he arranged for a 1.1 metre (44-inch) equatorially-mounted dish to be installed on the roof of the 'Eagle's Nest' (Figure 18), a small room located at the very top of the Radiophysics Laboratory (location 17 in Figure 4). Between April and October 1948, Jack Piddington (Figure 19) and Harry Minnett used this recycled WWII searchlight mirror to observe the Sun at 24,000 MHz. At this frequency the radiation originates from the chromosphere, and from 5 April to 30 August it was found

> "... to vary from day to day by amounts of up to about ± 8 per cent. These variations are smaller among the latter results, being less than ± 3 per cent. for the period July 14 to August 30 (with one exception on August 4). The variations were principally due to instrumental errors and decreased as the measuring technique was improved ... It is evident that solar radiation at a wavelength of 1.25 cm. remains fairly constant for long periods. There is little or no sign of correlation with sunspot areas or relative sunspot numbers (Piddington and Minnett, 1949: 543-544).

This pattern is in marked contrast to that obtained by Lehany and Yabsley at 600 and 1,200 MHz.

Figure 19: Jack H. Piddington, 1910–1997 (ATNF Historic Photo-graphic Archive).

On rare occasions between July and October 1948 Piddington and Minnett (1949: 545-546) recorded intervals of several hours when there were fluctuations of about ±5% in the solar radiation level. In at least one instance, on 6 August, the variations in 24,000 MHz emission mimicked those recorded at 200 MHz, but most major changes in 200 MHz noise levels were not accompanied by observable variations in solar emission at 24,000 MHz.

By the end of October 1948 the little radio telescope atop the Eagle's Nest had served its purpose and revealed the basic pattern of solar emission at 24,000 MHz. This modest aerial and the Georges Heights radar antenna were then transferred to the Potts Hill field station in anticipation of the November 1948 solar eclipse (see Section 2.4, below).

Before examining the key role played by this solar eclipse, let us return briefly to Dover Heights—the site of many of RP's most important solar discoveries in 1945-1946. By the end of 1947, the various Yagi antennas there were being used very effectively by John Bolton, Gordon Stanley and Bruce Slee in their search for new 'radio stars' (Bolton, 1982; Kellermann et al., 2005; Orchiston, 2004a; Slee, 1994), so Payne-Scott decided to transfer her solar program to Hornsby Valley.

The Hornsby Valley field station (Figure 20) occupied a radio-quiet valley on the isolated northern fringes of suburban Sydney (location 9 in Figure 4), and initially was used by Frank Kerr and Alex Shain to bounce radar signals off the Moon in a quest to investigate the Earth's ionosphere and the nature of the lunar surface (see Orchiston and Slee, 2005a, 2005b). At the end of 1947 Payne-Scott set up single 60, 65 and 85 MHz Yagi antennas at Hornsby Valley, and from January through to September 1948 she used these, plus an 18.3 MHz broadside array and Kerr and Shain's 19.8 MHz Moon-bounce rhombic antenna, to study the characteristics of solar burst emission. Most of the observations were made at 60 and 85 MHz; a pair of crossed Yagis was used at 85 MHz so that circular polarization could be studied. While all of the Yagi antennas could track the Sun during the day, the 18.3 MHz and 19.8 MHz aerials were fixed and could not be directed exactly at the Sun so measurements made with them were subject to correction factors.

The accumulated observations revealed the existence of two distinct types of variable high-intensity radiation which Payne-Scott (1949: 215) termed 'enhanced radiation' and 'unpolarized bursts'. She describes the first of these phenomena:

> The intensity reaches a high level and remains there for hours or days on end; there are continual fluctuations in intensity, both long-term and short-term. The short-term increases are somewhat similar to [isolated] bursts ... but usually have a lower ratio of maximum to background radiation. This type of radiation will be called "enhanced radiation" ... Superimposed on it may be bursts ... There may be short periods during which the polarization is indefinite, either because two sources of opposite polarization are superimposed or because the radiation is linearly or randomly polarized, but for the great part of its life the enhanced level shows circular polarization of one sense or the other. (Payne-Scott, 1949: 216-217).

Enhanced radiation normally occurred during the passage of large sunspot groups, and the presence of circular polarization suggested "… that the magnetic field of the spot group plays a part in its production." (Payne-Scott, 1949: 225). Figure 21 shows examples of enhanced radiation recorded at 60 and 85 MHz on 30 August 1948. Payne-Scott (1949: 219) noted that at the two lowest frequencies surveyed (18.3 and 19.8 MHz) enhanced radiation was rare.

The second type of solar radiation Payne-Scott investigated at Hornsby Valley was the 'unpolarized bursts', which showed

> … a very good correspondence on different frequencies, though their shapes and relative amplitudes may vary considerably … the closer the frequencies and the larger the bursts, the closer their relationship. Corresponding bursts do not appear to skip frequencies. Thus, if a burst appears on 95 and 19 Mc/s., there will be a corresponding burst on 60 Mc/s. ... A characteristic unpolarized burst shows a finite rise time, rounded top, and slow decay, reminiscent of the transient response of a medium with a natural resonant frequency … There is no marked connexion between the rate of decay and the intensity of the burst … [but bursts recorded at 18.3 and 19.8 MHz] have a markedly slower decay rate … (Payne-Scott, 1949: 219-221).

Payne-Scott noted that single unpolarized bursts were rare (they generally occurred in complex groups), and double-peaked bursts were particularly common, suggesting that "… the second peak may be an echo of the original disturbance." (Payne-Scott, 1949: 222).

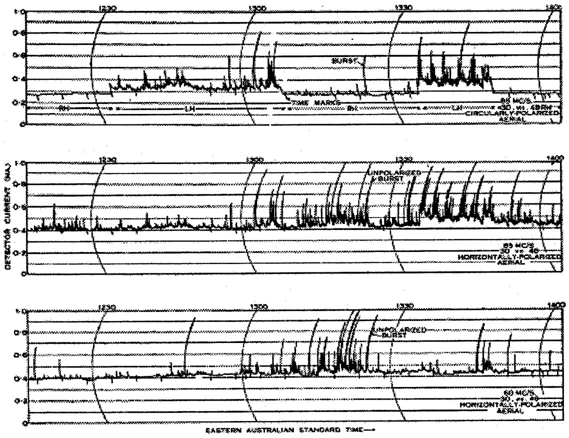

Figure 21: Examples of enhanced radiation with superimposed bursts of short duration. The enhanced radiation shows left-hand circular polarization (after Payne-Scott, 1949: 218).

Payne-Scott concluded that the generation of unpolarized bursts was not associated with sunspot magnetic fields—even though bursts were most common when sunspot groups were visible on the solar disk. Rather, they originated well out in the solar corona.

Figure 20: The picturesque Hornsby Valley field station showing one of the low frequency arrays used by Shain and Higgins for Galactic research. Unfortunately, no clear images exist of the Yagi antennas that Payne-Scott used for her Hornsby Valley solar radio astronomy program (ATNF Historic Photographic Archive: B2802-10).

Figure 22: View looking south showing the eastern water reservoir at Potts Hill. The radio astronomy field station flanked this reservoir, and the radio telescopes used to observe the November 1948 solar eclipse were located on the flat sparsely-wooded area in the foreground, immediately to the north of the reservoir (ATNF Historic Photographic Archive: B3253-1).

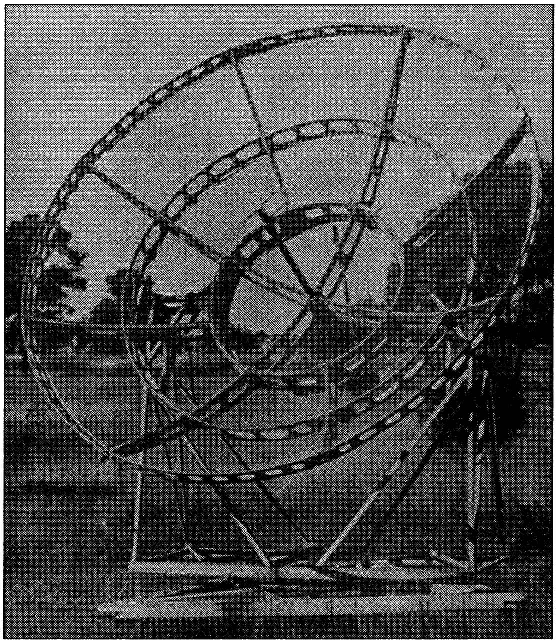

Figure 23: The 3.05 metre (10-ft) dish at Potts Hill (ATNF Historic Photographic Archive: B1511).

Payne-Scott (1949) also investigated the relative arrival times of associated unpolarized bursts at different frequencies, and found that 60 MHz bursts arrived on average 0.7 seconds later than their 85 MHz counterparts; 60 MHz bursts 0.3 seconds later than their 65 MHz counterparts; and 18.3 and 19.8 MHz bursts about 9 seconds later than their 60 MHz counterparts. However, considerably variations were encountered in all three studies. Nonetheless, these delays differed markedly from those shown in Figure 13, and indicate that Payne-Scott's relatively common 'unpolarized bursts' represented a very different type of solar event.

Upon completing her Hornsby Valley solar studies, Payne-Scott transferred to the Potts Hill field station (see Orchiston and Slee, 2005a), which then became the home base for most of the Division's burgeoning Solar Group.

2.4 The Source of the Solar Emission and the 1948 Solar Eclipse

The Potts Hill field station (Figure 22) in suburban Sydney (location 16 in Figure 4) was located beside Sydney's main water reservoir and apart from the relocated Georges Heights and Eagle's Nest radio telescopes, by November 1948 boasted a single Yagi antenna that was used by Alec Little to observe the Sun at 62 MHz and a simple 3.05 metre (10-ft) altazimuth-mounted wire-mesh dish (Figure 23) that would later be employed by Piddington and Minnett for solar and Galactic research at 1,210 MHz.

The principal incentive for the consolidation of solar astronomy at Potts Hill was the 1 November 1948 partial eclipse of the Sun. In the late 1940s the angular resolution of radio telescopes was poor, and observations of total and partial solar eclipses offered an elegant way of pinpointing the positions of localised regions responsible for solar radio emission and also of determining the distribution of radio brightness across the disk of the Sun. The reasoning was that as the Moon's limb moved across the Sun's disk and masked different radio-emitting regions there would be obvious dips in the chart record (Hey, 1955). Covington (1947) was the first to pioneer this technique, in 1946, and the RP radio astronomers were keen to take advantage of the 1948 eclipse which was visible from Australia.

If there was only one observing site, then any dip in the chart record would simply indicate that the source region was located *somewhere* along the arc subtended by the lunar limb *at that particular moment*, but by using several widely-spaced observing sites the intersections of the different limb profiles allowed the precise positions of the radio-active regions to be determined. For the 1948 eclipse, the refurbished ex-Georges Heights radar antenna—complete with a new equatorial mounting (Figure 24)—was used at Potts Hill, while 3.05 metre wire mesh dishes (identical to the antenna shown in Figure 23) were installed at temporary observing stations at Rockbank, near Melbourne, and Strahan, on the west coast of Tasmania (see Figure 25 for non-Sydney locations). For the States of Victoria and Tasmania, this eclipse represented their very first forays into the exciting new world of radio astronomy (e.g. see Orchiston, 2004d). All three radio telescopes operated at 600 MHz, a frequency where the radio emission was known to be associated with sunspot activity. Meanwhile, photographs taken at the Sydney Technical College's observatory (Figure 26) provided optical coverage of the event (Millett and Nester, 1948).

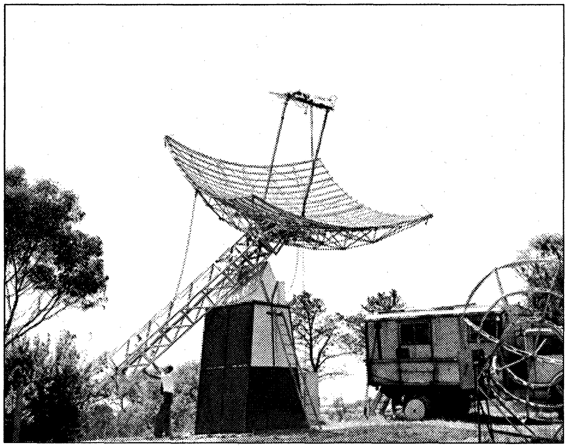

Figure 24: The refurbished ex-Georges Heights antenna at Potts Hill. In the background is one of the WWII mobile equipment huts (ATNF Historic Photographic Archive: B2649-3).

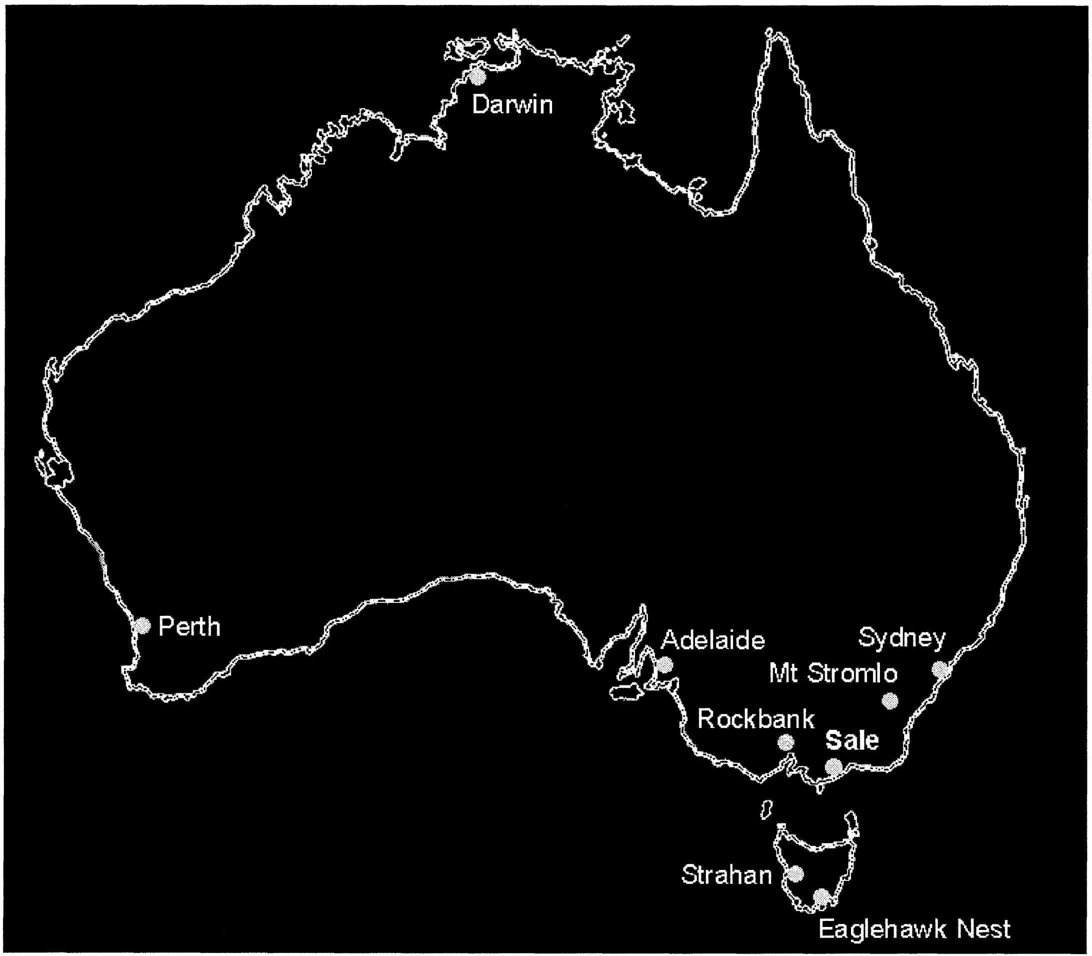

Figure 25: Map showing non-Sydney locations mentioned in the text.

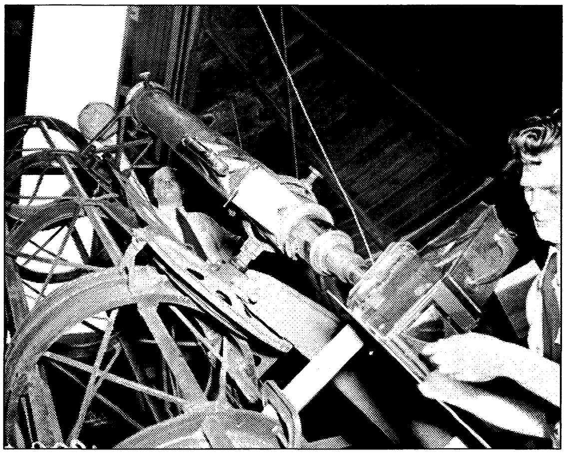

Figure 26: Photographs of the Sun during the eclipse were taken through the 15 cm (6-in) guide-scope attached to the 46 cm (18-in) reflector at the Sydney Technical College (ATNF Historic Photographic Archive: B1899-7).

The observations at the three sites were co-ordinated by Chris Christiansen, Bernie Mills and Don Yabsley. This was the first solar research project conducted by Christiansen (see Figure 9 on page 25), who would go on to establish an international reputation in this field with innovative new radio telescopes and associated research programs at Potts Hill, and later at the Fleurs field station (location 6 in Figure 4).

Successful observations were made at all three sites, and small, but obvious, variations in the levels of solar emission were noted during the eclipse (see Figure 27). Meanwhile, photographs taken in Sydney revealed the presence of six groups of sunspots, but their total area was small, amounting to only ~0.085% of the total area of the visible disk of the Sun.

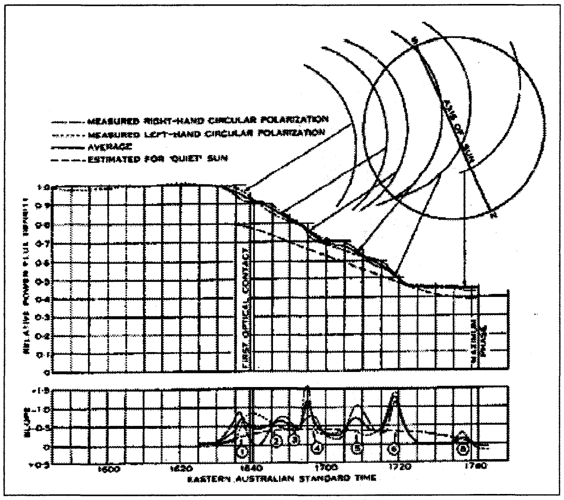

Figure 27: The solar eclipse chart record obtained at Rockbank (upper plot). In the lower plot this has been corrected for the slope and vertically-exaggerated, and the different emission peaks are numbered (after Christiansen et al., 1949a: 511).

The radio observations made at Potts Hill, Rockbank and Strahan showed that radio emission from the Sun began to decrease ~10 minutes before the commencement of the optical event (consistent with the idea that 600 MHz radio emission originates in the corona). As the eclipse progressed, the troughs in the declining emission curve indicated that several different localized regions of enhanced solar emission were present, and their precise positions—projected onto the solar disk—are shown in Figure 28.

Calculations indicated that the eight localized regions of enhanced emission shown in this figure contributed ~20% of the total solar radiation received on 1 November 1948. These emitting regions were assumed to be approximately circular, and their areas varied by little more than a factor of two, with a mean of ~0.4% of the total area of the visible disk of the Sun. Their effective temperatures varied by more than 10:1, and if we assume a quiet Sun temperature of ~0.5 × 10^6 K at 600 MHz, then the brightest localized regions in Figure 28 (numbers 4 and 6) would have had effective temperatures of ~10^7 K.

Figure 28 shows that peak number 1 was located ~1.7 × 10^5 km beyond the solar limb, and above a magnetically-active region in the chromosphere marked by a conspicuous prominence. All other emission peaks were on the solar disk, and in the case of numbers 2, 7 and 8 coincided with sunspot groups. However, peaks 3–6 did not appear to be associated with any obvious photospheric features, although three of these were close to the positions occupied by sunspot groups exactly one solar rotation earlier. Meanwhile, two small sunspots groups and one large group (near the western limb) were not associated with measurable levels of solar radio emission.

In addition to determining the positions of radio-active regions, the RP radio astronomers also wanted to use the 1948 eclipse to determine whether limb-brightening existed at a frequency of 600 MHz (as postulated by Martyn in 1946) and see whether radio-emitting regions in the northern and southern hemispheres of the Sun exhibited opposite senses of circular polarization—as also predicted by Martyn.

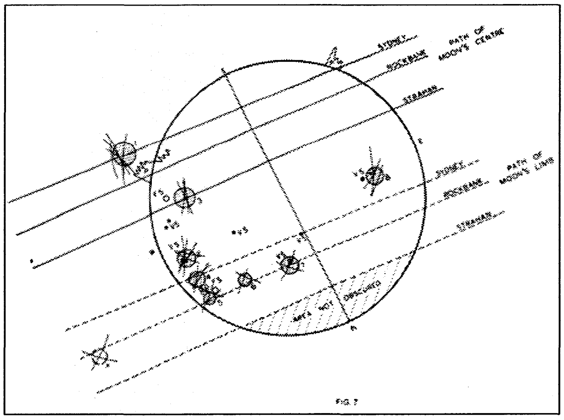

Figure 28: Map showing the distribution of 600 MHz active regions (hatched) during the 1 November 1948 eclipse. The small black dots indicate visible sunspots (VS), P indicates a prominence, and FS marks the position of a sunspot group that was prominent 27 days earlier (after Christiansen et al., 1949a: 513).

Unfortunately, the limb brightening investigation produced inconclusive results:

> … roughly half the (presumed) thermal component of the radiation originated close to, and predominantly outside, the edge of the visible disk of the sun. The details of the brightness distribution could not be derived from the records. The latter were shown to

> be consistent with two tentative distributions, the first a theoretical one, involving limb brightening … and the second a uniform one over a disk having 1.3 times the diameter of the optical disk of the sun. The existence of limb brightening, therefore, was not proved. (Christiansen *et al.*, 1949b: 570).

The polarization analysis proved interesting in that Rockbank was the only site to provide relevant data. Before the eclipse the two modes of circular polarization differed in amplitude by less than 2%, but on 1 November 1948, "The eclipsing of the active areas produced changes that sometimes were confined to one or other circularly-polarized component, or in some cases involved both components." (Christiansen et al., 1949a: 521). The changes were of short duration, and the two components quickly returned to equality. This is illustrated in Figure 27, where the most significant variations in the relative levels of left-hand and right-hand circular polarization are associated with active regions 1, 4 and 5. Since the difference in the two polarizations curves was <3% at the maximum phase of the eclipse, this indicated that the general magnetic field strength of the Sun at the poles was <8 gauss. We should note that this is in line with current thinking, but that in 1948 a value of ~50 gauss was assumed. Christiansen, Yabsley and Mills summarized their three-station observations in a letter to *Nature* (1949b) and provided a full account in a paper published in the *Australian Journal of Scientific Research* (1949a).

Two other small RP teams at Potts Hill carried out observations of the 1 November 1948 eclipse in conjunction with Christiansen's group. Jack Piddington and Jim Hindman used a 1.7 metre (68-in) dish to secure observations at 3,000 (Figure 29), while Harry Minnett and Norman Labrum observed at 9,428 MHz with the relocated 1.1 metre (44-inch) Eagle's Nest antenna (see Figure 30).

Pre-eclipse observations made by Piddington and Hindman at 3,000 MHz and Covington at 2,600 MHz showed that variations in solar emission were correlated with sunspot area, as at 600 and 1,200 MHz. During the eclipse, the emphasis therefore was on explaining variations in the intensity of the emission, and investigating its polarization. Piddington and Hindman (1949: 525) examined the latter "… at intervals during the eclipse, a method of excluding either right- or left-hand circularly polarized component being employed. It was hoped to associate certain polarization with given sunspots and also to measure the general magnetic field of the sun." They also examined the distribution of background radiation over the solar disk.

Unlike the 600 MHz observations, the 3,000 MHz Potts Hill curve showed few obvious variations during the eclipse, the only one of consequence being associated with active region #3 in Figure 28. The polarization results were even less impressive:

> Owing to the apparent random variations of polarization of the order of one per cent, and the rather long intervals between measurements, it is impossible to attribute any definite degree of polarization to any particular area of the solar disk … [However,] continuous measurement of polarization was in progress between 1743 and 1747 hours and the change of polarization by 2 per cent. took place within one minute. This change took the form of an increase in LH polarization while the RH component remained unchanged thus indicating that the change was probably due to the uncovering of an area with predominantly LH polarized radiation. (Piddington and Hindman, 1949: 531).

Piddington and Hindman were unable to identify a possible spot group or target region.

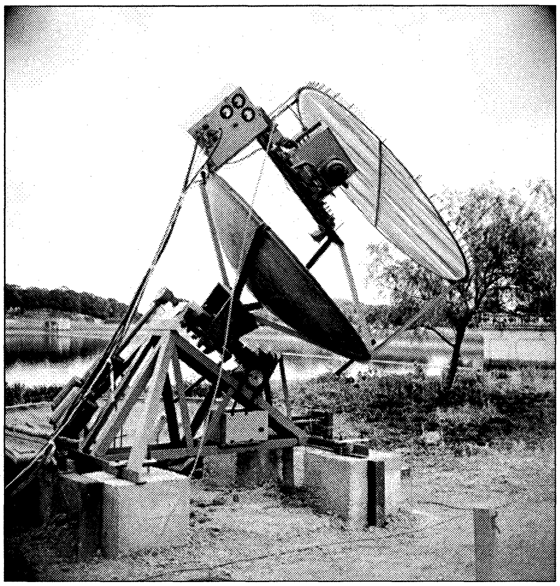

Figure 29: The 1.7 metre dish used by Piddington and Hindman to observe the 1948 solar eclipse. Particularly conspicuous is the full-aperture screen used to study polarization (ATNF Historic Photographic Archive: B1624-7).

After allowing for a single radio 'hotspot', Piddington and Hindman (1949: 532) investigated the distribution of 3,000 MHz emission over the solar disk, and concluded that "The distribution of radiation consisting of 32 per cent. from a thin disk around the solar limb and 68 per cent. from a uniform disk will, therefore, provide an eclipse curve very similar to that observed." This is illustrated in Figure 31).

The final aspect of 3,000 MHz eclipse program was an investigation of the magnetic field of the Sun, and "… although the results were not definite, they suggest that if a general magnetic field exists at all it is considerably smaller than the usually accepted value of 50 gauss at the poles." (Piddington and Hindman, 1949: 534).

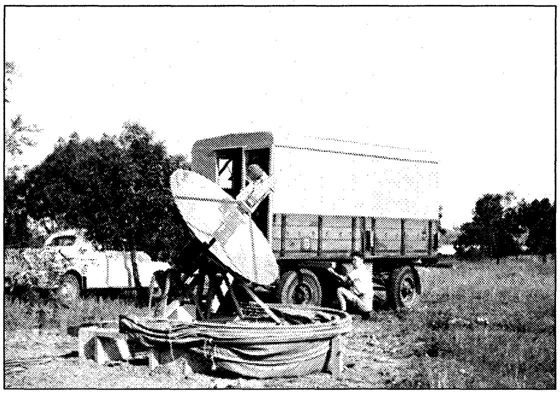

Figure 30: The 1.1 metre dish at Potts Hill, used by Minnett and Labrum for solar observations at 9,428 MHz. Norman Labrum is crouching beside the old mobile instrument trailer (ATNF Historic Photographic Archive: B1581-2).

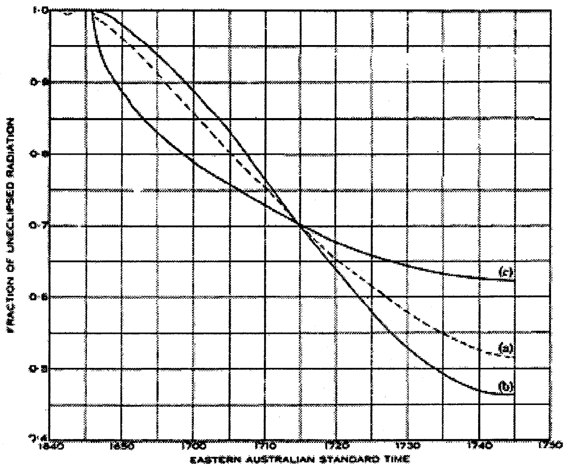

Figure 31: Alternative 3,000 MHz eclipse curves. (a) is the curve obtained during the eclipse; (b) is the theoretical curve for a uniform disk; and (c) is the theoretical curve for a circumferential ring (after Piddington and Hindman, 1949: 533).

Let us now examine Minnett and Labrum's 9,428 MHz solar research program carried out before, during and after the 1 November 1948 eclipse with the modest antenna shown in Figure 30. Ongoing measurements of mean daily radiation levels revealed a clear correlation with variations in sunspot area, as illustrated in Figure 32. During the eclipse, the declining radiation curve included minor variations, but "Any changes due to the covering and uncovering of the spots [present at that time] are too small to be distinguished from instrumental variations." (Minnett and Labrum, 1950: 69). However, the authors did note that the radio event began seven minutes before first optical contact, which "... could possibly have been caused by a localized emitting region extending from the sun's limb." (ibid.).

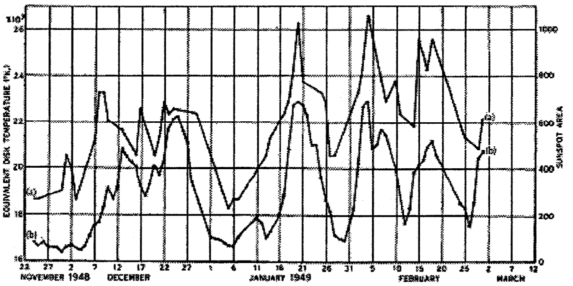

Figure 32: Correlated daily variations in solar emission at 9,428 MHz (a) and sunspot area (b) between November 1948 and March 1949 (after Minnett and Labrum, 1950: 65).

Despite the inherent instrumental limitations, Minnett and Labrum were able to investigate the brightness distribution of 9,428 MHz radiation across the disk of the Sun, and they found that the smoothed eclipse result was best represented by curve (d) in Figure 33, which requires 74% of the radiation to originate in a source of type (a) and the remaining 26% in a source of type (b).

Flushed with the success of their 1948 eclipse program, the RP radio astronomers looked with anticipation at another partial solar eclipse that would be visible from Australia in October 1949. Once again a multi-frequency campaign was planned, and the two portable 3.08 metre dishes were readied for further service interstate. One was sited in Sale, in eastern Victoria, and the other was transported to Eaglehawk Neck on the east coast of Tasmania, near Hobart. Once again successful observations were made but, strangely, no papers reporting this second eclipse program were ever published.

So from an international perspective, the Sydney, Rockbank and Strahan observations of 1948 marked a watershed in solar radio astronomy. They not only signalled the end of an era, but they were also the trigger that inspired Chris Christiansen (1984: 117) "... to devise some method of viewing the Sun [at high resolution] more frequently than was possible with eclipse observations. This of course meant devising some antenna system of very great directivity." The result was the first solar grating array, an innovative 32-element solar grating array operating at 21 cm that was constructed at Potts Hill in 1951 (see Christiansen and Warburton, 1953).

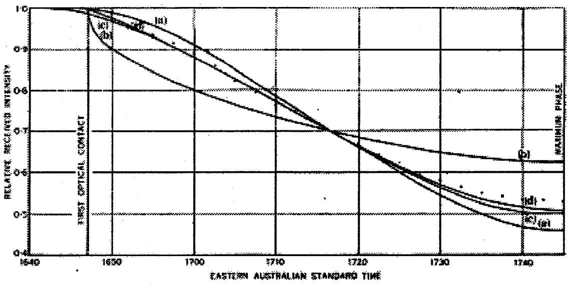

Figure 33: Comparison of the eclipse measures (dots) with various theoretical results for brightness distributions over the solar disk at 9,428 MHz. (a) is the theoretical curve for uniform distribution over the disk; (b) is the theoretical curve for a circumferential ring; (c) is the theoretical curve for a radius 1.1 times that of the optical disk; and (d) is the theoretical curve for a disk with 74% of the radiation from a type (a) source and 26% from a type (b) source (after Minnett and Labrum, 1950: 70).

2.5 The Pawsey Review Papers

The initial appearance of review papers in a new research field typically herald its 'coming of age', as the landslide of discoveries and technological developments demands an initial stock-take. This was certainly so of 'solar noise' studies after the hectic years of post-war achievement, and by 1948 optical astronomers were beginning to recognize the value of this avalanche of new data derived from the distant 'long' end of the electromagnetic spectrum. Only then did the term 'radio astronomy' begin to slip into common usage.

Given RP's international supremacy in solar radio astronomy is it little wonder that the first two major reviews of this new research field were penned by Sydney's Joe Pawsey? One of these, titled "Solar radio-frequency radiation", was completed in September 1948, revised through to early February 1949, and then submitted to The Institution of Electrical Engineers in London (reflecting radio astronomy's early affinity with radio engineering rather than astronomy). This 21-page paper was read before the Radio Section of the Institution on 7 December 1949, and appeared in the *Proceedings* exactly nine months later (Pawsey, 1950). The initial introductory section is followed by sections on "Observed Characteristics", "High-Frequency Characteristics of the Solar Atmosphere" and "Discussion and Hypothesis", before a succinct Conclusion introduces a very useful Bibliography featuring sixty-six different entries. The final two and a half pages record the discussion that followed the presentation of the paper, and Pawsey's rejoinder. The paper is profusely illustrated, and includes a number of previously-unpublished diagrams, mainly deriving from the Bolton's investigation of polarized bursts in 1947.

Pawsey's second review paper was titled "Solar radio-frequency radiation of thermal origin". It was co-authored by Don Yabsley, received by the Editor of the *Australian Journal of Scientific Research* on 17 January 1949, and published in the journal later that same year. Although it does discuss the RP achievements, this 16-page paper includes many results from overseas workers. Pawsey and Yabsley (1949: 198) found that

> … a relatively constant component can be identified throughout the whole of this wavelength range [from 1cm to 4m] despite the complication introduced on the longer wavelengths by the presence of highly variable components. This steady component has the properties expected of thermal radiation and it is concluded that it is, in fact, thermal radiation from the ionized gases of the outer atmosphere of the sun.
>
> The intensity of radiation is found to increase fairly uniformly from that corresponding to black-body radiation at about 10^4 °K. at 1.25 cm. to about 10^6 °K. at 1.5 m.
>
> The results yield direct confirmation of the hypothesis that the corona has a kinetic temperature of about a million degrees.

3 BRUCE SLEE AND RADAR 59 NEAR DARWIN

While the RP group was carrying out its first solar observations at Collaroy in October 1945, Owen Bruce Slee (Figure 34) was making independent observations at RAAF radar station 59 near Darwin (Figure 25) that would constitute an independent discovery of solar radio emission. Station 59 at Lea Point featured a British 200 MHz COL Mk5 radar unit situated ~0.5 km from the coast and ~70 m above sea level (Figure 35).

Slee was trained as a radar mechanic, but he also served as a radar operator in order to monitor the stability and sensitivity of the equipment. Between October 1945 and March 1946 he noticed from time to time that in the hour leading up to sunset

> … the 'grass' on the range display increased its height by up to a factor of ten when the antenna was pointing towards the setting Sun. By slowly scanning backwards and forwards through the Sun, he was able to establish that the source of the signal lay at the solar azimuth to within the errors of measurement. Furthermore, when he stopped the antenna while pointing at the Sun, he noticed that the amplitude of the 'grass' varied regularly by a large factor with a period of about 3 minutes. He concluded that this behaviour was consistent with the setting Sun passing through the sea interference fringes formed by the antenna and its image in the sea. (Orchiston and Slee, 2002a: 27).

In early March 1946 Slee read in a newspaper that the CSIRO Division of Radiophysics had been carrying out solar radio observations, and on 4 March he wrote them a letter describing his own work. In this letter he comments that "Tonight for example the interference first made itself evident on a bearing of 264 degrees at 1830 hrs. gradually becoming stronger until at 1845 hrs the Sig/noise ratio was 2:1. Then it faded out gradually and was gone by 1900 hrs. The sensitivity of the receiver at present is 91 d.b. below .1 volt for a signal equal to noise, and this may give you some idea of the signal strength of the interference." (Slee, 1946a). Elsewhere in this letter Slee (ibid.) states that he is "… convinced that the interference is solar radiation …", and he asks the Sydney scientists to confirm these latter suspicions. He also offers to make further observations and to submit these to the Division of Radiophysics.

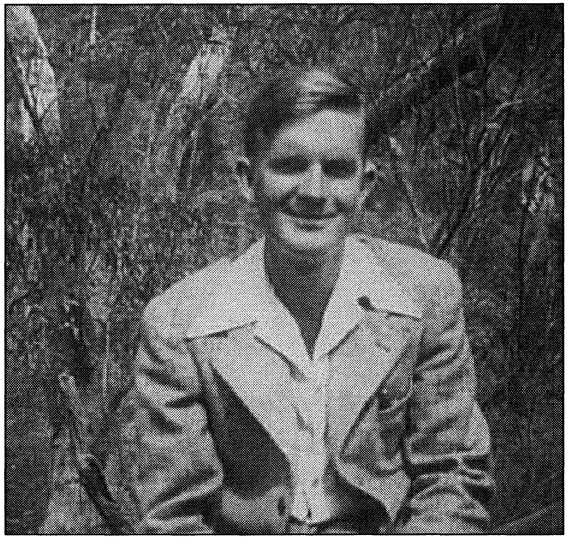

Figure 34: A youthful Bruce Slee (1924–) in 1948, three years after his Darwin solar observations.

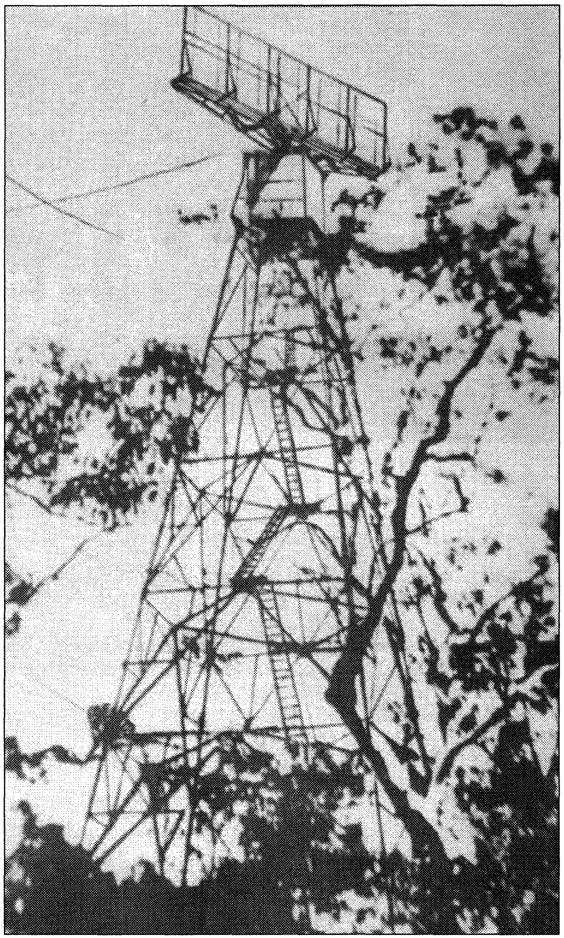

Figure 35: The 200 MHz COL Mk 5 radar antenna at Station 59, Lee Point (after Fenton and Simmonds, 1993: 27).

As might be expected, Slee's letter caused considerable excitement when it reached Sydney and was circulated among senior RP staff. Pawsey wrote "Good Stuff" above his signature, while Burgmann recommended that they "Tell this chap a lot". The reply letter came from J.N. Briton, Chief of the Division, and began very positively:

> We were very interested in your observations made at No.59 Radar Station. In particular the information regarding bearing, receiver sensitivity, height of the array etc. was important, and such data is [sic] often omitted by observers reporting abnormal phenomena. There is very little doubt that the noise you observe originates in the sun. (Briton, 1946b).

This 5-page letter contains an account of the Division's solar observations, including the relation between emission levels and sunspots, four different diagrams, and an accompanying booklet with (amongst other information) charts that could be used to calculate the position of the Sun on cloudy days. Briton made it clear that he and his colleagues were eager to receive observations from Slee towards the time of sunset:

> We would be interested in obtaining maximum readings (e.g. 3/2 [sic], 2, 3, 4 ... 10 times noise level) during periods of high sunspot activity ... [These] would be valuable data to supplement readings taken in Sydney … In the event of any abnormal radiation being recorded in Sydney we may send you a signal …" (ibid.).

Unfortunately, this planned collaboration between Sydney and Darwin never eventuated, for soon after Slee sent his letter of 4 March the radar station was unexpectedly closed down and all of the equipment was removed. Obviously disappointed with this development, Slee (1946b) wrote apologetically from his home in Adelaide following his discharge: "As a result, I shall not be able to make the required readings, as much as I would like to do so." He also enquired about possible vacancies at RP, noting that "I am intensely interested in your experimental work, and plan to do Radio Engineering, as soon as possible. Having had three years experience in the radar game on various types of gear, I thought that perhaps you may have some use for me." These were indeed prophetic words, for later in the year he was appointed a Technical Assistant in the Division, and went on to build an international repuation in radio astronomy (see Orchiston, 2004a; 2005b).

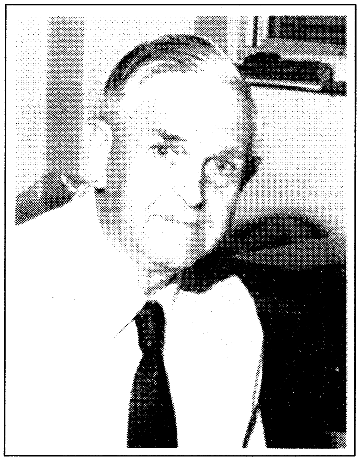

Figure 36: Dr S.E. Williams (1910–1979) (courtesy of the University of Western Australia).

4 UNIVERSITY OF WESTERN AUSTRALIA

It is not widely known that a small group at the University of Western Australia in Perth (see Figure 25) was active in solar radio astronomy in 1946–1948, even though attention was drawn to the group's work in papers published by Burman (1991) and Burman and Jeffery (1992), and this research is mentioned in Robertson's (1992) history of the Parkes radio telescope and in the Haynes et al. (1996) history of Australian astronomy.

The project was set up by Dr Sydney E. Williams (Figure 36), a Lecturer in Physics with a background in optical astronomy at the Commonwealth Observatory, following a seminar on radar held at the Radiophysics Laboratory, Sydney, in January 1946. At the end of April 1946 Williams installed a 75 MHz Yagi and receiver on the flat roof of one of the buildings on campus in suburban Perth. In a letter to Pawsey, Williams (1946a) describes how

> We made a Yagi (dismountable for portability) on a wooden polar axis with synchronous motor drive. Matching aerial to coaxial and receiver has so far been done simply by fiddling with the dipole and director lengths till we got the best polar diagram and sensitivity. We are using simply milliameter but soon will have a film camera recorder on the oscillograph.

For two years, Williams, assisted by three Honours (fourth-year) students, P. Hands, E. Denton and P.M. Jeffery, carried out studies of solar bursts and enhanced levels of solar emission using this Yagi, and an example of one of their chart records is reproduced here as Figure 37 (after Williams, 1946b). The focus was on temporal variations in the intensity of solar radio emission, correlations with sunspots, solar flares and ionospheric radio fadeouts, and the shapes of short pulses of radiation.

This research resulted in an editorial note (Williams and Hands, 1946) and two letters in *Nature* (Williams, 1947, 1948b), and a full-length paper in the *Journal of the Royal Society of Western Australia* (Williams, 1948a), which was based on a lecture presented to that Society. There were also unpublished papers given by Williams at the 1946 and 1947 ANZAAS Congresses (Burman and Jeffery, 1992). Of particular note, in the context of the present study, is the fact that the four papers by the Perth group comprise 33% of all observation-based papers on Australian solar radio astronomy published during the interval 1946-1948 (see Burman, 1991, for a relevant bibliography).

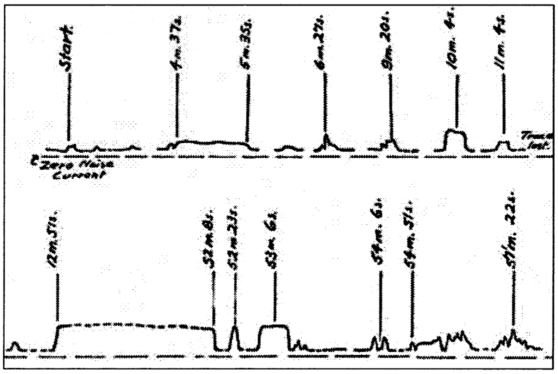

Figure 37: Annotated chart record showing 75 MHz bursts and enhanced emission recorded at Perth on 25 September 1946.

Williams ended these radio astronomical investigations in 1948, and later returned to optical astronomy. Burman and Jeffery (1992: 168) conclude that "Although the work was noticed internationally, its influence on the course of radio astronomy seems to have been slight …" They also note (page 169) that "Probably the main innovation introduced by the Perth group was the analysis and interpretation of the time decay of the pulses …" We shall look more closely here at that aspect of the work (Williams 1948b; also 1948a: Section 6).

The third of the *Nature* papers, a brief letter by Williams entitled "Shape of Pulses of Radio-frequency Radiation from the Sun", seems to have been the only one of the four Perth papers to have had any direct influence on contemporaneous radio astronomers. The paper, based on some 400 hours of observations, was aimed at examining the tails of pulses for exponential decay, as an indication of the decay of the source when it is no longer subject to the influence of an exciting agency, or of the decrease in influence of that agency. Filmed records of a vibration galvanometer output of detected radio bursts were used. There were 99 single pulses, lasting a few seconds each, that were considered to be sufficiently clear of others to indicate a faithful record of the variation of the power from the source: 78 of the 99 had a peak at least 25% above background and a sufficient length of falling slope for significant measurements to be made. Of these, 58 were found to be very probably exponential, 11 less probably so, 4 probably not so and 5 definitely not (see Figure 38).

Williams' table binning the half-lives (time for the power to reduce to half) of the 58 exponential tails shows them to range over 0.4–2.2 seconds, with a distribution peaking in 0.8–1.2 s. A second table lists the half-lives of 30 of these 58 pulses that occurred either consecutively or within a short interval. (The intervals listed are mostly 30 s, but 5 minutes for one sequence of 6 pulses.) From this table, Williams (1948b) noted that "Although it might be assumed that pulses closely connected in time come from the same region and should therefore show similar half-value times, the results given above do not offer firm support for such a hypothesis." The paper concludes with the remark that no success had been obtained in attempts to interpret the rising portions of the pulses in terms of exponential functions.

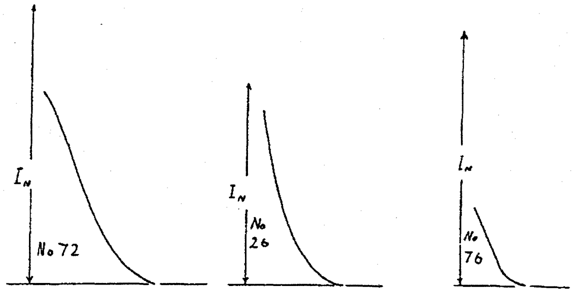

Figure 38: The tails of three pulses illustrating three of four categories; left = exponential, centre = doubtfully exponential, right = not exponential (after Williams, 1948a: 28).

5 COMMONWEALTH OBSERVATORY

The Commonwealth Observatory (CO) was located at Mt Stromlo, near the national capital, Canberra, and under the guidance of its talented Director, Dr Richard Woolley, it sought to reinvent itself in the immediate post-war years by moving from solar to Galactic and extragalactic studies (see Frame and Faulkner, 2003). However, before this quantum shift was able to take place, the CO enjoyed one serious foray in radio astronomy. This had its origin in a letter that Radiophysics Chief, Taffy Bowen sent Richard Woolley on 13 February 1946. Almost certainly drafted by Joe Pawsey, it reads:

> During the recent intense solar activity we have been taking extended observations on radio frequency noise originating in the sun. The level shows a large variation with a high maximum in the middle of last week.
>
> We wish to examine this data [sic] together with visual and ionospheric data, which you will have, in order to search for possible relations between them. I consider it essential that this be done with personal discussion between members of this Laboratory and your Observatory as each are expert in their different fields.
>
> As we have about four officers who have been concerned in the recent observations, it is difficult for us to go to Canberra, and I should appreciate it if you could arrange to send one of your officers to Sydney to bring the relevant data and discuss it with us. I anticipate the discussion should occupy a few days … (Bowen, 1946a).

Dr Cla W. Allen (Figure 39) was dispatched to Sydney, and the planned meeting took place on 20 and 21 February 1946 (Hogg, 1946). Although Allen specialized in solar spectroscopy and the terrestrial effects of solar flares (McNally, 1990), war-time research into the causes of short-wave radio fadeouts whetted his appetite to investigate solar radio emission.

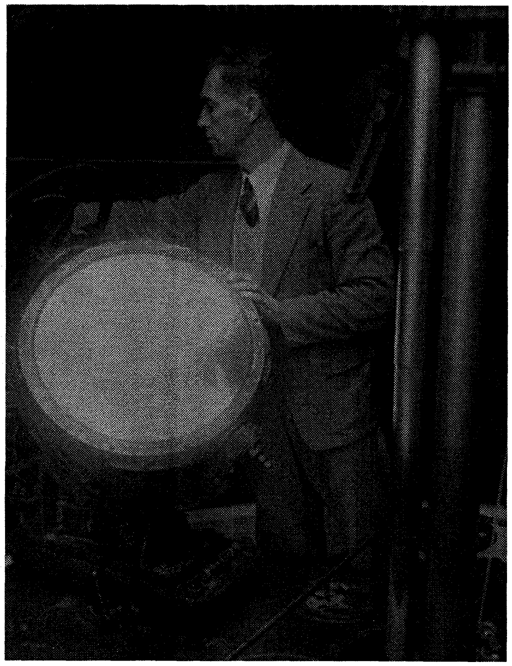

Figure 39: Cla Allen (1904–1987) with the coelostat of the CO solar telescope (courtesy Mt Stromlo and Siding Spring Observatories).

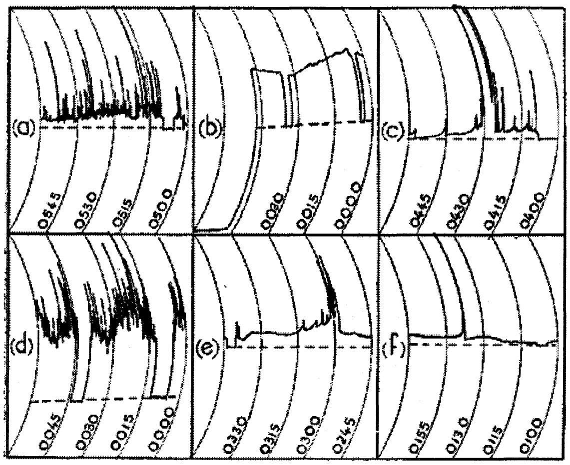

Figure 40: Six examples of solar radio emission recorded at Mt Stromlo between 14 September 1946 and 18 March 1947. (a) frequent bursts and a low noise level; (b) no bursts but a high noise level; (c) a large outburst; (d) frequent bursts and a high noise level; (e) a small outburst; and (f) an isolated burst and a low noise level (after Allen 1947: 388).

A direct outcome of this meeting was the decision to install a 200 MHz steerable 4-Yagi array at Mt Stromlo. This was a replica of an identical antenna then in service at Dover Heights (see Figure 9), and it was loaned by the Division of Radiophysics. Two of the RP radio astronomers, Lindsay McCready and Gordon Stanley, arrived at Mt Stromlo with the antenna and receiving equipment on 1 March, and it was installed the following day (Briton, 1946a). On 2 April, soon after the equipment became operational, Allen (1946) sent Pawsey the first of what was to become an ongoing series of reports. In this instance, his telegram read:

> WE ARE RECORDING 9 TO 5 ON WEEK DAYS AT THREE INCHES AN HOUR STOP BURSTS ON FRIDAY WERE AT 1431 AND 1435 STOP SUN ACTIVITY ON SATURDAY MORNING BUT NOTHING MONDAY NOR TODAY TUESDAY STOP NO RECORD SUNDAY.

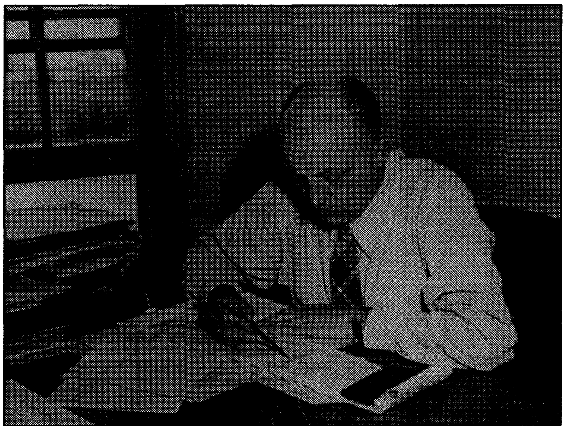

Figure 41: David Martyn (1906–1970) at Mt Stromlo (courtesy Mount Stromlo and Siding Spring Observatories).

Between April 1946 and March 1947 Allen carried out regular solar monitoring at the CO, and upon analyzing the accumulated observations found that even on radio-quiet days

> … there has always been a detectable amount of radiation which appears to be quite variable … [In addition] there are occasional sudden "bursts" of solar radio-noise which last for periods of the order of 1 sec … [and] rather rarely, sudden outbursts of radio noise, which last for a few minutes, fluctuating violently, and then disappear. (Allen, 1947: 387).

Examples of generally-enhanced emission levels, bursts and outbursts are shown in Figure 40.

Allen confirmed that solar emission was closely related to the central meridian passage of sunspots, just as Pawsey et al. (1946) had reported, although not all sunspots produced solar noise. Meanwhile, his analysis failed to show a general correlation between solar noise and Hα features or geomagnetic storms, although some solar flares were associated with outbursts.

While Allen was conducting these investigations, his CO colleague, David F. Martyn (Figure 41),[4] was using data obtained with the 200 MHz array to research the polarization of solar radio emission. He reasoned that since solar bursts were associated in some way with sunspots and in their turn sunspots were associated with strong magnetic fields,

> … we should expect to find evidence of the magnetic field in the production of gyratory effects at the source of the [radio] emissions, and/or in differential absorption of right-handed and left-handed components of polarization during transmission through the corona. (Martyn, 1946a).

The passage of the large sunspot group of July 1946 provided an ideal opportunity for Martyn to test this hypothesis, by turning two of the Yagis in the array at right-angles to the other two Yagis. Observations made on 26 July revealed

> … that the right-handed circularly polarized power received was some seven times greater than that received when the system accepted only left-handed circularly polarized radiation … Three days later, when this group had crossed the meridian, these conditions were reversed, five times more power being then received on the left-handed than on the right-handed system. (ibid.).

Part of the 26 July chart record is shown in Figure 42. This overall result, incidentally, was confirmed at 60 and 100 MHz by RP's John Bolton during observations made at Dover Heights in March and April 1947 (see Pawsey, 1950: Figures 17 and 18). Nonetheless, Martyn's polarization studies drew high praise from the Astronomer Royal:

> SIR HAROLD SPENCER JONES said that he considered the discovery of circular polarization of solar noise to be an important piece of work. Solar noise promised to be a very fruitful field of investigation and the Commonwealth Observatory was taking a place second to none in this particular branch. (Commonwealth Observatory …, 1947: 3).

Martyn followed up this important paper with the theoretical contribution on the 10^6 K coronal temperature referred to earlier (Martyn, 1946b) and two other theoretical papers on solar radio emission. In the first of these (Martyn, 1947), he invokes plasma oscillations to account for the energetic burst emission seen at low frequencies (cf. Solar radio noise, 1948). Martyn's next paper, "Solar radiation in the radio spectrum. I. Radiation from the quiet Sun", related solely to thermal emission, and was published in the prestigious *Proceedings of the Royal Society* in 1948. Undoubtedly, the most important predictive aspect of this seminal

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paper was Martyn's calculation of the distribution of thermal emission across the face of the Sun at wavelengths ranging from 20 cm to 30 m. As Figure 43 shows, between 20 and 60 cm there should be conspicuous 'limb brightening'. Despite the title of this paper—and the promise of a further paper, or papers, in this series—this was to be Martyn's final publication in solar radio astronomy, and he turned to other research interests (see Piddington and Oliphant, 1972). Allen, meanwhile, used the 200 MHz equipment at the CO to research Galactic radio emission (Allen and Gum, 1950), but his appointment to a Chair at University College, London, in 1951 brought Mt Stromlo's escapade in radio astronomy to an end. It had been a brief but profitable research diversion.

In addition to the research outcome of these radio astronomy initiatives at Mt Stromlo, there were direct benefits for RP during these nascent years of solar radio astronomy, as Robertson (1992: 109) points out:

> Despite some tension this early collaboration with the Commonwealth Observatory undoubtedly benefited the Radiophysics group. The radio scientists, turned radio astronomers, came into contact with Australia's leading astronomers at a time when the Sydney group was only learning the basics of the science. Clay Allen in particular provided a steady flow of information to Radiophysics on solar phenomena and astronomical objects. The association between Mt Stromlo and Radiophysics was the first major collaboration between optical and radio astronomers anywhere in the world.

6 CONCLUDING REMARKS

The birth of solar radio astronomy in Australia occurred towards the end of WWII, soon after news of secret war-time detections by radar units in England and New Zealand reached Sydney, and between October 1945 and December 1948 major advances were made in the study of 'solar noise'. Initially these involved wartime radar antennas and receivers, but Yagis and other types of antennas specifically dedicated to solar (and non-solar) radio astronomy soon emerged. This instrumentation was used at wide-ranging frequencies to investigate emission from the quiet and the 'radio-active' Sun. Of special interest were flux levels at different frequencies; the various types of burst emission; locations of the emitting regions and their association with photospheric features and magnetic fields; and emission mechanisms.

While much of this research was accomplished by staff in the CSIRO's Division of Radiophysics, it is important to remember that initially two other small research groups, at the Commonwealth Observatory (Mt Stromlo) and the University of Western Australia (Perth), were active in solar radio astronomy. To illustrate their contribution, we should note that of the twelve Australian research papers reporting observations of solar radio emission made in 1945-1947, the RP scientists contributed exactly half, the Mt Stromlo group two and those from the University of Western Australia, four. In addition Martyn (from Mt Stromlo) published three theoretical papers. However, Williams' research on solar radio emission ceased in 1948 and Mt Stromlo's involvement ended just three years later, leaving the growing RP group as the sole Australian participants. December 1948 therefore is an appropriate chronological point at which to end this paper.

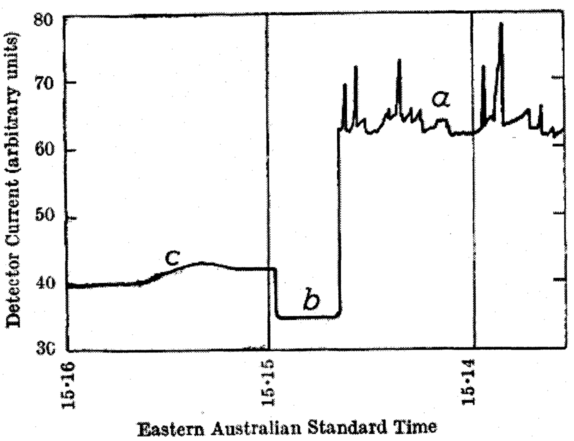

Figure 42: Circular left-handed (a) and right-handed (c) polarized solar emission recorded at 200 MHz on 26 July 1946. The background sky level, when the array was directed away from the Sun, is indicated by (b) (after Martyn 1946a).

Over the next three years, major RP initiatives would lead to a new perspective on energetic solar radio emission. Wild's first solar radio spectrograph at the short-lived Penrith field station would provide the basis for a new classification of low frequency burst emission, while the position interferometer developed by Payne-Scott and Little at Potts Hill would offer precise positions and polarization signatures for the various types of bursts (and outbursts). Meanwhile, Christiansen (Figure 43) would use the first of his innovative solar grating arrays at Potts Hill to track the on-going pattern of 1,420 MHz emission and the evolution of the enigmatic 'radio plages' (see Christiansen, 1984).

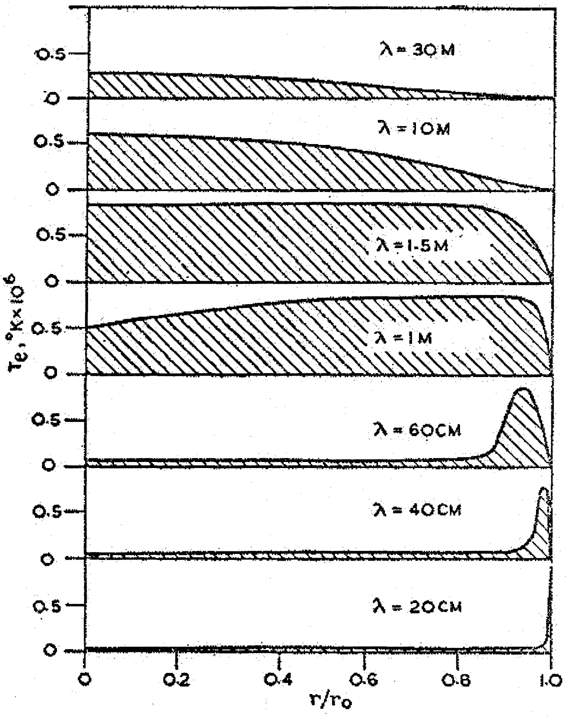

Figure 43: The calculated distribution of effective temperature (T_e) across the solar disk at various wavelengths (after Martyn, 1948: 54).

By 1952, the RP solar radio astronomy program was entrenched at the Dapto and Potts Hill field stations, and five years later Fleurs became a major solar research centre (Orchiston and Slee, 2002b) with the opening of the Chris Cross, the 'next generation' solar radio telescope (see Orchiston, 2004c). By this time, RP was widely regarded as the leading solar radio astronomy research group in the world (Sullivan, 2005).

From the mid-1950s, the journal *Vistas in Astronomy* acquired a reputation for publishing major review papers (see Duerbeck and Beer, 2006). In a paper that appeared in the inaugural issue, Wild (1955: 573-574) wrote:

> The characteristics of the radio-frequency radiation ("noise") from the Sun are complex. In the absence of disturbed conditions on the Sun, the intensity of the noise remains at a steady level corresponding to the thermal radiation emitted by the solar atmosphere. But when sunspots are visible on the Sun's disk the level may become enhanced and show rapid fluctuations above the basic thermal level. At times sudden increases ("bursts"), lasting some seconds or minutes, may increase the level a thousandfold.

It was largely through the pioneering research efforts of the young men and women from the CSIRO's Division of Radiophysics, the Commonwealth Observatory (Mt Stromlo) and the Physics Department at the University of Western Australia (Perth) that the basis of solar radio emission between 60 MHz and 24,000 MHz was unraveled during 1945-1948. These were the formative years of solar radio astronomy.

Figure 44: Sir Edward Appleton (extreme right), discussing Christiansen's first Potts Hill solar grating array in 1952. Chris Christiansen is on the far left, beside one of the antennas (adapted from ATNF Historic Photographic Archive: B2842-66).

7 NOTES

1. The location of these 600 and 1,200 MHz observations is not mentioned, but Payne-Scott (1945: 10) does mention that at the latter frequency "... a large aerial was available having a gain of about 5,000, and a receiver with a noise factor of about 20." This 'large aerial' was possibly the large experimental radar antenna at the Georges Heights radar station (location 8 in Figure 4), which was subsequently used by RP for solar monitoring at 200, 600 and 1,200 MHz (see Section 2.3 above).

2. Orchiston (2005a) and Sullivan (2005) have both discussed the practice that persisted from that time through into the early 1950s for some Australian radio astronomy papers submitted to British journals to be inexplicably held up for many months while British researchers wrote up their own papers and rushed these into print. Sir Edward Appleton and Jack Ratcliffe were identified as two of the main 'offenders'. It would seem that the Collaroy paper met this fate. Although submitted to *Nature* on 23 October 1945, it was only published in the issue of 9 February 1946. In the interim, two other solar radio astronomy papers (by Appleton, and Hey) were published.

3. An interesting case of protocol also relates to this paper in that it referred to the confidential British and New Zealand wartime reports by Hey and Alexander. Subsequently, Sir Edward Appleton sent letters to Frederick White (Chairman of the CSIR, as it then was), Taffy Bowen (Deputy Chief of RP) and Professor A.V. Hill (British Committee of Post-War Publications) pointing out that in the RP paper, "... reference was improperly made to two confidential reports." (Bowen, 1946b). White was forced to write Appleton an apologetic reply, but a letter penned to Bowen on the same day reveals that he was far from amused:

> There is nothing much that can be done about it as far as I can see beyond what I have done in my letter. No doubt we ought to have taken the correct steps to get the acknowledgement of confidential reports cleared by the Committee in England. *Personally I do not think it matters a great deal – it is only a focal point.* (White, 1946; our italics).

It is interesting that these letters from Appleton came after he had been quizzed by the Australians about the delay in publishing their *Nature* paper and was forced to defend the *status quo* (see Orchiston, 2005a: 82-83 for further details). We should note that more amiable relations existed between Appleton and RP by 1952, when Sir Edward led a sizeable overseas contingent to Sydney for the first URSI Congress held in the Southern Hemisphere. Figure 44 shows Appleton on a visit to Potts Hill field station at this time.

4. Martyn was at one time Chief of the Division of Radiophysics. No administrator, he was edged out of the post and seconded to the Commonwealth Observatory, a situation which caused him great bitterness (see Sullivan, 2005). He welcomed the chance to get involved in solar radio astronomy, not just because of the obvious research potential of this promising new field, but—one suspects—because it would give him a chance to compete with his former colleagues at RP.

8 ACKNOWLEDGEMENTS

We are grateful to Mary Harris, Gordon Burns and Miller Goss for supplying Figures 1, 2 and 6 respectively; the Australia Telescope National Facility for Figures 7, 9, 12, 14, 18-20, 22-24, 26, 29, 30 and 44; the University of Western Australia for Figure 36; and the late Don Faulkner for Figures 39 and 41. Finally, we wish to thank Woody Sullivan for providing archival material relevant to this study and for reading and commenting on an earlier draft of the manuscript.

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THE SIGNIFICANCE AND ERRORS OF ERATOSTHENES' METHOD FOR THE MEASUREMENT OF THE SIZE AND SHAPE OF THE EARTH'S SURFACE

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Abstract: We briefly explain Eratosthenes' method for the measurement of the meridian of the Earth, and based on modern astronomy and celestial mechanics we describe the errors inherent in his measurement. Also, we stress the significance of Eratosthenes' method to cartography, geography and astronomy. Finally, introducing new aspects of this method we show how it can be used in order to find the size and shape of the Earth's surface using curvature theory.

Keywords: Eratosthenes, geographical measurements, astronomy

1 INTRODUCTION

Since the earliest times, various attempts have been made to measure different distances on the Earth's surface, as well as the size of the Earth's. Anaximander, Pytheas, Eudoxus of Cnidus, Dicaearchus, Aristotle, Archimedes, Eratosthenes, Hipparchus, Posidonius of Rhodes, Strabo and Ptolemy were among those who made important contributions in this area. Among the early methods used for the measurement of the meridian of the Earth, the two most important ones were the *geometrical* method of Eratosthenes and the *astronomical* method of Posidonius of Rhodes. Elsewhere, we have described and explained Posidonius' astronomical method of measuring the length of a terrestrial meridian, using the celestial sphere (see Pinotsis, 2000: 406).

In order to understand how Eratosthenes conceived his method and how he applied it we performed a comparative study of the evolution of the ancient Greek geographical ideas and measurements until the time of Eratosthenes (Pinotsis, 2005). This study led to the conclusion that the novel method of Eratosthenes did not arise suddenly and was not a peculiar mathematic method. It appears that it was the final outcome of a long-lasting intellectual activity. Anaximander, Pytheas, Eudoxus, Dicaearchus, Aristotle and Archimedes were all predecessors of Eratosthenes who created a fervent atmosphere of blossoming ideas which affected Eratosthenes' thinking and contributed to his discovery.

The purpose of this paper is to present the error sources within Eratosthenes' measurement using astronomy and celestial mechanics. Many of these sources appear for the first time in the literature. Also, we stress the significance of Eratosthenes' method to cartography, geography and astronomy. Finally, we introduce new aspects of this method in order to determine the shape and size of the Earth's surface using curvature theory. A preliminary version of this work appeared in Pinotsis (2003).

2 THE METHOD OF ERATOSTHENES (276-194 BC)

According to Strabo, Eratosthenes was the first eminent geographer of antiquity; but he was also a mathematician, an astronomer, a philosopher and an outstanding orator. He was a broadly-educated man. Eratosthenes wrote several works on geographical, astronomical, mathematical and philosophical topics, but also on poetry and literature, and according to other ancient writers he left behind a rich scientific work. Archimedes (287–212 BC) is said to have appreciated him very much and dedicated certain of his writings to him. This is the reason why the king of Egypt, Ptolemy III the Benefactor, invited him to assume the Directorship of the famous Library in Alexandria.

Among Eratosthenes' astronomical books one of the better known ones was *Catasterismoi*. In this he calculates the inclination of the ecliptic with an accuracy that amazes given the absence of the telescope, deriving a value of 23° 51′ at a time when the actual value was about 23° 43′; as we know, today's value is approximately 23° 27′ (Ptolemy, 1898: 67; see, also, Dicks, 1960: 40; Goldstein, 1983: 3-14). Of equal importance was his opus, *Geographica*, which consisted of three books that described the whole known world of that era. Fragments of this work were reproduced in the writings of later geographers such as Polybius, Hipparchus, Posidonius, Cleomedes, Strabo, Ptolemy, and others; while they found much useful information in it, they also criticized it.

According to Cleomedes 1891, i, 10: 100), Eratosthenes had the idea to measure the length of the shadow of a vertical stick (*gnomon*) in both Syene (geographical latitude φ_S) and Alexandria (geographical latitude φ_A) at local noon (the moment of the upper culmination of the Sun) during the summer solstice in order to find the difference in the Sun's altitude at the two cities.[1] He observed that the solar rays were falling vertical to the surface of the Earth in Syene and therefore the Sun was at the zenith, the end of the vertical line KS in Figure 1. At the same moment it was also culminating in Alexandria, and the solar rays formed an angle β with the gnomon (i.e. with the vertical line KA). By measuring the length of the shadow and knowing the length of the stick he was able to determine the angle β. So the Sun was to the south of Alexandria's zenith by an angle β. This angle is equal with the angle ω formed by the vertical KS of Syene and the vertical KA of Alexandria, which in turn equals the difference of the geographical latitudes of the two cities, $\Delta\varphi = \omega = \varphi_A - \varphi_S$. KS and KA also define the celestial meridian passing over these cities, which lies exactly over the terrestrial meridian. Thus, from a known theorem of geometry we have (Pinotsis, 2003; 2005):

$$\frac{(SA)}{2\pi R} = \frac{\text{angle}(SA)^\circ}{\text{cel.merid.} = 360^\circ} \tag{1}$$

where R is the radius of the Earth. The angle of the arc (SA) is measured by means of the epicentric angle ω. Therefore, since the length of the arc (SA) is known in stades, equation (1) gives immediately the length of the terrestrial meridian as 2π times the radius of the Earth.

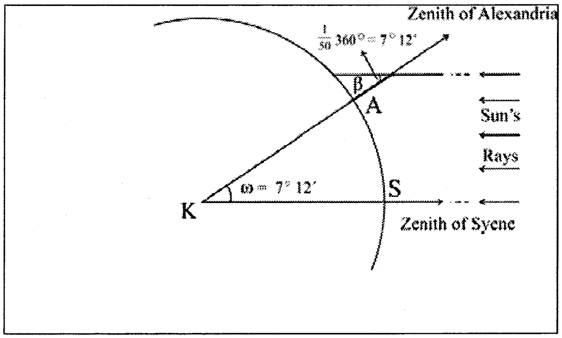

Figure 1: Eratosthenes' geometric method for the measurement of the meridian of the Earth.

Eratosthenes measured the angle β = ω and he found it to equal 7.2° or 7° 12′. This value is exactly 1/50 of a circle, and so it corresponds to 1/50 of the terrestrial meridian. The length of the meridian corresponding to the angle ω is 5,000 stades, equal to the distance between Syene and Alexandria (Figure 1), and so from equation (1) this distance must be 1/50 of the length of the circumference of the Earth. Consequently, the circumference of the Earth, C, equals 5,000 stades × 50 = 250,000 stades. If we adopt for the stade used by Eratosthenes the value 1 stade = 157.5 m (see Hultsch, 1882: 364), then the circumference-meridian of the Earth is 39,375 km. Alternatively, if we consider a later value by Eratosthenes for the circumference of the Earth of 252,000 stades, then this equates to 39,690 km. This latter figure is very close to the true value of the circumference, which ranges from 40,075 km (equatorial) to 39,942 km (polar). The corresponding radius of the Earth (R) according to Eratosthenes is 6,281 km or, if we adopt the value 252,000 stades, R = 6,317 km. Thus, the radius calculated by Eratosthenes is only about 40 km smaller than the true value of the polar radius, which is 6,357 km, and about 61 km smaller than the true value of the equatorial radius (6,378 km). This estimation by Eratosthenes is a very good approximation of the true value, with a relative error of about 1%.[2]

2.1 Errors in Eratosthenes' Measurement

Eratosthenes' method is based on geometrical considerations as well as astronomical observations. Here introduce four new errors inherent in Eratosthenes' method. Eratosthenes determined the angle, ω, with a small error and obtained a result which was close to the actual value. This is due to the fact that in general the contribution of the error sources is small and the contribution of some of them is positive while for other errors it is negative. In the following discussion we present all error sources associated with Eratosthenes' measurement.

It is difficult to determine the exact point in Syene, as well as in Alexandria, where Eratosthenes placed the gnomons, and therefore it is impossible to ascertain the *precise* geographical coordinates of these two locations. The latitude of Aswan (which is very near ancient Syene) is approximately 24° 07′, so the value for ancient Syene must have been about 24° 05′. As far as Alexandria is concerned, two buildings of great cultural and scientific significance were the Museum and the Library. The latitude of the Library was about 31° 12′, the area of the Museum was about 31° 11′, while the Phare of Alexandria was about 31° 13′. The most likely place for Eratosthenes to have made his observations was the area of the Museum, since this complex, among the other facilities, not only included an observatory but was also the residence of famous scientists—many of whom were acknowledged as the best astronomers, mathematicians and philosophers of the day. Whatever the precise location of Eratosthenes' observations, the difference between the latitudes of Syene and Alex-andria was approximately $\omega_d = 7^\circ\ 06'$. Eratosthenes determined with an error, $\Delta\omega = \omega - \omega_d \approx 6'$ the angle ω, and calculated the circumference of the Earth.

We shall now examine each of the errors inherent in Eratosthenes' method.

2.1.1 Error 1: The Gnomon Shadow

The measurement of the length of the shadow thrown by the gnomon is not accurate because of the existence of a penumbra.

2.1.2 Error 2: The Rotation of the Earth

The rotation of the Earth around its axis causes an additional error, which is different from the one due to the geoid shape of the Earth. The two errors would coincide if the Earth did not rotate. In a place, T, of geographic latitude, φ, the gnomon should have the direction of the vertical, that is, the direction of gravitational field intensity (force), $\bar{g}$, if the Earth was not rotating around its axis. Provided that we consider the Earth spherical, as people believed in antiquity, the vertical is perpendicular to the surface of the Earth and includes its center, K. The direction and the magnitude of $\bar{g}$, however, are influenced by the rotation of the Earth. Therefore, in a place, T, in the northern hemisphere the total intensity is the geometrical sum of the intensity of gravity and the centrifugal intensity $\bar{g}_c$ (Figure 2), where (Arya, 1990: 417)

$$\bar{g}_c = -\bar{\Omega}\times(\bar{\Omega}\times\bar{R}_E) \Rightarrow g_c = \Omega^2 R_E \cos\varphi \tag{2}$$

where $\bar{\Omega}$ is the angular velocity of the rotation of the Earth. Hence, the vertical (plumb line direction) does not pass from the center of the Earth, but forms an angle θ with the direction $\bar{g}$, that is, along the resultant $\bar{g}_r$. The error caused is measured by the angle of deviation, θ, of resultant $\bar{g}_r$ with the direction of gravity, $\bar{g}$, and is given by the trigonometric relation

$$\frac{\sin\theta}{\Omega^2 R_E \cos\varphi} = \frac{\sin\varphi}{g_r} \tag{3}$$

Because the angle θ is very small, we develop sin θ in a Taylor series; then equation (3) becomes

$$\sin\theta \approx \theta = \frac{\Omega^2 R_E}{2g_r}\sin(2\varphi),\quad 0^\circ \leq \varphi \leq 90^\circ \qquad (4)$$

From equation (4) it follows that:

i) For $\varphi = 0^\circ$ and for $\varphi = 90^\circ$ the angle of deviation is $\theta = 0$; that is, on the Equator and at the poles the error is zero; and

ii) The maximum divergence is at geographic latitude $\varphi = 45^\circ$. If we replace Ω and R_E in equation (4) by their numerical values we get $\theta = 0.1^\circ = 6'$. This error, although small, has as a consequence the increase of the epicentric angle by 6′ at geographic latitude $\varphi = 45^\circ$. In Syene, Eratosthenes placed the gnomon parallel to the direction of the solar rays and not in the direction of the plumb line. Thus, the resulting error is due to the parallel to the plumb line placement of the gnomon in Alexandria (Museum, $\varphi = 31^\circ\,11'$) and is $\theta = 5.31' = 0.0885^\circ$. This error, being inherent in the angle measured by Eratosthenes, must be subtracted from the value of 7.2°. Consequently, the circumference of the Earth is 253,129 stades = 39,868 km.

2.1.3 Error 3: Atmospheric Refraction

In Syene and Alexandria, because of the phenomenon of atmospheric or astronomical diffraction, the solar rays were not parallel, so that angle β, as measured, was smaller than the real value (see Figure 1). The diffraction is due to the bending of the Sun's rays as they pass through the atmosphere, and results in the Sun appearing higher in the sky than its real position; that is, its altitude above the horizon, υ, is increased. This introduces an additional error into the calculation of angle β, and therefore 'makes' the geographic latitude of the place smaller than it actually is. For a given latitude, the error of diffraction, measured by the angle of deviation, ε, which is formed between the real and the apparent position of the Sun (Figure 3), depends on the height of the Sun above the horizon, and it increases as the Sun's altitude decreases (see Nassau, 1948: 69; Pinotsis, 1994: 180). The altitude of the Sun at upper culmination decreases as the latitude increases, as shown by equation (6), and consequently the error of diffraction increases with latitude. Since Alexandria is at a higher latitude than Syene, the error of diffraction is larger in Alexandria than in Syene. More specifically, in Syene the solar rays fall almost vertically on the surface at the summer solstice, as it results also from equation (6). Consequently, the difference of their geographic latitudes, angle ω, was calculated smaller than the real value and thus the error should be added to the angle of 7.2°.

We will calculate approximately the error of diffraction, ε. This is given by the equation (Nassau, 1948: 70)

$$\varepsilon = (60.5\ \text{arcsec})\cot\upsilon \qquad (5)$$

which is good enough for $\upsilon > 25^\circ$. For small altitudes, tables give the angle ε. The relation

$$\varphi = \delta + z \;\Rightarrow\; \upsilon = \delta + 90^\circ - \varphi \qquad (6)$$

holds true, where z is the zenith distance of the Sun and δ is its declination. We can see that the diffraction in Syene is almost negligible. The declination of the Sun at summer solstice was $\delta = 23^\circ\,43'$ at the time of Eratosthenes. Therefore, the error of diffraction in Alexandria with latitude 31° 11′ is, if we combine relations (5) and (6), equal to 7.93″ = 0.132′. From equation (1), for $\omega = 7^\circ\,12.132' = 7.202^\circ$ we obtain 249,931 stades = 39,364 km.

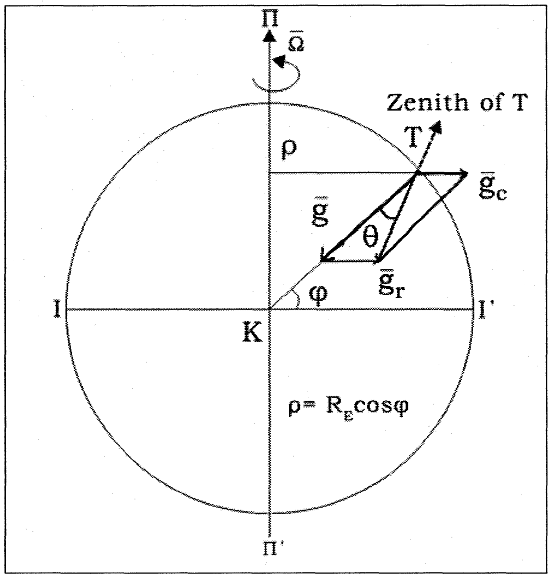

Figure 2: The rotating Earth. The combination of $\overline{g}$ and $\overline{g}_c$ gives the resultant $\overline{g}_r$. The direction of the zenith (plumb line) is along $\overline{g}_r$. The deviation, θ, is from the normal to the Earth's surface.

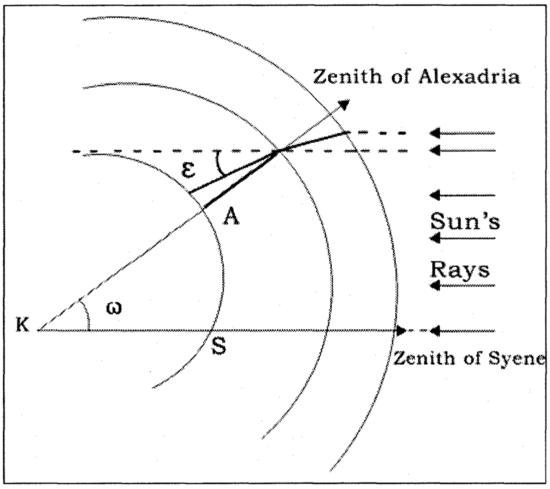

Figure 3: The refraction of sunlight by the Earth atmosphere at Alexandria.

2.1.4 Error 4: The Shape of the Earth

The Earth in antiquity was considered at best a perfect sphere, while actually it has the shape of a geodic: flattened towards its poles. In particular, its surface in first approximation is ellipsoidal from rotation around its axis ΠΠ'-*spheroid* (see Figure 4). This fact influences the calculation of the epicentric angle, ω (in Figure 1). The meridians of the Earth are thus approximated by ellipses, and the eccentricity, e, of each one is given by equation (11). The geocentric latitude of a place T, φ_{gce}, that is the angle formed between the radius of the Earth at T and the plane of the Equator, is smaller than the geographic latitude of this place φ. The geographic latitude is the angle formed by the perpendicular to the surface of the Earth (horizontal plane) and the plane of the Equator. Consequently the angle φ measured by Eratosthenes was larger than the real one (that is to say the geocentric latitude φ_{gce}) by the angle of deviation, γ, which is formed between the direction of the radius of the geoid and the direction of the perpendicular to the surface.

The deviation between the astronomical and the geocentric zenith is given, after Bozis (1967: 31), by

$$\gamma = \varphi - \varphi_{gce}$$

It can be proven that

$$\tan\gamma = \frac{e^2 \sin 2\varphi}{2(1-e^2\sin^2\varphi)} = \frac{\kappa \sin 2\varphi}{1+\kappa\cos 2\varphi} \qquad (7)$$

where e is the eccentricity and $\kappa = \frac{e^2}{2-e^2} = 0.0034.$

Expanding equation (7) into Taylor series and ignoring terms of order higher or equal than two, we get

$$\gamma = \kappa\sin 2\varphi(1 - \kappa\cos 2\varphi) + \ldots$$

for the latitude of Alexandria, φ = 31° 11′, we take $\gamma = 0.18' = 10.8''$.

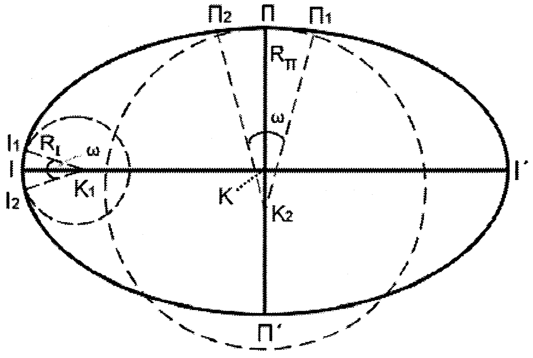

Figure 4: The oblateness of the Earth and the difference in the length of the degree at the Equator and at the poles. The circles of curvature at the Equator, I, and at the pole, П, and R_I, $R_П$, the radii of curvature of the closest circles, K_1 and K_2.

2.1.5 Error 5: The Longitudes of Syene and Alexandria

Syene, with a geographical *longitude* of about 32° 53′, and the area of the Museum in Alexandria, with a geographical longitude of about 29° 55′, do not lie on the same meridian, the latter being around $\Delta\lambda$ = 3° to the west.

2.1.6 Error 6: The Distance Between Syene and Alexandria

The distance between Syene and Alexandria was actually slightly less than 5,000 stades.

2.1.7 Error 7: Syene and the Tropic of Cancer

In Syene, Eratosthenes placed the gnomon at the direction of solar rays because he believed that the Sun was above the Tropic of Cancer. That is to say, he supposed that the solar rays fell vertical at the surface of Earth at Syene. However, Syene was not exactly located on the Tropic of Cancer at the time when Eratosthenes was living, but was about 8′ to the north of it because the obliquity of the ecliptic back at that time was 23° 43′, whereas Eratosthenes had determined it to be about 23° 51′. Given the modern value of 23° 27′, Syene is actually located 35 arc minutes to the north of the Tropic of Cancer.

Cleomedes (1891, i, 10: 100) gives the circumference of the Earth as 250,000 stades, while Strabo (1994, 2.5.7 and 34) explicitly states that according to Eratosthenes it is 252,000 stades. Theon (1878: 124) and Pliny (ii.247) also mention this latter figure, which was the most widely-quoted one in ancient writings and was adopted by later philosophers, geographers and astronomers (Neugebauer, 1975: 653). For example, it was used by Hipparchus and Ptolemy in their various measurements. However, it was not accepted by Posidonius of Rhodes, who calculated the meridian of the Earth, using a new method based on astronomical observations. He calculated the figure of 240,000 stades and later the figure of 180,000 stades for the Earth's size. However, in his calculations of the sizes of the Sun and Moon, and their distances from the Earth, Posidonius preferred Dicaearchus' rounded figure of 300,000 stades for the size of the Earth (Dreyer, 1953: 177; Harley and Woodward, 1987: 168-169; Heath, 1981b: 345; Pinotsis, 1993; 2000).

There has been much discussion about the different values given by Cleomedes on the one hand and people like Strabo, Theon and Pliny on the other (e.g. see Berger, 1880: 141 and 1903: 410; Heath, 1921, ii: 107). It is difficult to dispute the figure that Cleomedes reports by saying that it was not the original value calculated by Eratosthenes, since Cleomedes gives us an explicit and thorough description of the method. But neither can the figure reported by Strabo, Theon and Pliny be disputed. It seems more plausible to suggest that Eratosthenes—or perhaps a later author—altered the value from 250,000 to 252,000 in order to get a round number of 700 stades for one degree (Berger, ibid.; Dicks, 1960: 33, 161; Dreyer, 1953: 175; Heath, ibid.).

According to Goldstein (1984: 411-416), in order to obtain the value of 252,000 stades, Eratosthenes did not perform any actual measurements of observations but simply based his figure on estimated distances and approximations. We should note that the same applies to Posidonius: although Cleomedes mentions the value of 240,000 stades for the Earth's meridian, later sources ascribe the value of 180,000 stades to Posidonius (see Dicks, 1960: 150-151; Dilke, 1985; Dreyer, 1953: 177; Strabo, 1994: 2.2.2).

We conclude, as we explain below, that Eratosthenes adopted the value of 252,000 stades so that 1° would correspond to the round number of 4,200 (252,000/60°) stades. Some years later Hipparchus adopted the same value, and after dividing by 360° he obtained the ratio of 700 stades for one degree.

Eratosthenes did not know that one could subdivide a circle, or the circumference-meridian of the Earth, into 360°, for this concept only emerged later, during the second century BC (see Dicks, 1960: 32, 107, 148-149; Neugebauer, 1975: 671). However, it appears that Eratosthenes knew the significance and the importance of errors in measurements. He knew, therefore, that certain of his measurements contained errors, and consequently that his figure of 250,000 stades was approximate. He therefore followed a new approach by adopting the value 252,000 stades, which was divisible by 60°. This approach resulted from the need for an integer number of stades to correspond to 1°, so that one could conveniently measure distances between various places, the length of a parallel circle, and the extent of the inhabited world. Eratosthenes measured the circumference of the Earth in 60° (Strabo, 1994, 2.5.6, 7); in this case, 4,200 stades corresponded to 1°.

2.2 The Significance of Eratosthenes' Method

Harley and Woodward (1987: 154) believe that the method that Eratosthenes used to measure the size of the Earth was both simple and brilliant. Assuming that a stade was equal to 157.5 m, Eratosthenes accurately calculated the meridian of the Earth, making a relative error of just 1%. His method was therefore an impressive achievement of the ancient Greek intellect. It causes admiration even today, first because Eratosthenes did not have modern astronomical instruments at his disposal, and secondly because the errors mentioned above are independent of his method and are due to various other factors that can be limited today.

Applying his method, Eratosthenes made important measurements that gave a new impulse to the development of geography and cartography. He derived a geographical map of the then-known world (*oikoumene*), on which he traced meridians and parallels. This was a very important development, because Eratosthenes was the first to introduce coordinates that could be used to determine the position of a location on the Earth's surface. We also know that he used astronomical observations and positions of the constellations in order to determine the distances of various places, and he was the first to successfully measure distant locations with some accuracy. For example, he calculated the distance from Alexandria to Rhodes as 3,750 stades (Strabo, 1994, 2.5.c126). The differences in geographic latitudes and longitudes, which were expressed in fractions of a circle (i.e. in degrees) were converted to stades, using the relation that 4,200 stades corresponded to 1°. Furthermore, he converted the latitudes of different places to celestial arcs, and then measured the length of each geographic parallel in stades. In this way, he first estimated that the parallel circle of Athens was shorter than 200,000 stades, while the distance difference between India and the Iberian Peninsula was 78,000 stades (Strabo, 1994, 1.4.6). He also traced on his map a central meridian which passed from Meroe, through Alexandria, Rhodes and other known cities, and he calculated the distances between them. In this way he calculated the extent along this meridian of the inhabited part of the Earth and found it spanned 37,600 stades. Strabo (ibid.) often reports the distances of various places calculated by Eratosthenes, and then compares them with values given by other geographers. We can say that his aims were to estimate the extent of the inhabited world, and to determine the geographical coordinates of each occupied place on the surface of the Earth. This was a major development in geography, and the various distances were reported in his book *Geographica*.

Eratosthenes also divided the known world into parallels and meridians. The parallel that passed through the Straits of Gibraltar, the strait of Messina, the southern edge of the Peloponnese, Rhodes and the Taurus mountains, was the main parallel, while the main meridian passed through Syene, Alexandria and Rhodes (Strabo, 1994, 2.1.1). This division into parallels and meridians and thus the formation of smaller sections—*σφραγῖδες* (seals)—represents an important contribution to cartography (see Dicks, 1960: 39, 159; Harley and Woodward, 1987: 157).

Hipparchus knew that the circle is divided into 360°, and he also introduced this division into the circumference-meridian of the Earth. He adopted Eratosthenes' value of 252,000 stades for the size of the Earth (Strabo, 1994, 2.5.6, 7), meaning there were 700 stades in 1°. Hipparchus wanted to describe celestial phenomena visible between the Equator and the North Pole, and he proceeded to provide tables of astronomical data at 1° intervals of latitude. Obviously he was motivated by Eratosthenes, but what he did not do was measure lengths of various arcs, in different latitudes, along one meridian. Had he done so he may have discovered that the Earth is not a perfect sphere but is oblate towards the poles. We believe that he neglected to make these measurements because he accepted the widely-held belief among philosophers, astronomers and mathematicians of the day that the Earth—like other celestial bodies—was spherical.

Eratosthenes' method can be applied even today in order to determine the size and the shape of the Earth, as we will show in the following paragraph. In our opinion, the achievements of Eratosthenes and Hipparchus in calculating geographic latitudes and distances between various locations on the Earth's surface, provided the motivation for subsequent geographers to determine the size and the shape of the Earth by more accurate means.

2.3 The Application of Eratosthenes' Method

Let us now apply this method to determine the shape and size of the Earth using modern curvature theory. More specifically, by introducing new aspects of Eratosthenes' method we can find approximately the shape and size of the Earth's surface.

We shall consider two places on the Earth's surface that are on the same meridian, and at a distance (length of arc) Δs. Eratosthenes' method allows us to measure the epicentric angle, ω, corresponding to the closest circle (circle of curvature), which is the circle tangential to the arc Δs. The distance Δs can be easily measured. We select lengths of arcs Δs_i, $i = 1, 2, \ldots, N$, corresponding to equal or even different epicentric angles ω_i, $i = 1, 2, \ldots, N$, which, however, should be small ($\omega_i < 7^\circ$) so that the arc Δs can be approximated by the part of the circumference of the closest circle. Measuring the length of various arcs of the same terrestrial meridian at the same height above the sea's surface (in this way the arcs Δs_i will be on the same meridian and not on major circles with different radii) in different latitudes it is proven that the lengths of these arcs per 1° of epicentric angle increase as we advance from the Equator towards the poles. Consequently, the lengths of arcs of the same meridian that correspond to the same epicentric angle (1°) are not equal. If the Earth were spherical, the length of arc per 1° would be constant on the same meridian and independent of the geographic latitude.

We consider now a meridian of the Earth, ΠΙ′Π′ΙΠ in Figure 4, and measure the length Δs of the arc of the meridian between two different points, which corresponds to an angle of $\Delta\varphi = \omega = 6^\circ$. For simplicity, in Figure 4 we take Δs at the Pole Π and at the point of intersection of the meridian with the Equator, I, because these points present the largest difference of curvature. We then trace the closest circles, K_1, K_2, at the points I and Π respectively, and take an angle $\omega/2 = 3^{\circ}$ at both sides of points Π and I. Consequently, the corresponding epicentric angles are $\Pi_1\hat{K}_2\Pi_2 = I_1\hat{K}_1I_2 = \omega = 6^{\circ}$. In Figure 4 the closest circles are depicted with dashed lines. K_1 and K_2, which are the centers of the closest circles at points I and Π respectively, do not coincide and are different from the center of curve ΠI΄Π΄IΠ, K. We consider that the curve in the Figure 4 is an ellipse (i.e. it has one center). In the general case, the curve will not have a center. The elementary length of arc Δs in the region of a point is measured as a function of the radius of curvature (R_I or R_Π) of the corresponding closest circle. That is,

$$\Delta s_\Pi = \Pi_1\Pi_2 = 2\pi R_\Pi(\omega^{\circ}/360^{\circ}) \text{ and}$$

$$\Delta s_I = I_1I_2 = 2\pi R_I(\omega^{\circ}/360^{\circ}) \quad \Rightarrow \tag{8}$$

$$\frac{\Pi_1\Pi_2}{I_1I_2} = \frac{R_\Pi}{R_I} \tag{8a}$$

In the general case ($\omega_1 \neq \omega_2$), equations 8 and 8a become,

$$\Delta s_\Pi = \Pi_1\Pi_2 = 2\pi R_\Pi(\omega_2{}^{\circ}/360^{\circ}) \text{ and}$$

$$\Delta s_I = I_1I_2 = 2\pi R_I(\omega_1{}^{\circ}/360^{\circ}) \quad \Rightarrow \tag{8b}$$

$$\frac{R_\Pi}{R_I} = \frac{\Pi_1\Pi_2}{I_1I_2}\frac{(\omega_1{}^{\circ}/360^{\circ})}{(\omega_2{}^{\circ}/360^{\circ})} \tag{8c}$$

From equation (8a) it seems that the radius of curvature of the closest circle that corresponds to the poles is larger than the one that corresponds to the Equator ($R_I < R_\Pi$), hence $I_1I_2 < \Pi_1\Pi_2$ (Figure 4). In practice, measuring the distances Δs_Π and Δs_I and the epicentric angles $\Pi_1\hat{K}_2\Pi_2 = I_1\hat{K}_1I_2 = 6^{\circ}$ (or $\omega_1 \neq \omega_2$) we have the result that $\Delta s_I < \Delta s_\Pi$ and consequently $R_I < R_\Pi$. Therefore, the Earth is flattened at the poles. The epicentric angles $\Pi_1\hat{K}_2\Pi_2$ and $I_1\hat{K}_1I_2$ can also be measured by means of astronomical observations.

In order to determine the type of the curve, we should find its vector equation (equation (10) from the relation of the radii of the closest circles. Thus, along one of the meridians and proceeding from the Equator to the North Pole (and the South Pole) we consider N arcs Δs_i at the points s_i, i = 1, 2, ..., N. Then we trace N closest circles to the corresponding arcs Δs_i. The arcs Δs_i may correspond to equal or different epicentric angles $\Delta\varphi_i = \omega_i$, i = 1, 2, …, N, assuming that they are small in order to achieve better precision in the determination of the curve. Hence, since Δs_i and $\Delta\varphi_i$ are known, from equation (8) (or (8b)) we determine the radii of curvature $R_i(s)$, i = 1, 2, ..., N, of the closest circles and consequently the curvature of the curve (and also the vector of curvature $\overline{K}_i(s)$):

$$K_i(s) = \frac{1}{R_i(s)}, \quad i=1,2,..,N \tag{9}$$

at the N points which are at the distances s_i from the point I. The arcs, s_i, are measured along the meridian from the point I on the Equator towards the North Pole. To this end, we make the following table:

s_1	Δs_1	$\Delta\varphi_1$	$K_1(s)$
s_2	Δs_2	$\Delta\varphi_2$	$K_2(s)$
.	.	.	.
.	.	.	.
s_N	Δs_N	$\Delta\varphi_N$	$K_N(s)$

The next step is a curve-fitting of the values of the table_in order to find the function K = K(s) (and $\overline{K} = \overline{K}(s)$), which will describe in the best possible way the curve of the meridian. Consequently, after solving numerically or analytically, depending on the measurements, the following differential equation (Pinotsis, 2002: 48),

$$\ddot{\overline{r}}(s) = \overline{K}(s) \tag{10}$$

where $\overline{r}(s)$ is the position vector, we find the equation of the curve and thus the shape and the size of the curve (ellipse, hyperbola, etc). It can be proved, therefore, from the relation of the radii R_i, that the meridian of the Earth has the approximate shape of an ellipse. For the approach to become more and more precise, the arcs Δs_i (or the angles ω_i) have to be smaller and smaller. For an arbitrary meridian that can be approximated by an ellipse, the eccentricity, e, is given by the relation (Pinotsis, 2002: 139)

$$e = \left(1 - \frac{R_\Pi{}^2}{R_I{}^2}\right)^{\frac{1}{2}} \tag{11}$$

where R_I is the axis on the Equator (the semi-major axis of the ellipse) and R_Π is perpendicular to the Equator (the semi-minor axis).

We can also repeat this process along the Equator at the equinox so that the Sun's rays are parallel to the plane of the Equator. Then equal epicentric angles correspond to approximately equal arcs, and all the centers of the closest circles coincide or, in other words, all the closest circles coincide in one circle. Therefore the Equator can be taken as a circle.

Upon repeating this process for various other symmetrically-distributed meridians, we will determine equations 8a (or 8c), 10 and 11 for each meridian. Thus, we can find approximately the size and shape of the Earth's surface by combining all of the meridians, using an analytical or numerical geometrical method (depending upon the actual data).

The accuracy of the method depends on the number, N, of the arcs, Δs_i, along a meridian, and the epicentric angles, ω_i, of the closest circles. This method could also be applied to any other celestial body.

3 NOTES

1. The time of local noon at Syene during the summer solstice was confirmed when the Sun's rays reached the bottoms of several deep vertical wells there at the same time with the local noon of Alexandria and were reflected off the water.

2. Although there has been some discussion regarding the precise length of the stade used by Eratosthenes, the majority of authors, including Dilke, Dreyer, Heath, Hultsch and Thomson, accept the value of 157.5m. It seems that certain values for the length of a stade were used in ancient Greece during different periods. A known stade was the Olympic stade of 185 m, while another was the Ptolemaic or Royal stade of 210 m. But according to Pliny (1865, xii, 53, c13), the stade used by Eratosthenes was smaller than the Olympic stade and was about 157.5 m (Hultsch, 1882: 364). The stade of Eratosthenes was a unit used in antiquity in order to calculate distances measured in steps by professional pacers (*bematistai*). According to Rennell (Dreyer, 1953: 176), this stade was about 154 m rather than 157.5 m. In the following example

we compare the accuracy of the results that are obtained using different values for the length of 1 stade. If we use the Olympic stade, then 250,000 stades × 185 m = 46,620 km, and we have an error ~16.5%. If we use the value of 154 m, then 252,000 stades × 154 m = 38,808 km and the error is reduced to ~3%.

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MEASURING THE WORLD: EXCURSIONS IN GEODESY AND ASTRONOMY

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Abstract: The authors provide a brief history of attempts to measure the size and shape of the Earth, stressing the close relationship between astronomers and geodetic surveyors who, indeed, were often the same people. Particular interest is placed on F.G.W. Struve's measure of the meridian through Dorpat (modern Tartu, Estonia), which extended over 25° 20′ and was made in the early nineteenth century. Sites connected with this measurement were designated as World Heritage sites by UNESCO in 2005, and the authors describe the steps taken to achieve this result.

Key Words: geodesy, meridian arc, size and figure of the Earth, World Heritage, F.G.W. Struve

1 INTRODUCTION

In July 2005 UNESCO declared a number of sites connected with the Russian arc measurement undertaken by F.G.W. Struve in the nineteenth century to be a World Heritage monument. In particular, the old Observatory building in Tartu, Estonia (known to Struve as Dorpat) was so designated. The declaration by UNESCO was the climax of an initiative undertaken by the Fédération Internationale des Géomètres (FIG) and the National Land Survey of Finland, with the support of the International Association of Geodesy (IAG) and the International Astronomical Union (IAU). In this paper, we aim to give an account of the contributions to surveying made by Struve and other astronomers throughout two millennia.

Astronomers and surveyors are scientific cousins; indeed, until the rise of astrophysics they were often the same people, using the same mathematical formalisms and very similar skills and instruments in their two spheres of activity. The invention of the telescope at the beginning of the seventeenth century eventually made possible much more accurate measurements of both celestial and terrestrial positions. Meridian circles and theodolites are specialized versions of the same basic instrument; anyone competent to use the one could quickly acquire basic skills with the other. Paradoxically, however, the instrument that so greatly increased the potential of both professions was ultimately responsible for their practitioners drifting apart. Astronomers became astrophysicists, and few of them now know the techniques of the old positional astronomy: surveyors continued to refine their specialized techniques and, in recent years, have greatly profited from Electronic Distance Measurement (EDM), radar and the Global Positioning System. Nowadays, members of neither profession could practice the other without undergoing a second apprenticeship.

This division of the two professions is, however, a modern development. Throughout recorded history, right up to the end of the nineteenth century, we find the same names recurring in the history of both subjects. We may not know who produced the first known map found on a Babylonian clay tablet dated to the third millennium BC, or exactly who surveyed the Nile valley each year after the annual flood, but we do know that the name of Eratosthenes (c.276–c.195 BC) appears prominently in the early history of both astronomy and surveying. His work is a good starting point for us, since our interest is in geodetic surveying, the aim of which is to determine the size and figure of the Earth. Eratosthenes' attempt to do this is the earliest that has come down to us, and his method, in its essentials, is perfectly sound, containing elements that are still used. By his time, it was well known that the Earth is at least approximately spherical; Aristotle (384–322 BC) had given good reasons for believing that. If the Earth were exactly spherical, all that would be necessary to determine its size would be to measure the distance between two points on the same meridian of longitude and to determine the latitude difference between them by astronomical observations. Then the circumference and radius of the Earth follow by simple calculation.

2 THE EARLIEST EFFORTS

As is well known, Eratosthenes is said to have understood that at Syene—usually identified with modern Assuan—the Sun at the summer solstice was directly overhead. Some say this was deduced at Syene from a deep well into which the Sun shone right to the bottom. Others followed Cleomedes who referred to the absence of visible shadows from the pointers on sundials. It is usually inferred that, in this text, 'sundial' meant the pointer within the bowl of a sciotheron—a device invented by Aristarchus (c.310–230 BC). The sciotheron was a vertical gnomon in a hemispherical bowl, so the absence of a shadow indicates that the Sun is directly overhead, just as the appearance of the reflection of the Sun in a deep well would. It was understood among the astronomers in

Egypt that at any point within a roughly circular tract of ground at Syene, no gnomon would cast a shadow at noon on the day of the summer solstice. This effect was said to extend to a radius of 150 stades around the sundial, which meant, presumably, that up to a distance of 150 stades (or about 24 km, if the 'stade' is taken to be ~157 m) north and south no shadow could be distinguished. This implies that a shadow could only be distinguished in the sciotheron when it reached an angular length of approximately 13 arc minutes.

By measuring the meridian altitude of the Sun on the same day at Syene and Alexandria, and determining the distance between the two towns, the size of the Earth can, in principle at least, be easily deduced. Eratosthenes probably also used a sciotheron at Alexandria to measure the Sun's altitude. The hemispherical bowl of the sciotheron was carved out of a block of stone, and the inside surface bore graduated elevation lines which permitted the determination of the Sun's altitude from the shadow it cast (see Figure 1). The result of the measurement was 1/50th of a circle (7.2° in our terms) for the latitude difference between Alexandria and Syene. The method used for measuring the linear distance between the two towns is open to speculation. It might have been paced by specially trained Bematistes (surveyors), or measured by the number of days of journey by camel, or by some crude technique of linear measurement.

Figure 1: Example of the hemispherical bowl of a sciotheron found near Alexandria, at the base of Cleopatra's needle (British Museum No. 1936 3-9.1. Photograph courtesy of the British Museum).

Combining the angular value with the conveniently rounded value of 5,000 stades for the measured distance, Eratosthenes deduced a circumference for the Earth of 250,000 stades, rounded up to 252,000 (possibly to make 1° equal to 700 stades) which probably corresponds to about 46,250 km. Thus 1° of latitude would correspond to about 128.5 km, if the relation between stades and kilometres is correct (there were several units of that name ranging between the equivalents of 157.5 m to 210 m). As we shall see, relating old units to modern ones is a recurrent difficulty in assessing old measurements, and direct comparisons should be treated with caution since apparent variations may reflect the use of different units with the same name. Whatever the value of the stade used, there were, however, other sources of error. Some of these probably cancelled one another out so that the results appeared to be better than they really were.

For all the uncertainties in Eratosthenes' work, it remained standard in Europe for almost two millennia. Posidonius, in about 100 BC, and Ptolemy, in about AD 140, gave values for the circumference of the Earth, but there is no evidence that they made measurements of their own. Posidonius (c.135 BC–c.50 BC) was said to have noticed, or to have had pointed out to him, that the star Canopus grazed the horizon in Rhodes. In Alexandria its meridian altitude of transit was a fourth part of a sign of the zodiac, or 7½°. The distance between the two locations he took to be 5,000 stades, which could only have come from mariners' estimates. Ptolemy (fl. AD 120) brought together in a single work, which came to be known as *The Almagest*, all that was known at that time about astronomy. He is credited with the discovery of atmospheric refraction, the idea of latitude and longitude and a rectangular grid on maps. It is thought unlikely that he made any measurements of his own of the size of the Earth, but he accepted the idea of a spherical Earth with a circumference of 180,000 Egyptian stades. The Egyptian Stade was 210 m, compared with 157.5 m for the short stade believed to have been used by Eratosthenes; even so, as Appendix 1 shows, Ptolemy's estimate for the size of the Earth is the smallest that has come down to us.

Claudius Ptolemy, nevertheless, is a good example of the identity of astronomers and geodesists. In antiquity his reputation as a geographer was as high as that of an astronomer, and the publication of a Latin translation of his *Geographica* in 1405 was one of the stimuli of early modern surveying. The value that he accepted for the circumference of the Earth stands out as much less than earlier or later ones and this also was to resonate at the beginning of the modern era, since Christopher Columbus based his arguments for the feasibility of sailing westwards to Cathay and the Indies on Ptolemy's estimate of the size of the Earth. Contrary to widespread belief, Columbus did not have to convince his opponents that the Earth was spherical, only that it was smaller than they believed—and they were more nearly right than he was.

3 ASIAN MEASUREMENTS

Until the sixteenth century of our era, other attempts to measure the size of the Earth were only made outside of Europe. Around AD 724, I-Hsing (682–727) and the Astronomer Royal, Nankung Yüeh, in China arranged for a series of nine gnomons to be set up close to what we call the 114° E meridian of longitude. Each gnomon was eight Chinese feet high, and they were erected at intervals over a distance of about 34° in latitude. From simultaneous measurements of shadow lengths at each gnomon, at both the summer and winter solstices, the latitude differences between successive gnomons could be computed. Once more, there are obvious sources of error, but I-Hsing appears to have settled for a change of shadow length of about 4 Chinese inches for each 1,000 li of distance or a value equivalent to 1° = 157.52 km, but, again, there is uncertainty which of two units called a 'thang' he was using.[1]

Almost one hundred years later, c.829, Al-Mamun (786–833), the seventh Abbasid Caliph of Baghdad, had an observatory built in the city that overshadowed any built before in that part of the world. Continuous and detailed observations were possible, some of which were considered so important that they were specially recorded and signed on oath. In c.827 he requested his astronomers to measure an arc of meridian on the Plains of Sinjar. Reports suggest that probably more than one arc was measured. Among the possibilities are two successive degrees in the Plains of Sinjar; a single degree between Palmyra and Regga, and a single degree between Baghdad and Kufa. Detailed references are scant, but the generally-accepted figure for the length of 1° is 56⅔ Arab miles, or about 111 km. With regard to the measure at Sinjar it is thought that two measuring parties started from a central point, one going due north and the other due south, until their respective astronomical observations each indicated that they had traveled by 1°. On comparing results one group had recorded 56 Arab miles and the other 56⅔. Why the latter was accepted rather than a mean is not reported. But what of the Arab mile? Values for its equivalent vary considerably, although a figure around 2 km is generally accepted. The linear distances were possibly measured by using two ropes, each about 50 coudees (25 m) long, placed so that they overlapped by half, and then 'leap-frogged' with each other along the line. Alternatively three pickets were aligned then the last moved forward, and so on. Other than by the naked eye, it is not known how alignment was maintained.

4 THE INTRODUCTION OF TRIANGULATION

After the above effort, the scene of activity returned to Europe. In 1536, a few decades after the voyage of Columbus, the Frenchman Jean Fernel (1485?–1558) was to become Astronomer and Physician to King Henry II. About ten years before this, he is said to have observed the meridian altitude of the Sun from Paris—probably at the Collège de Ste Barbe. Then he calculated the position that the Sun would have on subsequent days at a position one degree further north. For this he made allowance for the change of about 22′ in the Sun's declination between observations. He then traveled northwards towards Amiens for three days to find the required position some 25 leagues from Paris. He measured the altitudes with an 8-ft high triquetrum, a fixed wooden right angled triangle set with one side vertical, bearing a moveable arm carrying sights pivoted at the right angle and able to move over the hypotenuse, which was graduated in degrees and parts thereof. The distance travelled from Paris was determined by counting the revolutions of his carriage wheel which had a radius of 6 ft 6 doigts. The total number of revolutions was 17,024, each equal to just over 20 ft, depending on the value used for π. Fernel expressed his result as 68 Italian miles plus 95¼ paces, and there is again uncertainty about the value of this unit in modern terms. The figure of 56 746 toises, or 110.69 km, for the final result is subject to uncertainties in the conversion of units. As with all earlier measures of arcs of the meridian there were again great doubts expressed about the validity. In fact, Lalande (1732–1807) and later Delambre (1749–1822) were most emphatic in saying that the details published by Fernel in 1528 were no more than a fanciful imagination of principles never executed (see Delambre, 1819; Lalande, 1789).

Just a few years later a new approach became available with the introduction of triangulation. The original idea for this is usually associated with Gemma Frisius (1508–1542) although, since he was the teacher of Mercator (1512–1594) of the projection, it is highly likely that the two developed the method jointly. In fact, new research in Belgium is now indicating that Mercator might well be credited with the first practical use of triangulation.

In 1533 Frisius published his work *Cosmographica* and introduced the modern form of triangulation, by which large distances could be measured by surveying a chain of triangles. The modern formalisms of plane and spherical trigonometry were beginning to be developed at about this time, and their application to surveying would soon become obvious. Frisius does not appear to have done any surveying himself, but his methods were applied by Tycho Brahe (1546–1601), who may have had some contact with a nephew of Frisius who was an instrument-maker (Thoren, 1990). Brahe surveyed his island fief of Hven, probably during or shortly before 1579, mapping the island and its relationship to major towns on the mainland. According to Thoren (*ibid.*), Brahe had plans for a survey of the whole kingdom of Denmark (which then included what is now southern Sweden), but apparently these were never completed.

The first to use triangulation in a measurement of a meridian arc appears to have been Willebrord Snellius (1580–1626), chiefly remembered today as the discoverer of the law of refraction. Snell, to use the anglicized form of his name, was somewhat of a prodigy since he was lecturing on mathematics and astronomy at Leiden University when he was only 19 years of age. From 1615 he measured five short baselines near Leiden. These varied from 87 to 475 roods (327 m to 1,788 m) in length, perhaps only a fifth or less of the main triangulation sides. Because the baselines were so short, a small scheme had to be measured to reproduce the short baseline through a series of triangles of increasing size onto one of the sides of the main triangulation. Snell introduced this practice, which has become known as a base extension net to extend the base to one of the longer sides of the triangulation. His arc spanned 1° 11′ 30″ and 33,930.2 roods to give a value of just in excess of 55,000 toises (107.4 km) for the length of one degree. Triangulation methods were not yet perfected, and because of the use of such short bases, the use of open sights, his tendency to measure only two angles of a triangle and to deduce the third, and errors of calculation due to the absence of logarithmic tables (although invented by Napier in 1614, Snell did not use them), Snell's result is now thought to be in error by about 2,000 toises. The measurement, however, remains an important landmark in the history of geodetic surveying (see Haasbroek, 1968).

Not everyone rushed to embrace the method of triangulation, however. In England between 1633 and 1635, Norwood (1590–1676) reverted to the use of a 99-ft long chain and pacing to determine the distance from the Tower of London to York. To quote from his publication:

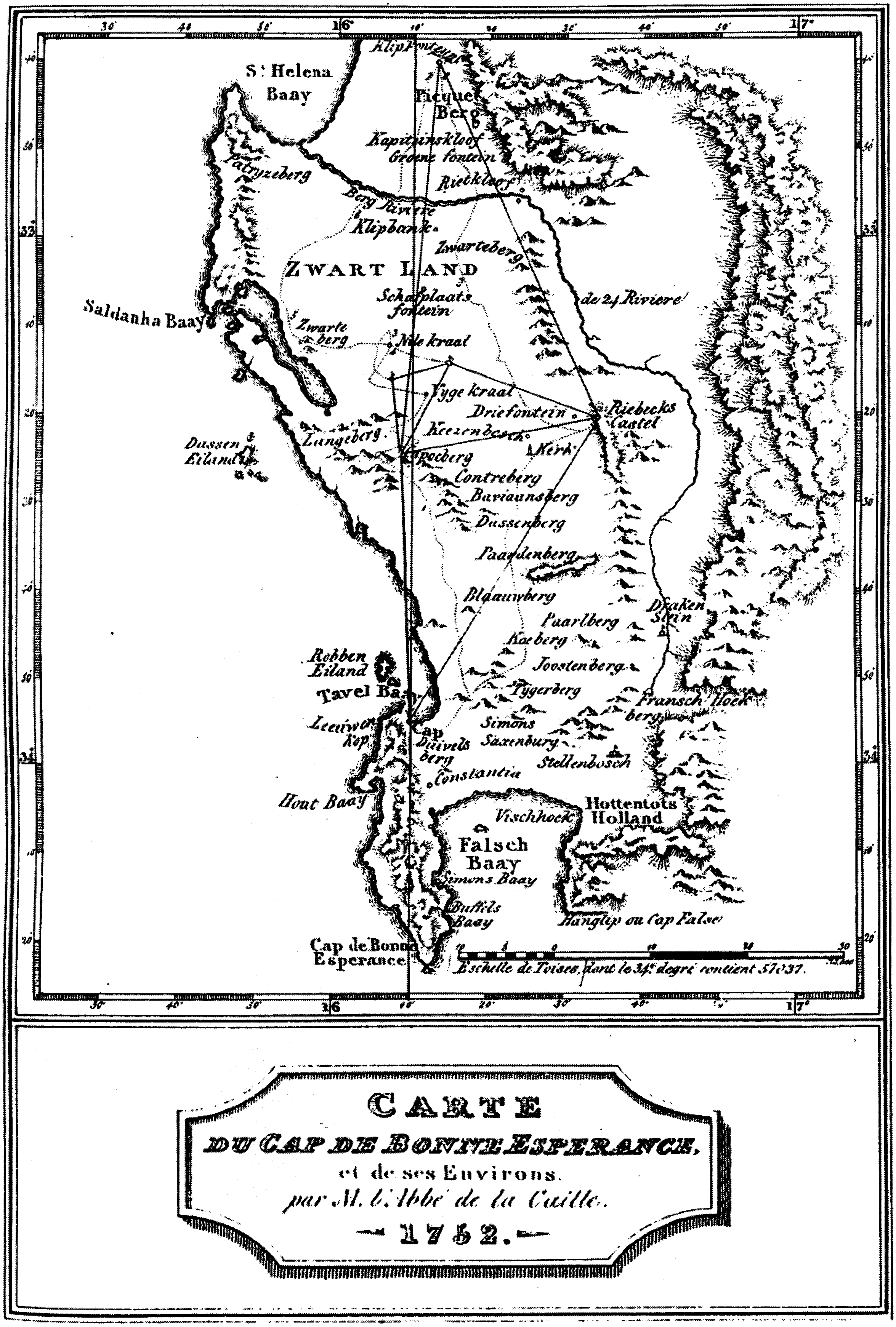

Figure 2: Map of LaCaille's South African Arc (after LaCaille, 1763).

> Upon the eleventh of June 1635, I made an observation near the middle of the city of York, of the meridian altitude of the sun, by an arc of a sextant of more than 5 foot semi-diameter, and found the apparent altitude of the sun that day at noon to be 59° 33′. I had also formerly, upon the 11th of June 1633, observed in the city of London ... (Norwood, 1637).

He found the latitude difference as 2° 28′ and the linear distance 9149 chains. He states that he made due allowance for the windings of the way in arriving at the distance. From this he deduced the length of 1° to be 367,196 ft (111.92 km). Norwood's reasons for disdaining triangulation are unclear. Snell had demonstrated the feasibility of the method nearly twenty years earlier, and surely that fact would have crossed the English Channel by the time Norwood was measuring, although he was in America from 1614 to 1627 and therefore somewhat out of touch with developments.

About a decade after Norwood, the Jesuits Riccioli (1598–1671) and Grimaldi (1618–1663) used a flawed technique to measure the distance between the tower of the Cathedral in Modena and the Jesuit Residence on Mount Serra-Paterna near Bologna. The flaw was to neglect the effects of refraction on measured zenith distances in a technique that relied upon it. This was strange because Riccioli, a notable Italian astronomer and later author of a 1,500-page volume on the history of astronomy, was considered by Delambre to be among those who marked the dawn of geodesy. Delambre (1819: 382-385), outlined the basis of their method in general terms thus:

> He then devised a method for measuring a degree of the spherical earth. Take two places each visible from the other and with zeniths of Z and B; at Z observe a good star at A in the vertical ZBAO, then you have ZA, PZ and PA and can calculate the angle A.
>
> At B observe the zenith distance BA and you have ZB = ZA – BA with PB, PA and the angle A, Riccioli calculated BA which he deduced from ZA; he apparently intended to save the trouble of transporting his quadrant from Z to B. He did not speak of refraction of the star A, which he said was at 69° 47′ from the zenith. Such refraction would have been 2′ 35″
>
> Since from Z one could see B, one could alternatively measure the azimuth PZA, then instead of calculating A one calculated B, with Z, PB and PZ from ZB. Then the method resembled that of Ptolemy. With ZB always an arc of a third or of a half degree or so; the error of the observation was double or triple in the value of a degree. It was perhaps the penalty for reproducing an idea of ancient times.

From a baseline of 1094 Bolognese paces Riccioli (1661) and Grimaldi obtained a result of 20,394 paces, and from measurement of zenith distances at each point they deduced the latitude difference to get the length of a degree as 61,478 toises (119.8 km). This value is clearly too high (Smith, 1986).

5 THE FRENCH SURVEYS

In Paris the French Academy of Sciences was founded in 1666 and among the initial members in the astronomy section was Jean Richer (1630–1696). In 1670 he used a quadrant bearing a telescope with cross-hairs to determine the latitude of the fort at Penobscot Bay. Two years later he sailed to Cayenne where he was to collaborate with astronomers in Paris in work on several fundamental questions in astronomy. While there, he found that the length of a pendulum beating seconds was about 1¼ lignes (2.8 mm) shorter in Cayenne (near the Equator) than the 3 ft 8.6 lignes in Paris.[2] The length of a pendulum of given period is, of course, directly proportional to the acceleration due to gravity and, in fact, at least in the late seventeenth century, measurements of that acceleration by means of the pendulum were capable of much greater precision than surveying could deliver. Even the small difference found by Richer would, if uncorrected, make a clock constructed in Paris lose 148 vibrations a day in Cayenne.

From the mid-seventeenth century onwards, however, surveying methods were remarkably improved and probably reached the maximum precision possible (before the advent of EDM, radar and global positioning techniques) by the middle of the twentieth century. In the late seventeenth century, also, a new element was introduced by the prediction of Newton (1642–1727) in his 1687 *Principia* that the Earth should be an oblate spheroid with the equatorial and polar radii in the ratio 230/229, rather than a perfect sphere. Clearly very precise measures were needed to detect a difference of less than 0.5%. Not only would the length of a degree be somewhat greater near the poles than near the equator, but the acceleration due to gravity would also change in the same sense, as suggested by Richer's observation.

From the time of Richer and throughout the eighteenth century, France became the centre of geodetic activity. Prominent among those working in the field were the first two generations of the Cassini family. Although the family was of Italian origin, its members in four generations were Directors of the Paris Observatory. Of particular interest in our context were G.D. Cassini (1625–1712), who had sent Richer on his expedition, and his son J. Cassini (1677–1756). They undertook extensive projects of arc measurement throughout France, making altogether a vast number of measurements during the sixty years from 1681 to 1742. In particular they found the following, the first result being the joint effort of father and son, the others, of course being the work of the younger Cassini:

- In 1701 an arc of over 6° was measured between Paris and Collioure, to give 1° = 57,292 toises; re-observation in 1713 gave a result only slightly different.
- In 1718 the arc was extended northwards to Dunkirk to give 1° = 56,960 toises.
- In 1733 an arc of parallel at 49° N from Paris to St Malo gave results that similarly when corrected, indicated a prolate Earth and an extension of the arc a year later gave a similar result.

All these results were in conflict with Richer's pendulum observations which should, as we have remarked, have inspired the greater confidence; but the Cassinis' results were the product of field measures of the greatest precision attainable at the time and the Cassinis, at least, gave them more weight than Richer's observations. Thus it seemed that Newton was wrong about an oblate Earth. Not only was doubt thrown on some aspects of Newton's work, but practical problems were raised. Even an error of 1 km in the length of a degree could have serious consequences for those sailing across oceans. The truth was that the differences the Cassinis were looking for were smaller than the observational errors inherent in their equipment, a fact that they had not appreciated.

The Academy of Sciences in Paris was more forward-looking than the slightly older Royal Society of London, and wished to settle the question once and for all. The Academy proposed to mount two expeditions, one to go as near the equator as possible and the other as far north as possible. So, in 1736, a group under Pierre Maupertuis (1698–1759) left Paris for Lapland, while in the previous year a group nominally under Louis Godin (1704–1760) had departed for what was then Peru, but is now mostly in Ecuador. The Lapland group endured harsh conditions; at one time when the team was working at –22°C on the baseline measurement the tongue of one member became glued by frost to the silver cup from which he was drinking brandy! At –36°C the spirit-of-wine thermometers froze, yet the mercury thermometer was still recording when the temperature fell to –46°C. The party was however able to report back after less than 18 months overall, with a value of 1° = 57,438 toises at latitude 66° 20′ N.

The Peruvian group suffered from poor leadership and split into factions. Bouguer (1698–1758) and La Condamine (1701–1744) ran the operation more than Godin did, but in addition to continual arguments finances were short and the weather was harsh. Unlike Lapland, where elevations were a few hundred metres at most, Peru was mountainous and the triangulation had to be made between numerous mountain peaks; fifteen of these were higher than 4,000 m, and some were over 4,500 m. Progress was painfully slow, and not until 1745 did the expedition's members begin to return to Paris, reporting a value of 1° = 56,748 toises at 1° 31′ S latitude. Meanwhile, more measurements in France by the younger Cassini and his son, C.F. Cassini de Thury (1714–1784), gave 1° = 57,061 toises at latitude 46° 30′ N: altogether, a definite 'yes' to Newton's theory and a 'no' to the Cassinis' earlier work.

A little later the Jesuit physicist Père Ruggieri Giuseppe Boscovich (1711–1787), by the age of 20, was discussing the shape of the Earth and planetary motion. In 1736 he observed a transit of Mercury, and during his lifetime he wrote at least fifteen texts on astronomy, mathematics, the shape of the Earth and related topics. Boscovich is remembered principally for his ideas about atoms, but he undertook many diverse investigations in what we would now call physics and astronomy. In the early 1750s he teamed up with Christopher Maire (1696–1787) to measure an arc from Rome to Rimini of some 2½°. His arc measurement must have been a major feat since it involved crossing the Appenines, although not at their highest point. His result, equivalent to 1° = 111.03 km, fitted well with the French results.

Accordingly, attention now turned to refining knowledge of the size of the Earth and of the ratio of the two principal radii. A new question presented itself, however: if the Earth is not a perfect sphere, might there not be other differences than that between the two principal radii? For example, is the Earth necessarily symmetrical about the equator?

In 1752, another Frenchman, the Abbé LaCaille (1713–1762) went to Cape Town, South Africa, primarily to measure and catalogue the positions of 10,000 stars (for which he was to gain international fame). He was to produce numerous papers, books and catalogues during his lifetime. He nevertheless found time while at the Cape to measure an arc of 1° 13′ north from the city (Figure 2). For this he was able to draw upon his experience on the arc measure from Dunkirk to Perpignon where he had assisted Cassini de Thury over large sections of the work. For his astronomical observations he used a zenith sector of 6-feet radius with a graduated arc of 51°. His sextant was similarly of 6-ft radius bearing two telescopes one of which was fixed parallel to the radius and through the zero of the limb. For his baseline he used four fir rods each 18 pied 3 pouce long to obtain a length of 6,467 toises (12,600 m). From this, his distance between the terminals of the triangulation was 69,669.1 toises. Combining this with his arc length of 1° 13′ 17.33″ determined from sixteen different stars he found a value of 1° = 57 037 t (111,165 m) at 33° 08′ S. This was very similar to the result from France at 46° 30′ N.

If the two hemispheres were similar, the value from the Cape should have been about 200 m smaller. Subsequent investigations by Sir George Everest (1790–1866) and an extension of the arc by Thomas Maclear (1794–1879) showed that there was an error in LaCaille's work caused by a gravity anomaly that deflected the plumb line by about 8.5 arc seconds from the true vertical. Something similar had troubled the Peruvian expedition, but attempts to investigate it were inconclusive, again because the amounts to be measured were smaller than the errors inherent in the equipment. The Peruvian expedition was the first ever effort to quantify the attraction of mountain masses on the plumb line, and hence the survival of the name 'Bouger' for one of the anomalies within the science of gravity observations. So it seemed that the hemispheres were broadly similar in shape. The question came up again in the twentieth century when some of the earliest geodetic satellites showed a somewhat larger southern hemisphere. The Earth, it was said—perhaps somewhat misleadingly in view of the magnitudes involved—is 'pear-shaped'.

The French unit of the toise quoted in all these results was a little less than 2 m. The metric system, of course, did not exist when the work cited was being done; it was the result of an initiative taken by the early post-revolutionary government in France, although definition of the system was delayed by the Reign of Terror. The aim was to create a uniform system of weights and measures throughout the country. In fact, perhaps partly because of the activities of Napoleon, the system was widely adopted through continental Europe, and subsequently throughout much of the world. It was decided to adopt as unit of length one ten-millionth part of the quadrant of the meridian of Paris from the North Pole to the Equator. In many ways this was an unfortunate way to define a fundamental unit, and there were people at the time who argued against it. Another proposal was to adopt the length of a pendulum beating seconds at the

Equator (Richer's work had shown that the length would depend on where the pendulum was situated). Delambre (1749–1822) and Méchain (1744–1804) were commissioned to re-measure some 9° of the Paris meridian, and it was this survey that was delayed by the Terror—at different times both Delambre and Méchain were arrested (Murdin, 2005). To take account of the fact that the Earth is a spheroid, the results of the Peruvian (or Ecuadorean) expedition were combined with this new measurement. Unfortunately, the idea has gained ground that the metric system was derived solely from the work of Delambre and Méchain. This is not correct; because the Earth is a spheroid there are two unknowns to be found and two equations are therefore needed. A second arc was essential and the Peruvian one was the best one available. From the French arc of 9° 40′ 25″, with a mid-latitude of 46° 11′ 58″ N and a length of 551,584.7 toises, and the Peruvian arc of 3° 07′ 01″, with a mid-latitude of 1° 31′ 00″ S and a length of 176,873 toises, the ellipticity of the Earth was found to be 1/304 and the length adopted for the metre was (approximately) 0.513 times the toise of Peru, which contained 864 Paris lignes. Thus, in terms of the Parisian unit, the metre is 443.296 lignes (Smith, 1997).

Subsequent measures, not surprisingly, showed that this length was *not* one ten-millionth part of the quadrant. By 1841 Bessel gave the Earth quadrant as 10,000,856 m; in 1857 Alexander Clarke quoted 10,001,983 m, so, in terms of the original intention, the metre is 'too small'. The accepted value, however, has long since been enshrined in a metal bar in Paris that, kept in a controlled environment, defines the standard metre. The original intent in setting up the system has become irrelevant.

The story of the Paris meridian does not end there. In 1806, the physicist and astronomer Arago (1786–1853)[3] was sent to Spain to extend Méchain's survey to the Balearic Islands. In the nineteenth century, however, the centre of activity moved eastward, first to Germany, and then to the Russian Empire and Scandinavia. Argelander (1799–1875), Bessel (1784–1846), Encke (1791–1865), Gauss (1777–1855), Schumacher (1780–1850) and, briefly, F.G.W. Struve (1793–1864) himself, were involved in an arc measurement in northern Germany in the early 1820s. Gauss was in fact criticized by some for wasting his time, and therefore his mathematical talent, on a routine task that others could have done as well. He thought, however, that he should be involved in the applications of his work. Considering that he gave us the first, and still the most commonly-used, method of assessing observational errors, he was perhaps right.

6 STRUVE'S ARC OF THE MERIDIAN

Struve's involvement in the German work was brief because he was already resident in Dorpat (modern Tartu) in Estonia, then a province of the Russian Empire. In the early 1800s, Struve (Figure 3) was already surveying Livland—the northern parts of what are now the Baltic republics. Concurrently, Lambton (1756–1823) was working in India on the Great Indian Arc of some 22°, which was completed in 1843 by Sir George Everest. Struve's work was to grow into an even longer arc of 25° 20′, which, together with Everest's, formed the two major nineteenth-century contributions to our knowledge of the figure of the Earth. Struve was a university professor (see Batten, 1988), and while he was working in the northern Baltic region, Colonel (later General) Carl Tenner (1783–1859) of the Imperial Russian army was working in Lithuania. Both surveys were primarily local, but each man, independently, had the idea of extending his work. When they became aware of each other's activity, they soon agreed to collaborate. Struve was to work northward, the arc eventually being extended to Hammerfest in Norway, while Tenner was to work southward, eventually reaching Staro-Nekrassowka at the mouth of the Danube River. Tsar Nicholas I agreed to finance the project.

Figure 3: Portrait of F.G.W. Struve painted by C.A. Jensen in 1843, and reproduced from a lithograph in the possession of Anne Lindhagen).

There were two problems about joining the projects, however. First, each man was working on a different meridian—Struve referred to that of Dorpat and Tenner to that of Vilnius. Dorpat was finally selected, and the origin adopted for all measurements, east-west as well as north-south, was a point under the centre of the cupola of the Old Observatory. Since 2002, that point has been marked with a commemorative plaque.

The second problem was one we have noted several times before: the reconciliation of units. The metric system had not yet become so widely adopted as it is now, even for scientific purposes. Struve used the French toise (approx. 1.949 m), because his basic standard had come from France, whereas Tenner used the Russian sajen (approx. 2.134 m), because his basic standard had been built in St Petersburg. In the eighteenth century the sajen had been defined as equal to seven English feet. By careful comparison of their respective standards, Struve and Tenner were able to relate the sajen and the toise precisely, and Struve presented all the final results in the later unit. This was probably the last time in history that the toise was used in such a major project, and that is perhaps a fitting tribute to the lead taken by French scientists in measuring the Earth. Struve also went to some trouble to have exact copies, not only of the French standard unit, but also of the British unit used in the Indian survey. Despite these precautions, there is still some uncertainty about the precise relation used for the toise to the metre: conversion factors ranging from 1.949 m to 1.949087 m to the toise can be found in the literature. Given that the arc extends nearly 3,000 km, even these small differences can become important.

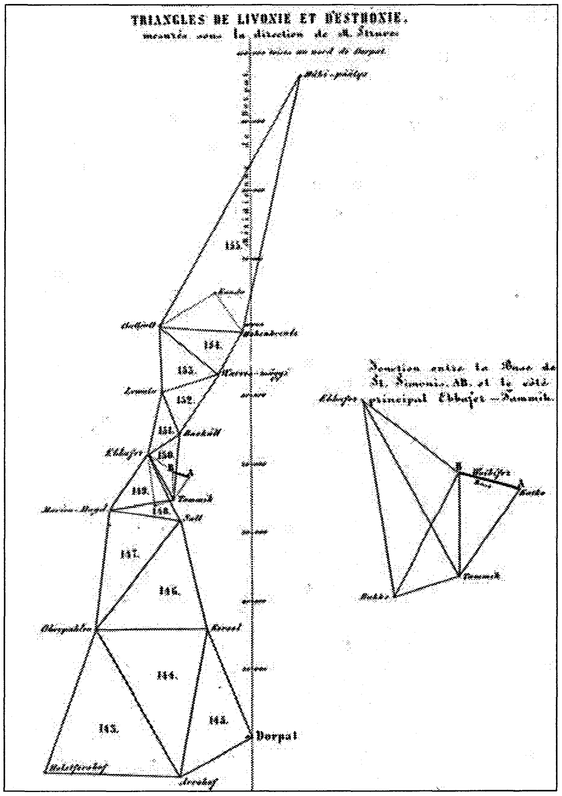

Figure 4: Extract from Struve's original survey diagrams, showing the Estonian section of the Arc (after Struve, 1860).

The survey of this long arc required 258 main triangles, plus some subsidiary ones, and the measurement of ten baselines varying in length from 2.2 km to 11.8 km. Much of the territory covered was within the Russian Empire (see Figure 4), but the extension into Scandinavia required international cooperation. Norwegian and Swedish scientists were responsible for this northward extension. Recent changes in the polity of lands through which the arc passes have resulted in relics of the arc being located in ten countries: Norway, Sweden, Finland, Russia, Estonia, Latvia, Lithuania, Belarus, Moldova and the Ukraine. Achieving recognition as a World Heritage Monument for even a selection of these sites proved to require more complex negotiations and even greater international cooperation than did the survey itself!

Before discussing that aspect, however, let us return to Struve himself. An account of the survey and its results is contained in a major publication comprising two volumes of text and one of plates (Struve, 1860), all of which are now rare books. Struve planned a further volume which would have included a critical evaluation of world-arc measurements, but a serious illness in 1858 incapacitated him for scientific work for the rest of his life; even the second volume was completed and seen through the press by his son and successor, Otto Struve (1819–1905), who was also a fully competent astronomer *and* surveyor. In 1844, Otto Struve had determined the longitude difference between the major Russian observatory at Pulkovo and Greenwich. In those days before telegraphy linked the continents, the only way of doing this was to transport large numbers of chronometers between the two points. The Struves were much assisted in this matter by their close friend, G.B. Airy (1801–1892), the Astronomer Royal. The elder Struve also hoped to measure a parallel of latitude from the west coast of Ireland to the Ural mountains, which would have been another major contribution to the study of the size and figure of the Earth. He was actively involved in negotiating this, again with Airy's help, when the first signs of his illness forced him to return home. The plans were never executed.

F.G.W. Struve would also have liked to extend the arc southward, across Anatolia to Crete. During his lifetime, relations between the Russian and Ottoman empires were too strained to permit this, but after his death, Otto Struve planned such an extension and began preparations for it. Even these plans, however, appear not to have been carried out, perhaps because of renewed political strains. Nevertheless, an arc of over 25° had been measured in sufficient detail to show conclusively that the Earth is oblate.

Wilhelm Struve's dream was, however, eventually to be realized. In the 1920s another arc measurement was proposed by the IAG, and the project was undertaken in the 1930s. The new arc passed through what was then Poland (and is now Belarus) and the Ukraine, where it had several points in common with the Struve arc, and ran through Roumania and what was then Yugoslavia (now Serbia and Montenegro and the FYROM) and Greece to Crete.

In the meantime, there had been renewed activity in Africa. Sir David Gill (1843–1914), Her Majesty's Astronomer at the Cape (Figure 5), planned and executed a completely new arc measurement in the Cape Province. We know from surviving correspondence between Gill and Otto Struve, who were close friends, that Gill was directly inspired by the example of the elder Struve (Batten, 1988: 191-192). Gill also had a dream: that his arc should eventually be linked with Struve's, and this, too, was to be realized. His own measurements were gradually and intermittently extended northward by others, reaching North Africa in 1954, close to the site of Eratosthenes' original efforts to measure the Earth. North Africa had been linked to Crete in the previous year by Hiran, a form of radar triangulation. From North Cape to Cape Province is an arc of 105° latitude. Elements of both the Struve arc and the African arc were used by Chovitz and Fischer (1956) in a new determination of the figure of the Earth.

7 THE WORLD HERITAGE PROPOSAL

Among World Heritage sites and monuments, the Struve arc is unique in two respects. It is the first, and at present the only, site commemorated for its scientific importance. Secondly, whereas previous sites have been either specific buildings or large areas of either natural beauty or ecological importance, the Struve-arc sites are mostly very small (sometimes only a mark on a rock), scattered over a north-south distance of nearly 3,000 km. Among the buildings involved are the Tartu Observatory (Figure 6), and a church in Tornio (modern Torneå) whose spire was one of the triangulation points. Now there are also obelisks at each terminus of the arc. Most of the remaining points are very inconspicuous. For the representation to UNESCO, each country was asked to select two or three sites within its boundaries (except for Russia, where the only two sites are on the island of Gögland in the Gulf of Finland). Several practical considerations led to this restriction: not all the sites are identified, or perhaps even identifiable; others are not accessible, and the costs of maintaining all may be beyond a country's resources.

The idea of seeking World Heritage designation was first proposed at a conference in Tartu in 1993 (the bicentenary of the elder Struve's birth) by the Finnish surveyor, Aarne Veriö. The conference adopted a resolution urging the countries that still had identifiable relics of the arc to take steps to approach UNESCO. The following year, both the FIG meeting in Melbourne and the IAU meeting in The Hague endorsed the original resolution, and support was also received from the IAG. A constituent permanent body within the FIG (the International Institution for the History of Surveying and Measurement) undertook the arduous task of approaching governments and preparing documentation. Progress was slow, partly because of the number of countries involved—many of which were newly independent—and partly because of language barriers. Preparation of the final submission to UNESCO was undertaken by the office of the National Land Survey of Finland (UNESCO, 2004). Finally, UNESCO met in Durban (South Africa) in July 2005, and it granted World Heritage status to the selected sites associated with the arc. Perhaps in due course, this status may be extended to sites connected with the African arc, and it is significant that plaques have already been unveiled at each end of that arc, one at Buffelsfontein, near Port Elizabeth, and the other near Cairo.

Tsar Nicholas I of Russia and King Oscar I of Norway and Sweden had, of course, very practical motives for supporting such a large-scale survey. Similarly, in the age of intercontinental ballistic missiles, modern governments and their military, regrettably, needed to know exactly the relative positions of the continents. Tsar and King could not possibly have foreseen the Global Positioning System and the many uses, peaceful as well as military, to which it has been put. Yet the arcs of Struve, Everest, Gill and the intrepid French savants were essential steps towards that achievement.

We know from contemporary accounts that Struve was fully aware of the practical value of the arc measurement, and that he considered the time and effort spent on it as part of what he owed to his country in return for the privilege of engaging in research in what we would call 'pure science' (Smyth, 1862: 183-184). Struve's friend, Airy, who, as we have seen, was also involved in geodetic work, went further when he wrote of his own visit to Pulkovo (Airy, 1848), suggesting that involvement in practical applications was necessary to keep astronomers on the right lines:

> ... the Observatory of Pulkowa [*sic*] is connected with the Astronomical Survey of the Russian empire, and here also is a certain analogy with the Observatory of Greenwich, which is connected in various ways with the Nautical Astronomy of Great Britain. It is, in my opinion, exceedingly important, as well for preventing astronomers from wasting their time in the mere fanciful abstractions of science, as for giving to the Observatory its proper place in public estimation, that it should be in part devoted to some distinctly useful purpose of this kind.

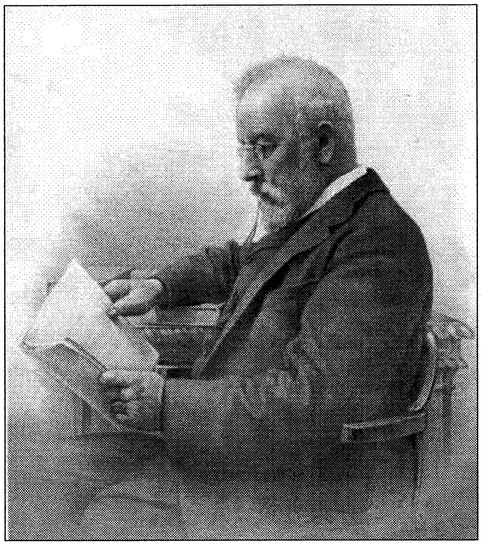

Figure 5: Sir David Gill (courtesy RAS Archives).

Airy was writing only a decade after the first distances to the fixed stars had been determined (by Struve and others), and when astronomy still consisted mainly of positional astronomy and celestial mechanics. Speculation about the age of the Earth and Sun, and the source of the latter's energy, such as Kelvin (1824–1907) was then making in his attempts to show that Hutton (1726–1797) and Lyell (1797–1875) were wrong in supposing the Earth to have had an indefinitely long past, probably did seem to Airy "... mere fanciful abstractions of science."

8 SURVEYING THE SKIES

For all that, Airy and Struve knew well that there were good scientific reasons, as well as practical ones, for measuring the size and figure of the Earth. As just mentioned, Struve himself was a pioneer in the measurement of stellar distances, measurement that depended on the same technique of triangulation that he used in the arc survey. The baseline for stellar triangulation is the diameter of the Earth's orbit and is determined by observations of the distance of the Sun. That distance, in turn, depends on knowledge of the size and figure of the Earth. The first realistic measurement of the distance of the Sun (only about 8 per cent too small) was made by the first Cassini in 1674, when he compared his own observations of Mars made from Paris with those of obtained by Richer from Cayenne (see Hughes, 2001). Even with modern radar measurements of interplanetary distances, we need to know the position of the observer with respect to the centre of the Earth.

We have called this paper "Measuring the World" rather than "Measuring the Earth" because "World" has the twofold meaning of our globe and the Universe, and measuring the first is an essential step to measuring the second. That connection was clear to Aristarchus of Samos who, in the third century B.C., showed how to use lunar eclipses to measure the distance of the Moon in terms of the Earth's radius, and devised a method for determining the relative distances of Sun and Moon. While his method was correct in principle, in practice it was extremely sensitive to observational error. As is well known, he arrived at a serious underestimate of the distance of the Sun.

Figure 6: Tartu (Dorpat) Observatory in 1860, from a lithograph by Louis Höflinger now in the possession of Anne Lindhagen.

Nowadays, we measure not only the distances of the stars, but also of galaxies. Of course, any form of triangulation is impossible for objects distant millions of light-years from the Earth; indirect measurements must be used, and only within the last decade have the observational uncertainties been reduced to reasonable size. Nevertheless, those indirect methods depend on the trigonometric parallaxes determined for the nearest stars, which depend in their turn on our knowledge of the distance of the Sun, which, again in its turn, depends on the great arc surveys of the last three centuries. Astronomers and surveyors are indeed scientific cousins. Airy wanted astronomers to keep their feet on the ground, to prevent themselves from getting lost in the skies. If he were still with us, he probably would regard modern cosmology as "… mere fanciful abstractions of science …", but he would appreciate the irony that only by being firmly anchored to the Earth are we able to scale the Heavens!

9 NOTES

1. One Chinese foot of 10 inches was about 0.25 m and one *li* about 0.5 km, although there were noticeable variations to these figures.
2. 12 lignes equals 1 Paris inch, which was between five and six per cent longer than the English inch.
3. Despite his own Spanish-sounding name, Arago was French, from the Basque country. The mailing address of the Paris Observatory is still "Boulevard Arago".)

10 ACKNOWLEDGEMENTS

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12 FURTHER READING

Smith (1986) contains 269 references pertinent to the period up to the mid-eighteenth century.

13 APPENDIX 1

The first part of Table 1 gives, for completeness only, a selection of the early values for a 1° section of a

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meridian, although here, in particular, varying units used at the different times and the uncertainty of conversions make any direct comparisons unreliable.

The second section illustrates how a selection of the values derived since the advent of triangulation shows a noticeable change with latitude, decreasing—as they should—as one goes towards the Equator. Omitting the doubtful values, such as those by Brahe, Snellius and Riccioli, there is, over the 71° north, a total change equivalent to about 19 m per degree (where 1° is around 111, 000 m, or a ratio of about 1 in 5,800). Whilst that would be readily possible today, it illustrates the small amount that observers like the Cassinis were trying to resolve with equipment and techniques that were perhaps not quite sufficiently developed at that time to be up to the task.

Table 1: Lengths for 1°, in Order of Latitude.

Date	Observer	Location	Mid-latitude	Length of 1° (m)
1525	Fernel	France	49° 20′ N	110 690
724 AD	I Hsing	China	35 N	157 520
820 AD	Al Mamun	Iraq	35 N	111 000
c100 BC	Posidonius	Mediterranean	33 50 N	122 000
c140AD	Ptolemy	Mediterranean	33 50 N	91 500
230 BC	Eratosthenes	Egypt	27 40 N	128 500

1855	Struve	Norway	70 40 N	111 589
1737	Maupertuis	Lapland	66 20 N	111 950
1855	Struve	Russia	60 04 N	111 431
1580	Brahe	Hven	56 N	112 840
1855	Struve	Lithuania	54 39 N	111 292
1633	Norwood	England	52 49 N	111 920
1615	Snellius	Holland	52 N	107 400
1718	Cassini II	France	49 56 N	111 010
1668	Picard	France	49 10 N	111 210
1681-01	Cassini I & II	France	49 10 N	111 280
1738	Maupertuis	France	49 10 N	110 950
1734-42	Cassini II & III	France	46 30 N	111 210
1799	Delambre & Mechain	France	46 12 N	111 131
1769	Leisganig	Hungary	45 55 N	110 863
1855	Struve	Ukraine	45 20 N	111 230
1740	Cassini III & LaCaille	France	45 15 N	111 240
1645	Riccioli & Grimaldi	Italy	44 34 N	119 800
1751	Boscovich	Italy	43 N	111 027
1766	Mason & Dixon	America	39 12 N	110 670
1847	Everest II	India	26 49 N	110 837
1847	Everest II	India	23 47 N	110 759
1830	Everest I	India	22 37 N	110 904
1830	Everest I	India	19 35 N	110 721
1800-21	Lambton	India	16 35 N	110 664
1800-21	Lambton	India	13 07 N	110 629
1830	Everest I	India	13 06 N	110 634
1800-21	Lambton	India	09 35 N	110 601
1743	La Condamine	Peru	01 31 S	110 655
1752	La Caille	S Africa	33 08 S	111 165

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NOTES ON TRANSLATIONS OF THE EAST ASIAN RECORDS RELATING TO THE SUPERNOVA OF AD 1054

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Abstract: The East Asian records of the 'guest star' of 1054 that produced the Crab Nebula have been re-evaluated more than once during the past decade. Although some of the apparent inconsistencies in the records have now been addressed, doubts about the reported position of the supernova still persist. The published translations of the records, moreover, are still unsatisfactory in certain respects. Here I offer corrections to the translations of several records, present a previously-unreported contemporaneous account of the guest star, and suggest an explanation of why the Song Dynasty astronomers erroneously placed SN1054 southeast of ζ Tau.

Keywords: Crab Nebula, supernova, SN1054, East Asia, China, Song Dynasty, history of astronomy

1 BACKGROUND

In the book *Historical Supernovae and their Remnants,* Stephenson and Green (2002) provide a thoroughgoing and authoritative study of the East Asian records that report the appearance of a 'guest star' in AD 1054. A subsequent paper by Stephenson (2004) adds the results of further study of the technical terminology. A previous re-evaluation of the East Asian and Western reports by Breen and McCarthy (1995) made progress toward dispelling misconceptions and apparent contradictions between the dates of the Japanese and Chinese records. Stephenson and Green's painstaking analysis has now advanced this project further toward a definitive account. When I was translating the relevant passages some time ago, my curiosity was also piqued by the apparent discrepancies among the surviving accounts, which I thought must have been a consequence of misreading or misdating of the records. Stephenson and Green have now corrected some of these problems, but notably a copyist's error in the date (*yichou* for *jichou*) in *Xu zizhi tongjian changbian* and Duyvendak's (1942a) erroneous Julian date for the guest star's disappearance in 1056. Given the evident contradictions among the records, it is surprising that in the sixty years since Duyvendak's initial publication no one actually went back to the original sources to re-examine their context until Stephenson and Green (2002) did just this—and to such good effect.

In what follows I propose to comment on the translations of the records. In the process I add a newly-discovered contemporary reference to the guest star from a highly reliable source, and add some thoughts on a lingering problem—the reported location of the guest star southeast of ζ Tau in the "Treatise on Astronomy" in the *Song shi*, which is inconsistent with the actual position of SN1054 northwest of that star.

2 TRANSLATIONS

For the most part, inaccuracies in the previous translations of some of the records do not materially affect the astronomical import of the texts. Since these records are apt to be frequently quoted in the future, however, perhaps it would be useful to have more precise translations on record, especially for historians of science who might find the astrological implications worthy of study. The best translations to date are those offered by Stephenson and Green (2002; 2004), so I will take these as the basis for my comments. Let me begin with their translation of the record from the *Song huiyao* (2002: 120; modified 2004: 96).

2.1 *Song huiyao (Composition of Essential Documents of the Song Dynasty)*

> Zhihe reign period, first year, seventh lunar month, 22nd day [= 27 April 1054] ... Yang Weide said, 'I humbly observe that a guest star has appeared; above the star in question there is a faint glow, yellow in colour. If one carefully examines the divinations concerning the emperor (i.e. Renzong), the interpretation is as follows: The fact that the guest star does not invade (fan) Bi and its brightness is full means that there is a person of great worth ...'

Yang Weide's language follows the normal linguistic conventions when submitting an opinion to the Throne. That being the case, it is inconceivable that he would have addressed Emperor Renzong directly using the term *huangdi* (皇, 'Emperor'). The required form of indirect address would be *bixia* (陛下). The string that contains the word *huangdi* (*Huangdi zhangwo zhan*) reads very much like the title of a book or compendium of divinations, of which there are many examples on record ending with *zhan* (or 'prognostication'). The title of the work might therefore be rendered *Prognostications in Respect of the Emperor*, from which Yang then quotes, signaled by *yun* (云). Since Yang's prognostication does, indeed, refer to the person of the Emperor, he certainty would have cited precedent, rather than 'going out on a limb' by proffering an unsupported personal opinion. To my knowledge, no book entitled *Huangdi zhangwo zhan* has survived, but this is hardly surprising as it would certainly have been closely held at Court and, consequently, lost in the conflagration when the Dynasty fell. Therefore, I suggest that Stephenson and Green's, "If one carefully examines the prognostications concerning the Emperor (i.e. Renzong), the interpretation is as follows ...", should be emended to: "I respectfully submit that the *Prognostications in Respect of the Emperor* says ..." Similarly, the text that immediately follows, which Stephenson and Green (2002: 120) render as "The fact that the guest star does not invade (fan) Bi and its brightness is full means that there is a person of great worth ..." should actually be "... if the guest star does not trespass on BI [LM #17], an Abundantly Enlightened One is ruler, and the State has Great Worthies [in office]."

In Stephenson and Green's translation the same *Song huiyao* record concludes:

> First year of the Jiayou reign period, third lunar month, the Director of the Astronomical Bureau said: 'The guest star has vanished, which is a portent of the departure of the guest'. Earlier, during the first year of the Zhihe reign period, fifth lunar month, it appeared at daybreak at the eastern direction, guarding (shou) Tianguan. It was seen in the daytime, like Venus. It had pointed rays in all directions and its colour was pale red. In total it was seen (in daylight) for 23 days.

The first of the astrological pronouncements in *Song huiyao* is attributed to Yang Weide by name. Yang was the most experienced senior official in charge of the Directorate of Astronomy and the Calendar at the time. That prognostication dates from the seventh month (27 August 1054), by which time the guest star was no longer seen in daylight, although it had been for nearly half the time since it first appeared on 4 July, seven weeks earlier. One can only speculate about the reasons for Yang's delaying until late August to report his astrological interpretation, although the stylistically very similar report of SN1006 a half-century earlier was also delayed by a month, and as Stephenson and Green suggest (2002: 152), "... evidently there was much deliberation before a report and prognostication could be released." The most likely explanation is that the responsible officials prudently awaited further developments before committing themselves to an interpretation. No doubt they would have wanted to confirm whether the object was a guest star or a comet (i.e. whether it moved), whether it would soon disappear, and so on, before offering an interpretation.

The second astrological pronouncement in the text followed the disappearance of the anomaly some twenty-one months later and is attributed to the Director of Astronomy (*sitianjian*), which was Yang Weide's official title. Its purpose was to bring satisfactory closure to the event. Yang, who was the senior official authorized to make such reports to the Court, quotes from the original observation the interesting facts that the guest star first appeared at dawn in the east and was visible in daylight for twenty-three days. Why this daylight appearance is mentioned here and not elsewhere is unclear, but it is likely that it had to do with the fact that the star had initially appeared reddish-white (presumably due to its low altitude), before it assumed a yellowish cast. The latter coloration is almost certainly due to the yellowing of the summer sky in North China by the fine loess dust blown in from the northwest by seasonal winds, a phenomenon observable to this day.[1] Yellow being the Imperial color, the star's apparent shifting coloration may have been an important astrological consideration. It is perhaps also worth noting here that the subsequent change of reign title, from *Zhihe* ('Attained Harmony') to *Jiayou* ('Auspicious Aid'), from the ninth month of 1056, may well have been intended to commemorate the appearance (and disappearance) of the supposedly 'auspicious' astral omen (but see below).

2.2 *Qidan guo zhi (History of the Qidan Kingdom)*

The report in the *Qidan guo zhi* is translated by Stephenson and Green (2002: 124) as follows:

> Year yiwei [32]; Chongxi reign period of (King Xingzong), 23rd year, eighth lunar month, the ruler of the kingdom died, having reigned for 25 years; Song dynasty, Zhihe reign period, second year ... Previously there had been a solar eclipse at midday (zhengyang), and a guest star had appeared at Mao. The Deputy Officer in the Bureau of Historiography, Liu Yishou [sic] said: 'These are omens that Xingzong will die'. The prediction indeed came true.

The style of this record parallels that of the *Song huiyao* passage above in combining brief reference to the astral anomalies with astrological prognostication. Here again, this represents a summing up of the significance of, in this case, what are taken to be inauspicious astral omens. Stephenson (2004: 97) corrects the discrepancy in the Song date, showing that "... Zhihe reign period, second year..." is an obvious mistake for 'first year' (although his discussion mistakenly states "... the 23rd year of the Zhihe reign period was A.D. 1055-1056 ...", when he meant to say "... the 23rd year of the Chongxi reign period was A.D. 1055-1056 ..."). Liu Yisou's title, *zhuzuo zuolang*, is better rendered, "[Palace Library] Assistant Editorial Director" (Hucker: 1978). Liu Yisou's remark, *Xingzong qi si hu* (興宗其死乎, or "might Xingzong die"), is a hypothetical and is best rendered that way. Stephenson attributes only the first comment to Liu; however, it is equally plausible that the last two sentences are also Liu's, recalling his earlier prediction after the fact, especially since the King is being referred to by his posthumous title, *Xingzong*. Therefore, an alternative translation would be: "[Palace Library] Assistant Editorial Director, Liu Yisou said, '[were they signs that] Xingzong might die? Now what was anticipated has come to pass'."

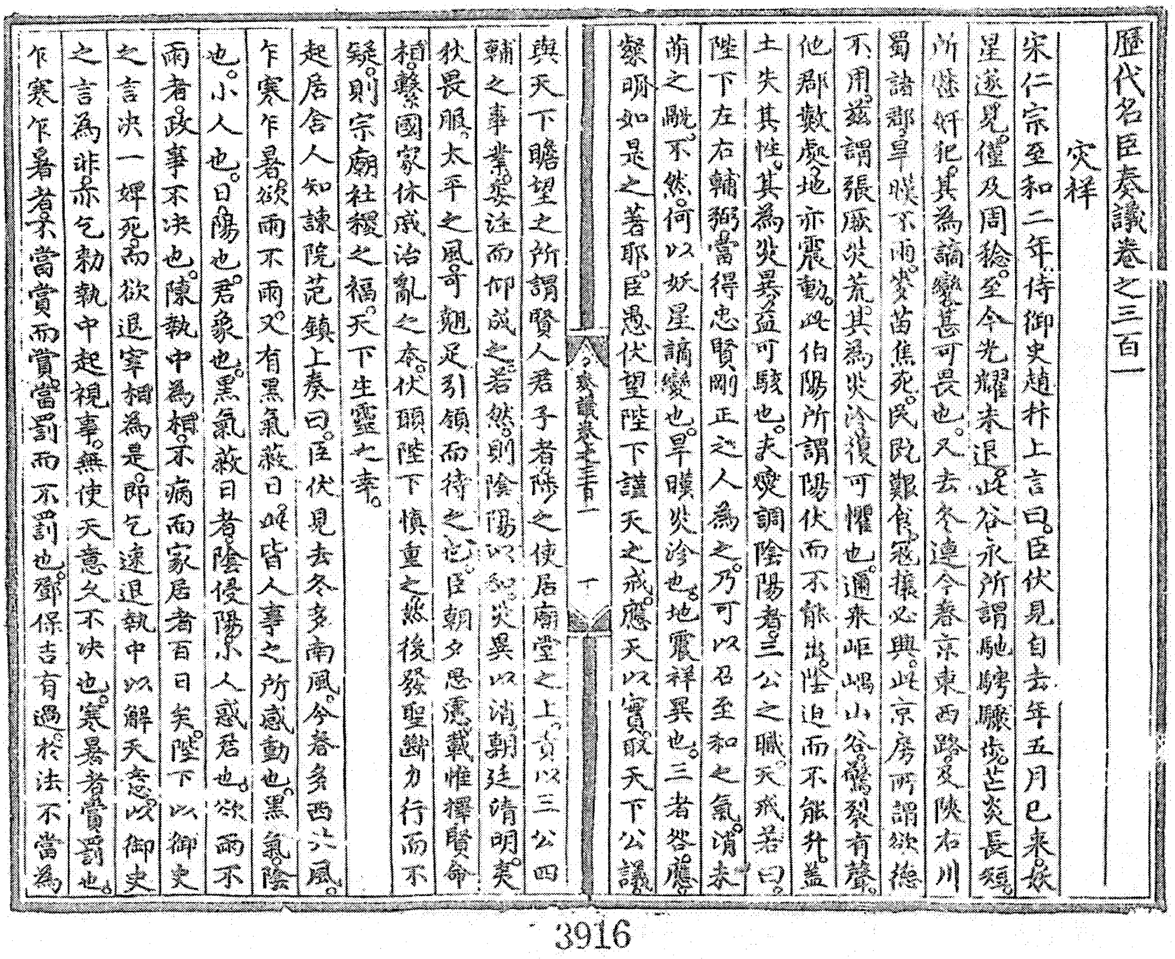
The timing of the eclipse is said to be *rishi zhengyang* (日食正陽). Duyvendak (1942a: 177) originally misread the text and punctuated between *rishi* (eclipse) and *zhengyang*, leading him to mistranslate *zhengyang* as 'first month' and attach it to the following "... a guest star appeared." Breen and McCarthy (1995: 367) follow Duyvendak. Ho et al. (1970: 4) follow Duyvendak in punctuating after *rishi*, but offer 'at midday' for *zhengyang* instead. Stephenson and Green (2002: 124) follow Ho in translating *zhengyang* 'at midday' and put forward the novel suggestion that the reference is to the partial solar eclipse (maximum 0.53 at 14.2h) on 13 November 1053. This seems only a modestly better fit than the eclipse of 10 May 1054 (maximum of 0.39 at 16.9h), since the 1053 eclipse maximum was over an hour past the noon double-hour of 11:00-13:00, which hardly qualifies as 'midday'. While the translation of *zhengyang* as 'at midday' might be defensible if either eclipse had occurred at noon, in fact, there is another explanation. It should be remembered that the chief significance of this passage is astrological; therefore, it is not surprising that the terminology should depart from the typical observational record. Had the author been an astronomer, he would doubtless have been more precise about the location of the guest star, as well as the hour of the eclipse. As it happens, however, in certain contexts the term *zhengyang* has a specialized astrological significance of '4th month', which fits the context perfectly here.[2] Because this is precisely when the dominance of the *yang* force is culminating, the portentological interpretation stresses that a solar eclipse in this month is particularly ominous (*mutatis mutandis* in the tenth month). This explains why the astrological term *zhengyang* appears here in conjunction with a solar eclipse, rather than the more standard '4th month'.[3] Therefore, what we have in this record is not a competing date for the guest star, but a correct dating of the 10 May 1054 solar eclipse to the fourth month.

2.3 *Lidai mingchen zouyi* (*Memorials by Famous Officials Through History*)

The *Lidai mingchen zouyi* (see Figure 1) dates to AD 1414, and includes the following account:

> 2nd year of the Zhihe reign period of Emperor Renzong of Song [1055]; Attendant Censor Zhao Bian submitted a letter saying: "Your servant considers that, since the 5th month of last year [when] the baleful star appeared, a full year has passed and until now its brilliance has not faded [lit. 'retreated']. This is what Gu Yong meant by 'its rapid movement, the variations in the length of its flaming rays, and the [asterisms] on which it has trespassed successively,' as a censorious anomaly it is greatly to be feared." (Ch. 301: 3916b).

This is a passage that has not been placed in evidence previously. It is excerpted from a memorial to the Throne written by Palace Censor Zhao Bian (赵卞) (1008–1084), a high-ranking official in the Censorate, which independently investigated malfeasance among Palace personnel and court officials (see Hucker, 1985: 431). Zhao is writing in connection with an impeachment case involving the Grand Chancellor, in late July or August of 1055 (see Wu, 1990: 240).[4] Immediately following Zhao Bian's memorial is another by a lower-ranking official, Fan Zhen. In it Fan also mentions the guest star's period of visibility as "… one year up to the present." Based on an internal reference to the autumnal equinox on "… the 23rd day of the present month …" (16 September 1055), Fan's memorial may be dated to the eighth month of 1055. It is worth noting that both Zhao's and Fan's memorials interpret the significance of the astral omen quite differently from the Director of Astronomy, Yang Weide, who put a positive spin on the anomaly. Unlike Yang, who was clearly out to flatter the Emperor, it was Zhao Bian's official duty to investigate wrongdoing and to remonstrate with the Emperor concerning the Government's shortcomings. In quoting the Former Han Dynasty official, Gu Yong (谷永), Zhao is alluding to the dismal portents associated with the ominous appearance of Halley's Comet in 12 BC (cf. Cullen, 1991: 117). He is not asserting that the guest star of 1054 has moved, but is drawing an analogy with the dismal implications of the precedent of 12 BC. Zhao has truncated Gu's statement, leaving it to the informed reader to draw the appropriate implications. Gu Yong's original comment is worth quoting in full:



歷代名臣奏議卷之三百一

災祥

宋仁宗至和二年侍御史趙抃上言曰臣伏見自去年五月已來妖星遂見僅及周歲至今光耀未退此谷永所謂馳騁驟步芒炎長短所歷奸犯其為譎變甚可畏也又去冬逮今春京東西路及陝右川蜀諸郡旱暵不雨麥苗焦死民既艱食寇攘必興此京房所謂欲德不用茲謂張厥災荒其為災沴復可懼也邇來岷嶓山谷驚裂有聲他郡數處地亦震動此伯陽所謂陽伏而不能出陰迫而不能升蓋土失其性其為災異益可駭也夫燮調陰陽者三公之職天戒若曰陛下左右輔弼當得忠賢剛正之人為之乃可以召至和之氣消未萌之亂不然何以妖星譎變也旱暵災沴也地震祥異也三者咎證粲明如是之著耶臣愚伏望陛下謹天之戒應天以實取天下公議

奏議卷三百一 十

與天下瞻望之所謂賢人君子者俾之使居廟堂之上責以三公四輔之事業委注而仰成之若然則陰陽以和災異以消朝廷清明矣狀長服太平之風可翹足引領而待之也臣朝夕思慮載惟擇賢命相繫國家休戚治亂之本伏願陛下慎重之然後發聖斷力行而不疑則宗廟社稷之福天下生靈之幸

起居舍人知諫院范鎮上奏曰臣伏見去冬多南風今春多西北風乍寒乍暑欲雨不雨又有黑氣蔽日此皆人事之所感動也黑氣陰也小人也日陽也君象也黑氣蔽日者陰侵陽小人惑君也欲雨不雨者政事不決也陳執中為相不病而家居者百日矣陛下以御史之言決一婢死而欲退宰相為是耶乞速退執中以解天意以御史之言為非耶乞勅執中起視事無使天意久不決也寒暑者賞罰也乍寒乍暑者不當賞而賞當罰而不罰也鄧保吉有過於法不當為

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Figure 1: Zhao Bian's memorial to Emperor Renzong from *Lidai mingchen zouyi*. The shaded passages refer to the guest star.

> 谷永对曰："上古以来，大乱之极，所希有也. 察其驰骋骤步，芒炎或长或短，所历奸犯，内为后宫女妾之害，外为诸夏叛逆之祸
>
> Gu Yong responded: "This is an omen of extreme disorder such as has rarely been seen since high antiquity. If we examine its rapid movement, the variations in the length of its flaming rays, and the [asterisms] on which it has trespassed successively, [it clearly signifies] harm to the women of the rear palace within, and the disaster of rebellion in the realm without." (after Cullen, 1991: 117, but with modifications).

In fact, the Former Han Dynasty fell barely twenty years after this ominous portent in 12 BC. In the present case, among the recent calamities referred to in Zhao's and Fan's memorials are severe drought, widespread banditry, and earthquakes, all indicative of an imbalance of *yin* and *yang*, which reflected badly on the Imperial administration. Even allowing for a certain amount of rhetorical exaggeration, specifically the implication that the guest star had not faded for a full year (unless, of course, he simply meant it had not disappeared), Zhao Bian's memorial provides independent confirmation from an impeccable source that the supernova was first observed in the fifth month of 1054.

2.4 *Meigetsuki* (*Diary of the Bright Moon*)

The *Meigetsuki*, by Fujiwara no Teika [Sadaie] (1162–1241), states:

> Tenki reign period of Emperor Go-Reizei, second year, fourth lunar month, after the middle ten-day period (zhongxun yihou). At the (double) hour of chou (1-3 am), a guest star appeared in the degrees of (the lunar lodges) Zui (xi) and Shen. It was seen in the eastern direction and emerged [bo] at the star Tianguan. It was as large as Jupiter.

Stephenson is translating the record found in the poet Fujiwara's diary, written ca. 1230, nearly a century and a half after the event, although it draws on some unknown eyewitness testimony. Like all Japanese records of astronomical observations from this period, it is in Classical Chinese. Breen and McCarthy (1995: 372) suggest the possibility of a common origin with the *Song huiyao* account above, which Stephenson and Green (2002: 126) discount on the grounds of linguistic variation. In fact, the *Song huiyao* records were kept secret within the Palace, suffered great losses when the Song Dynasty fell in 1279, and were only partially preserved in the form of Li Xinchuan's (1166–1243) *Xu zonglei huiyao* which was later copied into the *Yongle dadian* encyclopedia in the Ming Dynasty (Hervouet, 1978: 177-178). Since Li and Fujiwara were exact contemporaries, it is most unlikely that Fujiwara or his predecessors in Japan who were responsible for the record could have had access to the *huiyao*. The *Meigetsuki* account is an independent record of the guest star.

Most researchers now agree that the '4th month' date in *Meigetsuki* must be a mistake for '5th month', not least because in the humid climate of Kyoto in summer ζ Tau's heliacal setting ought to have occurred not later than 8-9 May. The fourth month did not begin until 10 May, and the report states that the guest star was not observed until after the twentieth day of the month, or 29 May, at which time ζ Tau was still in conjunction with the Sun. Some who have discussed this passage in the past have also been perplexed by the use of *bo* ('fuzzy/bristling/tail-less comet'), and have questioned whether the final clause refers to the original guest star or to the appearance of a new comet. Ho Peng Yoke et al. (1972: 5) express doubt, but Duyvendak (1942a; 1942b) does not, and Breen and McCarthy (1995: 372) ultimately also draw the correct conclusion, as do Stephenson and Green (2002: 126) and Stephenson (2004: 98): this last clause is not reporting a separate phenomenon. In fact, the verbal use of *bo* here, which actually means 'fuzzy' or 'bushy', is entirely regular and consistent with cometary records where it is used to describe the appearance of tail-less comets, or the changed aspect of a comet after having lost its tail. Classical Chinese grammar requires that the common term for this circumstance, *xing bo* ('star' + 'fuzzy'), be understood, not as a noun modifying a following noun, but as a subject followed by a verb (literally 'a star fuzzied'), which may suggests a change from a pre-existing condition. This ancient usage derives from a conception in which stars were thought to be capable of spontaneously becoming fuzzy, growing a tail, and even moving about at will. It is therefore inappropriate to translate *xing bo* as 'bushy star' by analogy with *hui xing* ('broom' + 'star'), which is quite properly translated 'broom star'. Thus, when one reads in the record of the comet that appeared in the sixth month of AD 396, *you xing hui yu maotou* (有星彗于髦头) (*Wei shu: tianxiang zhi*, Chapter 10), while this is conceivably an accidental inversion of *hui xing* ('broom star'), one cannot rule out the rendering "… there was a star that became broom-like in MAOTOU [i.e. lunar mansion 19, MAO]." In the passage from *Meigetsuki, bo* is intended to suggest that when first spotted the guest star resembled the head of a comet. Only on continued observation would it become clear that the stationary object was not a comet. Moreover, where there is no explicit change of subject, the grammar and syntax of Classical Chinese also require following verbs to refer to the nearest antecedent subject. Stephenson and Green, sensing the need for a verb in the clause, resort to rendering *bo* here as 'emerged', but this is inappropriate as it substitutes a different meaning instead of verbalizing the existing word whose sense in such contexts is well known. Therefore, I suggest the following is more appropriate: "… it appeared in the east, fuzzy [like a tail-less comet] at the star Tianguan, and as large as Jupiter."

2.5 *Song shi: Tianwen Zhi (Treatise on Astrology)*

Stevenson and Green (2002: 123) translate Chapter 56 in the *Song shi* as follows:

> Zhihe reign period, first year, fifth lunar month, (day) jichou [26]. [4 July 1054] (A guest star) appeared (chu) to the southeast of Tianguan possibly several inches away (ke shu cun). After a year and more it gradually vanished (shaomo).

2.6 Synthesis

Once the problematical passages are accounted for, as Stephenson and Green demonstrate, the collected records from a variety of East Asian sources on the whole provide a detailed and consistent account of this exceptionally long-lasting and luminous object. Consistent, that is, with one exception. A remaining non-trivial problem is the reported position of the guest star southeast of *Tianguan*, or ζ Tau, which is at odds with the actual position of the Crab Nebula about 1.1° northwest of that star.

Now, Ho et al. (1972: 9) quite rightly point out that "… the eleventh century in China happened to be a period renowned for its astronomical instruments and accurate observations." Stephenson and Green (2002: 131) and Stephenson (2004) provide detailed analysis of contemporaneous records of planets trespassing on *Tianguan*, which confirms the observational precision of which medieval Chinese astronomers were capable. Stephenson's (2004: 100) investigation of angular separations of recorded planetary conjunctions establishes the equivalence 1 *cun* ≈ 0.1°. Faced with the problem of reconciling the Song astronomers' observational acumen and the reported location of the guest star with the actual location of the Crab, Breen and McCarthy (1995: 370) propose that the use of the term *shou* ('guard') in several records carries the implication that the guest star was positioned above the *Tianguan* star, and they conclude that the records placing the guest star southeast of ζ Tau must have resulted from an inadvertent substitution of 'southeast' for 'northwest'. Stephenson (2004: 101) re-examines records of *shou* ('guarding') and concurs with Breen and McCarthy that, "… when a planet or other celestial body was said to 'guard' a star it nearly always lay a little towards the north of the star …" and that a reversal of relative directions must have occurred. However, directionality does not figure in any of the several definitions of *shou* cited by Ho (1966: 36). Nor are there any Chinese terms corresponding to *shou* for the circumstance when a planet or other object 'guards' a star to the south, east, or west. Therefore, the suggestion that *shou* implies a specific direction relative to the object being 'guarded' requires both a convincing explanation of the term's usefulness as well as further instances before it can be accepted. An inadvertent confusion of 'southeast' (東南) with 'northwest' (西北), whether by copyist's error or mental lapse, while not inconceivable, seems an unsatisfying explanation.

3 DISCUSSION

In drawing attention to the comparatively precise measurements of which the Song astronomers were capable, Ho et al. (1970), Stephenson and Green (2002: 132) and Stephenson (2004: 100) offer a clue that may point in a fruitful direction. Given that Song Dynasty astronomers routinely achieved an observational accuracy of ±0.5°, what could explain the vagueness of the positional information found in the *Xu zizhi tongjian changbian* and *Song shi* records which place the guest star "… about a few inches southeast of *Tianguan*"?[5] How can such a seemingly crude report be reconciled with the excellent reputation of the Directorate of Astronomy? If we can satisfactorily answer this question, perhaps it will be possible to arrive at a different explanation for the contradiction between the reported position of the guest star and the actual location of SN1054, besides the conjectured substitution of 'southeast' for 'northwest'.

Let me briefly suggest a scenario that might account for the discrepancy. We know from the guest star's subsequent visibility in daylight that its maximum apparent visual magnitude would have equaled or exceeded –3.7, and that it appeared to emit rays in all directions. The imprecision of the recorded linear distance from ζ Tau, at a time when the astronomers were capable of measuring the location of fixed stars to within a few tenths of a degree, certainly seems to indicate some impediment to direct observation. Recalling that the guest star's luminosity at this time may have approximated that of Venus,[6] and that it is reported to have been seen to 'sparkle' or scintillate, as a practical matter it may not have been possible even to see ζ Tau (m_v = +2.96) with the guest star only 1° away.

Under the best of circumstances, from the Song capital at Kaifeng (34.47° N, 114.20° E) with good seeing (k = 0.20) and a limiting visual magnitude of +6.0, ζ Tau's heliacal rising could ideally have been observed on 2 July, with the star at an altitude of 6° and the Sun at –14°. As has already been mentioned, however, the atmosphere in North China in late spring and summer is often loaded with loess dust, particularly in a year of severe drought such as 1054. Under such conditions, observation of ζ Tau's heliacal rising could well have been delayed by quite a few days. In addition, in the pre-dawn hours on 4 July, ζ Tau's angular separation from the waning Moon (21% illuminated and at magnitude –7) was only 21°, probably resulting in a limiting visual magnitude of about +3.0 ± 20%. Taken together, these considerations—SN1054 as bright as Venus and only 1° away, poor seeing, the Moon's proximity—suggest that it is highly unlikely ζ Tau could have been directly observed on 4 July. Indeed, Collins et al. (1999: 875) show that "… it would have been more than a week after July 4 before ζ Tau became visible in the dawn sky."

4 CONCLUSION

Given the above circumstances, perhaps an alternative explanation may be conjectured. First, given the likelihood that ζ Tau was invisible on 4 July, it is probably safe to conclude that the report of the guest star's position relative to that star is not the result of direct measurement at all. Since the guest star was observed only in daylight for the next three weeks, it may be that the position of SN1054 was measured as soon as practicable using an armillary sphere, perhaps based on the location of a known body some distance away (such as β Tau, or some other nearby reference star). Although routine, such indirect procedures would have been prone to larger error than direct measurement. Error could also arise from the previously-measured position of the reference star chosen, or depending on the method employed, as a result of clock error in timing meridian transits. In either case—and certainly the latter—an error of the order of less than two degrees in the presumed position of the guest star would hardly be surprising, and is comparable with positional measurements of the guest star of 1006 (see Stephenson and Green, 2002: 171). Whatever the case may be, it is difficult to reconcile the vagueness of the language employed in the 1054 record with the supposition that this is the result of actual measurement of the separation of SN1054 from ζ Tau on 4 July.

One must also keep in mind the astrological imperative motivating the observations. This was not dispassionate scientific inquiry. Rather, it would immediately have been a matter of great urgency to determine the nature of the occurrence and its approximate position, so as to report the phenomenon and its implications to the Court, at least preliminarily. Subsequently, it would have been essential to watch for any movement of the guest star in order to provide a definitive astrological interpretation. Given the major controversy at the Court a half-century earlier provoked by the appearance of the guest star of 1006, it is likely that the astronomers would have been working under intense pressure. Therefore, the reported position "... about a few inches southeast of *Tianguan* ..." may simply represent the best approximation possible under the circumstances, which the astronomers clearly signaled by the imprecision of their language—*ke shu cun* ("... about a few inches ..."). Positional reports concerning stationary objects amenable to precise measurement otherwise lack that telltale *ke* ('about') and *shu* ('a few'). Once the approximate position of the guest star was established accurately enough for astrological purposes, bureaucratic inertia or a lack of scientific curiosity meant that its precise position was never reported—even when it was later possible to measure it—and the official interpretation of the phenomenon was settled at the time of Yang Weide's report to the Emperor on 27 August 1054. However, as we have seen from Zhao Bian's remarks, when the brilliant guest star continued to linger after a full year, the initial auspicious interpretation of the phenomenon was very much open to question in the minds of some high-ranking officials.

5 NOTES

1. Chinese reports of the supernova of 1006, which was observed on 1 May that year, stress the yellowish coloration of that guest star as well. Simultaneous Japanese observations (Stephenson and Green, 2002: 161) report the color as bluish-white, which shows clearly that the Chinese were accurately reporting local atmospheric effects in North China.
2. For this definition of *zhengyang*, see *Zhongwen da cidian* (*The Encyclopedic Dictionary of the Chinese Language*), Volume 5, page 7575, sub 16611.324, glosses 3 & 4.
3. In Stephenson's (2004: 97) re-examination of this record he alludes to this possible alternative rendering, but without further comment.
4. Wu Yiyi (1990: 241) provides a date of 20 February 1055 for this memorial, but there is no precise date given in the text and, in any case, Zhao Bian clearly states that a full year had elapsed since the first appearance of the guest star. Wu also misinterprets (and mistranscribes) *zhouren* ('a round year') in the passage as the name of an asterism.
5. Stephenson and Green (2002) and Stephenson (2004) consistently translate *ke* in this context as 'possibly'. They and others appear to have been influenced by the modern binome, *keneng*, which does mean 'possibly'. In Classical Chinese, however, depending on the context, *ke* by itself means 'can/may', 'permit' or 'approximately', but not 'possibly'.
6. The *Song shi* (Chapter 12), reports a near simultaneous daytime observation of Venus (apparent visual magnitude –4.1) on 7 July 1054, three days after the discovery of the guest star, so the *Song hui yao* report comparing the daylight appearance of SN1054 with Venus could well imply that the two were comparable in brightness.

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NOTES ON TRANSLATIONS OF THE EAST ASIAN RECORDS RELATING TO THE SUPERNOVA OF AD 1054

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Abstract: The East Asian records of the 'guest star' of 1054 that produced the Crab Nebula have been re-evaluated more than once during the past decade. Although some of the apparent inconsistencies in the records have now been addressed, doubts about the reported position of the supernova still persist. The published translations of the records, moreover, are still unsatisfactory in certain respects. Here I offer corrections to the translations of several records, present a previously-unreported contemporaneous account of the guest star, and suggest an explanation of why the Song Dynasty astronomers erroneously placed SN1054 southeast of ζ Tau.

Keywords: Crab Nebula, supernova, SN1054, East Asia, China, Song Dynasty, history of astronomy

1 BACKGROUND

In the book *Historical Supernovae and their Remnants*, Stephenson and Green (2002) provide a thoroughgoing and authoritative study of the East Asian records that report the appearance of a 'guest star' in AD 1054. A subsequent paper by Stephenson (2004) adds the results of further study of the technical terminology. A previous re-evaluation of the East Asian and Western reports by Breen and McCarthy (1995) made progress toward dispelling misconceptions and apparent contradictions between the dates of the Japanese and Chinese records. Stephenson and Green's painstaking analysis has now advanced this project further toward a definitive account. When I was translating the relevant passages some time ago, my curiosity was also piqued by the apparent discrepancies among the surviving accounts, which I thought must have been a consequence of misreading or misdating of the records. Stephenson and Green have now corrected some of these problems, but notably a copyist's error in the date (*yichou* for *jichou*) in *Xu zizhi tongjian changbian* and Duyvendak's (1942a) erroneous Julian date for the guest star's disappearance in 1056. Given the evident contradictions among the records, it is surprising that in the sixty years since Duyvendak's initial publication no one actually went back to the original sources to re-examine their context until Stephenson and Green (2002) did just this—and to such good effect.

In what follows I propose to comment on the translations of the records. In the process I add a newly-discovered contemporary reference to the guest star from a highly reliable source, and add some thoughts on a lingering problem—the reported location of the guest star southeast of ζ Tau in the "Treatise on Astronomy" in the *Song shi*, which is inconsistent with the actual position of SN1054 northwest of that star.

2 TRANSLATIONS

For the most part, inaccuracies in the previous translations of some of the records do not materially affect the astronomical import of the texts. Since these records are apt to be frequently quoted in the future, however, perhaps it would be useful to have more precise translations on record, especially for historians of science who might find the astrological implications worthy of study. The best translations to date are those offered by Stephenson and Green (2002; 2004), so I will take these as the basis for my comments. Let me begin with their translation of the record from the *Song huiyao* (2002: 120; modified 2004: 96).

2.1 *Song huiyao (Composition of Essential Documents of the Song Dynasty)*

> Zhihe reign period, first year, seventh lunar month, 22nd day [= 27 April 1054] ... Yang Weide said, 'I humbly observe that a guest star has appeared; above the star in question there is a faint glow, yellow in colour. If one carefully examines the divinations concerning the emperor (i.e. Renzong), the interpretation is as follows: The fact that the guest star does not invade (fan) Bi and its brightness is full means that there is a person of great worth ...'

Yang Weide's language follows the normal linguistic conventions when submitting an opinion to the Throne. That being the case, it is inconceivable that he would have addressed Emperor Renzong directly using the term *huangdi* (皇, 'Emperor'). The required form of indirect address would be *bixia* (陛下). The string that contains the word *huangdi* (*Huangdi zhangwo zhan*) reads very much like the title of a book or compendium of divinations, of which there are many examples on record ending with *zhan* (or 'prognostication'). The title of the work might therefore be rendered *Prognostications in Respect of the Emperor*, from which Yang then quotes, signaled by *yun* (云). Since Yang's prognostication does, indeed, refer to the person of the Emperor, he certainty would have cited precedent, rather than 'going out on a limb' by proffering an unsupported personal opinion. To my knowledge, no book entitled *Huangdi zhangwo zhan* has survived, but this is hardly surprising as it would certainly have been closely held at Court and, consequently, lost in the conflagration when the Dynasty fell. Therefore, I suggest that Stephenson and Green's, "If one carefully examines the prognostications concerning the Emperor (i.e. Renzong), the interpretation is as follows ...", should be emended to: "I respectfully submit that the *Prognostications in Respect of the Emperor* says …" Similarly, the text that immediately follows, which Stephenson and Green (2002: 120) render as "The fact that the guest star does not invade (fan) Bi and its brightness is full means that there is a person of great worth …" should actually be "… if the guest star does not trespass on BI [LM #17], an Abundantly Enlightened One is ruler, and the State has Great Worthies [in office]."

In Stephenson and Green's translation the same *Song huiyao* record concludes:

> First year of the Jiayou reign period, third lunar month, the Director of the Astronomical Bureau said: 'The guest star has vanished, which is a portent of the departure of the guest'. Earlier, during the first year of the Zhihe reign period, fifth lunar month, it appeared at daybreak at the eastern direction, guarding (shou) Tianguan. It was seen in the daytime, like Venus. It had pointed rays in all directions and its colour was pale red. In total it was seen (in daylight) for 23 days.

The first of the astrological pronouncements in *Song huiyao* is attributed to Yang Weide by name. Yang was the most experienced senior official in charge of the Directorate of Astronomy and the Calendar at the time. That prognostication dates from the seventh month (27 August 1054), by which time the guest star was no longer seen in daylight, although it had been for nearly half the time since it first appeared on 4 July, seven weeks earlier. One can only speculate about the reasons for Yang's delaying until late August to report his astrological interpretation, although the stylistically very similar report of SN1006 a half-century earlier was also delayed by a month, and as Stephenson and Green suggest (2002: 152), "... evidently there was much deliberation before a report and prognostication could be released." The most likely explanation is that the responsible officials prudently awaited further developments before committing themselves to an interpretation. No doubt they would have wanted to confirm whether the object was a guest star or a comet (i.e. whether it moved), whether it would soon disappear, and so on, before offering an interpretation.

The second astrological pronouncement in the text followed the disappearance of the anomaly some twenty-one months later and is attributed to the Director of Astronomy (*sitianjian*), which was Yang Weide's official title. Its purpose was to bring satisfactory closure to the event. Yang, who was the senior official authorized to make such reports to the Court, quotes from the original observation the interesting facts that the guest star first appeared at dawn in the east and was visible in daylight for twenty-three days. Why this daylight appearance is mentioned here and not elsewhere is unclear, but it is likely that it had to do with the fact that the star had initially appeared reddish-white (presumably due to its low altitude), before it assumed a yellowish cast. The latter coloration is almost certainly due to the yellowing of the summer sky in North China by the fine loess dust blown in from the northwest by seasonal winds, a phenomenon observable to this day.[1] Yellow being the Imperial color, the star's apparent shifting coloration may have been an important astrological consideration. It is perhaps also worth noting here that the subsequent change of reign title, from *Zhihe* ('Attained Harmony') to *Jiayou* ('Auspicious Aid'), from the ninth month of 1056, may well have been intended to commemorate the appearance (and disappearance) of the supposedly 'auspicious' astral omen (but see below).

2.2 *Qidan guo zhi (History of the Qidan Kingdom)*

The report in the *Qidan guo zhi* is translated by Stephenson and Green (2002: 124) as follows:

> Year yiwei [32]; Chongxi reign period of (King Xingzong), 23rd year, eighth lunar month, the ruler of the kingdom died, having reigned for 25 years; Song dynasty, Zhihe reign period, second year ... Previously there had been a solar eclipse at midday (zhengyang), and a guest star had appeared at Mao. The Deputy Officer in the Bureau of Historiography, Liu Yishou [sic] said: 'These are omens that Xingzong will die'. The prediction indeed came true.

The style of this record parallels that of the *Song huiyao* passage above in combining brief reference to the astral anomalies with astrological prognostication. Here again, this represents a summing up of the significance of, in this case, what are taken to be inauspicious astral omens. Stephenson (2004: 97) corrects the discrepancy in the Song date, showing that "... Zhihe reign period, second year..." is an obvious mistake for 'first year' (although his discussion mistakenly states "... the 23rd year of the Zhihe reign period was A.D. 1055-1056 ...", when he meant to say "... the 23rd year of the Chongxi reign period was A.D. 1055-1056 ..."). Liu Yisou's title, *zhuzuo zuolang*, is better rendered, "[Palace Library] Assistant Editorial Director" (Hucker: 1978). Liu Yisou's remark, *Xingzong qi si hu* (興宗其死乎, or "might Xingzong die"), is a hypothetical and is best rendered that way. Stephenson attributes only the first comment to Liu; however, it is equally plausible that the last two sentences are also Liu's, recalling his earlier prediction after the fact, especially since the King is being referred to by his posthumous title, *Xingzong*. Therefore, an alternative translation would be: "[Palace Library] Assistant Editorial Director, Liu Yisou said, '[were they signs that] Xingzong might die? Now what was anticipated has come to pass'."

The timing of the eclipse is said to be *rishi zhengyang* (日食正陽). Duyvendak (1942a: 177) originally misread the text and punctuated between *rishi* (eclipse) and *zhengyang*, leading him to mistranslate *zhengyang* as 'first month' and attach it to the following "... a guest star appeared." Breen and McCarthy (1995: 367) follow Duyvendak. Ho et al. (1970: 4) follow Duyvendak in punctuating after *rishi*, but offer 'at midday' for *zhengyang* instead. Stephenson and Green (2002: 124) follow Ho in translating *zhengyang* 'at midday' and put forward the novel suggestion that the reference is to the partial solar eclipse (maximum 0.53 at 14.2h) on 13 November 1053. This seems only a modestly better fit than the eclipse of 10 May 1054 (maximum of 0.39 at 16.9h), since the 1053 eclipse maximum was over an hour past the noon double-hour of 11:00-13:00, which hardly qualifies as 'midday'. While the translation of *zhengyang* as 'at midday' might be defensible if either eclipse had occurred at noon, in fact, there is another explanation. It should be remembered that the chief significance of this passage is astrological; therefore, it is not surprising that the terminology should depart from the typical observational record. Had the author been an astronomer, he would doubtless have been more precise about the location of the guest star, as well as the hour of the eclipse. As it happens, however, in certain contexts the term *zhengyang* has a specialized astrological significance of '4th month', which fits the context perfectly here.[2] Because this is precisely when the dominance of the *yang* force is culminating, the portentological interpretation stresses that a solar eclipse in this month is particularly ominous (*mutatis mutandis* in the tenth month). This explains why the astrological term *zhengyang* appears here in conjunction with a solar eclipse, rather than the more standard '4th month'.[3] Therefore, what we have in this record is not a competing date for the guest star, but a correct dating of the 10 May 1054 solar eclipse to the fourth month.

2.3 *Lidai mingchen zouyi* (*Memorials by Famous Officials Through History*)

The *Lidai mingchen zouyi* (see Figure 1) dates to AD 1414, and includes the following account:

> 2nd year of the Zhihe reign period of Emperor Renzong of Song [1055]; Attendant Censor Zhao Bian submitted a letter saying: "Your servant considers that, since the 5th month of last year [when] the baleful star appeared, a full year has passed and until now its brilliance has not faded [lit. 'retreated']. This is what Gu Yong meant by 'its rapid movement, the variations in the length of its flaming rays, and the [asterisms] on which it has trespassed successively,' as a censorious anomaly it is greatly to be feared." (Ch. 301: 3916b).

This is a passage that has not been placed in evidence previously. It is excerpted from a memorial to the Throne written by Palace Censor Zhao Bian (赵卞) (1008–1084), a high-ranking official in the Censorate, which independently investigated malfeasance among Palace personnel and court officials (see Hucker, 1985: 431). Zhao is writing in connection with an impeachment case involving the Grand Chancellor, in late July or August of 1055 (see Wu, 1990: 240).[4] Immediately following Zhao Bian's memorial is another by a lower-ranking official, Fan Zhen. In it Fan also mentions the guest star's period of visibility as "... one year up to the present." Based on an internal reference to the autumnal equinox on "... the 23rd day of the present month ..." (16 September 1055), Fan's memorial may be dated to the eighth month of 1055. It is worth noting that both Zhao's and Fan's memorials interpret the significance of the astral omen quite differently from the Director of Astronomy, Yang Weide, who put a positive spin on the anomaly. Unlike Yang, who was clearly out to flatter the Emperor, it was Zhao Bian's official duty to investigate wrongdoing and to remonstrate with the Emperor concerning the Government's shortcomings. In quoting the Former Han Dynasty official, Gu Yong (谷永), Zhao is alluding to the dismal portents associated with the ominous appearance of Halley's Comet in 12 BC (cf. Cullen, 1991: 117). He is not asserting that the guest star of 1054 has moved, but is drawing an analogy with the dismal implications of the precedent of 12 BC. Zhao has truncated Gu's statement, leaving it to the informed reader to draw the appropriate implications. Gu Yong's original comment is worth quoting in full:

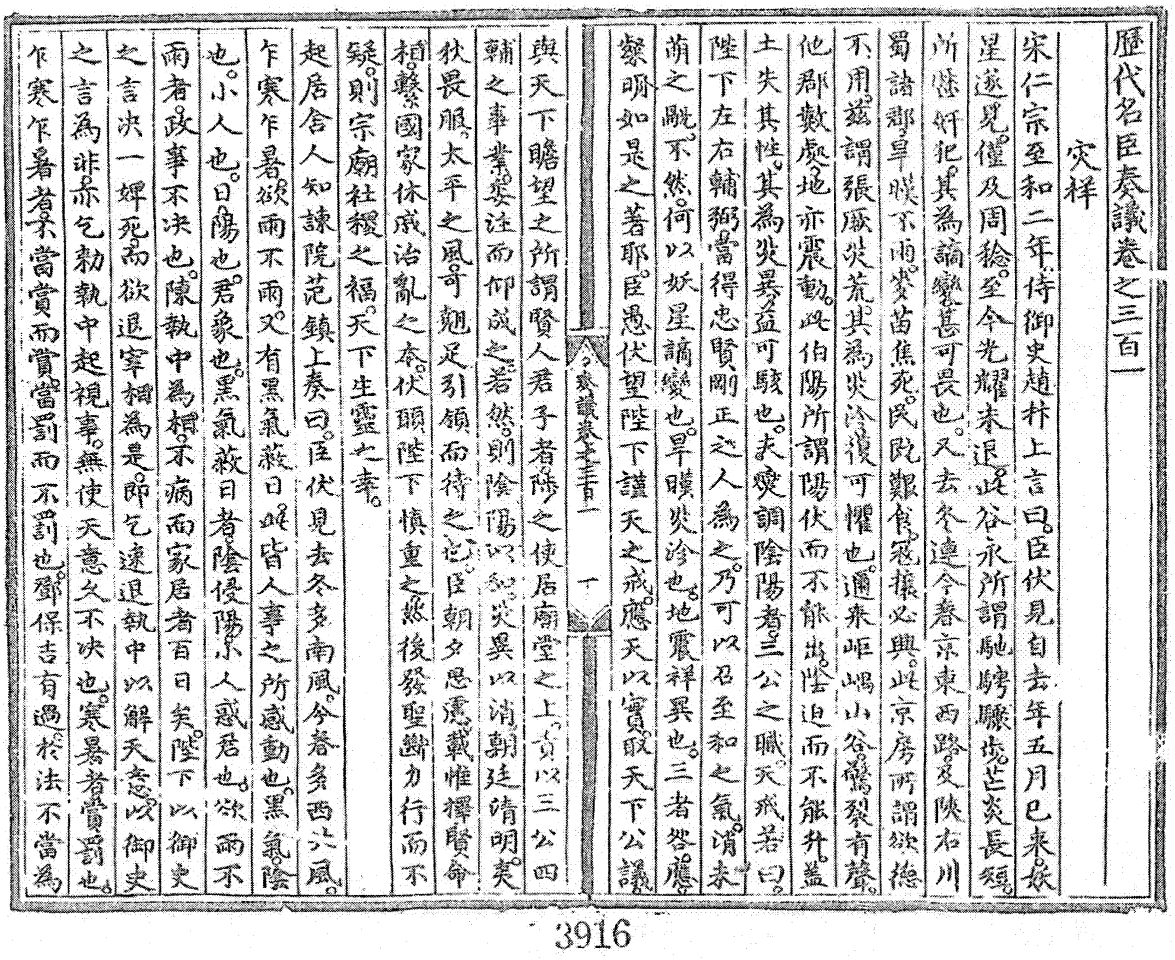
歷代名臣奏議卷之三百一
災祥
宋仁宗至和二年侍御史趙抃上言曰。臣伏見自去年五月已來。妖
星遂見。僅及周歲。至今光耀未退。此谷永所謂馳騁驟步。芒炎長縮。
所歷奸犯。其為譴變。甚可畏也。又去冬連今春京東西路及陝右川
蜀諸郡旱暵不雨。麥苗焦死。民既艱食。寇攘必興。此京房所謂欲德
不用。茲謂張厥災荒。其爲災冷。復可懼也。邇來岠嵎山谷。驚裂有聲。
他郡數處地亦震動。此伯陽所謂陽伏而不能出。陰迫而不能升。蓋
土失其性。其爲災異。益可駭也。夫燮調陰陽者。三公之職。天戒若曰。
陛下左右輔弼。當得忠賢剛正之人爲之。乃可以召至和之氣。消弭
萌之戒。不然。何以妖星譴變也。旱暵災冷也。地震祥異也。三者咎譴
彰明如是之著耶。臣愚伏望陛下謹天之戒。應天以實。取天下公議
奏議卷三百一　十
與天下瞻望之所謂賢人君子者。俾之使居廟堂之上。責以三公四
輔之事業。委任而仰成之。若然。則陰陽以和。災異以消。朝廷清明矣。
狄長服。太平之風。可翹足引領而待之也。臣朝夕思慮。載惟擇賢命
相。繫國家休戚治亂之本。伏願陛下慎重之。然後發聖斷力行而不
疑。則宗廟社稷之福。天下生靈之幸。
起居舍人知諫院范鎮上奏曰。臣伏見去冬多南風。今春多西北風。
乍寒乍暑。欲雨不雨。又有黑氣蔽日。此皆人事之所感動也。黑氣陰
也。小人也。日陽也。君象也。黑氣蔽日者。陰侵陽。小人惑君也。欲雨不
雨者。政事不決也。陳執中為相。不病而家居者百日矣。陛下以御史
之言決一婢死而欲退宰相為是耶。乞速退執中以解天意。以御史
之言為非耶。乞勅執中起視事。無使天意久不決也。寒暑者賞罰也。
乍寒乍暑者。不當賞而賞。當罰而不罰也。鄧保吉有過於法不當為
3916

Figure 1: Zhao Bian's memorial to Emperor Renzong from *Lidai mingchen zouyi*. The shaded passages refer to the guest star.

> 谷永对曰："上古以来，大乱之极，所希有也. 察其驰骋骤步，芒炎或长或短，所历奸犯，内为后宫女妾之害，外为诸夏叛逆之祸
>
> Gu Yong responded: "This is an omen of extreme disorder such as has rarely been seen since high antiquity. If we examine its rapid movement, the variations in the length of its flaming rays, and the [asterisms] on which it has trespassed successively, [it clearly signifies] harm to the women of the rear palace within, and the disaster of rebellion in the realm without." (after Cullen, 1991: 117, but with modifications).

In fact, the Former Han Dynasty fell barely twenty years after this ominous portent in 12 BC. In the present case, among the recent calamities referred to in Zhao's and Fan's memorials are severe drought, widespread banditry, and earthquakes, all indicative of an imbalance of *yin* and *yang*, which reflected badly on the Imperial administration. Even allowing for a certain amount of rhetorical exaggeration, specifically the implication that the guest star had not faded for a full year (unless, of course, he simply meant it had not disappeared), Zhao Bian's memorial provides independent confirmation from an impeccable source that the supernova was first observed in the fifth month of 1054.

2.4 *Meigetsuki* (*Diary of the Bright Moon*)

The *Meigetsuki*, by Fujiwara no Teika [Sadaie] (1162–1241), states:

> Tenki reign period of Emperor Go-Reizei, second year, fourth lunar month, after the middle ten-day period (zhongxun yihou). At the (double) hour of chou (1-3 am), a guest star appeared in the degrees of (the lunar lodges) Zui (xi) and Shen. It was seen in the eastern direction and emerged [bo] at the star Tianguan. It was as large as Jupiter.

Stephenson is translating the record found in the poet Fujiwara's diary, written ca. 1230, nearly a century and a half after the event, although it draws on some unknown eyewitness testimony. Like all Japanese records of astronomical observations from this period, it is in Classical Chinese. Breen and McCarthy (1995: 372) suggest the possibility of a common origin with the *Song huiyao* account above, which Stephenson and Green (2002: 126) discount on the grounds of linguistic variation. In fact, the *Song huiyao* records were kept secret within the Palace, suffered great losses when the Song Dynasty fell in 1279, and were only partially preserved in the form of Li Xinchuan's (1166–1243) *Xu zonglei huiyao* which was later copied into the *Yongle dadian* encyclopedia in the Ming Dynasty (Hervouet, 1978: 177-178). Since Li and Fujiwara were exact contemporaries, it is most unlikely that Fujiwara or his predecessors in Japan who were responsible for the record could have had access to the *huiyao*. The *Meigetsuki* account is an independent record of the guest star.

Most researchers now agree that the '4th month' date in *Meigetsuki* must be a mistake for '5th month', not least because in the humid climate of Kyoto in summer ζ Tau's heliacal setting ought to have occurred not later than 8-9 May. The fourth month did not begin until 10 May, and the report states that the guest star was not observed until after the twentieth day of the month, or 29 May, at which time ζ Tau was still in conjunction with the Sun. Some who have discussed this passage in the past have also been perplexed by the use of *bo* ('fuzzy/bristling/tail-less comet'), and have questioned whether the final clause refers to the original guest star or to the appearance of a new comet. Ho Peng Yoke et al. (1972: 5) express doubt, but Duyvendak (1942a; 1942b) does not, and Breen and McCarthy (1995: 372) ultimately also draw the correct conclusion, as do Stephenson and Green (2002: 126) and Stephenson (2004: 98): this last clause is not reporting a separate phenomenon. In fact, the verbal use of *bo* here, which actually means 'fuzzy' or 'bushy', is entirely regular and consistent with cometary records where it is used to describe the appearance of tail-less comets, or the changed aspect of a comet after having lost its tail. Classical Chinese grammar requires that the common term for this circumstance, *xing bo* ('star' + 'fuzzy'), be understood, not as a noun modifying a following noun, but as a subject followed by a verb (literally 'a star fuzzied'), which may suggests a change from a pre-existing condition. This ancient usage derives from a conception in which stars were thought to be capable of spontaneously becoming fuzzy, growing a tail, and even moving about at will. It is therefore inappropriate to translate *xing bo* as 'bushy star' by analogy with *hui xing* ('broom' + 'star'), which is quite properly translated 'broom star'. Thus, when one reads in the record of the comet that appeared in the sixth month of AD 396, *you xing hui yu maotou* (有星彗于髦头) (*Wei shu: tianxiang zhi*, Chapter 10), while this is conceivably an accidental inversion of *hui xing* ('broom star'), one cannot rule out the rendering "… there was a star that became broom-like in MAOTOU [i.e. lunar mansion 19, MAO]." In the passage from *Meigetsuki, bo* is intended to suggest that when first spotted the guest star resembled the head of a comet. Only on continued observation would it become clear that the stationary object was not a comet. Moreover, where there is no explicit change of subject, the grammar and syntax of Classical Chinese also require following verbs to refer to the nearest antecedent subject. Stephenson and Green, sensing the need for a verb in the clause, resort to rendering *bo* here as 'emerged', but this is inappropriate as it substitutes a different meaning instead of verbalizing the existing word whose sense in such contexts is well known. Therefore, I suggest the following is more appropriate: "… it appeared in the east, fuzzy [like a tail-less comet] at the star Tianguan, and as large as Jupiter."

2.5 *Song shi: Tianwen Zhi (Treatise on Astrology)*

Stevenson and Green (2002: 123) translate Chapter 56 in the *Song shi* as follows:

> Zhihe reign period, first year, fifth lunar month, (day) jichou [26]. [4 July 1054] (A guest star) appeared (chu) to the southeast of Tianguan possibly several inches away (ke shu cun). After a year and more it gradually vanished (shaomo).

2.6 Synthesis

Once the problematical passages are accounted for, as Stephenson and Green demonstrate, the collected records from a variety of East Asian sources on the whole provide a detailed and consistent account of this exceptionally long-lasting and luminous object. Consistent, that is, with one exception. A remaining non-trivial problem is the reported position of the guest star southeast of *Tianguan*, or ζ Tau, which is at odds with the actual position of the Crab Nebula about 1.1° northwest of that star.

Now, Ho et al. (1972: 9) quite rightly point out that "… the eleventh century in China happened to be a period renowned for its astronomical instruments and accurate observations." Stephenson and Green (2002: 131) and Stephenson (2004) provide detailed analysis of contemporaneous records of planets trespassing on *Tianguan*, which confirms the observational precision of which medieval Chinese astronomers were capable. Stephenson's (2004: 100) investigation of angular separations of recorded planetary conjunctions establishes the equivalence 1 *cun* ≈ 0.1°. Faced with the problem of reconciling the Song astronomers' observational acumen and the reported location of the guest star with the actual location of the Crab, Breen and McCarthy (1995: 370) propose that the use of the term *shou* ('guard') in several records carries the implication that the guest star was positioned above the *Tianguan* star, and they conclude that the records placing the guest star southeast of ζ Tau must have resulted from an inadvertent substitution of 'southeast' for 'northwest'. Stephenson (2004: 101) re-examines records of *shou* ('guarding') and concurs with Breen and McCarthy that, "… when a planet or other celestial body was said to 'guard' a star it nearly always lay a little towards the north of the star …" and that a reversal of relative directions must have occurred. However, directionality does not figure in any of the several definitions of *shou* cited by Ho (1966: 36). Nor are there any Chinese terms corresponding to *shou* for the circumstance when a planet or other object 'guards' a star to the south, east, or west. Therefore, the suggestion that *shou* implies a specific direction relative to the object being 'guarded' requires both a convincing explanation of the term's usefulness as well as further instances before it can be accepted. An inadvertent confusion of 'southeast' (東南) with 'northwest' (西北), whether by copyist's error or mental lapse, while not inconceivable, seems an unsatisfying explanation.

3 DISCUSSION

In drawing attention to the comparatively precise measurements of which the Song astronomers were capable, Ho et al. (1970), Stephenson and Green (2002: 132) and Stephenson (2004: 100) offer a clue that may point in a fruitful direction. Given that Song Dynasty astronomers routinely achieved an observational accuracy of ±0.5°, what could explain the vagueness of the positional information found in the *Xu zizhi tongjian changbian* and *Song shi* records which place the guest star "… about a few inches southeast of *Tianguan*"?[5] How can such a seemingly crude report be reconciled with the excellent reputation of the Directorate of Astronomy? If we can satisfactorily answer this question, perhaps it will be possible to arrive at a different explanation for the contradiction between the reported position of the guest star and the actual location of SN1054, besides the conjectured substitution of 'southeast' for 'northwest'.

Let me briefly suggest a scenario that might account for the discrepancy. We know from the guest star's subsequent visibility in daylight that its maximum apparent visual magnitude would have equaled or exceeded –3.7, and that it appeared to emit rays in all directions. The imprecision of the recorded linear distance from ζ Tau, at a time when the astronomers were capable of measuring the location of fixed stars to within a few tenths of a degree, certainly seems to indicate some impediment to direct observation. Recalling that the guest star's luminosity at this time may have approximated that of Venus,[6] and that it is reported to have been seen to 'sparkle' or scintillate, as a practical matter it may not have been possible even to see ζ Tau (m_v = +2.96) with the guest star only 1° away.

Under the best of circumstances, from the Song capital at Kaifeng (34.47° N, 114.20° E) with good seeing (k = 0.20) and a limiting visual magnitude of +6.0, ζ Tau's heliacal rising could ideally have been observed on 2 July, with the star at an altitude of 6° and the Sun at –14°. As has already been mentioned, however, the atmosphere in North China in late spring and summer is often loaded with loess dust, particularly in a year of severe drought such as 1054. Under such conditions, observation of ζ Tau's heliacal rising could well have been delayed by quite a few days. In addition, in the pre-dawn hours on 4 July, ζ Tau's angular separation from the waning Moon (21% illuminated and at magnitude –7) was only 21°, probably resulting in a limiting visual magnitude of about +3.0 ± 20%. Taken together, these considerations—SN1054 as bright as Venus and only 1° away, poor seeing, the Moon's proximity—suggest that it is highly unlikely ζ Tau could have been directly observed on 4 July. Indeed, Collins et al. (1999: 875) show that "… it would have been more than a week after July 4 before ζ Tau became visible in the dawn sky."

4 CONCLUSION

Given the above circumstances, perhaps an alternative explanation may be conjectured. First, given the likelihood that ζ Tau was invisible on 4 July, it is probably safe to conclude that the report of the guest star's position relative to that star is not the result of direct measurement at all. Since the guest star was observed only in daylight for the next three weeks, it may be that the position of SN1054 was measured as soon as practicable using an armillary sphere, perhaps based on the location of a known body some distance away (such as β Tau, or some other nearby reference star). Although routine, such indirect procedures would have been prone to larger error than direct measurement. Error could also arise from the previously-measured position of the reference star chosen, or depending on the method employed, as a result of clock error in timing meridian transits. In either case—and certainly the latter—an error of the order of less than two degrees in the presumed position of the guest star would hardly be surprising, and is comparable with positional measurements of the guest star of 1006 (see Stephenson and Green, 2002: 171). Whatever the case may be, it is difficult to reconcile the vagueness of the language employed in the 1054 record with the supposition that this is the result of actual measurement of the separation of SN1054 from ζ Tau on 4 July.

One must also keep in mind the astrological imperative motivating the observations. This was not dispassionate scientific inquiry. Rather, it would immediately have been a matter of great urgency to determine the nature of the occurrence and its approximate position, so as to report the phenomenon and its implications to the Court, at least preliminarily. Subsequently, it would have been essential to watch for any movement of the guest star in order to provide a definitive astrological interpretation. Given the major controversy at the Court a half-century earlier provoked by the appearance of the guest star of 1006, it is likely that the astronomers would have been working under intense pressure. Therefore, the reported position "... about a few inches southeast of *Tianguan* ..." may simply represent the best approximation possible under the circumstances, which the astronomers clearly signaled by the imprecision of their language—*ke shu cun* ("... about a few inches ..."). Positional reports concerning stationary objects amenable to precise measurement otherwise lack that telltale *ke* ('about') and *shu* ('a few'). Once the approximate position of the guest star was established accurately enough for astrological purposes, bureaucratic inertia or a lack of scientific curiosity meant that its precise position was never reported—even when it was later possible to measure it—and the official interpretation of the phenomenon was settled at the time of Yang Weide's report to the Emperor on 27 August 1054. However, as we have seen from Zhao Bian's remarks, when the brilliant guest star continued to linger after a full year, the initial auspicious interpretation of the phenomenon was very much open to question in the minds of some high-ranking officials.

5 NOTES

1. Chinese reports of the supernova of 1006, which was observed on 1 May that year, stress the yellowish coloration of that guest star as well. Simultaneous Japanese observations (Stephenson and Green, 2002: 161) report the color as bluish-white, which shows clearly that the Chinese were accurately reporting local atmospheric effects in North China.

2. For this definition of *zhengyang*, see *Zhongwen da cidian* (*The Encyclopedic Dictionary of the Chinese Language*), Volume 5, page 7575, sub 16611.324, glosses 3 & 4.

3. In Stephenson's (2004: 97) re-examination of this record he alludes to this possible alternative rendering, but without further comment.

4. Wu Yiyi (1990: 241) provides a date of 20 February 1055 for this memorial, but there is no precise date given in the text and, in any case, Zhao Bian clearly states that a full year had elapsed since the first appearance of the guest star. Wu also misinterprets (and mistranscribes) *zhouren* ('a round year') in the passage as the name of an asterism.

5. Stephenson and Green (2002) and Stephenson (2004) consistently translate *ke* in this context as 'possibly'. They and others appear to have been influenced by the modern binome, *keneng*, which does mean 'possibly'. In Classical Chinese, however, depending on the context, *ke* by itself means 'can/may', 'permit' or 'approximately', but not 'possibly'.

6. The *Song shi* (Chapter 12), reports a near simultaneous daytime observation of Venus (apparent visual magnitude –4.1) on 7 July 1054, three days after the discovery of the guest star, so the *Song hui yao* report comparing the daylight appearance of SN1054 with Venus could well imply that the two were comparable in brightness.

6 ACKNOWLEDGEMENTS

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SIR DAVID BREWSTER'S CHANGING IDEAS ON THE PLURALITY OF WORLDS

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Abstract: In the course of his long life the Scottish physicist David Brewster wrote copiously about the plurality of worlds. *More Worlds than One* (1854), perhaps his strongest statement on the question, was written as an answer to William Whewell's *On the Plurality of Worlds* (1853), which argued that life was a privilege of the Earth. Brewster's ideas changed drastically along the years in many crucial issues such as the habitability of the Sun and the Moon, the possibility that extraterrestrials could be different from humans, and the occupation of the Earth by intelligent races in the distant past. This paper succinctly surveys Brewster's main lines of thought about the plurality of worlds underlining the significance of his first two articles devoted exclusively to this topic. They were published in 1838 in *The Monthly Chronicle*, and affirm the habitability of the planets while denying that of the Moon. As is the case with many Victorian scientists, belief in pluralism was for Brewster part and parcel of a complex of ideas and attitudes in which it is hard to distinguish science from religion. I shall argue that a fair number of the shifting opinions and inconsistencies detectable in Brewster's ideas on the plurality of worlds can be attributed to the fact that these were used as pliable apologetic instruments in his scientific writings, many of which are permeated by strong religious concerns.

Key words: David Brewster, William Whewell, plurality of worlds, extraterrestrial life

1 INTRODUCTION

The Scottish physicist Sir David Brewster (Figure 1) enjoyed during his lifetime a scientific reputation far larger than the slim fame bestowed on him by posterity. If he is known at all to present-day science students, it is on account of the law of polarisation by reflection named after him ('Brewster's Law'). Indeed, all his major achievements belong to optics. On the theoretical side, his studies on the colours of the spectrum and his distrust of the hypothetical ether motivated his staunch defence of the emission-particulate theory of light, at a time when the wave theory was growing undisputed. Practical issues such as photography also occupied his attention and he was the inventor of the kaleidoscope as well as of other optical instruments—his *Treatise Upon New Philosophical Instruments* (1813) bears witness of this interest.

David Brewster (1781–1868) was born in Jedburgh, a town in the Scottish lowlands. He was educated at Edinburgh, where he obtained an honorary degree in divinity and, although he was not ordained as a minister, he was licensed to preach in the Church of Scotland; he was a pious evangelist and a close friend of the Reverend Thomas Chalmers, the leader of the Free Church. He can be seen in this connection as a distinguished representative of the circle of Scottish evangelical scientists bent on the popularisation of scientific knowledge, people like John Fleming, Thomas Dick and Hugh Miller.

Brewster cut a major figure in the Scottish scientific milieu of the first half of the nineteenth century and many honours were bestowed upon him: he was one of only eight Foreign Associates elected to the *Institut de France*; was knighted in 1832 by William IV; and was awarded the Copley, Rumford and Royal Medals of the Royal Society of London, the Keith Prize of the Royal Society of Edinburgh and honorary degrees from the Universities of Aberdeen and Oxford.

Unable to obtain a teaching position until late in his life, Brewster earned his living chiefly as an editor of encyclopaedias (notably the *Edinburgh Encyclopaedia*), and scientific journals. He took part in the creation of several scientific societies, among them the British Association for the Advancement of Science (1831). His hectic activity as editor and author of books and articles of scientific popularisation explains why the bibliography of his writings lists 1,241 titles (Morrison-Low, 1984). During the last decades of his life, Brewster acted as a principal at St. Andrews University and later at Edinburgh University, being elected Vice-Chancellor of the latter institution in 1859.

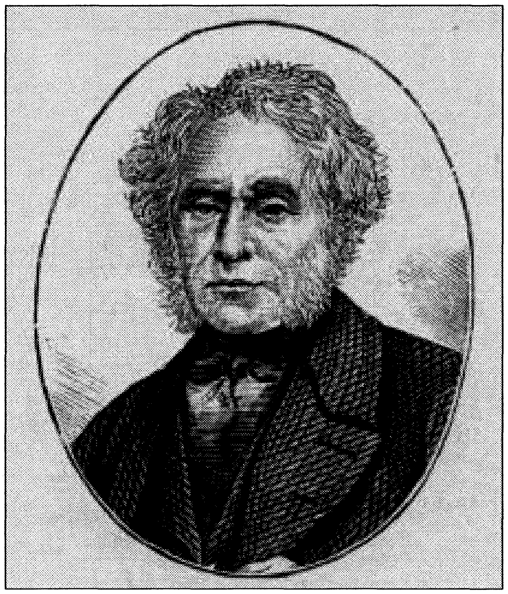

Figure 1: Sir David Brewster, 1781–1868 (after Keeling, 1897).

Brewster's ideas on the plurality of worlds have already been explored by John H. Brooke (1977) and Michael J. Crowe (1988: 300-305), who showed how many of the opinions expounded in *More Worlds than*

One (1854) can be seen as a reaction against the publication of Whewell's *Of the Plurality of Worlds. An Essay* (1853), which denied the existence of extraterrestrial life. This paper charts the shifting character of Brewster's pluralist pronouncements from 1811 to 1854. We shall focus on two 1838 articles on the habitability of the planets and the Moon, which will allow us to appreciate the significance and depth of his change of mind between the late 1830s and 1854. We shall consider in turn Brewster's texts up to 1838, the two 1838 articles, those corresponding to the period between 1838 and 1854, and his *More Worlds than One* and other writings of 1854. I shall argue that the mercurial character of Brewster's opinions can be attributed to the fact that his belief in pluralism was for him not only a scientific question, but a significant component of his religious beliefs and as such a conceptual instrument to use as he saw fit in his apologetically-oriented writings.

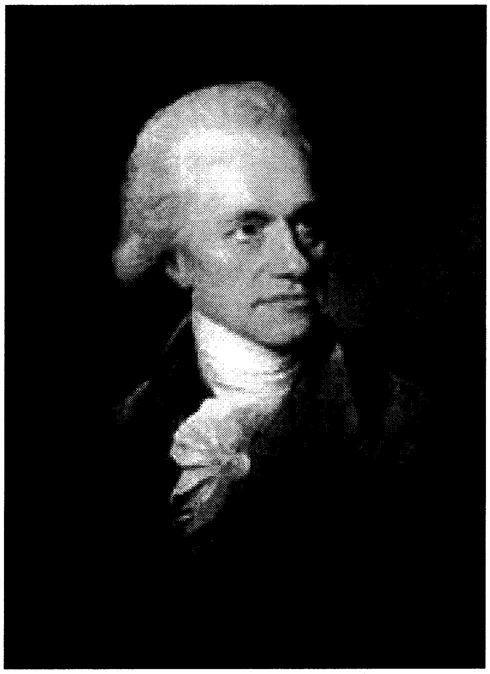

Figure 2: Sir William Herschel, 1738–1822 (after en.wikipedia.org/wiki/William_Herschel).

2 THE TEXTS EDITED UP TO 1838

It has been claimed (Crowe, 1988: 62-63) that the reading of Ferguson's *Astronomy* aroused William Herschel's interest in astronomy and his belief in the plurality of worlds. James Ferguson (1710–1776) was a self-taught astronomer, an instrument maker and a successful populariser of science, who wrote a number of books on astronomy, all of them rich in pluralist ideas. Ferguson's *Astronomy Explained upon Sir Isaac Newton's Principles*, first published in 1756, went through seventeen editions. In 1811 Brewster issued his own edition of the book adding twelve chapters and numerous notes (Ferguson and Brewster, 1811). Ferguson claimed that all the planets in the Universe are inhabited, but his position about the presence of life on the Moon was ambiguous on the grounds of its lack of an atmosphere (Ferguson and Brewster, 1811(I): 27). Brewster's note on this passage (ibid.) indicated that the opinion of astronomers on the issue was divided. But in a subsequent note commenting on Ferguson's denial of the existence of seas in the Moon, Brewster affirmed that "... the existence of a lunar atmosphere is completely ascertained." (Ferguson and Brewster, 1811(I): 30).

It should be recalled here that from 1776 to 1783 William Herschel (Figure 2) carried out a program of lunar observation motivated by his strong conviction that there was life on the Moon. His manuscripts of the period and his lunar observation book show that he believed that the Earth's satellite had an atmosphere, although he was aware of the evidence to the contrary i.e., the sharpness with which stars were occulted by the Moon (Crowe, 1988: 63-66). Herschel provided 'evidence' of the existence of a lunar atmosphere in a communication to the Royal Society, where he reported observations of three volcanoes on the Moon, carried out on 19 and 20 April 1787. He described one of them as "... an actual eruption of fire, or luminous matter." This occurrence made him recall a previous observation, on 4 May 1783, that he also considered to be of a volcanic eruption (Herschel, 1787). Brewster could have had Herschel's observations in mind when he affirmed the existence of a lunar atmosphere, but this does not explain the slight inconsistency between his notes to Ferguson's text.

On the claim by Ferguson that belief in inhabitants of the Sun is an opinion "... which posterity will rank among the aberrations of the human mind." (Ferguson and Brewster 1811(I): 204), Brewster limits himself to mentioning the case of a Dr Elliot, accused of having set fire to the cloak of a lady and whose attendant, Dr Simmons, had alleged madness in his client's defence, mentioning the fact that Elliot had submitted a paper to the Royal Society claiming that the Sun is inhabited. This case had been published in the *Gentleman's Magazine* for 1787 July (Kawaler and Veverka, 1981: 47). Brewster makes no reference to Herschel's paper of 1795, where the discoverer of Uranus supports the inhabitability of the Sun (Herschel, 1795).[2]

In 1801, Herschel proposed that the Sun could be enveloped by two layers of clouds, an outer 'luminous shell' generating its light and heat and an inner shell of 'planetary clouds', which would work as an isolating layer helping to keep the temperature on the surface of the Sun within a range compatible with the existence of life (Herschel, 1801). In his edition of Ferguson's *Astronomy* Brewster expressly argues against Herschel's solar theory of 1801, on the grounds that the luminous clouds could not be the cause of the Sun's 'blaze of light', and the 'planetary' clouds could shield the surface of the star from the enormous heat. Besides, he points out that the Sun does not agree with the density gradient of the planets, because if it did it should be denser than Mercury. Thus, he goes on, the exemption of the Sun from this law "... is a virtual admission, that analogical reasoning, on which Dr. Herschel's opinion is founded, cannot be fairly applied in this case." (Ferguson and Brewster, 1821). Thus, Brewster dismisses Herschel's solar theory not only on the grounds of its intrinsic problems, but also because he detects an inconsistency in Herschel's analogy (between the planets and the Sun).

The publication of the *Bridgewater Treatises* between 1833 and 1836 provided Brewster with a new opportunity to manifest his opinions on the plurality of worlds, this time in connection with his views on natural theology. These eight treatises were the result of a substantial legacy left to the Royal Society by the Reverend Francis H. Egerton, Eight Earl of Bridgewater, for the purpose of publishing a book that should illustrate the power and wisdom of God as manifested in the creation. Those in charge, decided to publish a series of books instead of a single volume, and the various authors were chosen by the President of the Royal Society, Davies Gilbert, who also sought the advice of the Bishop of London and the Archbishop of Canterbury (Brock, 1966; Gundry, 1946; Topham, 1995). In 1834 Brewster published anonymously in the *Edinburgh Review* his review of the *Bridgewater Treatise* authored by William Whewell (Figure 3), *Astronomy and General Physics Considered with Reference to Natural Theology* (Brewster, 1834). In this work Whewell (1833) does not expatiate upon extraterrestrial life, but in the chapter entitled "Of the Vastness of the Universe" he suggests that "… no one can resist the temptation to conjecture, that these globes [the planets]...are, like ours, occupied with organisation, life, intelligence." In his review, Brewster takes a much firmer stand on the inhabitability of the planets on the strength of a teleological argument:

> The mind cannot admit the sentiment, that a light-and-heat giving sun will carry round it, with an annual and diurnal motion, a light receiving planet, where there is no living eyeball upon which that light can fall, and no animal frame which that heat can cheer. (Brewster 1834: 441).

Brewster strongly believed that every element of the Universe, which he supposed had been created by God, should have a purpose (Brooke, 1977: 228; Crowe, 1988: 304). Whatever his changes of opinion in other issues were, he never modified his strong conviction on the power of the teleological argument in natural theology. Brewster's teleological style of reasoning has been distinguished from Whewell's model based on the nomological form of the design argument, according to which the existence of God is proved not by the perfect fitness of each organism to its environment but by the orderly display of the laws of nature that would express the harmony of the whole (Crowe, 1988: 266 and 269).

A problem Brewster saw in Whewell's nomological approach was that dependence on the laws could hide dependence on hypotheses and, as he puts it in his review of Whewell's *Astronomy*, natural theology has "… nothing to do with speculation and theories, however ingenious or well founded." (Brewster, 1834: 428). Edgar Morse has shown that Brewster's defence of the particulate theory of light and his opposition to the wave theory were linked to his idea that to accept hypotheses is equivalent to undermining the foundations of natural theology (Morse, 1972). After paraphrasing Whewell's passage about the ether, Brewster concludes that

> … the theory is still spoken of as only probable, and the ether only as a consequence of the theory. Yet Mr. Whewell goes on to deduce from this theory and this ether, proof of divine wisdom and skilful adaptation. (Brewster, 1834: 438).

In an 1847 article, Brewster would again insist on the primacy of facts over hypothesis in physico- and astrotheological arguments, claiming that geology and astronomy are specially important as the basis of natural theology because they are "… received as facts to be believed, and not as truths to be previously demonstrated or even explained." (Brewster, 1847: 210).

In his review of Whewell's *Bridgewater treatise* Brewster not only defends the idea of the plurality of worlds but also argues in favour of an infinity of worlds, and he opposes Whewell's claim that the Sun is the largest body in the Universe (Brewster, 1834: 444). Brewster's unwillingness to set limits on the creator in matters astronomical has a symmetrical counterpart in the realm of microscopic life, for he advocates an indefinite progression of animal life, which would take the form of a succession of 'orders' of creatures ever more and more diminutive, so that

> The Monades, in place of being the lower boundary of life, may thus be the commencement of a new order of living beings; and where the link to which they belong terminates, there may be other links in the descending chain, which man, with his present organs and instruments, may never be able to discover. (Brewster, 1834: 445).

Figure 3: English scientist and philosopher, William Whewell, 1794–1886 (after en.wikipedia.org/wiki/William_Whewell).

This emphasis on a chain of being progressing beyond the limits of what was by then conceived as the most elementary form of life was no a casual remark. In his review of the *Bridgewater Treatise* by Peter Mark Roget, *Animal and Vegetable Physiology*, Brewster (1834-1835: 151). calls an 'heresy' the assertion that infusoria are "… the ultimate term of animality."

In 1836 William Buckland, Professor of Geology at Oxford University, published the two volumes of his treatise *On Geology and Mineralogy Considered with Reference to Natural Theology*. In his commentary on Buckland's *Bridgewater Treatise*, Brewster suggests a cyclic conception of the inhabitability of the Earth. From a conviction urged by the 'buried monuments' of the past that the present "… cycle of the intellectual occupation of the globe …" must terminate, he jumps to the question whether "May not this, then, be the first of a series of cycles…?" (Brewster, 1837: 3). Brewster would not admit a limit to the size of objects in the Universe—be it an upper limit for the largeness of celestial bodies or a lower limit for microscopic organisms. And neither was he willing to concede that the occupation of the Earth by intelligent beings could soon come to an end. As we shall see below, this train of thought would be developed further in *More Worlds than One.*

3 THE ARTICLES OF 1838

In 1838 Brewster published anonymously two articles on the plurality of worlds: "Are the Planets Inhabited?" (Brewster, 1838a) and "Fortifications of the Selenites" (Brewster, 1838b). These are the first papers he wrote that were devoted exclusively to a discussion of the pluralist question (he was then 54 years old). The papers were published in the first two issues of *The Monthly Chronicle*, a London journal founded by the English politician and novelist Edward Bulwer-Lytton and the Irish scientific writer Dionysius Lardner. The latter had studied for the ministry, but worked as a lecturer and writer of encyclopaedias and popular scientific books and was at that time Professor of Natural Philosophy and Astronomy at University College, London. He enthusiastically endorsed the idea of extraterrestrial life (Crowe, 1988: 226-227).

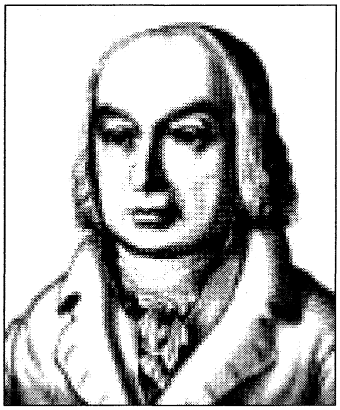

Figure 4: Bavarian physician and astronomer Franz von Paula Gruithuisen, 1774–1852 (after commons.wikimedia.org/wiki/Franz_von_Paula_Gruithuisen).

The significance of the 1838 papers for our study is that Brewster displays in them a coherent and empirically-founded account of his ideas about the habitability of the planets. It has already been emphasised that Brewster's endorsement of pluralism was based on a logic built upon teleological *and* analogical inferences (Crowe, 1988: 300-305). This twofold strategy is most evident in his "Are the Planets Inhabited?". The teleological approach is carried out through the description of the fitness of the Earth as the dwelling place of the human race: "Could we hesitate to infer that such a place [Earth] must have been *provided* expressly for *our* habitation?" (Brewster, 1838a: 102). Brewster describes the correspondence of a number of physical characteristics of "… this glorious earth …" with the physiological needs of human beings. For instance, he mentions the force of gravity,

> … just sufficiently great to give them stability, and no so great as to deprive them of the power of free and rapid motion …[and] the intervals of light and darkness, giving alternations of labour and rest, nicely corresponding with our muscular powers. (Brewster, 1838a: 101).

The 'beneficent intentions' of the creator, he says, are seen in "… these conveniences and luxuries …" more than in "… any great physical or mechanical laws, however imposing and important." (Brewster, 1838a: 101).

After this brief teleological excursion, Brewster turns to analogy seeking to demonstrate that physical conditions similar to those on Earth apply to other planets as well. The guiding idea that opens and justifies Brewster's careful exploration of "… physical analogies of irresistible force …" is that,

> … if they [the planets] are proved to be habitations similarly built, ventilated, warmed, illuminated, and furnished … can we doubt that such structures have been provided as the abodes for beings in all respect resembling us? (Brewster, 1838a: 102).

He examines in turn the alternation of day and night as a consequence of the rotation of the planets, the seasons as a result of the inclination of their axes from the perpendicular, the presence of an atmosphere (and consequently of water, clouds, rain and electricity), geographical accidents, gravity, and the heat and light provided by the Sun to all the planets of the Solar System.

For example, Brewster calculates the gravity in each planet from the values of mass and magnitude and arrives at the following results: bodies on Mercury, Venus and Saturn weigh the same as on the Earth; on Mars about one half; on Jupiter three times more than on the Earth; and on Uranus one fifth of Earth (Brewster, 1838a: 114). With respect to the periods of revolution, the figures provided by Brewster are: 23h 30min for Venus, 24h 40min 20s for Mars, 9h 56min for Jupiter, and 10h 30min for Saturn, while the period of diurnal revolution for Mercury is considered 'uncertain' (Brewster, 1838a: 104). The presence of an atmosphere is in each case ascertained on the basis of telescopic observations. Venus and Mercury are enveloped "… in atmospheres so dense and so loaded with clouds, that we rarely behold their surfaces."; that of the planet Mars is "… probably, less dense than that of the earth …" (Brewster, 1838a: 108); evidence for the atmosphere of Jupiter is inferred from a pattern of parallel streaks of light and shadow that the planet exhibits on its surface, no doubt proceeding "… from clouds floating in an atmosphere." A similar reasoning applies to Saturn. With respect to Uranus (Herschel's planet), Brewster says that it is too distant for this kind of observation, but "… if analogy has any force …" we should be justified in believing that it also has an atmosphere (Brewster, 1838a: 110).

The second 1838 paper, "Fortifications of the Selenites", was devoted to commenting on the lunar observations made by Gruithuisen. Franz von Paula Gruithuisen (Figure 4) was a German astronomer who wrote eagerly about the plurality of worlds and was eventually appointed Professor of Astronomy in Munich. In 1824 he published a lengthy paper titled

"Discovery of many distinct traces of lunar inhabitants, especially of one of their colossal buildings" in which he described different kinds of constructions and monuments supposedly erected by the lunarians. Among those who plainly rejected Gruithuisen's observations was J.H. Mädler, one of the first selenographers who, in collaboration with Wilhelm Beer, in the 1830s published a book and a map of the Moon (Crowe, 1988: 202-209). Brewster cites Mädler's contention that "… the appearances taken for forts by Gruithuysen [*sic*] are merely chains of hills of very regular outline …" and he goes on to argue that given the power of telescopes then available and the distance of the Moon, an object measuring 1 mile in diameter would be too small to be discerned (Brewster, 1838b: 150-151).

Since this question cannot be settled by direct observation, Brewster invites us to "… see how far analogy lends countenance to the supposition that the moon is the habitation of a race of beings like those to whom the earth is appropriated ...." (Brewster, 1838b: 151). Following his own advice, he then goes over the proofs at his disposal to see "… whether it [the Moon] is fitted to supply the wants of such beings." Among other things, Brewster contends that (a) telescopic evidence indicates that the Moon has no water; (b) that there are no seasons; (c) there is no available evidence of water in the Moon; (d) the length of the lunar day is 328 hours, and thus is unsuited for supporting life; (e) the existence of huge 'caverns' and mountain peaks turn the surface of the Moon into something very different from the terrain found on Earth; and (f) that the sharp disappearance of stars behind the disk indicates that "… the atmosphere of the moon, if it have any, must be so attenuated as not to produce any discoverable effect on the position of a star seen through it." (Brewster, 1838b: 152). Not surprisingly, he concludes:

> … we must, in the absence of any direct evidence on this question, come to the conclusion that our satellite is a barren uninhabited waste, playing doubtless some necessary part in the creation, but not the higher one assigned to the earth and planets ... (Brewster, 1838b: 154).

It is surprising to note that John Pringle Nichol, the Professor of Astronomy at the University of Glasgow and a believer in the plurality of worlds, in his *Views of the Architecture of the Heavens* quotes a letter by Brewster in which the latter states that

> … we need scarcely despair of discovering the structures erected by the [Moon's] inhabitants … an achromatic object-glass of the same size as the speculum of Sir William Herschel's forty-feet telescope, would certainly accomplish this. (Nichol 1838: 40).[3]

We have already discussed Brewster's note in the 1811 edition of Ferguson's *Astronomy*, where he affirms that existence of a lunar atmosphere "… is completely ascertained." It seems that when it comes to this issue his opinions were subjected to the winds of change! If we take his 1838 article on the fortifications of the selenites at face value—and I see no reason to suppose otherwise—we must accept that by that time Brewster was convinced that the Moon was not habitable. But as we shall see, this was not to be his last word on this.

4 TOWARDS 1854

Brewster was one of the main actors in the creation of the Free Church of Scotland in 1843, a momentous event for the evangelical movement led by the Reverend Thomas Chalmers (Figure 5). Chalmers was a precocious Scottish youth who studied mathematics at St. And-rews and was licensed to preach when he was only nineteen years of age. He spent two more years studying philosophy and science at Edinburgh University and was ordained in 1803. During his pastoral activity in a rural parish he underwent a conversion and changed his position from moderate rationalism to an emotionally-charged evangelism. Chalmers became the leader of the Scottish evangelicals and a close friend of Brewster's (Crowe 1988: 182-190). In the course of 1815 Chalmers preached a series of sermons in Glasgow where he boldly blended pluralism and evangelical feeling. The sermons were eventually published as *Discourses on the Christian Revelation Viewed in Connection with the Modern Astronomy*—usually cited as '*Astronomical Discourses*'—a book that enjoyed astonishingly popular success (Chalmers, 1817). This work bears on our argument, for it greatly contributed to the diffusion of pluralist ideas. Chalmers also was the author of one of the *Bridgewater Treatises*, titled *On the Wisdom, Power and Goodness of God*, which was published in 1833 (Chalmers, 1833). It has been argued (Cairns, 1956; Smith, 1979) that although Chalmers was keenly interested in the relationships between science and religion, he set limits on the possibilities of natural theology. Chalmers died in 1847 and, as Crowe (1988: 301) has suggested, "… Brewster may have felt that Chalmer's mantle as chief defender of pluralism had passed on to his shoulders."

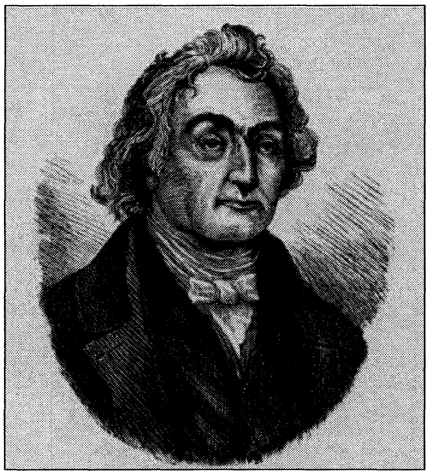

Figure 5: Scottish theologian Reverend Dr Thomas Chalmers, 1780–1847 (after en.Wikipedia.org/wiki/Thomas_Chalmers).

In 1844 the Edinburgh naturalist and editor, Robert Chambers (Figure 6), published his influential *Vestiges of the Natural History of Creation* (Chambers, 1844), a book with an evolutionary and deistic account of the Universe (Crowe, 1988: 224-225). Brooke (1977: 264-268) has argued that the publication of *Vestiges*, which associated ideas of the plurality of worlds with a version of Laplace's nebular hypothesis and advocated the idea that life on Earth could be explained by a 'principle of development', was what prompted Whewell to write his *Essay* in 1853. Baxter (1984: 47-48) has shown that during the late 1830s and early 1840s the nebular hypothesis was being popularised by the fervent evangelical astronomer, Nichol, and enthusiastically endorsed by Brewster. But the publication of *Vestiges* caused Brewster to turn about and to become a vocal opponent of the hypothesis. Baxter contends that this change of mind cannot be attributed to the publication of Lord Rosse's discoveries during the second half of the 1840s (the resolvability of the Orion nebula and the spiral structure of M51, among others), for the reason that Brewster was still writing in favour of the hypothesis *after* these observations became known.

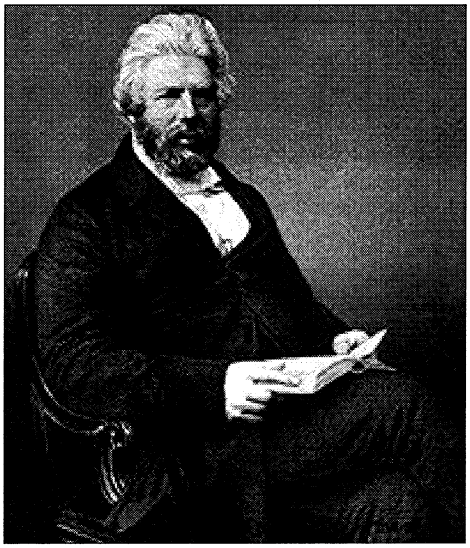

Figure 6: Scottish author and publisher Robert Chambers, 1802–1871 (after en.wikipedia.org/wiki/Robert_Chambers).

In the above-mentioned article "The Revelations of Astronomy"—which is actually a conjoint review of Smyth's *Cycle of Celestial Objects* (1844) and of a book by Nichol, both of which contain pluralist passages—Brewster once more resorted to his analogical–teleological argument to prove the existence of extraterrestrial life, along the lines of his 1838 "Are the Planets Inhabited?". Since the planets

> … have the same constitution … [and] perform the same function [as the Earth] … we are compelled to believe that the primary planets at least, are bodies like the earth, composed of land and sea, and are the theatres of animal and intellectual life. (Brewster, 1847: 241).

The Moon is still seen as a deserted wasteland, but Brewster now suggests the possibility that this dreary state of affairs will not last forever: "As our own earth was long in preparation for the occupation of man, the Moon may in like manner be preparing for the reception of inhabitants." (Brewster, 1847: 215).

Other texts of the period are Brewster's 1850 Presidential Address to the British Association for the Advancement of Science (Brewster, 1851), which has clear pluralist pronouncements, and his review of Grant's *History of Physical Astronomy* (Brewster, 1853), but these did not show any new departures from his previous ideas.

5 1854: PUBLICATION OF *MORE WORLDS THAN ONE*

This book, Brewster's answer to Whewell's (1853) *Of the Plurality of Worlds. An Essay*, can be seen as the culmination of his efforts in favour of the pluralist cause. Brewster wrote a review of the polemic work of the Cambridge polymath for the evangelically-oriented *North British Review* (Brewster, 1854a), and then submitted his *More Worlds than One* (Brewster, 1854b) for publication. I shall comment on the article and the book, focusing on Brewster's startling changes of opinion. We shall consider in turn the number of celestial bodies inhabited, the form of the extraterrestrial creatures, the habitation of the Earth in the distant past, and the planets as mansions for the souls in their afterlife.

Perhaps Brewster's most surprising change concerns the habitability of the satellite of the Earth. In his review of Whewell's *Essay*, the Moon ceases to be what in 1838 he had called a "… barren uninhabited waste." On the strength of conclusions reached by "… philosophers of high caste …", he declares that "… the Moon must be a world like the Earth ... fitted for the reception of, or already occupied by, animal and intellectual races like our own." (Brewster, 1854a: 2). Brewster also claims that "… the satellites of the planets must have been created for the double purpose of giving light to their primaries, and a home to animal and intellectual life." (Brewster, 1854b: 128).

As has been noted by Brooke (1977: 263), Brewster changed his mind with respect to the habitability of the Sun in the short period that elapsed between his review of Whewell's *Essay* and publication of his own book. In the review, he claimed that "There are certainly no grounds of analogy upon which we can support this theory [Herschel's] …" (Brewster, 1854a: 4), but in *More Worlds than One* he decidedly adopted Heschel's theory of the habitability of the Sun:

> The probability of the sun being inhabited is doubtless greatly increased by the simple consideration of its enormous size. Admitting with Sir William Herschel, that the sun may have a temperature adapted even for human constitutions, it is difficult to believe that a globe of such magnificence, 8,882,000 miles in diameter, upwards of one hundred and eleven times the size of our earth, and 1,284,472 times its bulk, should occupy so distinguished a place without intelligent beings to study and admire the grand arrangements which exist around them; and it would be still more difficult to believe, if it is inhabited, that a domain so extensive, so blessed with perpetual light, is not occupied by the highest orders of intelligence. (Brewster, 1854b: 114-115).

As has been pointed out by Brooke (1977: 263) and Crowe (1988: 304), in *More Worlds than One* the criterion for habitability turns out to be sheer size: a large celestial body could not have been made for nothing! With respect to Jupiter, Brewster stated that its size or bulk "… is about 1300 times greater than that of the Earth, and this alone is a proof that it must have been made for some *grand* and *useful* purpose ..." (Brewster, 1854b: 65). Of course, the 'grand and useful purpose' meant that Jupiter was the seat of intellectual life (we shall come back to Jupiter below).

Brooke (1977: 275) has underlined one of the main arguments utilised by Whewell in his *Essay*: "If the earth, as the habitation of man, is a speck in the midst of an infinity of space, the earth, as the habitation of man, is also a speck at the end of the infinity of time." Whewell saw no inconvenience in admitting cosmic waste. On the contrary, Brewster's teleologicaly-oriented mind felt compelled to fill in Whewell's cosmic void by populating the Sun, the Moon, the planets, and the satellites of the planets, no matter what he had said previously on these matters. He was certainly enthralled by what has been called 'the principle of plenitude' (Lovejoy, 1936: 52).

In his papers of 1838 Brewster's analogical argument gained force from the fact that he intended to demonstrate the existence of beings "... in all respect resembling us ...": similar conditions must be directed toward similar aims. Moreover, in *More Worlds than One* he opposes Chalmers' claim that perhaps the extraterrestrials would not need to be redeemed, arguing that to suppose the inhabitants of the planets sinless and immortal would entail breaking the analogy and, consequently, renouncing the faith in a plurality of worlds (Brewster 1854b: 158). In the same work, Brewster (1854b: 76-79) deals with a series of objections raised against the possibility of Jupiter being inhabited: scarce light and heat due to the great distance from the Sun, shortness of the day, and the great force of gravity—objections that were not discussed in his 1838 paper about the inhabitability of the planets. Curiously enough, to answer these objections, Brewster was forced to break the analogy, as has already been pointed out by Crowe (1988: 304). Brewster declares that to assume that the creatures living in the planets must be either human beings or anything resembling them "... is to entertain a low opinion of that infinite skill which has produced such a variety in the form and structure and functions of vegetable and animal life." (Brewster, 1854b: 80). And he uses mythological comparisons to speculate about the variety of organic forms of extraterrestrials. The immortal soul, he claims, should not necessarily be united to a human frame: "May it not reside in a Polyphemus with one eyeball, or in an Argus with a hundred? May it not govern in the giant forms of the Titans and direct the hundred hands of Briareus?" (Brewster, 1854b: 81).

Brewster concludes his discussion about Jupiter by asserting that the analogies between the Earth and the four 'superior planets' are "... sufficiently numerous and powerful to command the assent of an unprejuiced mind." (Brewster, 1854b: 84). He apparently ends in 1854 where he began in 1838, that is, with the conclusion that all the planets are inhabited. But there is a significant difference. In *More Worlds than One* he contemplates the possibility of extraterrestrial creatures being different from humans. This applies to Jupiter and the Sun, the largest bodies in the Solar System. Brooke (1977: 250) has remarked that with this move Brewster favours the 'resourcefulness' of the Creator over 'precision of adaptation' as an argument of natural theology. In his review of Whewell's *Bridgewater Treatise*, Brewster had already expounded this approach *in nuce*: "... the existence of an elastic energy in organic bodies by which they could accommodate themselves to a residence in every planet in the system, might be held to be a proof of divine wisdom and power." (Brewster, 1834: 436).

There are some passages in *More Worlds than One* that point toward the idea of a parallel between matter and life. In chapter 12 the idea of the infinity of life is linked to the notion of the infinity of matter (Brewster, 1854b: 204). Although it must be remarked that Brewster did not ever attribute to matter the capacity of producing life on its own, he affirms that life "... is virtually almost a property of matter, and therefore to conceive large masses of matter, that are warmed and heated to be destitute of life, is to do violence to our strongest convictions." (Brewster, 1854b: 115). In this argument we can see an instance of Brewster's abhorence of waste and purposelessness in the Universe (Brooke, 1977:263). The connection between matter and life is teleological: it is inconceivable to think of matter without life, because matter essentially demands to be granted meaning through the living beings that inhabit it.

In his last writings, Brewster also changed his mind with respect to the past history of the human race. It has been mentioned above that in his 1837 review of Buckland's *Geology and Mineralogy* Brewster suggested a series of 'cycles of life' projected toward the future. In his 1854 review of Murchison's *Siluria*, the succession of cycles is projected also into the past. Brewster suggests that 'more glorious creatures' (than human beings) might lie beneath the Earth, beings "... more lovely, more pure, more divine than man, may yet read to us the unexpected lesson that we have not been the first, and may not be the last of the intellectual race." (Brewster, 1854c: 506). Interestingly, in this review, Brewster establishes an explicit parallelism between the 'cycles of life', which could have preceded the cycle extending from the Silurian age to the present, and the distant celestial objects "... like the binary systems in the heavens far beyond our own, and to others remoter still, like the nebular worlds, which the science of the present day has been able to resolve." (Brewster, 1854c: 544). Thus, the overwhelming duration of geological time is compared with the immeasurable distances of interstellar space, and both are granted a meaning through the postulation of extraterrestrial creatures and of a series of cycles of life on Earth preceding the present one. In 1854 Brewster's mind was bent on a plenitude of creatures and his *horror vacui* could not suffer a swarming barren multitude of worlds any more than a succession of lifeless prehistoric aeons. And the latter were populated with some kind of pre-Adamite race, creatures deemed to have been more divine than human beings.

Brewster's papers of 1838 evolved into the exuberant passages of his 1854 writings. Perhaps one of the more curious doctrines in *More Worlds than One* is the idea that the planets, besides being inhabited by extraterrestrials, are at the same time the future homes of our souls. This view, though, had been advanced two decades earlier: in his 1834 review of Whewell's *Astronomy* Brewster (1834: 440) claimed that "... these magnificent spheres are worlds like our own,—the seats of animal and intellectual life,—the abodes of 'joy and gladness,'—the scene of preparation for a nobler existence." In his 1850 Report for the British Association for the Advancement of Science, he even claimed that

> ... if men of ordinary capacity possessed that knowledge which is within their reach, and had that faith

> in science which its truth inspires, they would see in every planet around them, and in every star above them, the home of immortal natures—of beings that suffer and of beings that rejoice—of souls that are saved and of souls that are lost. (Brewster, 1851: xxxiii).

It is difficult to discern in this text whether Brewster talks about the planets as the dwelling places of the departed souls or of extraterrestrials with a soul to be saved. In any case, in *More Worlds than One* Brewster explicitly expounds a full-blown doctrine in which celestial bodies are the homes of human souls in the afterlife:

> If there is no room then on our Earth for the millions of millions of beings who have lived and died upon its surface, and who may yet live and die during the period fixed for its occupation by man, we can scarcely doubt that their future abode must be on some of the primary or secondary planets of the Solar System, whose inhabitants have ceased to exist like those on the earth, or upon planets in our own or in other systems which have been in a state of preparation, as our earth was, for the advent of intellectual life. (Brewster, 1854b: 18).

In his review of Brewster's and of Whewell's books, the Reverend Baden Powell (1796–1860), Savilian Professor of Geometry at Oxford University, objected that "… it might rather seem that their being uninhabited would be more favourable to his doctrine, as affording more ample space for the reception of resuscitated humanity." (Powell, 1855: 294).

As a rule, Scottish evangelical scientists did not strive toward reconciling science and the Bible (Baxter, 1984: 48-49). In his review of Buckland's *Bridgewater Treatise* Brewster criticises the attitude of some geologists who had intended to harmonise the geological record with the biblical narrative of creation in six days. It was a motive of pride on his part to defend the independence of the scientific enterprise from the literal readings of the Bible: "The best interests of mankind, are invariably sacrificed when religion is intruded into questions of science and civil policy." (Brewster, 1837: 4). In the same article, he praises the abandonment of biblical geology by Hutton, and comments approvingly on how the catastrophist theory has superseded the idea of a single Flood (Brewster, 1837: 2). His attack on the intrusion of religion upon science seems to have arisen from his deep belief in the idea that God manifested himself through the book of nature as well as through the book of revelation, so that "Truth secular cannot be separated from truth divine ..." (Brewster, 1851: xli).

These words, uttered in the 1850 meeting of the British Association for the Advancement of Science, neatly express Brewster's belief in the uniqueness of truth. In the texts referring to the plurality of worlds he does not distinguish between scriptural and natural truth. In *More Worlds than One* he points out passages of the Bible which, from his point of view, cannot be explained without accepting pluralism (Brewster, 1854b: 15-16), and he enumerates another group of passages that "… use terms which clearly indicate that the celestial spheres are the seat of life." (Brewster, 1854b: 14). But given that "Scripture has not spoken with an articulate voice of the future locality of the blest …", the warrant for the notion of the planets as the future abodes of our souls is said to be the result of 'reason', which "… with a voice almost oracular, has declared that He who made the worlds will in the worlds which he has made, place the beings of His choice." (Brewster, 1854b: 289-290). A few lines below he claims that it is 'religion' which teaches us about our future lives: "It is impossible for intellectual man, with the light of revelation as his guide, to doubt a moment that on the celestial spheres his future is to be spent." (Brewster, 1854b: 290). Thus revelation completes the task begun by science and "… Faith 'takes up the wondrous tale' [of science], and associates with these bright abodes the future fortunes of immortal and regenerated man." (Brewster, 1854b: 292).

The papers of the late 1830s advocated only the habitability of the planets, not of the Moon or satellites. In his 1854 book Brewster extends this property to all the bodies of the Solar System—including the Sun—and he admits, rather inconsequentially if we accept the premises of his analogical reasoning, that extraterrestrials need not be similar to human beings. He also gives a new twist to his cyclic notion of an Earth, which he now sees as inhabited in the future *and in the distant past* by some kind of intellectual creatures. At the same time, his conception of the planets and stars as the future abodes of the blessed souls, which already lurked in his early papers, was presented as a doctrine of material existence after death, a materialist eschatology already pointed out by Brooke (1977: 263).

The style of *More Worlds than One* is more emotional and rather less consistent than that of Brewster's first papers. The 1838 articles were published in *The Monthly Chronicle* and intended for a metropolitan and supposedly highbrow readership. The book has a very different character. It was the answer to Whewell's *On the Plurality of Worlds* which, according to Brewster, had deeply hurt his feelings (Gordon, 1869: 250). Moreover, Crowe (1988: 305) has drawn our attention to the religious crisis into which Brewster was precipitated after the death of his first wife in 1850. By that time he began to attend sessions of 'spirit-rapping' and the turning of tables. Although in a letter of June 1855 he wrote that he did not believe that those phenomena were the actions of idle spirits, the important point is that he frequented those experiments for many years (Gordon, 1869: 253-258). Brewster's confidence in the possibility of visualisation of spirits, and his belief in ghosts—he told his daughter that he had seen a pair of them (Gordon, 1869: 294-295)—is consistent with his conception of a material existence after life. Mrs Gordon (1869: 294-295) tells of her father's ambivalence towards non-natural phenomena and 'ghostly marvels', like clairvoyance: "He really wished to believe in many wonders to which his constitution of mind utterly refused credence." Brewster's character, according to his daughter,

> … was peculiarly liable to misconstruction from its distinctly dual nature; it was made up of opposites, and his peculiarly impulsive temperament and expressions laid him open to the charge of inconsistency, although he never recognised it in himself … Accustomed to look at every subject with the critical investigation of the man of science, he yet united the feelings of the man of impulse, and he spoke as moved by either habit ... (Gordon, 1869:

294).

The striking combination of emotive style and dry physical arguments characteristic of *More Worlds than One* fits well with this description.

6 CONCLUSION

We have reviewed a series of changing attitudes in Brewster's pluralist ideas: the habitability of the Moon, the habitability of the Sun, the possibility that extraterrestrials are morphologically different from humans, and the possibility of intelligent races—other than the human race—inhabiting the Earth in the distant future and in the immemorial past. A number of inconsistencies are also evident: the occasional break of the analogical argument; his idea that the planets were inhabited by extraterrestrials, and at the same time were the future abodes of our souls; and his belief in the pluralist hypothesis, and its coexistence with a steadfast defence of empiricism. The contradiction between the hypothesis of extraterrestrial life and the strongly empirical approach to science as advocated by Brewster is implied in a passage of his 1854 review of Whewell's *Essay*: "… the doctrine of the plurality of worlds is a *theory* founded on analogy …" (Brewster, 1854a: 74; my italics).

Our discussion of Brewster's writings on the plurality of worlds between 1811 and 1854 suggests that not a few of the ambiguities, shifts and inconsistencies found in them can be accounted for if we accept that his pluralist views were linked more with his religious faith than to his science, notwithstanding his proclamation of the equal value of reason and the Bible, and the obvious difficulty of separating both strands in any historical account of the period. Brewster wielded his extraterrestrial weaponry as polemical necessity required, always prioritising his religious convictions over the demands and limits of empirical evidence which, despite his claims to the contrary, seems to have remained subservient to his other motives.

7 ACKNOWLEDGMENTS

I wish to express my gratitude to Professor Michael Crowe (University of Notre Dame) for his valuable criticisms and never-ceasing encouragement during the long time this paper took to be written. Dr Miguel Valvano and Ms Evellyne Ansara (University of Western Ontario) kindly helped me with some bibliographical items. Finally, I wish to thank Dr Wayne Orchiston (James Cook University) for helping me improve the quality of this paper.

8 NOTES

1. For a brief biographical account see Morse (1981). The most important source for Brewster's life is the biography by his daughter (Gordon, 1869).
2. For Herschel's ideas on the inhabitability of the Sun see Kawaler and Veverka (1981) and Crowe (1988: 59-70). I based my account on the latter work.
3. The letter quoted in Nichol (1838: 40) is cited by Crowe in *The Extraterrestrial Life Debate 1750-1900* (1988: xxi), where he acknowledges that David Dewhirst and Simon Schaffer drew his attention to this document.

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ARTHUR BEER AND HIS RELATIONS WITH EINSTEIN AND THE WARBURG INSTITUTE

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Abstract: We give an account of the scientific life of Arthur Beer (1900–1980). Born in Reichenberg, Bohemia, he studied in Leipzig, Vienna and Berlin. After obtaining his Ph.D., he worked with the Seewarte (maritime observatory) and with the Warburg Library, both in Hamburg. Because of his relations with Finlay Freundlich, Albert Einstein and Fritz Saxl, he succeeded in emigrating to England in 1934, where he obtained a temporary position at Cambridge Observatory, and carried out astrophysical research under F.J.M. Stratton. After shorter stays at the observatories of Mill Hill and Kew, both in the vicinity of London, he obtained, after World War II, the position of Senior Assistant Observer in Cambridge. Besides his studies in astrophysics and the history of astronomy, he is best known as the founding Editor of the series *Vistas in Astronomy*.

Key words: Arthur Beer, Albert Einstein, Warburg Institute, *Vistas in Astronomy*

1 INTRODUCTION

On the occasion of the Einstein Relativity centenary, the Berlin State Library/Prussian Cultural Heritage published a facsimile edition of Einstein letters kept in its Manuscript Department. An English translation of one of the letters to Mrs Clara Stern (spouse of the historian Alfred Stern, Professor of History at the ETH Zurich, and member of Einstein's circle of friends), written in Oxford on 11 June 1933, reads as follows:

> I know a very worthy person which you might support with your means. He is an astronomer of 33 years, held in high esteem by all specialists, a Bohemian Jew who has received his entire academic education in Austria and Germany, and has worked exclusively in Germany at the largest institutes with very good success. This man has now lost, with a brutal consequence, all possibilities, even the smallest ones, to earn his living, so that now he has become without subsistance, and is literally forced to be a beggar. I have a handful of brilliant testimonies of him and his work. He has a wife and a child. I have written and asked for his address. In any case, he can be reached via Dr. Freundlich, Potsdam Astrophysical Observatory. (Schneider-Kempf, 2005).[1]

So much for Einstein's letter. Of course, the reader is curious to know the name of the astronomer, but neither Einstein nor the editors give a name. Looking through the Portraitgallerie der Astronomischen Gesellschaft of 1931 (Tass, 1931), the first author of this article finds, after having looked through the first pages, Arthur Beer, born in Reichenberg, Bohemia, in 1900. Is this the proper identification? A. Beer died in 1980, but his son Peter, who edited the journal *Vistas in Astronomy* for some years, might know something. The retired Regius Professor of Astronomy in Glasgow, Archie E. Roy, another Editor of the journal, communicated Peter Beer's address, and a telephone call confirmed that indeed Arthur Beer and Albert Einstein were acquained with each other and had exchanged letters for decades (although Peter Beer did not know about Einstein's letter to Mrs Stern).

We now take the opportunity to review Arthur Beer's life, to discuss his correspondence with Albert Einstein, and also to describe his relations with the Warburg Library (the predecessor of the Warburg Institute).

Figure 1: Arthur Beer (1900–1980).

2 BEER'S LIFE UNTIL 1933

Arthur Beer (Figure 1) was born on 28 June 1900 in Reichenberg, Bohemia. Today's Liberec in the Czech Republic was then part of the 'crown land Bohemia', which belonged to the Austro-Hungarian Monarchy and was known for its textile industry. Arthur was the only son of Professor Johann Beer, a secondary school teacher of arts and crafts, and his wife Olga née Pollak. He attended a seven-year 'state secondary school' in Reichenberg, where he passed his final exam at the end of 1918 (after returning from war service), and then started studying natural sciences in Leipzig as a 'second grade student' (Studierender II. Ordnung), with astronomical laboratory courses under Friedrich Hayn (1863–1928). The four semesters in Leipzig were followed by a cure for the nervous complaint he acquired during his war service, and by two semesters in Vienna (1921/1922 and 1922). In order to become a 'first grade student' (Studierender I. Ordnung) and be able to complete a Ph.D., he passed another exam in Leipzig in 1922, and continued his studies in the winter of 1922-1923 in Berlin, where he worked at the Berlin-Babelsberg Observatory under the supervision of Paul Guthnick. In 1924, he contracted polio and was able to walk only after an operation. So he was only able to register for his doctoral examination on 25 June 1926. His dissertation "Zur Charakterisierung der spektroskopischen Doppelsterne" ("On the characterization of spectroscopic binaries") was judged to be "valde laudabile" (very laudatory) by Paul Guthnick and August Kopff. The examination took place on 17 February 1927, and the dissertation was printed as Veröffentlichung der Sternwarte Berlin-Babelsberg Band V, Heft 6 (1927).[2]

Beer had married Charlotte Vera Popielarski from East Prussia on 24 July 1925, and their children are Peter Beer (who was born on 15 October 1929 in Hamburg) and Nova Beer (who was born 13 March 1935 in Cambridge, England).

Until 31 December 1928, Beer was secondary assistant at the Breslau University Observatory, financed by the Notgemeinschaft Deutscher Wissenschaft. He worked on the radiation of planets, and also took part in the reduction of meridian circle observations of stars to be used to calibrate the photographic zone observations of the second catalogue of the Astronomische Gesellschaft.

Starting in January 1929, he was employed at the Deutsche Seewarte (German Maritime Observatory) in Hamburg as a 'tide astronomer' (Dominik, 1930), and several years later he published a popular account on the prediction of tides, which included photographs of his former working place (Beer, 1933b). In addition, he produced a regular radio program titled "News from Nature and Technology" for the North German Radio Station. In his curriculum vitae, he wrote: "My personal position at the Maritime Observatory as the first employed Jew was, from the beginning, a very difficult one and gave no perspectives for the future." (Beer, 1933a). Thus he left at the end of March 1930, and apparently looked for possibilities in Brazil and Canada, but he then accepted an offer from Fritz Saxl (1890–1948), Director of the Warburg Cultural Library in Hamburg, to take part in the installation of a permanent exhibition in the rooms of the new Hamburg Planetarium.

It is necessary to make a digression here. Aby Warburg (1866–1929), one of a group of five brothers, belonged to an important family of bankers in Hamburg. There apparently was an agreement made between the 13 year old Aby and his 12 year old brother Max: Max should take over the paternal bank, but had to promise to buy all books for his elder brother that he deemed necessary for his studies. Thus the art-dilettante Aby Warburg was able to collect about 65,000 books in the course of his life! A special, architecturally-unusual building was added to his living quarters in Hamburg-Eppendorf to house the library (Stockhausen, 1992). Fritz Saxl was the long-time Director of the Warburg Library.

When the Hamburg Planetarium was installed in a water tower of the city park in 1930, a 'sky museum' was also installed, equipped by Aby Warburg with mural paintings, drawings, models, show-cases, manuscripts, and in addition a small library with relevant books. The museum consisted of a few original items and many graphic displays, which illustrated the history of 'Star belief and star knowledge' from the beginnings of astronomical activity through to modern times. Arthur Beer was responsible for the installation of the section on 'Modern times', and was supported by the Einstein Tower, the Berlin-Babelsberg and Hamburg Observatories, and the Zeiss Company. He also prepared an illustrated report about the 'sky museum' (Beer, 1931). Another report by Beer (1930) deals with a planetarium lecture by Saxl given in December 1930 to the 'Religionswissenschaftliche Gesellschaft' (Theological Society). Saxl referred to the sky image of Qusayr 'Amra, another one in the Capella Pazzi in Florence, the Roman villa of Agostino Chigi which shows a planetary configuration for the year 1465, and finally the position of planets at the time of Martin Luther's birth. These topics continued to intrigue Beer right into the 1960s.

The Warburg Institute at the University of London has retained the old correspondence which reveals that the planetarium exhibit had both supporters and adversaries, and that there were also plans to transfer it to the Museum of Ethnology (Völkerkunde-Museum) in Hamburg. But the rise of the National Socialists made all these plans obsolete. In 1933, the Warburg Library was moved to London, initially for a period of five years, but it still exists today, in expanded form, as The Warburg Institute. The exhibit remained in the Hamburg Planetarium and gradually was transferred to the storage rooms, but some original manuscripts and prints were returned to London in 1935. In 1966-1968 and 1973, Arthur Beer (1968) and the Director of the Planetarium, Dr Josef Beller, used available archival material and old photographs to reconstruct an updated version of the original exhibit, and this was placed on display. But once again, the exhibit slowly vanished into the Planetarium's storage rooms. Rediscovered and reconstructed in 1987, the exhibit was then shown in Vienna, Hamburg and Berlin. In a book published in 1993, Fleckner et al. provide illustrations of the reconstructed exhibit, but minus the section on "Modern Times" that was originally arranged by Beer. This book also includes a biography of Aby Warburg, and contemporary newspaper and journal clippings about the exhibit. In the very latest development, a permanent display in Hamburg is currently under discussion.

Beer gave frequent lectures at the Hamburg Planetarium, and he also wrote many articles about new scientific and technological discoveries for the daily press, both in Germany and abroad. In addition, radio stations in Germany, Austria and Switzerland aired his radio programs. One of these, titled "Aus

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Natur und Technik", was among the very first regular scientific radio programmes. From 1931, he undertook lecture trips during the winter months within Germany and Czechoslovakia, and was able to earn a living from these activities. Yet he also found time for scientific work, including a study of "The Zodiac of Qusayr 'Amra" (Beer, 1932; 1967; 1972-1973), which he carried out in collaboration with F. Saxl. This long-term study related to a painting on the ceiling of a deserted Jordanian castle about 80 km east of Amman. Built in the first half of the eighth century AD, Qusayr 'Amra now has UNESCO world cultural heritage listing (see http://whc.unesco.org/en/list/327).

Beer concludes his Curriculum Vitae of May 1933 with the words:

> ... all the above-mentioned foundations of my existence, built up in year-long work, have now been destroyed by the revolutions in Germany. There is no chance in academy, and there is also no chance to work for the press, the radio, or as a lecturer. The insurmountable difficulties have deprived me in a systematic way of any future possibility inside Germany, as is clearly expressed in the attached copied letters. (Beer, 1933a: 3).

Already in 1930 Guthnick had described Beer as "... one of my most talented and most diligent disciples, whose advancement is unfortunately hampered by the crowding in all scientific disciplines, which forces us to employ, in first line, German citizens." Because Reichenberg was originally part of Czechoslovakia, Beer held a Czechoslovakian passport.

The situation became even more difficult in 1933. In March the North German Radio (1933) wrote that,

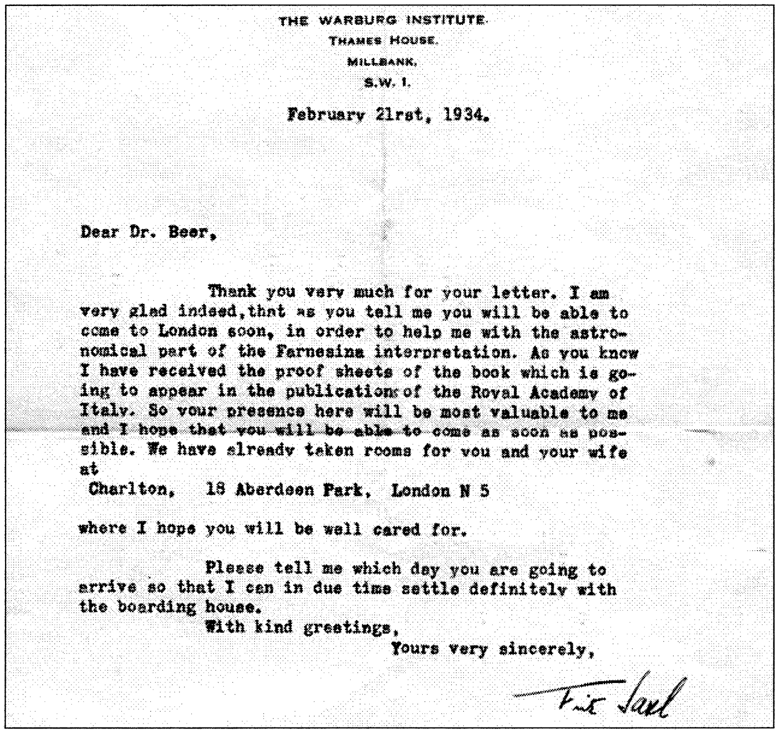
> ... because of the historical events, a thorough change in the programme of the German radio has become necessary. We are very sorry to be unable any more, at least for the time being, to air your regular talks "Aus Natur und Technik".

Two months later Beer received a letter from the Silesian Radio Hour G.m.b.H. (1933) where they indicated that, if at all possible, they had to give their current lectures to Silesian collaborators, and that they were not able to accept "Aus Natur und Technik" in their programme as a local unemployed engineer had taken over the 'technical talks'.

3 BEER IN ENGLAND

In this situation, Beer turned to Einstein asking for support, and thus the lines in the above-mentioned Einstein letter were written. In a letter sent from Reichenberg and dated 17 December 1933, Beer (1933c) thanks Einstein "... for pecuniary help ... via Professor Freundlich ... which allowed me to make it over the summer". It is not unlikely that this is the help that Mrs C. Stern had offered before. Einstein also tried to assist Beer (and many others) to find a position outside Germany. During his stay in England, Einstein met Fritz Saxl (who was then living in London), and he passed Beer's membership application on to the Royal Astronomical Society and approached the Academic Assistance Council in London on his behalf.

In early 1934, Einstein expressed some hope of finding a position for Beer in Princeton. When Beer received this letter, he was on his way from Reichenberg via the Netherlands and Belgium to London, since he had received a telegraphic (wire) invitation from Saxl, who was Director of the Warburg Library. An English translation of the text reads: "Dr Beer – do come over here as soon as you can – your expert help with our archive is urgently needed." (Saxl, 1934b). This had been followed by a vital letter (Saxl, 1934a), which is reproduced here as Figure 2.



THE WARBURG INSTITUTE.
THAMES HOUSE.
MILLBANK.
S.W. 1.

February 21rst, 1934.

Dear Dr. Beer,

Thank you very much for your letter. I am very glad indeed,that as you tell me you will be able to come to London soon, in order to help me with the astronomical part of the Farnesina interpretation. As you know I have received the proof sheets of the book which is going to appear in the publications of the Royal Academy of Italy. So your presence here will be most valuable to me and I hope that you will be able to come as soon as possible. We have already taken rooms for you and your wife at
Charlton, 18 Aberdeen Park, London N 5
where I hope you will be well cared for.

Please tell me which day you are going to arrive so that I can in due time settle definitely with the boarding house.

With kind greetings,
Yours very sincerely,
Fritz Saxl

Figure 2: Letter from Fritz Saxl supporting Arthur Beer planned migration to London.

In this way, Beer—and somewhat later, his family—were able to emigrate to England. On 25 March 1934, he wrote to Einstein, that he "... had been received in Cambridge, Oxford, Greenwich etc. ... most cordially, but nowhere could anyone give him any concrete prospects for the future." (Beer, 1934a). H.N. Russell (1934) of Princeton wrote to him as well, indicating that he was "... not able to find any vacancy as yet." Nevertheless, Beer (1934b) was able to report to Einstein on 15 July 1934 that

> ... a stipend could be secured for a year ... based on a kind invitation by Prof. Eddington, who had written to the London Committees ... and that he [Beer] had taken up work with him [Eddington] and Professor Stratton, at first in stellar spectroscopy.

In Cambridge, Beer started work in a field that had already been his favourite one in Berlin: binary stars. His first paper, which also included some material from his German colleagues Josef Hopmann and Kurt Walter, dealt with the eclipsing binary ζ Aurigae and was published in the prestigious *Monthly Notices of the Royal Astronomical Society* (Beer 1934c). At the end of 1934, Nova Herculis 1934 (DQ Her) erupted. Frederick J.M. Stratton (1881–1960), the Director of the Cambridge Observatory and a specialist in the field of novae (e.g. see Stratton, 1928), collaborated with Beer, and in subsequent years several papers on this object were published; these were subsequently summarized by Beer and Mustel (1974). Beer's daughter was born in 1935, and at Stratton's suggestion she was called 'Nova'.

A letter to Einstein indicates that in June 1936 Beer was still living on stipends, which were provided by the Academic Assistance Council in order "... to continue your work at Cambridge while searching for a position elsewhere." Beer made contact with Princeton, Yale, Cambridge (Mass.) and Yerkes, and he was also aware that Finlay Freundlich had moved from Istanbul to Prague (although his ambitious plans did not bear fruit, and with Eddington's help he had to find a new scientific home in Great Britain). Beer (1936) also mentioned in his letter to Einstein, "... that, according to my knowledge, the plan of a Palestinian observatory has not yet been discussed until now ... [and] even if it would be an institute with modest beginnings – it would have a great potential to develop! Also climatically the possibilities are ideal."[3]

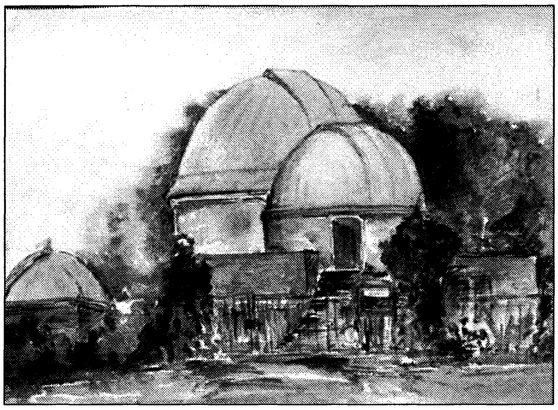


Figure 3 The University of London's Mill Hill Observatory, from a watercolour painting by Arthur Beer in 1944.

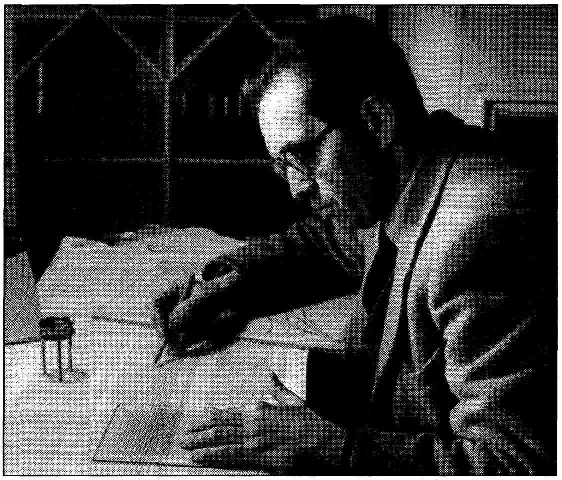


Figure 4: Arthur Beer with seismographic recordings at Kew Observatory, 1940s (P. Beer Collection).

But fate moved in other directions, and at the end of his stipend Beer transferred to London where he worked for some time at the Mill Hill Observatory of University College (Figure 3). It was, however, closed at the beginning of World War II, so Beer continued his work from Cambridge. Like many of his European colleagues, Beer was not able to continue his astronomy during the War, but as a 'friendly alien' (thanks to his Czechoslovak passport) he was able to work for the Air Ministry. From 1941 to 1945 he was based at the meteorological-seismological Kew Observatory, and was employed as a seismologist (Figure 4). In 1946, he obtained British citizenship and became Senior Assistant Observer at Cambridge Observatories (Figure 5). While he retained this position until his retirement in 1967, he spent extended periods at the Dominion Astrophysical Observatory in Victoria (Canada) and as a Visiting Professor at Swarthmore College (USA).

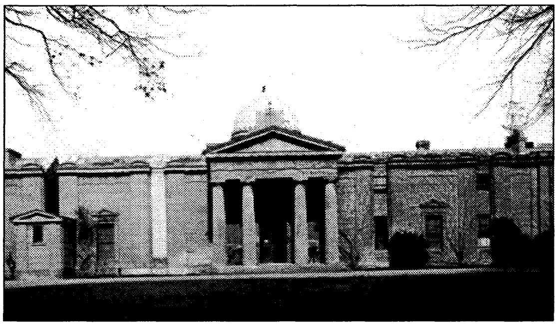


Figure 5: The main Cambridge Observatory building, ca 1920.

Beer was a long-standing member of the Royal Astronomical Society and the International Astronomical Union, and from 1935 he regularly attended IAU General Assemblies (see Figure 6). Besides his astrophysical work, he also busied himself with topics in the history of astronomy. On 10 December 1970, the Faculty of Natural Sciences at the Johann Wolfgang Goethe-Universität Frankfurt am Main awarded him an honorary doctorate (the "Doctor philosophiae naturalis causa") upon the recommendation of the famous historian of science, Willy Hartner. In his spare time, Beer took care of English editions of Russian astronomical books for Pergamon Press. He also translated Jordan's *Die Expansion der Erde* and the second edition of Roth's *Handbuch für Sternfreunde* into English, and they appeared in 1971 and 1975, respectively; the latter book had the inviting title *Astronomy: a Handbook*. In addition to his numerous scientific papers, Beer wrote hundreds of popular articles for newspapers and periodicals.

4 *VISTAS IN ASTRONOMY*

Let us step back two decades. In the early 1950s, Beer planned a book—neither a 'Handbook' nor a 'Festschrift', but rather a voluminous and thorough survey of present-day astronomy—which he intended to dedicate to F.J.M. Stratton, on the occasion of his 70th birthday. Frederick John Marrian Stratton (1881–1960), or 'Chubby' to his colleagues and 'Tubby' to his students, was one of the pioneers of astrophysics (e.g. see the group photograph of the 1939 Paris conference on novae in Tassoul (2004, 119), and was General Secretary of the International Astronomical Union from 1925 to 1935 and of the Council of Scientific Unions (ICSU) from 1937 to 1952.

Beer's 'Stratton Project' grew into a gigantic enterprise, and it is no wonder that its appearance was somewhat delayed. It ended up as an impressive two-volume panorama of astronomy, astrophysics and related fields of the mid-twentieth century, and almost everyone of fame in astronomy was represented. As Beer (1955-1956, I: xii) says in his introduction: "Ultimately 215 authors joined to produce the 192 contributions: 179 of them are astronomers, 36 physicists, geophyicists, mathematicians, and historians. They cover the span between the ages of twenty-four and ninety-one, and the more significant one between twenty-six nations." Some potential authors, like W. Grotrian and E.P. Hubble, died before they could provide their manuscripts. Einstein was also asked to contribute, but he wrote: "Overloaded with work and other obligations, it is impossible for me ... I also completely realize that I cannot contribute anything of special value to the book." (Einstein, 1954). At the end of 1954, Beer again tried to obtain Einstein's support, and asked him just to provide a motto, a single line, in order to underline the fact that the book was a serious example of internationalism. Einstein's early reply (1955) came just three months before his death, and it reads: "I don't lack good will. But something is opposed to my contribution, namely that I cannot say anything sensible. I have indeed become some sort of authority, but not enough, so that I would not realize when I say something foolish." And this is why the book appeared without a contribution by Einstein.

Nevertheless, the result—which constituted the first two volumes of *Vistas in Astronomy*—was so impressive, and the support for it so overwhelming, that a decision was made to continue this type of astronomical review. In this way, *Vistas* became a series of approximately annual books, which from 1960 featured astronomical reviews on a wide range of topics. In general, these papers tended to be somewhat less formal than those that appeared the *Annual Review in Astronomy and Astrophysics* (which started a few years later). Apart from Volumes 1 and 2 (the 'Stratton Festschrift' of 1955-1956), astronomical historians will particularly appreciate Volume 9, titled "New Aspects in the History and Philosophy of Astronomy" (which reproduced papers given at the 1964 Hamburg symposium that was organized jointly by the IAU and the International Union on History and Philosophy of Science), and Volumes 17 and 18 (on "Copernicus" and "Kepler", respectively), which appeared in 1975.

From Volume 19 (1975), *Vistas* appeared as a journal with three to four annual issues. In 1992, *Vistas* absorbed the *Astronomy Quarterly*, a journal founded in 1978 by G. Pacholczyk which specialized in historical and philosophical topics. Finally following the appearance of Volume 42 in 1998, *Vistas* itself underwent a metamorphosis and it now appears in the guise of *New Astronomy Reviews* under the banner of the Amsterdam publishing house, Elsevier. Since *New Astronomy Reviews* does not accept those historical papers which were so much appreciated by Arthur Beer, it was fortunate that the *Journal of Astronomical History and Heritage* was also founded in 1998, so that these papers can now find another home.

Figure 6: Arthur Beer at the 1955 IAU General Assembly in Dublin, Ireland. Arthur Beer (holding camera) is at the back; directly in front of him (left to right) are Karl-Otto and Sabine Kiepenheuer, Vera Beer (partially eclipsed) and Nova Beer (courtesy E. Landré, Collection of the Astronomical Institute of Utrecht University).

5 CONCLUSION

Finally, we would like to quote Arthur Beer's own words:

> This museum illuminate[s] the path up to Kepler and ... beyond him to the present ... [and shows] a dehumanising process ... [which] appears rather frightening ... This is well brought out ... by two of the pictures: on the first there is Tycho Brahe in command of his helpers at his sixteenth-century's observatory – quite visibly feeling himself "central" in his Universe. In the other, there is today's observer at the eye-piece of his 200-inch reflector on Mount Palomar ... but we have first to find him! It seems that if observing Man has grown ever smaller and more insignificant ... There will always be a place for astronomers at intricate control-desks, checking television screens from cushioned seats, perhaps in a warmth and comfort that dispenses entirely with a real look at the real stars. And the output will be great. Why then raise this issue here? Only to hint at a simple but vital truth: we need historical perspective today more than ever – this precious mirror of the

past held to the face of the living. (Beer, 1967: 223).

Arthur Beer not only knew the past, he also foresaw the present. He died on 20 October 1980 in Cambridge, England.

6 ACKNOWLEDGEMENTS

We thank the Albert Einstein Archives, Jerusalem University, for permission to quote from the Einstein-Beer correspondence; the archive of the Humboldt-Universität Berlin, for putting at our disposal the Ph.D. files of Arthur Beer; and Dr M. Maaser from the archive of the Johann Wolfgang Goethe-Universität Frankfurt a.M. for information. We also thank Professor P. Brosche (Daun) for information about Reichenberg, and Dr Mary Brück (Penicuik) for her help with the Dublin photograph. Last but not least, we thank Professor A.E. Roy, University of Glasgow, for establishing the contact between the authors.

7 NOTES

1. The original of this letter is in the Albert Einstein Archives (Jerusalem University, Israel) Call No. 39-443.00. The Call Numbers of all subsequent letters from these Archives are abbreviated as AEA.
2. These biographical details were drawn from his Curriculum Vitae, various certificates (AEA 49-203 to AEA 49-209) and promotion papers (Humboldt-Universität zu Berlin – Archiv – Bestand Phil. Fak., LiHr. P No. 4 Vol 450, 661, Bl. 323-331).
3. In fact, the very first major modern observatory in this region, the Wise Observatory of Tel Aviv University, was only inaugurated on 26 October 1971. This facility, which is located near Mitzpe Ramon in the Negev Desert, is equipped with a 1-m reflecting telescope.

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Peter Beer was born in 1929 into a scientific family – his father was a distinguished astronomer and editor. Peter gained an Honours degree in Physics at Manchester University, and for some years he researched and taught science. He then became a science producer with BBC World Service radio, and worked at the BBC for twenty-one years, where he was responsible for several hundred radio programmes and science scripts. During and after this time, he was much involved also as joint Editor of *Vistas in Astronomy*. For the past several years, he has acted as a reporter and associate Producer for Audio Medica in London, which is mainly concerned with the *Audio Journal of Oncology*. He lives in Soham with his wife, Barbara, and is active in community affairs.

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A FAIR USE OF ARCTURUS: A SYZYGY OF SCHOLARIANS AND THE LIGHTING OF THE CHICAGO CENTURY OF PROGRESS EXPOSITION, 1933-1934[1]

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Abstract: The objective of this paper is to relate the story of a small group of former Yerkes Observatory astronomers, who cooperated in the lighting of the 1933-1934 Chicago Century of Progress Exposition. It is also the story of the use of an early electronic instrument, a photo-electric cell, and utilization of the light from the star Arcturus, to signal the lighting of the Fair, thus furthering the application of astrophysics for the general public.

Keywords: Chicago Century of Progress Exposition, 1933-1934; Yerkes Observatory; photo-electric cell; Arcturus.

1 THE CENTURY OF PROGRESS EXPOSITION, CHICAGO, 1933-1934

The nominal objective of the Century of Progress Exposition was to celebrate the centennial of the founding of the city of Chicago (Figure 1). However, the early planners of the Exposition wanted to set a grander central theme which would have broader public appeal.

In 1927 the industrialist Rufus Dawes became President of the Board of Trustees of the newly-chartered 'Chicago Second World's Fair Centennial Celebration' Committee, the purpose of which was to promote and implement the Fair. Dawes immediately began to search for a unique central theme. In the autumn of 1927 he met with George E. Hale, the former Director of both Mount Wilson and Yerkes Observatories and a prime mover in the founding of the National Research Council. He suggested focusing the upcoming exposition on "… the services of science during the past hundred years." It was Hale's suggestion that later evolved into an official theme for the Fair, which was "… the dramatization of the progress of civilization during the hundred years of Chicago's existence." (Rydell, 1993: 93-94).

After accepting the idea, the Committee began to search for ways to interpret this theme. In 1931, Edwin Brant Frost, the Director of Yerkes Observatory, suggested to Philip Fox (Director of the newly-opened Adler Planetarium) that the Committee should try to symbolically link the 1893 Chicago World's Colum-bian Exposition and the 1933 Chicago Century of Progress Exposition. They could do this by capturing the light from the star Arcturus (then thought to be forty light years distant) with a photoelectric cell and use this to generate a current that could be transmitted from Yerkes Observatory to Chicago to signal the lighting of the Fair.

The idea was accepted by the Committee and the project was soon underway. The work of implementation fell to the Yerkes Observatory staff and involved several American astronomers located across the country, as well as Fair committee members, who were also, at one time or other, associated with Yerkes Observatory.

2 EARLY ROOTS OF THE PROJECT

The roots of this project can be traced back forty years to the display of the mounting of the world's largest refracting telescope at the 1893 Chicago World's Columbian Exposition and to the subsequent opening of Yerkes Observatory in 1897.

Figure 1: Sheffer's, *World's Fair (Spirit of Chicago)*, poster ca 1933 (reproduced from 2000 Poster Plus, Chicago).

2.1 The World's Columbian Exposition, Chicago, 1893

The World's Columbian Exposition (Figure 2) has been claimed by some scholars to be the greatest world's fair of all time. It was certainly the greatest pre-World War I fair and a watershed in American history because of its influence on American culture and ideas. It was also a forerunner in that it displayed many American achievements in science and every day life (e.g. see Adams, 1996: xix). It was created to celebrate the 400th anniversary of Columbus's journey to the new world, was open from 1 May 1893 until 30 October 1893, and was located on a 686 acre site on the south side of Chicago in the vicinity of the present day University of Chicago campus.

The largest major building at the Exposition was the Manufactures and Liberal Arts Building which was 1,687 feet long, 787 feet wide, and 236 feet high. It still holds the record as the largest roofed structure in history. Among the thousands of exhibits was the giant mounting for the 40-in Yerkes telescope (Figure 3), made by Warner & Swasey (Anonymous, 1893a, 1893b, 1893c; To Pierce ..., 1893; Work nearly done ..., 1893). When completed, this instrument would be the largest refracting telescope in the world.

2.2 The Yerkes Telescope

The 40-in telescope was scheduled to be the centerpiece of the new Yerkes Observatory (Figure 4), and was funded by Charles Tyson Yerkes (1837–1905), a wealthy Chicago financier with vast holdings in transit companies. He had been persuaded to provide the necessary finance by George Ellery Hale and University of Chicago President, William R. Harper (see Osterbrock, 1997).

2.3 George Ellery Hale

George Ellery Hale (1868–1938) was a Chicago-born American astronomer (Figure 5), and was the son of a rich Chicago engineer-salesman who made his money in the booming elevator business after the Great Chicago Fire of 1871. Hale was an early faculty member of the University of Chicago, and was brilliant at persuading other Chicagoans to invest in his astronomical projects. He first made his reputation as a solar observer, inventing the spectroheliograph and discovering the magnetic fields of sunspots (Wright, 1966; Wright, Warnow and Weiner, 1972).

In 1897, the Yerkes Observatory at Williams Bay (Wisconsin) was completed with Hale as founding Director, the 40-in telescope was installed, and a faculty in astronomy was assembled. One of Hale's earliest appointments was Edwin B. Frost, who had attended the World Congress of Astronomy, Astro-Physics and Mathematics, which was held during the World's Columbian Exposition in 1893 (Osterbrock, 1993).

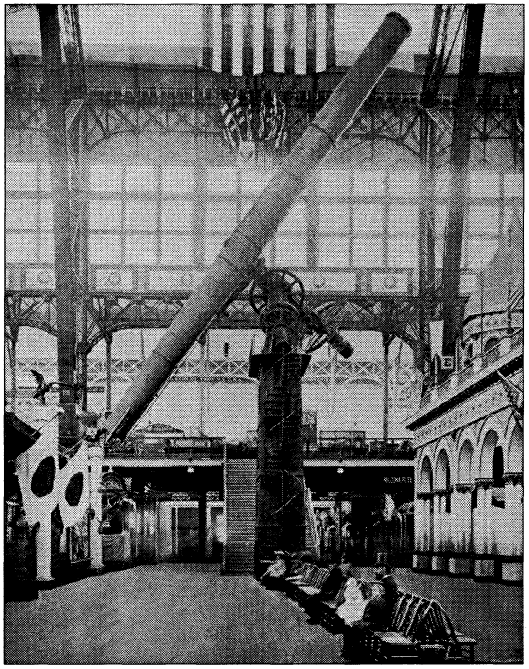

Figure 3: The 40-in Yerkes Telescope mounting and tube at the 1893 World's Columbian Exposition (after Yerkes Observatory Virtual Museum: Timeline, 1893).

Figure 2: The Grand Basin of the 1893 Columbian Exposition (courtesy of the Chicago Historical Society).

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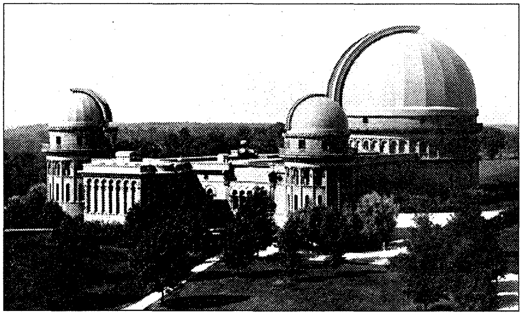

Figure 4: Yerkes Observatory in the 1930s (courtesy Yerkes Observatory).

Hale continued to develop the spectroheliograph, as a means of studying the Sun's atmosphere, but he later became involved in the construction of the 60-in and 100-in reflectors at Mount Wilson Observatory, near Pasadena, California. In 1903, when he left Yerkes to head the staff at Mt. Wilson, Edwin B. Frost (Figure 6) became Acting Director and later Director of the Yerkes Observatory.

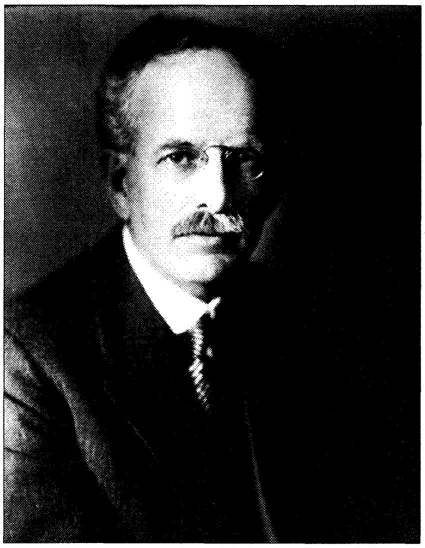

Figure 5: George Ellery Hale (courtesy Observatories of the Carnegie Institution of Washington).

2.4 Edwin Brant Frost

Edwin Brant Frost (1866–1935) graduated from Dartmouth College in 1886, where he specialized in physics. He then studied astronomy at Princeton University under Charles A. Young, and became interested in solar spectroscopy. He went on to study with Herman C. Vogel at the Potsdam Observatory. Returning to America, he taught sporadically at Dartmouth from 1892 until 1905. Between 1898 and 1905 he was Professor of Astrophysics at Yerkes Observatory, and became Director in 1905 (after Hale had moved to California). He remained Director of Yerkes Observatory until 1932, when he retired. His principal research was in stellar astronomy. Because of his very poor eyesight he was known as 'The Blind Astronomer'. He died in Chicago in 1935 (Obituary ..., 1935).

Figure 6: Edwin Brant Frost (after Yerkes Observatory Virtual Museum: People, Frost).

During Frost's tenure at Yerkes he became friends with, supported, or employed, numerous astronomers who would later play a part in the Century of Progress Exposition. These included Philip Fox, Henry Crew and Forest R. Moulton, all whom were involved in the 'Arcturus Project".

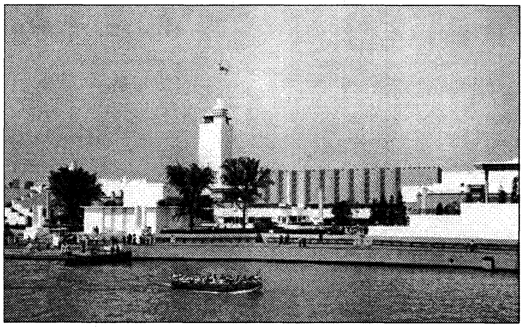

Figure 7: The Court of Honor of the Hall of Science (courtesy Chicago Historical Society).

3 THE ARCTURUS PROJECT AND ASTRONOMY EXHIBITS AT THE CENTURY OF PROGRESS EXPOSITION

Hale's 1927 idea of a science-oriented fair was picked up by other scientists and culminated in the endorsement of the theme by the National Research Council, which formed a special Science Advisory Committee for the new Fair, to make recommendations regarding the exhibits. It was decided that the exhibits were to show, "... only the fundamental discoveries, or milestones, that marked the path scientists had traveled in their search for knowledge." (Lohr, 1952: 117).

The principal building at the Fair was to be the Hall of Science (Figure 7). In it were exhibits and demonstrations displaying the unity of mathematics, physics, chemistry, biology, and geology, plus exhibits dealing with medicine. Henry Crew, then a Professor of Physics at Northwestern University, became Chief of the Division of Basic Sciences, and he was placed in charge of the exhibits in the Hall of Science.

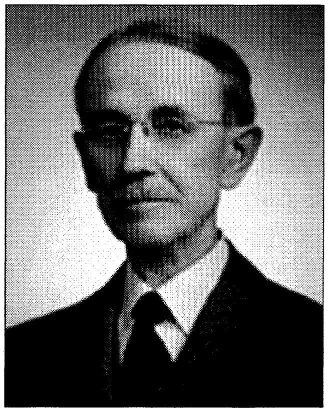

Figure 8: Henry Crew (courtesy American Institute of Physics ESVA).

3.1 Henry Crew

Henry Crew (1859–1953) (Figure 8) was born in Richmond, Ohio. He received his A.B. from Princeton University in 1882. He then received a graduate fellowship from Princeton to study physics, and in 1883 he obtained another fellowship to study under Hermann von Helmholtz in Berlin. He returned to the U.S.A. and received his Ph.D. under Professor Henry Rowland at Johns Hopkins University in 1887. He then taught physics at Johns Hopkins and at Haverford College. In 1891, Crew accepted a position as astrophysicist at the Lick Observatory in California. In 1892, he became Fayerweather Professor of Physics at Northwestern University and Chairman of the Physics Department. In 1930, he took a leave of absence from the University to perform his Exposition activities. He is best known for his work in the field of spectroscopy, its application to astrophysics, the building of new spectrographic instruments, and his work on solar spectra. He also had a deep interest in the history of physics, and published a translation of Galileo's *Two New Sciences* and the *Rise of Modern Physics*, as well as several textbooks that were widely used in physic classes. He was also an Associate Editor of the *Astrophysical Journal* for over thirty years. A close friend of George E. Hale and many contemporary astronomers and physicists, he continued to live in Evanston, Illinois, and died on 17 February 1953 (American Institute of Physics, Center for the History of Physics).

Despite Crew's strong affinity for astrophysics, there was a conspicuous absence of major exhibits on astronomy in the Hall of Science. The reason for this is best understood by quoting from *The Official Handbook of Exhibits in the Division of the Basic Sciences*:

> The Basic Science of Astronomy has not been included in the Hall of Science because it is completely and beautifically (*sic*) exhibited in the permanent Adler Planetarium. Professor Philip Fox, Director of the Planetarium, has cooperated so wholeheartedly with the Exposition that nothing is left to be desired in the way of an astronomical section. The Planetarium is located within the Exposition Grounds on Northerly Island, and is available to all visitors (Official Handbook, 1933: 19; Fox, 1933).

Exceptions were the display of a model of Galileo's telescope, a few transparencies of a spiral nebula taken with the reflecting telescope at Mount Wilson, and two movies, one titled "A Motion Picture Journey to the Moon" and the other "The Solar Eclipse of August 31, 1932". Related, however, was the commissioning of a series of books, "... by well-known scholars presenting the essential features of those fundamental sciences which are the foundation stones of modern industry." This series included a work on the state of astronomy at the time (see Baker, 1932).

To accommodate the Arcturus project, the Adler Planetarium, on the northern edge the Fair grounds, was to have a working model illustrating the method by which the light from the star Arcturus would be caught and turned into energy, which was transmitted to the Fairgrounds to turn on the lights at the Fair (Fox shows how, 1933).

3.2 Philip Fox

While the technical coordination of the Arcturus Project was in the hands of Edwin B. Frost, the negotiations with the Fair authorities were largely taken over by Philip Fox (1878–1944) (Figure 9), then Director of the new Adler Planetarium. Fox was a strong advocate for the popularization of astronomy, and he realized that the use of the light of a star to open the Fair would have great public appeal.

Fox was born and raised in Manhattan, Kansas. He attended the Kansas State Agricultural College and received a Bachelor's degree in mathematics. He enlisted in the Army during the Spanish-American War and was elevated to Second Lieutenant. He was discharged with a medical disability and he returned to Kansas State College where he earned a Master's degree in 1901. At the urging of his cousin, Ernest Fox Nichols, who was head of the Physics Department and later President of Dartmouth College, Fox was persuaded to seek a career in astronomy. At Dartmouth he became a student of Edwin B. Frost and earned a second Bachelor's degree, this time in physics. Later when Frost moved to Yerkes, he gave Fox a graduate fellowship at the Observatory. Fox was subsequently awarded Honorary Doctorates by Drake University and Kansas State College, in 1929 and 1931, respectively.

In 1909, Fox became Director of the Dearborn Observatory of Northwestern University at Evanston, Illinois. Later, he became Chairman of the Department of Astronomy. There, he worked with Henry Crew, head of the Physics Department. Except for a period of military service during WW I, where he achieved the rank of Colonel, Fox remained at Northwestern until 1929. He then took over the planning of the Adler Planetarium, the first planetarium in the United

States (Figure 10). He remained as Director until 1937, when he became Director of the Museum of Science and Industry in Chicago, a position later held by Major Lenox R. Lohr the General Manager of the Century of Progress Exposition (Menke, 1987).

Fox had one other connection with the astronomy establishment. He was married to Ethel Snow of Chicago who, in turn was related to Helen E. Snow, the benefactor who had given George E. Hale the funds for construction of the Snow Telescope which was first erected at Williams Bay, Wisconsin.

3.3 Assurance of Participation by the Yerkes Observatory

By the early 1930s, the Great Depression was already taking hold. Plans for the Fair included some compensation for the Adler Planetarium, as a percentage of the profits generated from the Fair concessions. However, the Yerkes Observatory was not originally included in a compensation plan and the monetary outlook for Yerkes' participation did not look good. By late 1932, Frost had retired from the Directorship of the Observatory and Otto Struve (1897–1963) was now in charge. Struve was somewhat skeptical about using research funds for 'popular science projects', especially now that the Depression was taking hold. This is where another former astronomer with ties to the Yerkes Observatory helped out. University of Chicago astronomer Forest R. Moulton (1872–1952), was appointed to the Fair's Executive Committee and placed in charge of Fair Concessions. Moulton was an innovative administrator and aided in setting Yerkes' share of the potential Fair receipts at 3%. With this assurance Struve, allowed the project to continue under Frost's direction (Osterbrock, 1997).

3.4 Forrest R. Moulton

In 1894 Moulton (Figure 11) graduated from Albion College in Michigan, and he entered the University of Chicago as a graduate student in 1895. In 1899, he received a Ph.D. in astronomy and mathematics, *summa cum laude*. His was the first Ph.D. that the Astronomy Department had produced. He continued to teach at Chicago, rising in rank from Instructor to full Professor by 1912. He specialized in theoretical astronomy and became the Department's chief representative on the Chicago campus. He also worked closely with Thomas C. Chamberlin (1843–1928), then a Professor in the Geology Department. Together they came up with the 'Chamberlin-Moulton planetessimal hypothesis' for the origin of the Earth.

Moulton was also a prolific writer on popular astronomy, popular science and mathematics. Edwin Hubble (1889–1953) was one of his undergraduate students. During WWI, Moulton served as a Major for the Ordnance Department, developing mathematical methods for computing artillery firing tables. After the War, because of his aggressive advocacy for popular science and popular astronomy, he realized he had little chance of becoming head of the Astronomy Depart-ment at Chicago, and resigned his academic position to become Director of the Utilities Power and Light Company, a holding company in Chicago. But because of the Great Depression and the collapse of holding companies, he was soon out of a job.

Figure 9: Philip Fox (courtesy Yerkes Observatory).

In July 1931, a Concessions Department was set up for the forthcoming Century of Progress Exposition, and Moulton was given charge of it.

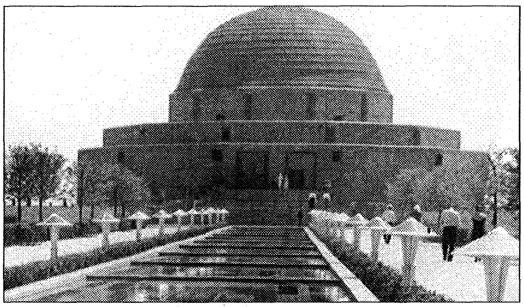

Figure 10: The Adler Planetarium and the Terrazzo Promenade, 1933 (courtesy Adler Planetarium).

After the Fair was over, Moulton became Permanent Secretary of the American Association for the Advancement of Science, and he played a role in the publication of its magazines, *Science* and *The Scientific Monthly* (Osterbrock, 1997: 164-167).

Figure 11: Forest Ray Moulton (courtesy Yerkes Observatory).

4 DEVELOPMENT OF THE ARCTURUS PROJECT

With the major monetary obstacles out of the way, Frost could now proceed with the Arcturus Project.

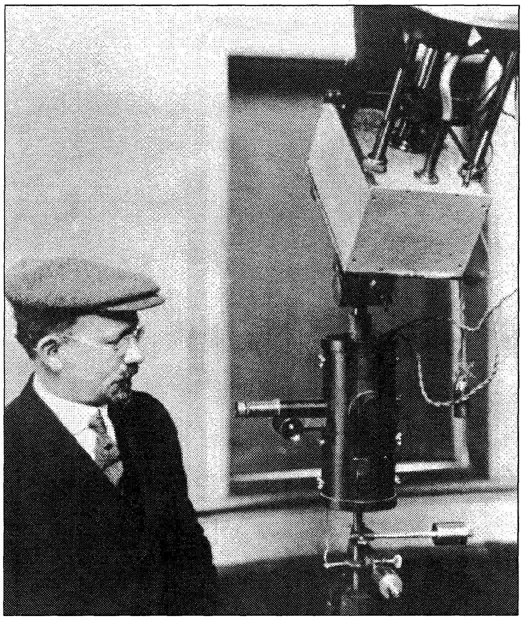

Figure 12.: Joel Stebbins (courtesy University of Illinois).

4.1 Arcturus

As stated earlier, the management of the Fair wished to make a dramatic connection between the World's Columbian Exposition, which had been staged, forty years earlier, and the new Century of Progress Exposition. At the time, it was estimated that the star Arcturus was 40 ly distant from Earth. Fair promoters could make a claim that the light from the star had left it at the time of the last Fair and arrived in time for the new Fair. Arcturus is the brightest star in the constellation Bootes. It is one of the nearest bright stars to Earth and its actual distance is 36-37 ly. It is pale orange in color and visible to the naked eye, especially in the Summer sky.

Figure 13: Christian T. Elvey (courtesy Geophysical Institute, University of Alaska).

Frost, in his role at Yerkes, was aware of current research on the radiation from stars including Arcturus. Two friends of his, with early connections to Yerkes, had done research using Arcturus as an easy-to-find astronomical reference. One was Ernest F. Nichols of Dartmouth. Nichols had visited Yerkes as early as 1900, to perform experiments in detecting heat from the stars. Using an improved radiometer, Nichols was able to detect a measurable difference between the radiation of Arcturus and Vega as part of his research (Frost, 1933: 126).

The other astronomer was Walter S. Adams, also a graduate of Dartmouth. Frost had offered him a fellowship at Yerkes in 1899, and worked closely with him. Adams later followed Hale to Mount Wilson. Using the Snow telescope, Hale and Adams began obtaining high-dispersion spectrograms of the Sun and of the bright stars Arcturus and Betelgeuse (Osterbrock, 1997: 60-61). In 1923, Adams became Hale's successor at Mount Wilson.

4.2 The Photoelectric Cell

Frost's idea of utilizing a photoelectric cell did not come from out of the blue. In the early 1930s, Frost was approaching retirement age and was considering a successor to his Directorship of the Yerkes Observatory. His principal choice was Joel Stebbins (1878–1966) (Figure 12). Stebbins at the time was a professor at the University of Wisconsin-Madison. Frost had tried to get him to join the Yerkes facility as early as 1926. Stebbins was also a friend of Hale and Adams. In 1907, Stebbins, then at the University of Illinois, became an expert in the new field of photoelectric photometry. He first used a selenium photoresistive cell to quantitatively measure the brightness of stars by the weak electric currents their light produced, when collected by a telescope. In 1912, he began to use the more sensitive potassium hydride cells. In 1922, he left Illinois to direct the Washburn Observatory in Wisconsin. Both Illinois and Wisconsin developed highly-skilled techniques in photoelectric photometry to produce valuable scientific results (Liebl and Fluke, 2004).

In 1929, Frost, with funds from the Julius Rosenwald Fund, arranged for Stebbins to construct a photometer for the 40-in refractor, and Stebbins spent the summer at Williams Bay installing it and training Christian T. Elvey, who was then a graduate student at Yerkes, in its use.

4.3 Christian T. Elvey

Christian Elvey (1899–1970) (Figure 13) was a graduate of the University of Kansas. When Otto Struve became head of the Yerkes Observatory, Elvey became his first Ph.D. student. After Stebbins set up the photoelectric cell, he took over the operation of the instrument as part of the regular research program with the forty-inch telescope. Elvey later became an eminent astronomer and geophysicist, serving as Assistant Director at the McDonald Observatory, Texas, and during WW II while at Caltech he worked on various rocket projects. After the war he worked at the Naval Ordnance Test Station at China Lake, California, doing research on the upper atmosphere. In 1952, he joined the University of Alaska as Director of their

Geophysical Institute and remained at the University until 1967.

5 DETAILED PLANNING FOR LIGHTING OF THE FAIR

As the date for the Fair's opening approached, Frost's health began to deteriorate. His eyesight became worse and he was left almost completely blind. In October 1931, he suffered a severe gallbladder attack that left him hospitalized for weeks. He retired as Director of the Yerkes Observatory in mid-1932. Elvey would be placed in charge of converting the captured light from Arcturus to signal to light the Fair, but Frost would continue as titular head of the project (Figure 14).

5.1 The Back-up Telescope Systems

Because of the fear that a cloudy night in Wisconsin might prevent the viewing of Arcturus, a back-up plan had to be developed to insure that the event would be carried out successfully. Several plans were suggested, including one by Harvey C. Rentschler, then Director of Research at the Westinghouse Lamp Company, where upon the light from Arcturus would be "... trapped by absorption in a phosphorescent substance and frozen in liquid air ..." and then released at the appropriate time (Scientist Planning, 1932). However, this approach was not adapted.

By 1932, however, other large observatories throughout the world had adapted photoelectric cells to their telescopes for research. It was decided for the purposes of the project that a network, connected via Western Union Telegraph lines, be set up to transfer the electric currents generated by Arcturus at four observatories within the Eastern and Central Time Zones of the United States. These were the Yerkes Observatory at Williams Bay, Wisconsin; Harvard University Observatory at Cambridge, Massachusetts; Allegheny Observatory at Pittsburgh, Pennsylvania; and the University of Illinois Observatory at Urbana, Illinois.

Since funds as well as observing times on the Yerkes telescope were limited, it was inconceivable that the observatories could be relied upon for the nightly lightings of the Fair throughout the season. As a result, arrangements were also made to utilize a reflector telescope with a 20.5-inch mirror, located at the Elgin National Watch Factory in Elgin, Illinois, some thirty-five miles west of Chicago. The owner and maker of the telescope was Professor Arthur Howe Carpenter of the Department of Metallurgy at the then Armour Institute of Technology in Chicago. Carpenter (Anonymous, 1934) was a metallurgist, and he was a specialist in the making of astronomical speculums (Figure 15).

6 THE OPENING CEREMONIES, 27 MAY 1933

On the opening day of the Fair, nearly 30,000 people crowded into the court yard of the Hall of Science (Figure 16).

The opening ceremonies were divided into two parts. In the morning, there was a parade of five hundred persons dressed in national costumes and carrying flags of forty different nations. This was followed by speeches by Postmaster General James A. Farley, Board President Rufus Dawes, Chicago Mayor Edward J. Kelly and presentation of the Queen of the Century of Progress.

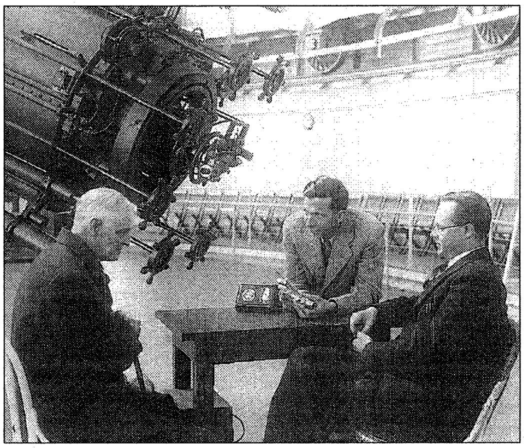

Figure 14: Edwin B. Frost, Christian T. Elvey and Otto Struve at the 40-in refractor with the photocell that converted the light from Arcturus to an electrical signal to start the Chicago World's Fair. 1933 (courtesy Yerkes Observatory).

The evening program was devoted to lighting of the Fair, symbolically linking the two Chicago fairs via the light from Arcturus. Phillip Fox was the Master of Ceremonies. To open the ceremonies, there was a concert given by the Chicago Symphony Orchestra, a 2,500 voice chorus, and a rendering of the National Anthem by opera star Lawrence Tibbett. This was followed by speeches from Dawes, Frost and Fox. The program was broadcast over Chicago radio station WGN.

Figure 15: The daily link between Arcturus and the Fair (after *Scientific American.* 151(1): 60, 1934).

7 TURNING ON THE LIGHTS

Finally, the moment arrived. Philip Fox, now took charge of the proceedings (Figure 17). In his book

Fair Management, Lenox R. Lohrv (1952: 198) describes the climax of the ceremony:

> In the courtyard of the Hall of Science at twilight a great hushed crowd waited, watching the great illuminated panel that towered above the rostrum. The lower half was a map of the eastern part of the United States showing the four observatories. The upper half contained the instruments that closed the circuit.
> 'Harvard, are you ready?'
> 'Yes.'
> A red glow ran across the map from Cambridge to Chicago.
> 'Is Allegheny ready?'
> 'Is Illinois ready?'
> 'Yes.' 'Yerkes?'
> 'Lets go.'
> In the center of the flaming circle a star flashed out. The switch was thrown, and a searchlight at the top of the Hall of Science shot a great white beam across the sky. It circulated slowly from one exposition building to another and, at the touch of its finger, one after another burst into full brilliant illumination. Lights were turned on at 9:15 p.m. (Figure 18).

Figure 16: The Court of Honor during Opening Ceremonies (courtesy Chicago Historical Society).

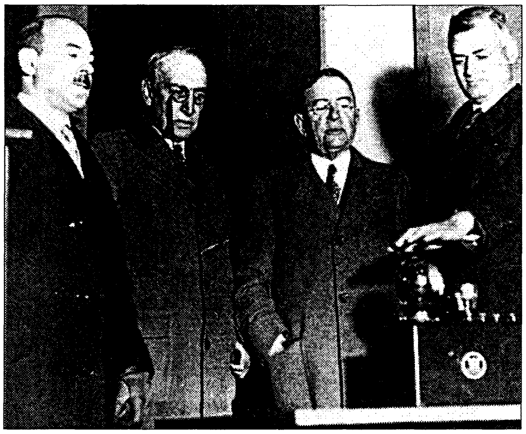

Figure 17: Turning on the lights, "A Scientific Miracle" (courtesy University of Illinois at Chicago, A Century of Progress Records).

Between 17 and 29 May 1933 the *Chicago Tribune* published numerous articles and photographs of the preparations for and opening day ceremonies of the Fair. Today, there are several commercially-available videos about the 1933 Century of Progress Exposition, showing opening day ceremonies and lighting of the Fair. Copies of the WGN broadcast are also available. For convenience a transcription of Edwin Frost's talk is printed below.[2]

8 EPILOGUE

Today, astronomical research is clearly divided into two major directions of endeavor. The oldest is observational astronomy, which is still primarily devoted to studies of the appearance, position, distance, and brightness of astronomical objects. Modern astrophysics, however, began with the developments of spectroscopy, photography, electronics, and other more recent technologies developed for research by the Department of Defense and the CIA. Through the use of these newer technologies, employed by ground-based telescopes and space vehicles, most of today's astronomical research is focused on measuring the compositions, temperatures and motions of astronomical objects.

With the lighting of the Chicago Century of Progress Exposition in 1933-1934, classical astronomy demonstrated a shift into the realm of modern astrophysics. Time was ending when a 'syzygy' of close-knit friends and colleagues within the profession could be relied upon to produce new understandings in the field. Directors of observatories and their sponsors realized that they had to hire professional scientists from disciplines other than astronomy to enhance astronomical research. Soon the sciences of electronics, geology, chemistry, physics, space science and information technology all became part of the astrophysics endeavor.

While these developments put most astronomical research physically and financially out of reach for the average citizen, through stunts like this, they became further aware of the vastness of the Universe and all that it contains. This also, however, brought a greater number of talented amateurs into the field, as new technologies became available off-the-shelf.

New insights and techniques also played a part in the gradual acceptance by the average American, of the spiraling costs necessary in maintenance of America's reputation in science.

This path was not altogether smooth. In 1939, for the opening of the New York World's Fair, elaborate ceremonies were staged, to illuminate the Fair with cosmic rays, just as Arcturus was used at the Chicago Century of Progress Exposition. The ceremonies proved to be a comedy of errors. First Albert Einstein, against his better judgment, delivered an introductory talk on cosmic rays which could neither be heard nor understood due to a faulty amplification system and his heavy German accent.

Then, fair officials and participating scientists set about to capture the ten [cosmic] rays needed to provide the requisite power, in a feat described as 'modern temple magic'. Ringing bells and flashing lights signaled the capture of each ray. But when the tenth ray was captured and the switch thrown to illuminate the huge light on the Trylon, the electrical system overloaded, which caused a power failure. Disappointed, the crowd turned its attention to the color, light, and sound display in the Lagoon of Nations. As the *New York Times* reported the scene: "The crowd dropped science in favor of a spectacle that they could applaud." (Kuznick, n.d.; Marche, 2005: 80-81).

An opening of another World's Fair twenty-nine years after the Century of Progress Exposition was more successful. This was the 21st Century Exposition in Seattle in 1962, the first major American World's Fair since WW II. America and Russia were then at the height of the Cold War. Russia had launched the Sputnik satellite in 1957, to the embarrassment of the American space program and public. America was looking to do something to regain face. Planners of the Fair again looked back at the event that sparked the opening of the Chicago Fair in 1933. This time a radio signal from a supernova remnant was used. When President John F. Kennedy pressed a key on 21 April 1962, it activated a computer in Andover, Massachusetts, that focused a radio telescope on the radio source Cassiopeia A. When the telescope picked up a radio signal from the object this was sent to Seattle to successfully open the Fair.

Sometimes, good ideas are worth using over and over again.

9 NOTES

1. Syzygy is a Greek word meaning "a yoking together" or "in line." Philip Fox, when a member of the Yerkes Observatory staff, utilized the word as a password or introduction for academic friends or astronomers (Frost, 1933: 207).

2. The text of Edwin Frost's address reads as follows:

It has seemed appropriate to add a celestial touch to this ceremony of illuminating the Century of Progress by employing the light of the great star Arcturus in the constellation Bootes. In so doing we are literally following the precept of Emerson to 'hitch your wagon to a star.' Arcturus is one of the brightest stars in our summer sky, and has been known since men first turned their eyes in wonder toward the glories of the heavens. This star is a yellow sun, having the diameter about twenty-five times greater than that of our sun and radiating about one hundred times as much light.

It is highly probable that Arcturus has been sending out its light toward us and in all other directions for thousands of millions of years. It has reached a stage of development not unlike that of our Sun, and we know that it contains the same chemical elements that occur in our Sun and that are found upon the Earth.

We astronomers call it one of the nearer stars because among the billions of stars forming our galaxy, or the system of the Milky Way, there are hardly more than on hundred known to be nearer to us than Arcturus. However, its distance is enormous, the star being more than two million times as remote as our Sun, or about 225 million, million miles away.

The vibrant waves of light, surging toward us at the rate of 186,000 miles per second, or eleven million miles a minute, require about forty years to span this bit of the void of space. Therefore the light rays reaching our telescopes tonight and actuating our photo-electric cells, left Arcturus at the time when the eyes of the civilized world were turned toward this central city of our continent at the great Columbian Exposition of 1893.

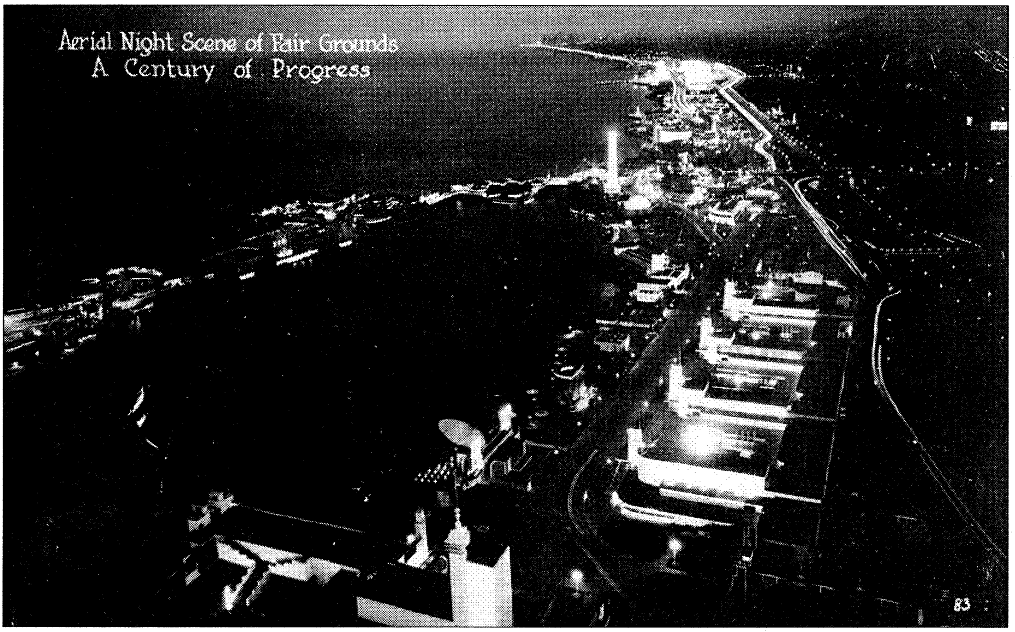

Figure 18: Night view of World's Fair grounds from the observation platform of the sky ride (Postcard WF 41).

One of the striking exhibits at that Exposition was the great forty-inch refracting telescope which had just been presented by Mr. Charles T. Yerkes to the new University of Chicago to serve as the principal research instrument of the Yerkes Observatory, which was soon to be erected at Williams Bay, Wisconsin, on the shore of Lake Geneva. This great refractor still remains the largest of its kind yet constructed.

As the idea of 'borrowing a light' from Arcturus originated with me at this Observatory, it has been the plan to use this telescope to catch the light of the star and to focus it upon the photo-electric cell which is to furnish the current needed for our purpose tonight. However, because of the danger of a cloudy sky at any one station, a circuit has been arranged to include the use of the telescopes at Harvard and at the Universities of Pittsburgh and Illinois to insure the success of our experiment.

The Columbian Exposition did not include the photo-electric cell among its many wonderful exhibits, but scientific men, especially in Germany, were already beginning to study the curious effect of light falling upon surfaces of alkaline metals whereby exceedingly feeble currents of electricity were generated. Impelled by an insatiable curiosity to understand the mysterious workings of nature, scientists have steadily continued this research during the two score years that the light now arriving from Arcturus has been coming toward us. And inventors, ever ready to make use of the discoveries in pure science, have helped to perfect its technique and have found many applications of this photo-electric cell, which in these days is often called 'the mechanical eye.' It has been of great service to astronomers in enabling them to measure with the highest precision the brightness of stars without depending upon the capriciousness of the human eye.

Hence science and invention have prepared the proper apparatus to receive the light from Arcturus and convert it into an electric current which, suitably amplified and transmitted over the telegraph lines to this Temple of Science, actuates the switch which will in a few moments start the illumination of this Exposition.

Is not this a fitting illustration of the amazing advances during this wonderful Century of Progress?

10 ACKNOWLEDGMENTS

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A BRIEF HISTORY OF THE ASTROPHYSICAL RESEARCH CONSORTIUM AND THE APACHE POINT OBSERVATORY

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Abstract: This history of the Astrophysical Research Consortium (ARC) and the Apache Point Observatory (APO) describes why and how the ARC was formed, the vision for the APO, and the technology used to implement that vision. In particular, it examines the building of a low cost, lightweight, f/1.75, 3.5 meter telescope with an experimental mirror cast at the Stewart Observatory Mirror Lab, and key features of remote observing, rapid instrument change and flexible scheduling. The organizational challenge of unifying distinct institutions and their astronomy programs, and the difficulty of gathering funds for this venture, are also explored. Key scientific results and achievements using the APO are noted. This paper is based on interviews with key personnel, documents in the ARC business files, and published papers and reports (including astronomy department annual reports).

Keywords: history, astronomy, Apache Point Observatory, remote observing, spincast mirror

1 INTRODUCTION

Like most human endeavor, astronomy depends on bigger and better tools to break through the frontiers of discovery and ensure the advancement of our knowledge. By the 1950's in the United States the biggest and best astronomy tools were concentrated in a handful of universities, guaranteeing the astronomers associated with them the best opportunities for new discoveries. Although there were a number of fine observatories supporting excellent astronomy programs, it was hard to compete with the Hale 200-inch telescope at the California Institute of Technology's Palomar Observatory and the 100-inch Hooker Telescope at the Carnegie Institute of Washington's Mt. Wilson Observatory or even the University of California's 3-meter reflector at the Lick Observatory, which began operation in 1960. The size of their departments and their ability to raise private funds ensured their continued leadership in the ever more expensive world of bigger and better telescopes (McCray, 2004).

With National Science Foundation (NSF) support and encouragement, a number of institutions in the United States formed the Association of Universities for Research in Astronomy (AURA) on 28 October 1957 to create and manage a national US optical observatory available to all US scientists based on the scientific merits of their proposals (Edmondson, 1997). Government funding made excellent telescopes available to astronomers with good research ideas, who would not otherwise have access to the equipment needed. Although serving an important need, the AURA national observatories do not adequately support the needs of a university astronomy department to implement long-term observing programs (York, 2004) that strengthen the department by attracting top faculty, graduate students, and post-docs.

Desirous of a first-class observatory for long-term programs, yet recognizing that none of them individually could fund or fully utilize it (Wallerstein, 2004), New Mexico State University, Princeton University, the University of Chicago, the University of Washington, and Washington State University formed the Astrophysical Research Consortium (ARC) in 1984 in order to create an observatory that would provide telescope time to each member university based on its investment (ARC Agreement, 1984). Figure 1 shows the Apache Point Observatory, which was ultimately built by the consortium. The cost of the biggest and best astronomical instrumentation has grown so much that today almost every new telescope project is a cooperative effort. Modest ones require a small group of institutions like ARC, while ambitious ones might require the cooperation of nations. Today's models of cooperation rely on the pioneering steps by groups like the ARC, where each institution's individual needs, dreams, and ambitions have been accommodated and unified into a single vision. How they came together, how they worked together, and what they created provide insights into the science and technology of astronomy today, the business of astronomy today, and the human effort required to implement a vision.

2 THE LONG PATH TO AN OBSERVATORY

Prior to the formation of the ARC in 1984 none of the consortium members had telescopes with apertures exceeding 1.0-meter, and their observatory locations were not ideal. In general, a common desire to gain access to a larger telescope in a better site brought these universities together, but it was a long and bumpy road even to get started. Since the astronomers at the University of Washington initiated the process, starting with their story provides the best illustration of how this group was eventually formed.

2.1 University of Washington (UW)

In 1965 the UW decided to expand its Astronomy Department and hired Paul Hodge and George Wallerstein (both from the University of California at

Berkeley) to join Theodor Jacobsen, the sole UW astronomer since 1928. As observers used to accessing excellent telescopes, Hodge and Wallerstein soon began to plan for an observatory and that first year they hired Ed Mannery to help with site selection and optical design. Three issues soon became apparent. Firstly, Washington State did not offer a suitable site for an outstanding observatory. Secondly, funding for the telescope required a larger resource base, especially since State funds would be hard to get for an out-of-State project. Finally, they needed a partnership with other astronomy departments because their small, but growing faculty would underutilize and have difficulty funding and operating a large facility (Wallerstein, 2004).

As early as October 1965 the UW Regents authorized construction of a large telescope using external (versus State) funds. Although Professors Wallerstein and Hodge entered into discussions with many potential partners in the following ten years, including the Jet Propulsion Lab and the University of Wisconsin at Madison, private funding at UW and the other institutions had not been secured and Federal funding from the NSF was channeled to other projects, like the National Observatory at Kitt Peak (ibid.). In the meantime, in 1971 the UW built a small observatory on Manastash Ridge in central Washington State that initially housed a 16-inch telescope, but was replaced a year later with a 30-inch telescope (ibid.). Additionally, as the Department continued to grow, its many observers came to rely heavily on the National Optical Astronomy Observatory facilities at Kitt Peak in Arizona and Cerro Tololo in Chile. By 1981, the UW ranked second in allocation of time among all US institutions and ranked first in per-capita allocation. Although the UW obtained a lot of observing time, the constraints were growing as the demand increased and NSF funding for Kitt Peak failed to keep pace. Three-quarters of all requests for time were denied, and programs like the one at the UW were in a difficult position as the growing team of astronomers and graduate students at the University realized that they could no longer rely on gaining access to NOAO facilities (Balick, 1981). They needed their own large telescope.

In 1975 Mr Alex Kane died and left an estate worth $250,000[1] for the purpose of building a telescope, so the UW finally had startup funds for a major telescope project. Kane, from Ashland, Oregon, had first offered the money to Oregon State University, but they told him they were not interested; the UW did not make the same mistake. But even with secure funding, finding a partner was not easy. The UW talked with Stanford University about locating a telescope on Mauna Kea in Hawaii, but Stanford could not justify the project without hiring four additional faculty members, so they dropped out of the discussion. Another possibility was acquiring a 40-inch telescope from the University of Vienna and partnering with them to build an observatory on Mauna Kea for it. In addition to the mediocre optics and awkward mount, the mirror was just too small (Wallerstein, 2004). The situation changed in 1978-1979 when Professor Bruce Balick began exploring a partnership with Howard University, New Mexico State University (NMSU), and Washington State University (WSU).

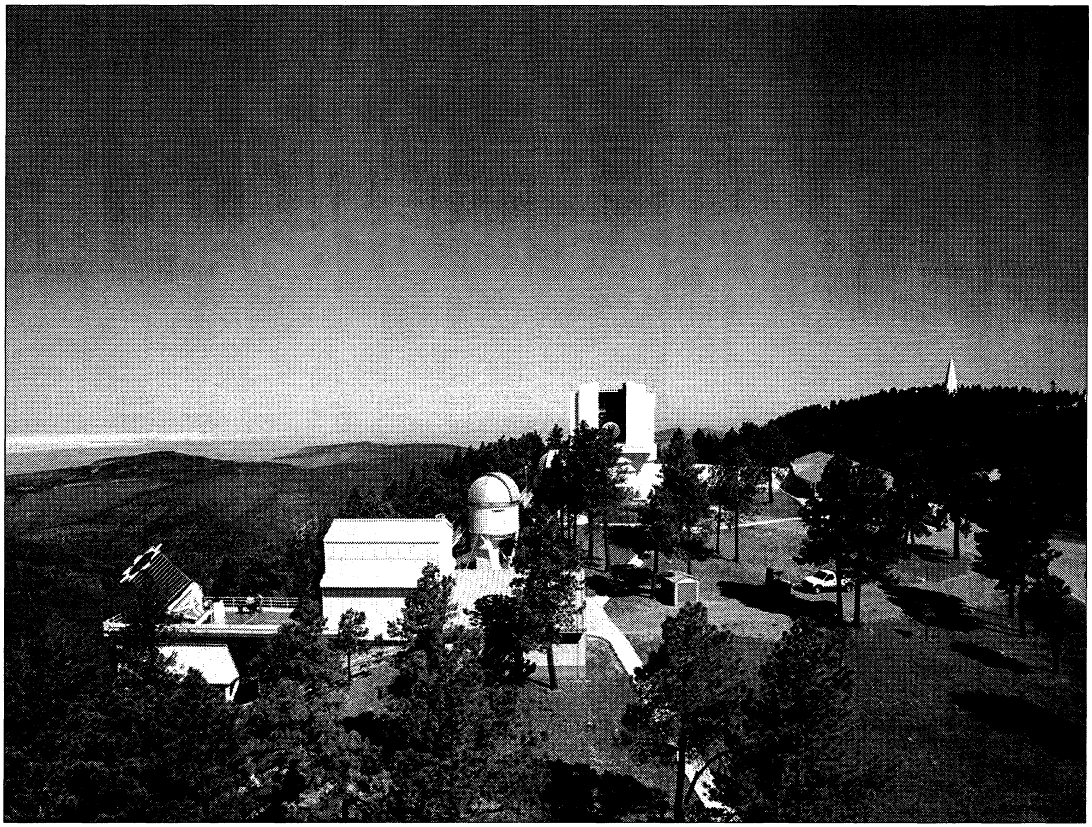

Figure 1: The Apache Point Observatory in 2000, including from left to right the Sloan Digital Sky Survey (SDSS) 2.5 meter, the SDSS 0.6 meter, the New Mexico State University 1.0 meter (hidden by a tree), and the ARC 3.5 meter telescopes (courtesy of ARC, photo by Dan Long, 2000).

Balick had been presenting ideas for an advanced technology telescope to groups around the country that, through personal and professional contacts, he heard might be interested in joining with the UW to build a telescope. The initial ideas for the new telescope came from the Kitt Peak Advanced Development Program and from radio astronomy. Balick's background in radio astronomy led him to invite Sebastian Von Hoerner and Wun Yuen Wong from the National Radio Astronomy Observatory in Greenbank, West Virginia, to visit Seattle for a week to help explore novel, low-cost, intensively-engineered approaches to optical telescope design by applying techniques from radio telescopes (Balick, 2004).

What emerged through the brainstorming with new partners at Howard, the NMSU and the WSU, and through the gathering of ideas from Kitt Peak and Greenbank, was a well-developed concept for a lightweight, 2-meter mirror only 6 centimeters thick, with a support that would use tube structure members on an altazimuth mount with a servo control system driven by computer for precision pointing. The lower mass of this structure meant lower thermal noise and lower cost. Overall dimensions were only half and the weight was just 10-20% of a traditional telescope of similar aperture, so a small and inexpensive building was possible. Instead of placing an instrument in the standard position behind the mirror, which causes load flexure during use and costly equipment changes when a new instrument is needed, up to four instruments could remain attached to the sturdy telescope mount. The incoming beam could then be redirected to the appropriate instrument. The group even found a cost-effective site at Sunspot, New Mexico, near the Sacramento Peak campus of the National Solar Observatory in a region officially protected for astronomical use. It had excellent seeing, on a par with any mainland site, and was close to support facilities, an airport and the NMSU. The overall project cost was estimated at $3.6 million, with a 15% error margin (Balick, 1981).

By 1981, armed with a well-conceived proposal and partners in the venture, the UW astronomers had good reason to be optimistic as they anticipated using their 40% share in a world-class telescope that would also attract other grants. They still needed approval from their own administration for the $1.44 million UW share in the project, but to help sway the argument they had already secured a sizable portion of the needed funds, including $300K from the Kane Estate (earning interest), $200K from UW matching funds, $100K from a Boeing pledge, and $100K from a Kenilworth Foundation pledge (ibid.). In particular, Malcolm Stamper, President of Boeing and a friend of the University, was very supportive (Balick, 2004). Both Wallerstein and Balick were ready to increase the fundraising effort, once approval was gained. In the meantime, their partners also enthusiastically pursued approval and funding for the project.

2.2 New Mexico State University (NMSU)

In the fall of 1978, the NMSU Astronomy Department had six regular faculty members and two emeritus, including Professor Clyde Tombaugh, the discoverer of Pluto (New Mexico State University, 1979). With a size similar to UW and also a heavy user of Kitt Peak, the Department recognized that to support faculty and graduate student research programs they needed to secure access to a 2+-meter class telescope. They could no longer rely on national facilities to meet their needs. Although they originally planned on building their own telescope and had actually been exploring sites, the advantages of a partnership that brought more resources and more personnel support convinced the NMSU to join with the UW (Anderson, 2004). The UW brought telescope-engineering expertise and the NMSU had site management capability.

Initially the NMSU thought they could contribute $500K in cash and provide a site and an empty operations building (the former NMSU Cosmic Ray Lab about three miles north of Sunspot) to meet a $900K commitment for 25% participation, but the partners decided that the Sunspot site was better and it was free (Balick, 1981). Actually, the NMSU had considered the Sacramento Peak area for a telescope as early as the 1960s and Professor Kurt Anderson had extensive meteorological data acquired by others since the 1950s, so the choice was supported scientifically (Anderson, 2004). As it stood in July 1981, the NMSU committed to $576K for a 16% share, but held out hope that it might raise more funds to buy a 25% share.

2.3 Washington State University (WSU)

Although the WSU had a small astronomy program with only two observers, Professors Tom Lutz and Julie Lutz, the UW invited them to join. The astronomers at both schools knew and liked each other and had collaborated in the past at both Kitt Peak and Manastash Ridge. Both groups realized the political advantage of the two Washington State research institutions working together to create a state-wide resource, even though that resource would likely be located in New Mexico. The WSU's astronomy program was part of the Mathematics Department at that time and had no specific plans to grow, but its observers would get a tremendous resource. The WSU could not add personnel expertise, but they could contribute modest funding for a small share of observing time, and they also agreed to help out where they could. For a 5% share, the WSU's commitment was $180K, and an initial part of the funding came from both the Graduate School and the School of Arts and Sciences (Lutz, 2004).

2.4 Howard University

In the late 1970s Professor Ben Peery moved from Indiana University to Howard University in Washington (D.C.) to start an astronomy program. Balick contacted Peery in hopes that Howard University might have an interest in a telescope project, since this would certainly help grow their new program. Peery was Howard University's only astronomer at the time and a member of the Physics Department, and he responded to his University's call for proposals to improve the graduate programs by suggesting that they buy a 30% share in the partnership with the UW, NMSU and WSU to build a new observatory. University officials liked the idea, and agreed to provide $1.08 million in funding if Congress would approve the budget. Howard University is funded directly by the U.S. Congress, and it had to convince Congress to support the project with an appropriation in the line item that funds them. Because it had such a small Department, Howard University, like the WSU, would only contribute money, not expertise (Anderson, 2004; Wallerstein, 2004).

By August 1981 the presidents of the UW, Howard University, the NMSU and the WSU had given tentative approval for the project and the UW attorney general was drawing up the actual agreement. The astronomers had even found a 2-meter mirror blank made of Cervit for $35,000. A new one would be $500K. It was available from Norman C. Cole of Tucson, who would also figure and polish it for $160K (Balick, 1981). Throughout the rest of the year an optimistic group waited for approval of Howard University's funding, the last roadblock. They even pooled money for a celebratory bottle of champagne. Alas, Howard University's request to Congress was mistakenly excluded from President Reagan's budget, and it never resurfaced. Although the University maintained an active role until the end, it had to drop out in early 1982 (Anderson, 2004). The project was then in jeopardy, and it might never have gotten back on track were it were not for a timely disappointment experienced by Princeton University.

2.5 Princeton University

In the late 1970s Princeton University submitted a proposal to manage Hubble Space Telescope (HST) data acquisition and reduction. This included a $1 million endowment for postdoctoral positions associated with the project, if the University won the contract. In January 1981 Princeton heard that they had not been selected, nor did they get approval to build the wide field camera for the HST. With balloon experiments winding down and an unsuccessful bid to enter space-based astronomy, the Astronomy Department decided to focus on ground-based astronomy, which pleased Professors Jim Gunn and Ed Turner, two observational astronomers who had recently joined the faculty (Gunn, 2004; Wallerstein, 2004).

Although Princeton University was not selected for the HST projects, the $1 million donation was still available to them. During a research-related visit to Princeton in early 1982, Wallerstein happened to mention that the UW was forming a consortium to build a 2.5-meter telescope and that Princeton's $1 million would buy a substantial share of telescope time. Notice, incidentally, that with the passage of time the mirror size continued to creep upwards in order to keep the telescope competitive (Wallerstein, 2004)! Professor Jerry Ostriker was Chairman of the Astronomy Department at that time and he liked the idea. He had hoped to build a much larger telescope, but realized that Princeton University did not have funding for it, so he asked Professor Don York to investigate participation in the UW project. Even though York moved to the University of Chicago soon after evaluating the consortium idea, Princeton University remained interested, but only at a $500K level (York, 2004). In addition, they lobbied for a larger telescope. Gunn felt that a 2- or 2.5-meter telescope would not give the consortium a leading edge in aperture, and he insisted on a mirror of at least 3 meters and with a wide field of view. At a Departmental meeting Gunn showed that a 3.5-meter mirror located in a good seeing location would perform as well as the Hale 200-inch telescope that he frequently used. Meanwhile, a telescope with that aperture would be the second largest university-owned optical telescope. Princeton University brought prestige, money and expertise to the project, and Gunn agreed to build a dual imaging spectrograph for it (Gunn, 2004; Wallerstein, 2004).

Now that the consortium expected to build a larger 3-meter, wide field, advanced design telescope, Balick, Ed Mannery and Walt Siegmund (from the UW's telescope engineering group) spent the remainder of 1982 working with Princeton University, the NMSU and the WSU on optical concepts that would deliver a 1° field of view with 0.2 arcsecond image quality (NMSU, 1983; UW, 1983). Moving up to a larger mirror increased costs, yet currently-planned contributions from the associated Universities did not even cover a smaller telescope. Clearly, another partner was needed.

2.6 The University of Chicago

The University of Chicago has a long association with astronomy and astrophysics, and its Yerkes Observatory features the world's largest refractor. From 1932 to 1962, the Astronomy Department also managed the MacDonald Observatory in Texas, which gave them access to the 2.1-meter Otto Struve Telescope. By 1982, though, access to newer and larger telescopes was more difficult, so the Department formulated a strategy to build instruments and trade their use off for observing time on large telescopes. In the fall 1982 Don York moved from Princeton University to the Astronomy Department at Chicago. Early in that academic year the Dean, Dr Stuart Rice, attended a Departmental meeting and suggested they build a large telescope (York, 2004). At this time Rice happened to be on the National Science Board, the National Science Foundation's (NSF) governing body, which had control over its budget and plans. In July 1982 he received a letter from Dr Leo Goldberg, a long-time leader of AURA and Kitt Peak, which discussed the issues of funding national ground-based telescopes, space telescopes, and private university telescopes. Goldberg suggested that perhaps the space telescopes should be the national telescopes and that the NSF should go ahead and fund other ground-based projects (McCray, 2004). This may have emboldened Rice to encourage the Astronomy Department to 'think big'. Fortunately, York had just explored this topic for Princeton University, and he was still enthusiastic about the UW-led consortium. He therefore had just the solution for the University of Chicago, and they decided to sign up for a share equal to the UW's. The team was formed; now they had to get started.

3 FORMATION OF THE ARC

Acquiring telescope time on a first-class telescope brought these institutions together, so equitably distributing the time and designing an effective form of governance was the first major administrative hurdle. Professor Bruce Margon, the UW Astronomy Department Chair at the time, took on this difficult task, and he and Don Baldwin (the UW Assistant Provost for Research) shepherded the process of gaining agreement while at the same time building an atmosphere of trust and mutual respect. It took most of 1983, but the Consortium Agreement signed by all members by 26 January 1984 and effective from 1 January, spells out the obligations of each member and allocates telescope time to each institution based on its contribution (ARC, 1984). The available observing time, after removing small allocations for engineering developments and Director's discretionary time, breaks down as follows: the UW 31.25%, University of Chicago 31.25%, the NMSU 15.625%, Princeton University 15.625%, and the WSU 6.25%. In addition, the Board of Governors included two representatives from each university—one scientist and one administrator/business person (York, et al., 1984). At a summer 1983 meeting at WSU, Julie Lutz proposed "Astrophysical Research Consortium" as the name for the new organization and this was agreed to (Lutz, 2004). The ARC was incorporated as a non-profit entity in Washington State on 26 June 1984 (UW, 1985), and it received non-profit status from the IRS on 25 October 1984 (BOG, 1984b).

Project progress also continued in 1983 with the final selection of the Sacramento Peak site near Sunspot, selection of a 3.5-meter mirror, and development of detailed concepts and budgets for the telescope, enclosure and site. The Sacramento Peak site had been tentatively selected early on because a year-long monitoring program indicated that median seeing approached one arcsecond (Beckers, 1979); it involved low costs; and the National Solar Observatory welcomed and supported the ARC as a neighbor. Nonetheless, NMSU's Professor W.L. Sanders continued site testing there, at the Cosmic Ray site nearby, at South Baldy near Soccoro, the NMSU Blue Mesa Observatory site, and at the Cloudcroft (New Mexico) 48-inch telescope Air Force site. M. Walker from the Lick Observatory consulted with Sanders and helped him use his site evaluation methods and a Walker-type telescope (NMSU, 1981, 1982, 1983). The Sunspot site tested positively, and the consortium members discussed naming it Apache Point. In researching the use of this name, Anderson (1983) determined that calling it Apache Point would not offend anyone, and that nothing else in New Mexico was using that name. Actual final approval to use the site came on 17 April 1985 when the Forest Service signed a use permit for Apache Point (Margon, 1985a). The story of the mirror and the observatory designs will be told later in this paper.

In 1984, with the ARC formed, the group could elect officers, make appointments, and actually begin spending their contributed resources to build their dream. In the first meeting, on 20 January, the Board of Governors voted Margon as the Chair and Baldwin as the Secretary/Treasurer of the ARC. In addition, York was appointed Director of the Observatory, Anderson was appointed Associate Director for the Site, Balick became Associate Director for the Telescope and Doyal A. (Al) Harper of Chicago became Associate Director for Instruments (BOG, 1984a; NMSU, 1985; UC, 1985; UW, 1985). Appendix 1 lists everyone who has served on the ARC Board of Governors. Interestingly, at its next meeting, in October, the Board decided that no outside oversight would be needed for the project (BOG, 1984b), even though by early 1984 the overall cost had already grown to a projected $10 million (as illustrated by the figures in Table 1).

Table 1: Project expenditure and sources of funds (after York, et al., 1984)

Expenditure	
	$8.8 Million
Faculty costs for which member institutions are not charging overhead.	$1.2 Million
Total:	$10.0 Million
Sources of Revenue	
Provided by the ARC using non-Federal funds.	$3.2 Million
From member institutions.	$1.2 Million
Requested from the NSF.	$5.6 Million
Total:	$10.0 Million

3.1 Fundraising

Obviously, the ARC expected to get significant support from the NSF. Funds provided by the ARC came from State sources and private donations to the member institutions, as described earlier. The NMSU, for example, got $800,000 of its share through an award of State bond funds set aside for graduate programs. The consortium's well-conceived proposal helped the Astronomy Department win a sizeable portion of the $5 million available to all of the NMSU departments (Anderson, 2004). Member institutions continued to look for sources to fund their individual membership dues and developed a plan for the ARC to approach national organizations for funds that would reduce dues on a pro-rata share basis (BOG, 1984a). Although individual members met with success, the coordinated effort from the ARC did not. A $100K donation from the Perkins Fund solicited by Princeton University on behalf of the ARC was at first thought to be a gift to the ARC and as agreed the funds would be used to reduce all member dues (Margon, 1985b). However, when Princeton University actually received the money they discovered that the Perkins Fund trustees had voted to donate it to University itself and not the ARC (Eggers, 1985). Despite the ARC's difficulty in raising private funds, it met with huge success in winning NSF funding.

Obtaining NSF funding, and the challenges of managing cash flow until the funding was received, occupied a large portion of the Board's efforts. Once the ARC was formed, proposed budgets for 1984 of $1,010,854 and for 1985 of $4,007,005 were put in place so that the engineering and construction teams could start designing and building the telescope and buildings, and could complete site preparation, such as roads and power (BOG, 1984b). Quarterly invoices to members for contributions provided the cash for expenses prior to NSF funding, which was anticipated to cover the 1985 budget. With rising costs, tough decisions had to be made, such as eliminating an aluminizing facility at the Observatory—but only after confirmation that Kitt Peak would be able to provide optical coating services at a reasonable cost (Jeffries, 1984).

3.2 The NSF Proposal

The process to win NSF funding began early in 1983 when proposal-writing started. On 1 September of that year consortium members sent a letter to Dr Laura Bautz and Dr Francis Johnson at NSF telling them the Universities had agreed to create the ARC and planned to ask NSF for a grant of about $3.75 million. They anticipated submitting the proposal by the end of 1983 (BOG, 1983). Mannery, Siegmund and others worked with experienced fabricators to prepare a detailed concept design they called the 'Blue Book', which provided the basis for cost estimates, schedules, milestones, and specifications that were incorporated into the NSF proposal. By the time the request was actually submitted, on 15 May 1984, the amount had risen to $5.565 million. Donald G. York, Kurt O. Anderson, Bruce O. Balick, James E. Gunn, Doyal A. Harper, and Thomas O. Lutz signed the document, which became NSF proposal number AST-8414829 (York, et al., 1984).

After optimistically commencing work on the project and submitting the NSF proposal in early 1984, the reality of the challenge loomed by the end of the year. In a letter Margon sent to the Board on 31 December he bluntly stated that NSF money in 1985 was unlikely and that the ARC would have to spend carefully, while trying to move forward. He also cautioned to be careful when talking about the status of the NSF grant, in order to maintain fundraising momentum (Margon, 1984). By May 1985 he expressed some confidence in NSF approval and even expected a peer review date to be set shortly, but he had to counter some criticism of how aggressive York was being with NSF, by assuring the Board that York was doing a great job (Margon, 1985a). In early June the ARC implemented a project slowdown with a reduction of the calendar year 1985 budget from $4 million to $1.6 million (Baldwin, 1985b). Later that month NSF asked York to respond to a straw man budget that would give the full $5.6 million over several years, causing optimism to rise, even though no grant could be given before an in-person peer review, a date for which had yet to be set. Margon suggested communicating in a positive, but restrained manner, even though a stronger statement would help fundraising. He did not want to embarrass the NSF or cause competing projects to lobby harder for their projects before the ARC received approval (Margon, 1985c).

The in-person peer review took place on 23 October 1985. York and Margon prepared the agenda and Anderson, Baldwin, Gunn, Harper, T. Lutz, Mannery, and Siegmund also attended. Dr Roger Angel of the Steward Observatory Mirror Lab came as a guest of the ARC. The NSF representatives were Wayne Van Citters (Program Officer), Morris Aizenman (his boss) and Laura Bautz (Division Director). The peers were Jerry Nelson (University of California, Berkeley), Steve Beckwith (Cornell University), Bob Tull (University of Texas), and Mike Mumma (NASA, Goddard). Luckily, the peers were well known to the ARC scientists, both professionally and personally. Margon reported to the Board that the review went well and that the attendees were able to answer every question confidently and clearly. Some minor follow-up questions were expected, but none ever came. Van Citters let Margon know that the NSF could take no final action on the request until the FY86 budget was in place and that the straw man budget of $5.6 million was possible but difficult (Margon, 1985d, 1985e).

As 1985 ended, the Board hoped that the NSF money would become available in February or March, but it realized that it would still have to request first quarter 1986 dues from the members, to ensure that funds would be available to maintain progress on the telescope and the enclosure. The telescope fabrication contract with L & F Industries of Los Angeles had a delay clause in it that could be activated beginning 28 February in exchange for a modest fee, but by mid-1986 the ARC would have to dismantle the project and lay-off people if no NFS funding came through (Margon, 1985e). Baldwin presented some 1986 budget alternatives at the 10 December 1985 Board meeting, and these included a new project total estimate of $9,556,515, not including in-kind contributions of at least $1.2 million. The Board decided to continue to work for a February 1988 goal for project completion, but put the L & F contract on hold as of 1 February 1986, unless otherwise approved by the Board (BOG, 1985).

On 5 February 1986 Margon sent a letter to the Board informing them that the NSF had asked the ARC to consider a revised budget with a total of $3.3 million. If the ARC agreed, the proposal would go to the National Science Board in April or May. Given this funding situation, Margon (1986a) outlined some possible options, such as cutting expenses by delaying instruments; increasing member dues; adding new partners; and asking the NSF to grant funds far into future, then taking a loan against the future funds. On a conference call with Board members on 14 February, Margon expressed delight with the grant, because it would be the largest ever by the astronomy program at the NSF. Furthermore, it would be done in the face of Gramm-Rudman restrictions passed by Congress on 11 December 1985 to cut spending through 1990 in order to reduce the Federal deficit. During the conference call the Board discussed, but did not vote on, Margon's options. They agreed to accept the revised budget, but decided to ask for more. In addition, they decided to have L& F move forward on the telescope, because its fabrication was at a critical stage and they felt confident that the grant would be approved (BOG, 1986a).

The NSF agreed to give $450K more, but on condition that the ARC come up with a further $750K itself, which would guarantee completion of the telescope (but without any instruments). Margon suggested approaching member administrations asking for the additional $2.05 million not granted by the NSF, but 'begging' immediately for the $750K. The UW and University of Chicago portions were $232.5K, Princeton University and the NMSU needed to ask for $120K and the WSU share was $45K (Margon, 1986b). By 21 March all the institutions had guaranteed the $750K (Margon, 1986c). Recognizing that a telescope without instruments was useless, York then suggested that the University of Chicago finish its two main instruments and charge the ARC later, and he also proposed that the ARC trade time for instruments (York, 1986). At its 6-7 May 1986 meeting, the Board agreed to the suggestion to swap observing time (prorated from all members) for instruments, noting that any arrangement should have a 2-3 year lifetime and be renewable. Members were instructed to explore this idea with their Departments. Also, given the budget restraints of a likely lower grant from the NSF, only the échelle spectrograph, the 2-m camera and a makeshift CCD camera should be finished. The minutes state, "These are not scientific choices, but the affordable ones given the cost, current investment in these instruments and their progress." (BOG, 1986b).

On 11 July 1986 the NSF granted the ARC $3.74 million, with $890,000 paid in 1986 and $950,000 to be paid 1988, 1989 and 1990 (NSF, 1986). Interestingly, the amount received matched the expectations set in the September 1983 letter. This was a big win that assured completion of the Observatory as it was then envisaged (see Figure 2), but the one-year delay put the project behind schedule, even though the Board astutely funded critical path items. The key reasons for winning the grant were that it was a well-conceived proposal based upon Astronomy Survey Committee recommendations; it introduced advanced technology features; and it provided an opportunity to test new mirror fabrication processes to be used in a more ambitious project backed by the NSF. All three of these topics are discussed in more detail in the next section, which looks at the science and institutional goals that drove the Observatory design, as well as the actual design features.

4 THE APO DESIGN

The institutional goals, as noted earlier, were to provide abundant telescope time to faculty and students; enable long-term research; and build strong Astronomy Departments that attract top people and grant dollars. These goals called for a world-class facility with a telescope of competitive aperture. The scientific research conducted by the member institutions covered the full range of astronomy and astrophysics, from planets and asteroids to the most distant galaxies and difficult cosmological issues. The science perspective, therefore, called for a general-purpose design that could easily accommodate and adapt to a variety of uses. In addition, the US astronomy community, through its Astronomy Survey Committee process, identified a number of priorities for the 1980s that were important to the ARC astronomers. In particular, on page 16 in Section 4 the Committee report it suggests

> … the construction of an optical/infrared telescope in the 2-5 meter class for observing: transient phenomena, long-term survey and surveillance programs, provide ground-based support for space astronomy, and permit development of instrumentation under realistic observing conditions. The committee particularly encourages federal assistance for those projects that will also receive significant non-federal funding for construction and operation. (Astronomy Survey Committee, 1983).

The NSF proposal described 49 planned projects by the faculty at member institutions and these covered QSOs, the intergalactic medium, galaxies, and the Galaxy. Some examples of projects listed are: a redshift survey of a complete sample of distant galaxies; studies of light curves of a variety of astronomical objects; high-resolution studies of intergalactic and interstellar absorption; measurements of the velocity dispersion of stars in galaxies; and the determination of abundances in stars. These projects needed extensive telescope time, but this was not available at any public facility or on the telescopes then owned by member institutions (York, et al., 1984). The projects listed in the NSF proposal only represented a small number of what was contemplated (Gunn, 2004). Astronomers hoped for an easily-rescheduled telescope that could be operated remotely, because they wanted to be able to match projects to seeing conditions; respond to transient events, like supernovae, which had to be observed within hours of discovery; and follow up on opportunities identified by the Hubble Space Telescope and other space telescopes. Remote observing would enable rapid response without the need for travel, saving both time and money, and it would also increases opportunities for student access—even by undergraduates.

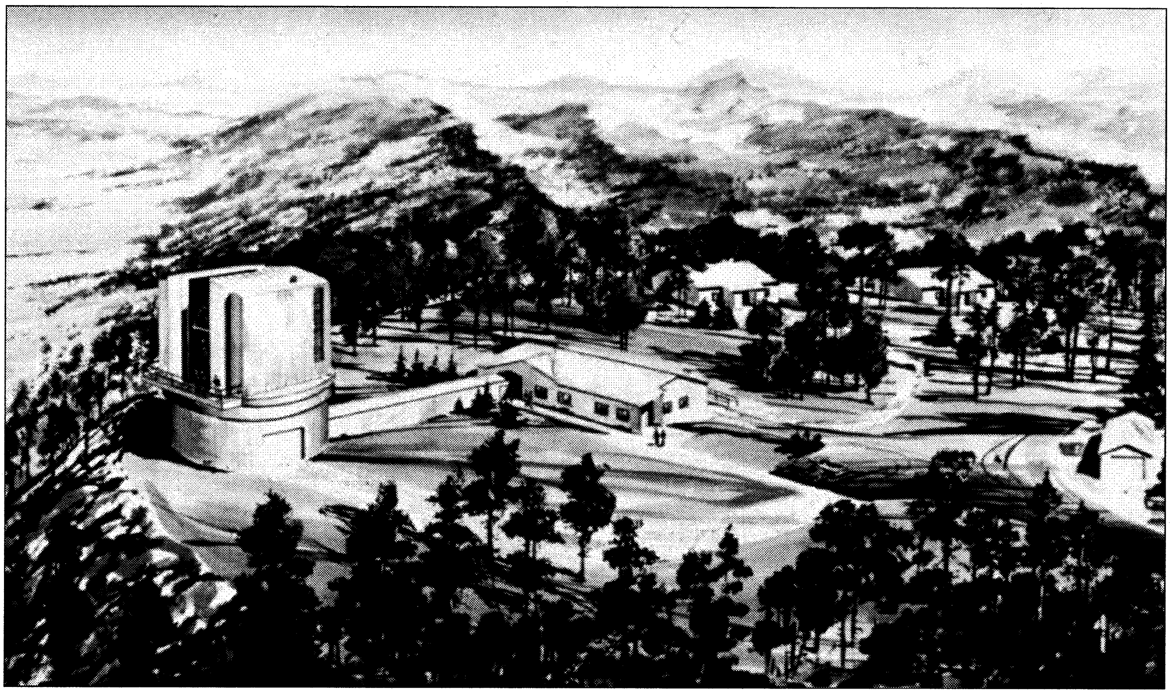

Figure 2: A 1986 concept drawing of Apache Point Observatory (courtesy of ARC, drawing by Kent Blair, 1986).

Key design features developed by the original consortium with Howard University back in 1981 carried through in the design submitted to the NSF that was eventually built: a lightweight mirror; a low-cost enclosure; remote observing capability; and rapid instrument changes. In 1983 and 1984 Balick and the UW engineering group worked with the new consortium, which now included scientists from Princeton University and the University of Chicago, to create a new conceptual design based on a 3.5-meter mirror. They wanted to design the optics, structure, and dome so that performance would only be limited by seeing on the best nights. In addition, the consortium needed to build instruments that would accommodate a wide range of astrophysical projects; provide for rapid observing program changes, including instrument changes to enable large routine surveys, programs that use sporadic excellent conditions and variable search work that only needs small portions of a night (PU, 1985).

Restricted remote observing was already available at other observatories using text-based commands to control a limited number of options. For example, the National Optical Astronomical Observatory at Kitt Peak had a remote tele-type terminal that could be used for observing. Of course, remote observing using the services of a staff observer had been available for a long time, if an astronomer wished to relinquish observing to a colleague. The goal for the APO was convenient and simple control of all aspects of the telescope and instruments by the astronomer him/herself. To accomplish this they would use a revolutionary graphical user interface built with Apple Macintosh computers, which would also include image feedback capability. The real innovation, though, was flexible scheduling and timesharing, which allowed multiple observers to share a night instead of the traditional scheme where one observer would be assigned a whole night or several nights in a row, even though the entire night was generally not required for the research (Mannery, 2004).

4.1 The Mirror

Detailed specifications for the telescope and enclosure start with the 3.5 meter, f/1.75, lightweight mirror. With a short focal length primary mirror, the overall telescope structure can be shorter and lighter, and the less massive mirror and telescope reduces noise from thermal heat. The APO secondary mirror gives a final focal length of f/10, but it is removable and can be replaced by an optional longer secondary (f/35) that is available for infrared observations. The flat tertiary mirror can be oriented to point the beam towards selected instruments that are already mounted and available. The optics follows a modified Ritchey-Chrétien design giving a 0.5° field of view and, theoretically, images smaller than 0.1 arcsecond (when the optics are perfectly supported and aligned). The Steward Observatory Mirror Lab was contracted to supply the blank for the primary mirror, while the Hextek Corporation, a technology spin-off of the Steward Lab, would provide the blank secondary mirror (York, et al., 1984).

In early 1983 Roger Angel contacted the UW astronomers forming the new consortium to tell them of an opportunity to get an experimental mirror free of charge. From 1980 to 1983 Angel researched and developed a process for spin-casting short focal length mirrors. This grew into the Steward Observatory Mirror Lab at the University of Arizona campus, which was eventually located in a large space built under the bleachers of the local football stadium. Angel wanted to create a production process for low-cost, lightweight mirrors, and his ultimate goal was to cast an 8-meter mirror for a nationally-funded new technology telescope. The NSF had funded some of his efforts, and supported the plan for the large new telescope. However, Angel needed to successfully cast smaller mirrors on his way to the 8-meter one. He planned to cast a 1.8-meter mirror in 1983, expected to complete a 3.5-meter mirror in 1986, and follow this with one of 6.5 meters (McCray, 2004). The NSF funded the 3.5-meter mirror in a separate grant to the Mirror Lab, but both Angel and the NSF wanted the mirror used in an observing environment that would provide valuable feedback to the casting process before an 8-meter mirror was attempted. Therefore, the mirror was available free of charge to the group with the best plans to use it (Williams, 1988). The ARC consortium astronomers responded quickly, since their plans were already well developed. They claimed to have the best plans for the mirror; they were willing to risk using an experimental mirror; and they agreed to conduct thermal and optical tests after is was installed in the telescope.

After selection of the Angel 3.5-meter mirror the ARC's Scientific Steering Committee debated the desired focal length for it. The faster the better, since it would reduce the overall size of the telescope and the enclosure. Several argued for f/1.5, but in the end the Committee recommended an f/1.75 mirror as the safer choice, because they thought polishing and finishing an f/1.5 mirror would take too long (even with double shifts), require risky testing methodologies and be difficult to align (Anderson, 1983).

4.2 The Telescope

The broad features of the telescope, shown in Figure 3, did not change much from the 1981 version described earlier. Of course, it would be larger, with a 3.5-meter mirror instead of the 2-meter one, and more than four instrument locations fit on the larger mirror weldment. Still, the moving mass was only ~30 tons, which is about 20% of the weight of the conventionally-designed Lick Observatory 3-meter reflector (PU, 1985). Engineers designed the telescope for 0.3-arcsecond image quality with wind speeds less than 20 mph, absolute pointing to 1 arcsecond, and tracking to 0.2 arcseconds for up to 10 minutes (York, et al., 1984). Pneumatic pistons support the mirror and provide dynamic compensation for variable wind loading and gravity changes as a result of altitude angle changes. The support and ventilation systems are integrated so that support does not block ventilation (Mannery, 1986).

The NSF proposal had seven possible mounted instruments, but by 1988 there were nine slots. Two of these were at the Nasmyth and seven were at the bent Cassegrain foci, located on the edges and top of the weldment holding the primary mirror. Light is directed to a particular focus by a rotatable flat tertiary mirror. The tertiary mirror assembly is removable in case a conventional Cassegrain focus is ever required. Four mirrors located in two of the corners direct the beam to the top, or retract to let it pass to the corner ports. Plans called for one Nasmyth port to be permanently reserved for a large échelle spectrograph, while at the other an instrument change could be accomplished by just one person in less than 15 minutes. The new instrument was on a cart which could easily be rolled into place. Since all the mounted detectors are continuously powered and ready to be used, changing to one of these was expected to take no more than 5 minutes (Balick, 1988). With this design the nightly schedule could accommodate multiple users with a variety of observing needs. In addition, users could respond quickly to unexpected situations.

4.3 Associated Instruments

Obviously, a key design goal for instruments was ease of use in remote observing, in addition to meeting scientific goals. In early 1984 the instruments planned at first light were (York, et al., 1984):

- An échelle spectrograph with a resolving power ($\lambda/\Delta\lambda$) of 50K.
- A Fabry-Perot narrow-band spectrograph, with imaging capability.
- Direct cameras for photographic and CCD imaging.
- A cooled IR/optical bench for a variety of IR sensors.
- A photo-electric photometer.
- A Ronchi astrometry machine.

As noted above, by the time the NSF grant was approved in 1986, most of these were put on hold until funding was available. In fact, the photo-electric photometer and the Ronchi astrometry machine were never built.

4.4 The Enclosure

The fast primary mirror and altazimuth mount allowed a compact barn-like enclosure to be built. This was modelled on the enclosure used for the Multiple-Mirror Telescope at Mt. Hopkins that was dedicated in 1979 (NMSU, 1985). Figure 4 demonstrates the size and, therefore, cost differences between this new design and conventional designs. The APO enclosure has a wide shutter and rotates with the telescope. In order to ensure the best seeing possible, the design goals call for the telescope and enclosure to cool quickly and remain isothermal with outside conditions (PU, 1985). Mounting the honeycombed mirror high on a pier above the structure lets airflow cool it more quickly (see Figure 3). To minimize heat production, observers and computers work from a separate operations building connected to the telescope enclosure by a covered walkway. Forced ventilation pulls outside air in, while the warm air is exhausting through the covered walkway, downwind of the telescope. Warm air is not allowed to cross the light path. Ventilation and airflow were modeled using dye, with an acrylic model of the enclosure placed in a water tunnel (Siegmund and Comfort, 1986). The enclosure design promotes the best possible seeing.

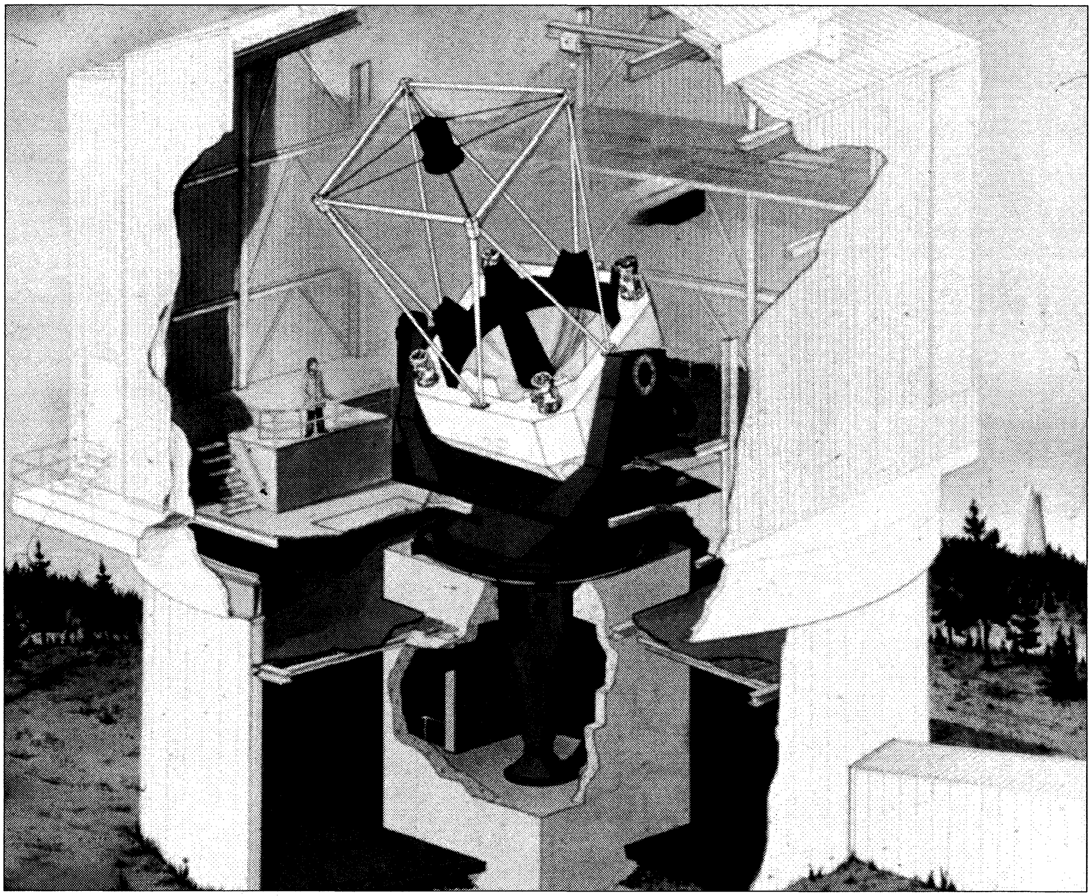

Figure 3: Cutaway view of the ARC 3.5-meter telescope (courtesy of the ARC, drawing by T. Asa Bullock, 1986).

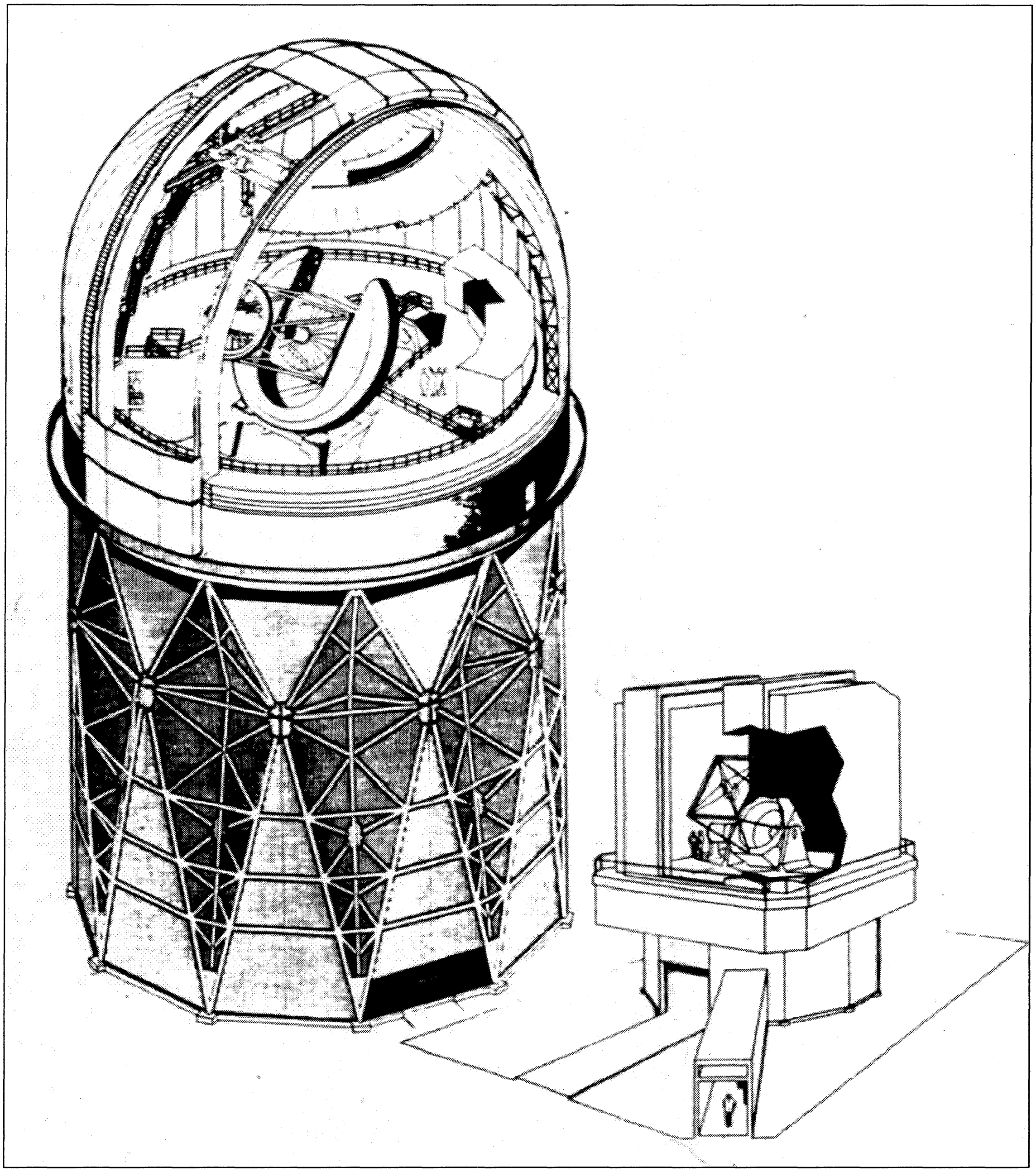

Figure 4: Size comparison between the Mayall 4-meter enclosure at Kitt Peak and the ARC's 3.5 meter enclosure at the APO (courtesy of the ARC, artist unknown, 1987).

5 BUILDING THE APACHE POINT OBSERVATORY

After a period of intense design through 1984 and into 1985, the ARC had firm plans for the site, the buildings, the telescope, the enclosure and the instruments. After the Forest Service granted the use permit, construction commenced in 1985 with the clearing of the site and the completion of the access road (NMSU, 1986), but major work was delayed until the NSF funding came through. The building of the APO went smoothly with few problems, except for long and costly delays involving delivery of the mirror and some of the instruments. One of the on-site problems that did occur was when the primer used on the beams of the dome and the skin of the enclosure degraded in the harsh ultraviolet light at the site's high altitude. Although the entire structure had to be sandblasted and reprimed, the project got back on schedule and the costs were shared by the ARC and the contractors. David Nordfors of Seattle designed the telescope enclosure (shown under construction in Figure 5), which followed the telescope design by six months, so that the telescope design drove the enclosure design and not visa versa (Baldwin, 1985a). Leedshill-Herkenhoff of Santa Fe completed the design of the site improvements and other buildings by May 1985, and L & F Industries of Los Angeles was hired in July 1985 to complete the detailed design of the telescope parts and then construct the telescope (Margon, 1985f). Mesilla Valley Construction (Las Cruces) erected the site buildings and infrastructure, Otero County Electrical

Cooperative (Cloudcroft) put in the power lines and Sunshine Services (El Paso) constructed the roads (Anderson, 2004). By 1 November 1987 contractors completed the telescope, enclosure, and support buildings and, after formal acceptance, the ARC occupied the site from January 1988.

The APO, site works and infrastructure, were all completed on time and within budget, but while the Observatory had a telescope, it lacked the mirrors and some of the instruments. The initial delay in obtaining the 3.5-meter mirror arose from the funding problems, as the NSF did not grant the Mirror Lab its full funding request. Eventually, the ARC was asked to provide money for materials and labor (UC, 1988). The NSF's smaller grant to the ARC also constrained the project: the ARC had to increase member contributions by $1.5 million, pro-rated by share, from the original $3.95 million ($3.2 planned, plus $750K more to get the NSF grant), because fundraising was slow and challenging. In struggling to find funds, the ARC decided not to build a second dormitory costing $116K, even though that was an excellent price (because it was cost-effective to build two dormitories at the same time). The ARC only had $54K, which the Board decided to reserve as a contingency fund, and they thought they might be able to use the AURA dormitories at the nearby National Solar Observatory if the need arose (Baldwin, 1987a; 1987b).

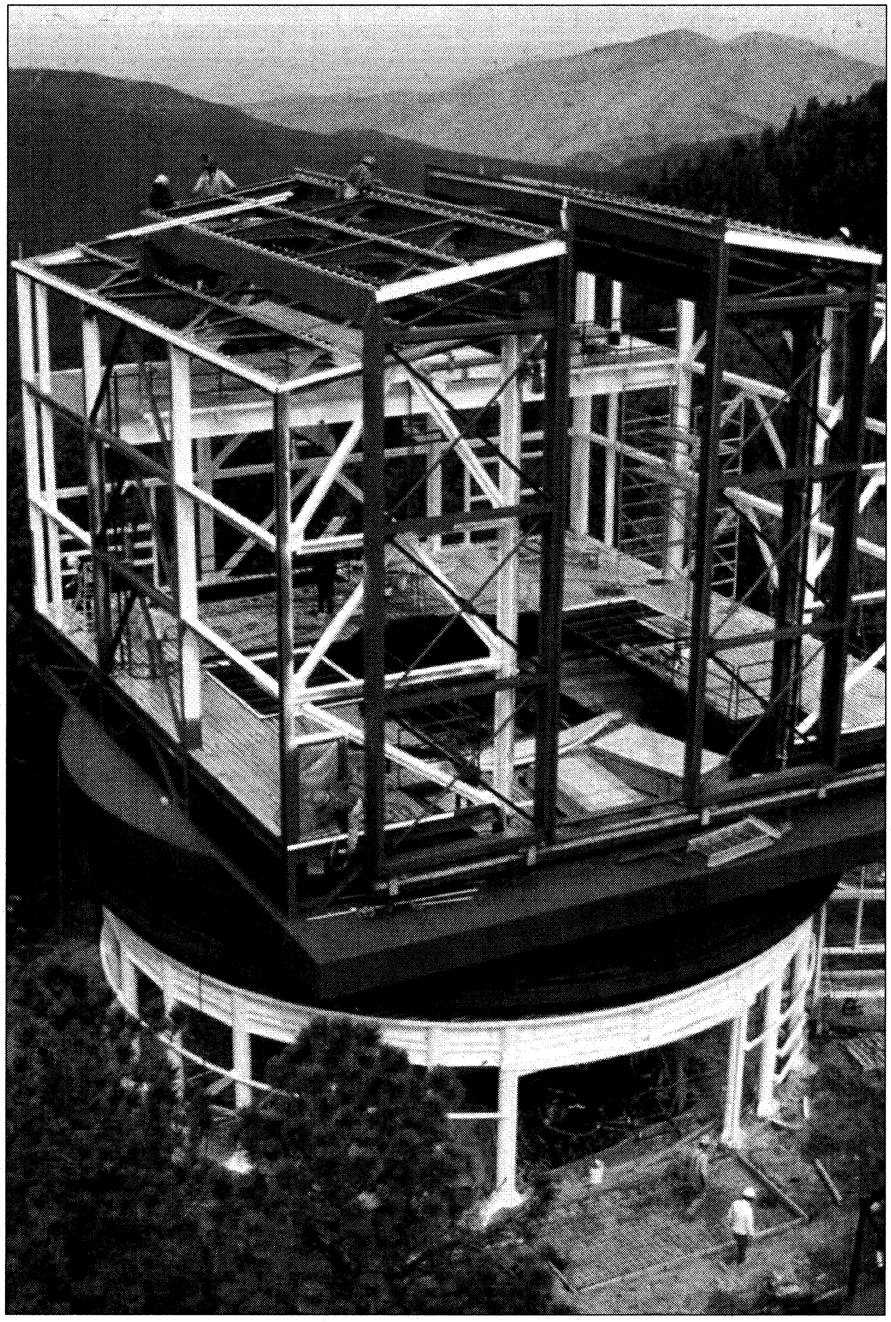

Figure 5: 3.5 meter telescope enclosure under construction in summer 1987 (courtesy of the ARC, photo by Dan Long, 1987).

5.1 The Mirror Delay

The real frustration came from the delay in the delivery of the mirror blank. The NSF funding problem was only the first delay of many. The ARC originally expected the Mirror Lab to deliver the mirror in August 1985, with first light scheduled in 1987 (York, et al., 1984), but after signing a contract for the 3.5-meter mirror with the University of Arizona (operator of the Steward Observatory Mirror Lab) in August 1986 they re-scheduled first light for February 1988 (UC, 1987). They expected the casting to start at any time, but further delays occurred and it only began in April 1988. *Bulletin of the American Astronomical Society* Annual Reports by the ARC member institutions from 1985 through 1988 give continually later dates for first light.

The process that was used to cast the mirror was complicated, and had not been tried before. The Mirror Lab only cast its first mirrors in April and August 1983. These were 1.8-meter mirrors for the University of Calgary and the National Optical Astronomy Observatory (NOAO), and they were done in a non-rotating furnace. In March 1985 the Lab cast its next mirror and the first using a rotating furnace. This was a 1.8-meter mirror for the Vatican Advanced Technology Telescope. The Mirror Lab then moved to its site under the stadium at the University of Arizona and built a larger rotating furnace. Its next mirror, from the new furnace, was 1.2 meters in aperture, for the Smithsonian Astrophysical Observatory, and this was cast in November 1987. Casting the ARC's 3.5-meter mirror started in April 1988 (Lampis, 2000). Prior to casting, engineers at the Mirror Lab built a mirror mold and placed it in the furnace. Casting began when five-pound chunks of borosilicate glass, known as 'E6' and supplied by the Ohara Corporation of Japan, were placed on top of the mold and melted by heating up the furnace (ibid). After heating the glass to 1,170° C and maintaining it at that temperature for three hours, the glass melted into a honeycombed shape around the mold. The heat was then turned off and the spinning started, which gave the mirror its parabolic shape. Spinning reached a top speed of ~8.5 revolutions per minute. After twenty hours engineers peeked inside the furnace to check progress, and they stopped the spinning about thirteen hours later, when the mirror was cool enough to hold its shape. It took another six weeks for the mirror to anneal (cool and temper). Figure 6 shows ARC and Mirror Lab personnel inspecting the just-cooled mirror on 27 June 1988. After inspection, engineers used water jets to wash the mold material out of the mirror. They then cleaned it up and subsequently delivered it (McCoy, 1988).

Spun mirrors have the short focal length desired by the ARC and Roger Angel's other clients. Casting the ARC 3.5-meter was a critical step in the Mirror Lab's progress to 8-meter mirrors, and Angel said that the problems were not with technology but with their own inexperience in managing large projects. Handover from astronomers to engineers earlier in the production process might have helped (McCoy, 1988). In addition, better expectation management could have relieved growing customer frustration. Don York, sick of explaining the mirror delay, grew a beard and proclaimed: "When the beard comes off, the mirror has been delivered." (Erickson, 1988). Figure 7 shows a happy York as Roger Angel shaves his beard off, with the completed 3.5-meter mirror in the background. The mirror was delivered on 10 August 1988, and Angel claimed it was a perfect 10 (ibis.). On 11 August the ARC moved the mirror to the optical shop of Norman C. Cole's Arizona Technologies Inc., where it was polished (UC, 1989). As it turns out, York should have waited to shave his beard.

Figure 6: The ARC's 3.5-meter mirror fresh out of the oven on 7 June 1988 (courtesy of the ARC, photo by Ed Mannery, 1988).

Assuming it would take eighteen months to polish and install the mirror, the ARC astronomers now projected first light in early 1990 (Balick, 1988). Because of situation regarding the mirror, there were also delays with instrumentation and programming. However, telescope and enclosure testing and shakedown continued, even without the mirror. During the fall of 1989 the NOAO, the Magellan Project, the Steward Observatory, the NSF, and the ARC all collaborated on thermal control tests using a dummy honeycomb mirror segment. The ARC agreed to these tests when it acquired the free mirror. Scientists studied thermal control under operating conditions at the APO through April 1990, and their tests showed that thermal surface deformation would produce image quality less than 0.2 arcseconds in all but the worst nights (York, 1991), thus validating plans for 8-meter mirrors.

5.2 The Temporary Mirror

An even more interesting test came when the ARC astronomers borrowed the University of Calgary's 1.8-meter mirror (the first made by the Mirror Lab). The idea for this emerged in late 1987 as Observatory construction ended but the mirror was still unpredictably delayed. It turns out that the University of Calgary had a mirror but no telescope, while the ARC had a telescope but no mirror. In exchange for borrowing the mirror, the ARC paid for it to be generated, polished, and aluminized. The mirror arrived on 19 June 1990 and was installed by March 1991 (ARC, 1992; Smith, 1990). It was installed with secondary and tertiary mirrors that gave an overall optical system of f/20, with a scale about the same as the final optical system planned for the 3.5-meter primary mirror (UC, 1991).

Besides being used for an engineering shakedown and refinement of the software written to remotely control the telescope, enclosure, and instruments (ARC, 1992), from February to 20 October 1992 full remote operation was carried out from the campuses of the ARC member institutions using a CCD camera, a guide camera and an infrared imager. Observing with this mirror successfully tested key goals of rapid instrument change and a shared nightly schedule with observers from different campuses (ARC, 1993). The APO even produced its first publishable results, observations of the cataclysmic variable, HV Virginis (see Ingram and Szkody, 1992; Szkody and Ingram, 1992; UW, 1993). Turner tested the synoptic advantages of the telescope by capturing sixty light curves of the gravitationally-lensed quasar Q2237+0305 over a three month period (PU, 1985). Images with the 1.8-meter mirror confirmed the expected good seeing at the site and the benefits of the enclosure design (ARC, 1992).

5.3 Delivery and Installation

By early 1990 Norm Cole completed rough generation of the 3.5-meter mirror and began polishing it. When the mirror was delivered by Roger Angel in 1988, the ARC scientists predicted the mirror would have seen first light by this time. Cole had a small shop and was an experienced optical worker, but this project stretched his capabilities since the curvature changes significantly from point to point on a short focal length mirror. When he was chosen for the task, the ARC knew it might be a risk, but the only other vendors for this kind of work were very expensive. The Mirror Lab wanted to do the work, but they did not have the facility at the time (Mannery, 2004). By November 1990 the Board authorized a search for another vendor (BOG, 1990). In the meantime, Roger Angel, knowing that he had to develop faster, cheaper methods of polishing and measuring if he hoped to achieve his goal of producing inexpensive mirrors, had developed computer controlled polishing tools and had designed an interferometric measuring method. He offered to finish the 3.5-meter mirror if ARC would pay for the development costs of the testing methods, a risky proposition since those costs were only roughly known (Balick, 2004). In February 1992, after months of no progress on figuring the mirror, the ARC took the risk and moved the mirror to the Steward Observatory Mirror Lab. Mirror polishing and test evaluations were completed in August 1992, the mirror was aluminized at Kitt Peak on 15 September 1992, and it arrived at the APO three days later. The process of installation began on 20 October 1992 (ARC, 1993). The secondary, cast by the Hextek Corporation, was delivered to the Optical Sciences Center in February 1992 and completed in January 1994. While waiting for the secondary and tertiary mirrors, the 3.5-m mirror was operated at the prime focus with a simple detector, in order to exercise and refine operations. Three-mirror first light happened on 5 April 1994, and the dedication of the Apache Point Observatory was held on 10 May 1994 (ARC, 1995).

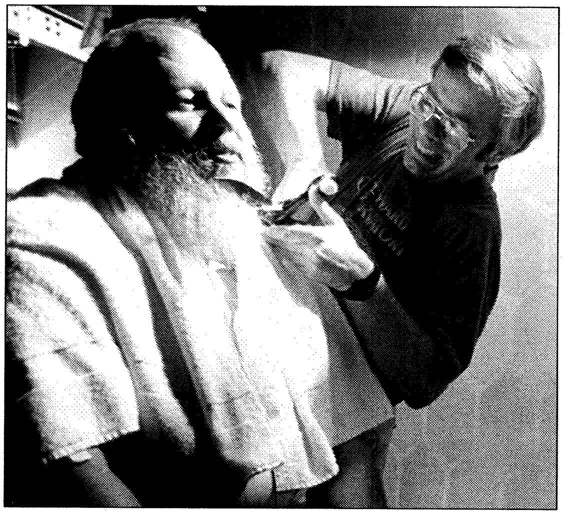

Figure 7: Roger Angel shaving Don York with the 3.5 meter mirror in the background taken 10 August 1988 (courtesy of the ARC, photo by Ed Mannery, 1988).

5.4 Dedication

Early 1993, with the primary mirror on site but still awaiting the secondary and tertiary mirrors, the ARC set a date for the dedication. A rare annular solar eclipse would pass directly over Apache Point in just a little over one year. Although it would be a challenge to get ready on time, they could not miss this unique opportunity (Gillespie, 2004). The 3.5-meter reflector, the 14th largest optical telescope in the world at the time, was dedicated on 10 May 1994 before an invited audience of about 300 people. The eclipse took place at 10:30 am, during which Don Jennings and Drake Deming, guests from Goddard Space Flight Center, imaged solar spectral lines with their 12-μm spectrometer. The telescope remained in operation until August as the astronomers and students were trained to remote observing techniques (ARC, 1995).

Luckily, some instruments were available. The dual-imaging spectrograph (DIS) built by Jim Gunn took its first images in April, barely making the dedication. It has since become the Observatory's workhorse (York, 2004; Anderson, 2004)). The near infrared spectrometer and imager built by Mark Hereld (University of Chicago), and a drift scan camera (DSC) built by Tim McKay at Fermi Lab, were also tested (ARC, 1995). The échelle spectrograph turned out to be more difficult to make and had to be rebuilt, so it was unavailable, although it eventually was commissioned and now performs above initial expectations (Anderson, 2004; York, 2004). In July, as part of a coordinated effort with other observatories, intensive observing of Comet Shoemaker-Levy 9's impact with Jupiter for up to 18 hours per day fully exercised the telescope (ARC, 1995).

The new optics exhibited performance problems, however. Earlier tests with just the primary mirror installed exceeded performance expectations, but tests with all three mirrors in place did not, and the problem was eventually traced to incorrectly-figured zones on the secondary mirror. In August the secondary was sent to Lick Observatory for measurements and then to Kodak for ion polishing. After re-measurement at Lick, the secondary was reinstalled in October with substantially improved image quality, but because it had too thin a faceplate to be successfully refigured, it still performed below expectations. The ARC's completed 3.5-meter telescope is shown in Figure 8, and it commenced full-time science observations in November 1994 (ARC, 1995).

6 THE SLOAN DIGITAL SKY SURVEY (SDSS)

At the time of the dedication of the ARC's 3.5-meter telescope, APO was home to three other telescopes: the NMSU 1.0-meter telescope dedicated the same day, the SDSS 2.5-meter telescope and the SDSS 0.6-meter telescope (both of which were under construction at the time). The Sloan Digital Sky Survey (SDSS) is a very successful project led by the ARC. It deserves its own history, but will get a brief description here, because its roots extend into the ARC's early history.

In fact in May 1988, Rice, then interim Chair of the ARC, sent a letter to Board members saying that the ARC needed a process to allocate and approve space for other telescopes at the APO. York had sent a proposal to the NSF for a dedicated 3.5-meter telescope for a cosmology program, and the UW and Princeton University were getting more serious in their desire for a 2.5-meter telescope for a survey they started discussing in 1982 (York, 1988). At the 23-24 October 1989 Board meeting the members discussed the 2.5-meter survey project and admitted the Institute for Advanced Studies (IAS) as an ARC member for the purpose of doing the survey that, at that time, only included Princeton University and the University of Chicago (BOG, 1989). The IAS actually joined in December 1990, when the ARC formed a subcommittee to investigate building a second telescope (UW, 1991; 1992). In the fall of 1991 this grew into an additional collaboration of the ARC with other institutions that were funded by the Alfred P. Sloan Foundation to carry out the Sloan Digital Sky Survey. Johns Hopkins University (JHU) joined the consortium in June 1992 to be part of the SDSS. The IAS and JHU joined the ARC, but they do not share in the time or expenses of the 3.5-meter APO telescope (UC, 1994). The UW did not join the SDSS until 1994, even though it had a lead role in the project. The NMSU joined in 2000, but the WSU never joined the SDSS. The SDSS telescope is dedicated to the survey, so membership in it gives access to the data, but not to telescope time.

Figure 8: The completed ARC 3.5-meter telescope at APO in 2000 (courtesy of the ARC, photo by Dan Long, 2000).

Although other institutions from around the world have since joined SDSS, they are not members of ARC. Today, the other members of the SDSS are: the Fermi National Accelerator Laboratory (FNAL), the Japan Participation Group (JPG), the Korean Scientist Group (KSG), the Los Alamos National Laboratory (LANL), the Max-Planck-Institute for Astronomy (MPIA), the Max-Planck-Institute for Astrophysics (MPA), University of Pittsburgh (Pitt) and the United States Naval Observatory (USNO) (see ARC, 2004a). Appendix 2 lists everyone who has served on the SDSS Advisory Council.

The SDSS telescope, software and instruments are tightly integrated to survey the North Galactic Polar Cap. The sophisticated design of the telescope gives about a 3° field of view and consistent images even out to the field edge, so that it works well with fiber-fed spectrographs. During excellent seeing, five-color imaging, with a limiting magnitude of 23 in R band, surveys the sky via drift-scans. These images are used to select galaxies and QSOs for spectroscopy with two fiber-fed spectrographs. A million redshifts will be obtained over a 5-year period to study the large-scale structure of the Universe. In addition, many other objects will be found, such as supernovae, brown dwarfs and asteroids (PU, 1994; UC, 1994). Originally it was expected that first light would occur in 1995 and the survey would be completed by 2001 (UC, 1995). Major site improvements at the APO associated with the SDSS were completed in the summer of 1993 (UW, 1994), but instrumentation problems meant the survey did not start until April 2000. The initial project formally ended in June 2005, when a 3-year extension project began (SDSS-II).

7 CELEBRATING SUCCESS

With the SDSS in full operation and the APO providing ample observing time to astronomers and students, the ARC celebrated its achievements on 27 May 2004, the twentieth year of its existence and the ten-year anniversary of the dedication of the 3.5-meter telescope. The accomplishments included creating a thriving organization, building a world-class observatory, and fulfilling the goals of developing strong astronomy departments and doing important scientific work.

Organizationally, the ARC proved sturdy enough to withstand the vicissitudes of long-term projects, the formation of a complex sub-organization (i.e. the SDSS), and even a membership change in July 2001 when the WSU sold its share to the University of Colorado (Boulder), which also agreed to build several new instruments. The WSU did not have an institutional commitment to grow its astronomy program, so it could not justify the cost nor fully use its plentiful telescope time—especially after Tom Lutz died in 1995 and Julie Lutz moved to the UW a few years later. Without these two observational astronomers and ARC founders, the costs outweighed the benefits and the WSU sold its share, smoothly transitioning participation to Colorado (Lutz, 2004). For the other members, the ARC and the APO worked as planned. By the end of the second full year of operation, in 1996, two hundred astronomers and students had been certified to operate the telescope remotely (ARC, 1997), and by 2004 the ARC members could say that having the 3.5-meter telescope made their departments attractive to faculty, students, and future grants (York, 2004; Anderson, 2004; Balick, 2004). In particular, students benefited from it: there was time available to them, and they did not have to find ways to fund travel to an observatory. They could do their work remotely.

Astronomers and students now do a variety of research work at the APO, and produce results in a range of astronomy sub-disciplines. The anniversary celebration in 2004 included papers with titles like “APO Insights into Cataclysmic Variables”, “Observations in Support of HST” and “A Decade of Planetary Science with the APO 3.5 meter Telescope” (ARC, 2004b). Meanwhile, a search for ‘Apache Point’ on NASA’s ADS server on 10 November 2005 resulted in 305 papers, 179 of which were selected when that search was limited by ‘3.5’. Many projects undertaken at the APO are multi-wavelength and collaborative, which the ARC 3.5 meter is very good at, but its contribution often is not specifically cited (see Lutz, 2004). Examples of these types of programs include asteroid detections (as part of Spacewatch Projects), or the impact of the Comet Shoemaker-Levy 9 on Jupiter, as mentioned earlier. Some of these collaborations do show up in published papers, though. Kurt Anderson, who was part of a large collaborative effort involved in monitoring the temporal behavior of radio galaxy 3C390.3, obtaining images and spectra using the DIS on the ARC 3.5 meter telescope at 10-day intervals (see O’Brien, 1998).

Flexible scheduling, both for unexpected opportunities and for long-term programs, separate the APO from other facilities and give the ARC astronomers a distinct benefit. Advantageous scheduling of the ARC 3.5 meter telescope enabled optical spectra to be obtained in conjunction with International Ultraviolet Explorer observations during the 43-day super-outburst cycle of ER UMa (Szkody, et al., 1996), and rapid instrument changes made it possible to observe the optical afterglow of gamma-ray bursts (Margon, et al., 1997). The ARC astronomers can commit to long-term programs using the APO that would be impossible to do by competing for time on other telescopes in the open ‘marketplace’. These kinds of projects yield important results. Turner and colleagues at Princeton used the APO to conduct a synoptic gravitational lens program consisting of half-hour observations every other night, starting later and later in the evening as the program progressed. Actually, the initial observations were done with the 1.8-meter, and these resulted in a time delay prediction for the 1996 B light curve of quasar 0957+561 (Kundic, et al., 1995). In 1996, the time delay was observed as predicted, allowing them to derive the Hubble Constant to a claimed accuracy of 10% (Kundic, et al., 1997). A graph from the later paper is reproduced here as Figure 9, and it shows the 1995 prediction and the 1996 observation. In yet another example, Professors Reiss, Diercks, Stubbs, and Hogan joined an international effort with the High-z Super-novae Search Team to study and monitor high-redshift supernovae using the 3.5 meter telescope (UW, 1996).

The wide range of astronomical interests at ARC member institutions results in a variety of research programs. Greenawalt and Walterbos made the first detection of oxygen lines in a truly diffuse medium, confirming the expected strength of [OII] and the weakness of [OIII] (Walterbos, 1996). Walter and Marley used the infrared camera to show evidence of substantial haze at the south pole Uranus, and were able to demonstrate that it had brightened in recent years (Walter, et al., 1996). Kibblewhite and his group at Chicago installed developmental adaptive optics instruments on the 3.5 meter telescope, including a laser beam for artificial sodium stars. Even though promising improvements in image size from 1 arcsecond to less than 0.2 arcseconds were recorded, indicating excellent progress, the system never became operational (Shi, et al., 1995; Larkin and Kibblewhite, 1998). After 1997, funding for the costly laser effort became too difficult to obtain. Interestingly, discussions between APO staff and local officials led to a national-level discussion involving several large observatories and Government agencies in a bid to develop guidelines for the safe use of laser-guided systems (ARC, 1997). Another laser project, the APOLLO–Apache Point Observatory Lunar Laser-ranging Operation, was used to test predictions of General Relativity using precise measurements of the Moon-Earth distance (see Strasburg, et al., 2002).

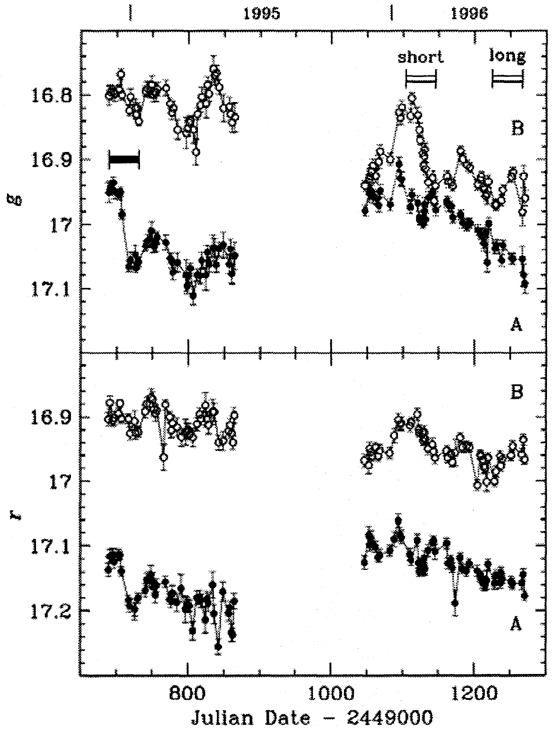

Figure 9: Observations and predictions of time delay for the gravitationally-lensed quasar 0957+561 (after Kundic, T. et al. 1997; reproduced by permission of the AAS).

Some of the most exciting results at APO have come from the combination of SDSS and the ARC 3.5 meter. For example, the infrared camera on the ARC 3.5 meter, following up on SDSS commissioning data, found some of the highest redshift QSOs in the universe at the time (Fan, 1999) and the first field methane brown dwarf (Strauss, 1999). Further spectral analysis of the distant quasars with the ARC 3.5 meter telescope and at the Keck Observatory led ARC scientists to announce the detection of the Gunn-Peterson Trough at redshifts of $z > 6$ and evidence of reionization at $z = 6$ (Fan, 2000; Becker, 2001).

Ultimately the continued success at the APO depends on the quality of the site, the telescopes and the instruments. The key functional design goals of remote observing—rapid instrument changes and flexible scheduling—have been achieved, are successful, and will remain fundamental to future plans. The site provides excellent seeing, as confirmed by continual and extensive monitoring. Between 1997 and 2000 the APO implemented an aggressive plan to improve telescope performance by replacing the secondary mirror (which had never quite met expectations), stiffening optical supports to reduce jitter, increasing baffling to reduce scattered light effects, and completing a host of other upgrades, such as new and updated instruments, new computers and rewritten software (ARC, 1998). In addition to an ongoing maintenance and upgrade program, the ARC is beginning to discuss future telescopes and instruments for the Apache Point Observatory. Don York, who heads a 'Futures Committee', not only sees the APO completing the large-scale surveys that are currently progress (such as SDSS II), but also initiating new ones. Survey follow-up can be done with current instrumentation, but his Committee will explore the construction of a 6-meter class experimental telescope (York, 2004).

8 CONCLUSION

Despite delays in building the telescope, the ARC successfully completed a world-class facility for its members that served to strengthen their astronomy programs and accommodate the types of long-term projects that are so important to university astronomy departments.

The University of Washington initiated the effort to build a world-class university observatory in the USA, but at the same time the other universities were discussing the same thing so the messages conveyed by Wallerstein and Balick resonated with them. Committees led by Margon and Baldwin formed the Astrophysical Research Consortium (ARC), an organization that unified different member-interests and created a process for long-term cooperation. This new organization faced difficult challenges in funding and budgeting, but it eventually succeeded, and even went on to develop other ambitious projects desired by its members, such as the SDSS.

The construction of the ARC 3.5 meter telescope at the APO was driven by the goals of convenient remote observing, rapid instrument changes and flexible scheduling. Without the benefit of a dedicated project manager, Don York guided this project to a successful conclusion by overseeing the work which was distributed among all of the member institutions and a number of vendors (including Angel's experimental mirror, which was vital to the telescope, yet contributed to its delay). Building the telescope exercised and developed the skills and talents of the astronomers at all of the ARC institutions. Anderson and NMSU faculty selected, prepared and manage the site. Gunn at Princeton, Harper at Chicago, and their various colleagues, built cutting-edge instruments that had to function in remote observing mode, and could be rapidly inter-changed. Balick, Mannery, Siegmund and colleagues at the UW designed the advanced technology telescope and enclosure. After a long gestation period, in 2005 the APO 3.5 meter telescope completed ten years service to the ARC. The first APO Director, Princeton's Ed Turner, took up duties in 1994, and he only stepped down on 1 January 2005. Suzanne Hawley from UW then replaced him, beginning a new era for the APO.

9 ACKNOWLEDGEMENTS

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10 NOTES

1 US dollars are used throughout this paper.

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12 APPENDIX 1: MEMBERS OF THE ARC BOARD OF GOVERNORS, 1984 – 2005, LISTED BY INSTITUTION*

Note that only two members from each institution are on the board at any given time. The members are listed in the order they served from their institution.

Princeton	Chicago	UW	NMSU	WSU	JHU	IAS	UC-Boulder
Ostriker	Rice	Baldwin	Beebe	Lutz	Heckman	Bahcall	Peterson
Sinisgalli	Rosner	Margon	Darnall	Radziemski	Poehler	Rowe	Shull
Gunn	Schramm	Hogan	Burnes	Spitzer			Barker
Tremaine	Oxtoby	Kwiram	Casillas	Brown			Pampel
	Konigl	Balick	Adams	Miller			
	Turner	Irving	Dwyer				
	Olinto		Walterbos				
	Fefferman		Paap				
			Czerniak				

*Provided by Mike Evans, Astrophysical Research Consortium.

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13 APPENDIX 2: MEMBERS OF THE SDSS ADVISORY COUNCIL, 1995 – 2005, BY INSTITUTION*

Chicago	**Princeton**	**FNAL**	**IAS**	**JPG**	**JHU**	**USNO**	**UW**
Rice	Ostriker	Peoples	Bahcall	Fukugita	Heckman	Johnston	Baldwin
Schramm	Sinisgalli	Nash	Rowe	Ikeuchi	Poehler	Pier	Margon
Rosner	Gunn	Kolb		Okamura	Davidsen		Hogan
Turner	Tremaine	Stanfield		Sekiguchi			Parks
Oxtoby				Ichikawa			Hawley
Fefferman				Suto			Irving

NMSU	**MP**	**Pitt**	**LANL**
Walterbos	Rix	Jasnow	Press
	White	Turnshek	

*Provided by Mike Evans, Astrophysical Research Consortium

Jim Peterson is Chief Technology Officer for Tamarac Inc., a Seattle software company, and a member of the Seattle Astronomical Society. He completed his Master of Science in Astronomy at Swinburne University of Technology in 2005. Jim enjoys bringing astronomy activities to Seattle area classrooms and he volunteers at the University of Washington's historical Jacobsen Observatory. He is interested in the history of astronomy, especially from economic and science management perspectives.

Dr. Glen Mackie is an Astronomer at the Centre for Astrophysics and Supercomputing, Swinburne University of Technology. He received a PhD ("The Stellar Content of Central Dominant Galaxies") from the Australian National University in 1990. His research interests include investigating the properties of brightest cluster galaxies (as tracers of large-scale structure and as probes of galaxy formation and evolution in the rich cluster environment), multi-wavelength properties of galaxies, galaxy mergers and astronomy education.

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BOOK REVIEWS

***The Composition of Kepler's Astronomia Nova*, by James R. Voelkel (Princeton, Princeton University Press, 2001), pp. 327, ISBN 0-691-00738-1 (hardback), £25.35.**

Some contemporary researchers in the history of astronomy inquiring into basic problems often discover strands or echoes of rhetoric in their methods and subject matters, whether in the persuasive mathematical definitions of celestial phenomena or in the circumstantial and shifting meanings of terms in scientific semantics. James Voelkel's *The Composition of Kepler's Astronomia nova* examines Johannes Kepler's monumental work as rhetoric. Voelkel takes a clear, forceful and comprehensive look at the 'how' and 'why' of a complex and technical book.

By way of introduction, 'rhetoric' was originally defined by the philosopher Aristotle as the art of persuasion and its use should be limited to judicial and political concerns. Today, in its broadest sense rhetoric encompasses all human communication. When it is found in scientific works rhetoric's definition and purpose remain the same: to persuade a particular audience of something, and in the creation and communication of knowledge, science like all persuasive discourse, as Gross reminds us in *The Rhetoric of Science* (1996), must convince us of the truth of its claims. For example when Copernicus, Kepler and Galileo suggested that the Earth revolved around the Sun they were not asking natural philosophers or scientists of their day to observe new data; rather, they were proposing a new framework for understanding over a millennium of old observations. Their new and yet unfinished paradigm constituted a radical assault upon tradition. Needless to say, many of their contemporaries regarded such new ideas as pointless. Therefore, each of them employed rhetoric to garner appeal, embellish, and argue their radical ideas. This was necessary, for to be truly persuasive at critical periods in history, science had to be buttressed by rhetorical argument.

Published some four hundred years ago, Kepler's *Astronomia nova* (1609) (henceforth referred to simply as '*Astronomia*'), is recognized today as probably Kepler's greatest contribution to astronomy and the Scientific Revolution. Containing Kepler's first two Laws of Planetary Motion, the *Astronomia* was written during a period when scientific treatises often mixed physics and metaphysics, astronomy and astrology, geometry and theology, and alchemy and numerology. The Late Middle Ages was a battleground encrusted with ideology, where the accepted method for proving a scientific theory was scientific demonstration according to Aristotelian principles; clear lines of demarcation were drawn between science and rhetoric when making scientific claims.

Wallace (1989: 7) notes that "To affect the thinking habits of men on important issues such as the nature of the universe strong forces had to be at work, forces that impinged not only on men's minds but on their hearts and instincts as well." So when Kepler introduced his new astronomy in seventeenth century Europe he knew that he would have a difficult task persuading other astronomers to give up a 'world view' that had existed for more than fifteen hundred years, and in anticipation of their objections he had the sense to carefully arrange his work.

In his own words, Kepler (1610) stated that he was trying to establish a new philosophical interpretation for reality, to "... provide a philosophy or physics of celestial phenomena in place of a theology or metaphysics of Aristotle." While everyone else saw circles Kepler saw ellipses, and he knew that—like Copernicus and Galileo—he would have to defend his radical ideas. But unlike his predecessors Ptolemy or Copernicus, Kepler wrote the *Astronomia* as a narrative story that described his war with the planet Mars as a way of unlocking the secrets of the Heavens. Kepler forgoes the time-honored tradition of keeping science and rhetoric separate, and employs rhetoric to justify his scientific claims.

Kepler's writing style is quite unappealing to the modern mind, as it was to his own colleagues. He adopts an unusual style, and chooses to personally narrate the *Astronomia*, appealing to and asking its readers to bear with him through every agonizing detail as he attempts to use medieval tools and medieval values, assumptions, residues, emotions and superstitions to carve out new mechanistic and mathematical paths. Kepler reveals the conflicts, the cross-currents and the confusions that surged in his mind as he struggled to find a new way of looking at reality.

Voelkel analyzes Kepler's composition in a detail never attempted before, putting everything in its correct historical context. In his Preface, Voelkel asks somewhat rhetorically: What drove Kepler to compose as he did? Voelkel then seeks to answer two central questions. First, if the *Astronomia* is didactic or rhetorical, what can be reconstructed about the development of Kepler's physical astronomy without reference to Kepler's account? Second, and more important to Voelkel, to what extent did the astronomical community influence how Kepler would compose and presentation of the *Astronomia*?

Voelkel pursues answers to these insightful queries by interpreting Kepler's account of his astronomical research with the informed skepticism of previous investigators. Fairly early in his Preface to the *Composition*, Voelkel acknowledges that Stephenson (1987) was the first to point out the argumentative nature of the *Astronomia.* But Stephenson 'dropped the ball' after mentioning the most important characteristic about Kepler's *magnus opum*: that it was one long, sustained, rhetorical argument. Stephenson proceeded to produce what I believe is probably the most definitive study ever done of Kepler's physical astronomy.

It would take Donahue's (1988) paper on "Kepler's fabricated figures: covering up the mess in the *Astronomia nova*" to clearly bring into question Kepler's presentation of fact and his skills in constructing the *Astronomia.* With the basic clue in hand that Kepler's scientific treatise was written with a rhetorical purpose, we find Voelkel exploring the *Astronomia* as a rhetorical pseudo-history. Kepler is indeed presenting one long sustained rhetorical argument, and Voelkel skillfully devotes eight chapters to tracing the evolution of Kepler's thoughts, from the conceptual seeds of his youthful *Mysterium Cosmographicum* (which was published in 1597) through to the publication of the *Astronomia.* By doing so, Voelkel exposes Kepler's motivations and the meaning of his work within the context of the time and the role played by his skeptical astronomical contemporaries. In the final chapter, Chapter Nine (which is erroneously referred to as Chapter Ten in the Introduction), Voelkel finally reviews the *Astronomia* as rhetoric. What he emphasizes is Kepler's rhetorical strategy to win persuasion by "... constructing his argument to make it appear as though he resorted to a physical approach to planetary theory only after a comprehensive failure of the most general kind of model in the classical form." (page 8).

According to Owen Gingerich (1989: 69), many generations of astronomers and historians of science have without comment reported Kepler's meandering attempts as 'flaws'. Voelkel correctly realizes that Kepler's recording of his "... many ... failed attempts ..." (page 8) and "... numerous redundant demonstrations ..." (page 243) serve a valuable didactic function. Voelkel freely admits that his investigation is not grounded in the art of rhetoric, nor is it a comprehensive rhetorical analysis of the *Astronomia.*

For me this is regrettable. It might be more appropriate to consider Kepler's rhetorical strategy here as much more than a carefully- and purposely-designed rhetorical ploy, that is to say, it should be studied as a method and subject matter in its own right. Upon closer investigation, Kepler's strategy appears to contain much more than traditional Aristotelian recommend-ations for using grammar, syntax, etc. to win over minds. Rhetoric, like revolution, can be a way to redefine reality, and Aristotle's principles of rhetoric are, by definition, too modest and powerless to bring about revolutionary change in science. It seems to me that Kepler's rhetoric was a 'new rhetoric' of argument, and that it goes beyond the mere cosmetics employed by Copernicus and Galileo. In the *Astronomia nova* Kepler wanted to redefine astronomy by wedding physics and astronomy into something 'new'—a *Physica Coelestis*.

Notwithstanding these concerns, Voelkel's excellent study has much to offer anyone interested in Kepler's astronomy, for it helps us understand Kepler's motivations in writing the *Astronomia* the way that he did. This is a highly-readable scholarly book, and is suitable for people with an interest in the history and development of astronomy and anyone who has been waiting to experience some of the essential aspects of Kepler's work. I thoroughly recommend it.

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Biographischer Index der Astronomie/Biographical Index of Astronomy **(Acta Historica Astronomiae, Volume 26) by Wilhelm Brüggenthies and Wolfgang R. Dick (Verlag Harri Deutsch, Frankfurt am Main, 2005), 481pp, ISBN 3-8171-1769-8 (softback), 210 x 145 mm, Euro 39.80.**

The searcher for information in the history of astronomy constantly encounters the task of locating sources. The basic facts concerning individual people—exact names, dates, nationalities—are often frustratingly difficult to find. The present volume, one in the excellent *Acta Astronomiae* series, provides the solution. This compilation, the fruit of what was originally a venture for private use, lists in alphabetical order the names of an astonishing 16,000 individuals, astronomers or persons connected in some way with astronomy from earliest to recent times, and provides for each one the date and place of birth and of death, and at least one reference to a source of biographical information. The sources, which number about 400, include biographical dictionaries, encyclopaedias, lists of obituary notices, books, journals and websites. Most of these are in either English or German, but publications in other languages have also been assiduously combed for references.

The work is a bilingual publication. The Introduction and explanatory notes are in both German and English. The place names and abbreviations in the actual list are in German, but tables of translations of these are provided. The authors emphasize that the Index is intended as a first guide in the cases of eminent astronomers, when the numerous sources cannot obviously all be listed. Many quoted sources incorporate earlier ones; such duplications are given in brackets, so that the user is spared the trouble of unnecessary searches. The entries also indicate commemoration on lunar and planetary formations; there is thus a delightful sprinkling of names of great artists and scholars of the past—Homer, Dante, Goethe, Shakespeare, Michelangelo—with their biographical data. Also recorded are those with named minor planets, many if not most of whom are happily living. Other contemporary astronomers who are given places—perhaps unnecessarily—are distinguished names taken from recent editions of *Who's Who* and similar national biographical dictionaries. However, the vast majority of subjects are of course persons of the past, which is the Index's chief purpose.

By dint of careful planning and editing, the authors have compressed a great deal of information into small spaces to provide an easily-used directory where, in the manner of a telephone directory, a name may be looked up and basic data checked at a glance. The book is of a convenient size and very modestly priced. Astronomers, especially historians of astronomy, will find it indispensable and will wish to keep a copy permanently at hand.

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***Stars and Numbers. Astronomy and Mathematics in the Medieval Arab and Western Worlds*, by Paul Kunitzsch (Aldershot, Variorum, 2004), pp. 356, ISBN 0-86078-968-3 (hardback), US$119.95.**

The majority of modern star names in the sky today have corrupt forms of Arabic names. This is evidence of the important influence that medieval Arabic and Islamic astronomy had on European science.

Paul Kunitzsch is one of those few scholars whose work has been fundamental in highlighting the development of this Arabic-Islamic astronomy and its transmission to Western Europe. Many of his studies are written in both English and German, and have been published in major astronomical journals. This book, in the Variorum Collected Studies Series, brings together a second collection of Kunitzsch's papers, and complements his earlier book, *The Arabs and the Stars*, which was published in 1989.

There are twenty nine papers in this book; seven are written in German, two in French and the remainder in English. The papers are divided into four groups. The first group, titled "Ptolemy in the Arabic-Latin tradition", focuses on Ptolemy and the ways in which his book, *The Almagest*, influenced Arabic astronomical traditions. The first of these papers describes the translation of this book from Arabic to Latin, and its subsequent transmission from Spain (or *Al-Aandalus*) to the rest of Europe in the twelfth century.

These papers are followed by a second group of papers on "Arabic Astronomy", where Kunitzsch first writes about a Persian astrological manuscript which includes a section on the fixed stars. The next two papers in this group are about the Persian astronomer Al-Sufi, and the final paper provides a description of an Islamic celestial globe which is in the Schmidt Collection in Vienna.

The third group of papers focuses on "Arabic Astronomy in the West", and describes the first period of Western contact with Arabic astronomy, and especially the use of astronomical instruments such as the astrolabe.

Papers in the final group relate to "Mathematics and Numbers", and describe the transmission of Euclid's elements as well as Arabic numerals from India to the Arabs, and then to the West.

Kunitzsch's papers are always a fascinating read, and his understanding of Arabic astronomy—and especially star nomenclature—is unparalleled in the world. I very much recommend this book to those with an interest in Arabic astronomy.

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***Empire of the Stars: Friendship, Obsession and Betrayal in the Quest for Black Holes*, by Arthur I. Miller (London, Little Brown, 2005), pp. xvi+400, ISBN 0 316 725552 (hardcover), £17.99, Can$39.00.**

This book has two main themes: the complex personal relationship between Sir Arthur Eddington and Subrahmanyan Chandrasekhar, with particular reference to their disagreements about relativistic degeneracy and the late stages of stellar evolution; and the history of theoretical studies stimulated by Chandrasekhar's work which eventually led to the prediction of what are now called black holes. The Eddington-Chandrasekhar story occupies most of the book's first part and Miller returns to it towards the end. The other theme is divided, somewhat arbitrarily, between the other two parts of the book.

A meeting of the Royal Astronomical Society in January 1935 provided the scene for the climax of the disagreement between Eddington and Chandra (as Miller consistently calls him). My own belief is that, as time passes, this disagreement will come to be seen as a minor, if somewhat puzzling, incident in the history of astronomy, but interest in it was stimulated by Wali's 1984 biography, *Chandra*. Miller is following a current fashion in the history of science, in which personalities and social contexts are stressed at least as much as the internal logic of science. Insofar as it acts as a corrective to the earlier one of seeing scientists as objective, passionless onlookers of the natural world, the new fashion is to be welcomed; but it also carries dangers of creating a false picture of what actually happened. The historian may be tempted to impute motives for which there is little or no evidence and thus to create verbal portraits of some of the actors that are little better than caricatures. In my opinion, Miller has not avoided these dangers.

Towards the end of his Preface, Miller writes that great scientists "… may act in ways that seem inexplicable to ordinary people…" and, he goes on, "Working out why has involved spinning a scenario based on what can be inferred from the information available." Fair enough; all historians must sometimes make inferences, but their readers are entitled to know what is inference and what is established fact. It is just here that Miller fails us. From several examples, I have chosen two to illustrate my point.

The first example concerns the relationship between Eddington and Jeans. Miller tries to convince us that Eddington's treatment of Chandra was of a piece with his treatment of Jeans and Milne. The case of Milne is perhaps complex, but I have always thought that Jeans could and did give as good as he got in his debates with Eddington, even though we now consider Eddington to have had the better scientific arguments. Jeans, being independently wealthy, 'retired' (the word used by his biographer, Milne) at an early age from his Stokes Lectureship in Cambridge and never held a paid university position again. Miller asserts (page 99) that on the death of the then Plumian Professor (Sir George Darwin) Jeans expected to succeed but "… as a result of some Machiavellian internal politics, the professorship went instead to Eddington … [and] Disgusted with academia, Jeans resigned." No sources are cited for these statements. I can find no more specific date for Jeans' retirement (or resignation) than 1912. Darwin died on 7 December of that year. If Miller's statements are correct, the Board of Electors must have met and reached a decision (of which Jeans became aware) within three weeks that included Christmas. Jeans would have had to make up his mind and act within a few days. Here a scenario has been spun that I find implausible; Eddington was certainly not officially appointed until 1913. Yet, by page 169, Eddington has become "… the evil genius who drove Jeans out of academia." To be fair, the term "evil genius" is Chandra's, but, driven or not, was Jeans "out of academia"? Although Jeans never held a paid university post again, Miller fails to mention that he held for ten years (1919-1929) the extremely influential position of Secretary of the Royal Society and was very much involved in the affairs of the academic community.

In his 1988 book, *The Eddington Enigma* (which Miller appears to have overlooked), David Evans also speculated about whether there were other possible candidates for the Plumian Chair and the Observatory Directorship (which also went to Eddington in 1914). Evans did not mention Jeans, but thought that A.R. Hinks, then Chief Assistant at the University Observatory, might have hoped at least for the Directorship. Hinks *did* leave Cambridge and astronomical research shortly after Eddington's appointment was announced but, even for him, the case is not clear. Hinks had become interested in geography and cartography; his change of occupation in 1913 could just have been a natural consequence of a gradual process.

The second example is Miller's account of the last time Eddington and Chandra actually met, at a conference in Paris shortly before the outbreak of World War II. In the last session they had an outspoken debate. Afterwards, Eddington made some attempt to apologize, but Chandra, still annoyed and upset rebuffed him. Wali and Miller both give accounts of this incident. Miller's is based on a taped interview of Chandra with Spencer Weart. Wali does not give his source but appears to be quoting verbatim, also from an interview. In each account the words spoken by the two men are identical, but the connecting passages are subtly different. In Miller's version, the incident becomes more dramatic and Eddington appears in a less favourable light. Even if, as is quite possible, Chandrasekhar recalled the incident slightly differently on different occasions, Miller appears to have chosen the account that better suits his case, without alerting the reader to the existence of a different version. More crucially, he omits supplementary information that Wali gives us: that Chandra was still annoyed and upset and that, when he had cooled down, he expressed regret that he had been "… rude and unforgiving …" when Eddington tried to apologize.

The last two parts of the book are less concerned with personalities and give a useful account of the history of the theory of neutron stars and black holes. Particularly thought-provoking is the link that Miller sees between the development of the hydrogen bomb and research on supernovae. The weakness of these sections, however, is that very little is said about the observational detection of collapsed objects. This weakness is connected to the controversies of the first part. On the front flap of the dust jacket, it is claimed that Eddington's 1935 encounter with Chandra hindered "… the progress of astrophysics for nearly forty years." Authors are not always responsible for what appears on the dust jacket, but on page 3 Miller himself writes that the "… story of these pioneering astrophysicists … [involved several factors that] led to a thirty-year delay before the notion of black holes was finally accepted." I do not believe these claims can be substantiated. First, the thirty years included one decade (the Second World War and its immediate aftermath) in which the pace of astronomical research was greatly reduced. During those thirty years, no-one could point to actual examples of neutron stars or black holes. The attitude of most observing astronomers during that period is well summed up in the words of Paul Merrill, an outstanding observer, quoted by Miller, although they refer to an earlier period: "… these discussions have, for the most part, the character of exercises in mathematical physics rather than astronomical investigations." If white dwarfs had been theoretically predicted before the discovery of Sirius B, very few would have found their high densities credible. Similarly, the (accidental) discovery of pulsars in 1968 is what led most of us to pay serious attention to neutron stars, and some to study optically inconspicuous X-ray sources in the hope of discovering a stellar-mass black hole. It is noteworthy that detection of both these kinds of collapsed object depended on observations made outside the visual region of the electromagnetic spectrum. Those observations could not have been made before radio and X-ray techniques each reached a certain stage of maturity and were made very soon after each reached the appropriate stage. I doubt if the debates between Eddington and Chandrasekhar had much influence on the development of non-optical methods of observation.

All this is not to argue that Eddington was blameless in this affair. It is now beyond dispute that he was wrong about the scientific point at issue and even his most fervent admirers must concede that he was, at the very least, insensitive to the insecurity that the young and homesick Chandra felt. Debate can range only around four questions: was Eddington's behaviour intentionally hurtful; was it 'in character'; did it materially affect Chandra's subsequent career; and was Chandra hurt by it? Only Chandra could answer the last question, and it appears from the records he has left that he was indeed hurt. The other three questions are now probably unanswerable. My own guess is that the answer to each is 'no'. Miller, I think, would answer 'yes' to each. That is an honest difference of opinion, but I feel that Miller presents the evidence in such a way as to make his own opinions seem more firmly established than, in fact, they are.

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***In Search of Dark Matter*, by Ken Freeman and Geoff McNamara (Canberra, Springer-Praxis, 2006), pp. xiii + 155, ISBN 13-978-0-387-27616-8 (paperback), Euro 29.95.**

In a sense this book is a biography of a life's work of the renowned Professor Ken Freeman and his involvement in the search for the elusive 'dark matter' since the early 1970s, climaxing in the MACHO search carried out at Mount Stromlo Observatory. It comprises the author's preface, list of illustrations, a prologue and fourteen chapters, with two appendixes. What is unusual, but certainly makes for easy reading and absorption of detail, is that each point discussed is highlighted with its own heading.

The author's preface opens the review with a short comment on how our understanding of the Universe has changed since Copernicus suggestion that the Earth was not the centre of the Universe. Our Sun became only a speck inside our Galaxy. Then in the 1930's Hubble confirmed that our Galaxy was only an 'island Universe' in a sea of galaxies. The book attempts to "... describe how far into the night we currently see ..." as a story beginning in the "... first few decades of the twentieth century." The building of larger and larger optical telescopes and numerous new wavelengths enabled astronomers to see further and more clearly. But the further they looked, in fact, the less and less of the Universe they saw. Once called 'missing mass', the search for 'dark matter' became the key to understanding our Galaxy and the Universe.

The first Chapter is appropriately-named "How to Weigh Galaxies", for without this seemingly-simple beginning, it is impossible to measure the content of dark matter. Jeans' and Newton's equations are simply discussed and Freeman then moves on to the historical background with a discussion on Oort's discovery of differential rotation and disk dark matter, the latter being proved to be false in 1988 by Konrad Kuijken (a Belgian student at Cambridge).

The narrative then moves on to the very large contribution made by Fritz Zwicky in the early 1930's, including the use of gravitational lensing to map the distribution of dark matter in the Universe. Zwicky's most noted works were his calculations of the dark matter in the Coma cluster and the use of the virial theorm. When Zwicky is compared with Oort in their contribution to science Zwicky does not receive the credit he is due, simply because, shall we say, his unfortunate habit of violently "... disagreeing with the deeply held scientific beliefs of his fellow astronomers ...", whereas Oort was the pre-eminent Dutch astronomer of his time and was much revered by fellow scientists.

Then for some reason the interest in dark matter waned for nearly fifty years, possibly due to the construction of very large optical telescopes, until the early 1970's when Ken Freeman and others using observations from radio telescopes measured the rotation of the galaxies and found the first evidence that dark matter exists in giant halos surrounding the galaxies.

The next few chapters discuss the measurement of the dark haloes and the discovery made by Freeman that galaxies do not rotate as expected. He compared the analytical consideration of rotation curves with what was actually seen for three or four galaxies and found that they were the wrong shape. The velocity of the stars and gases was not falling in proportion to their distance from the centre, and in some cases actually increased. Like his predecessor Zwicky forty years earlier, Freeman published a paper "... pointing out that something was definitely wrong." His conclusion was somewhat the same: "... galaxies contained considerably more invisible matter than visible matter."

The book goes on to discuss some aspects of dark matter in different types of galaxies, begging the question as to whether there are 'dark galaxies'. Freeman looks in more detail at the application of gravitational lensing in relation to Einsteins' Theory of General Relativity and how gravity affects light from a distant source. A study is made of the 'bits and pieces' that could have been left over from the Big Bang, such as the baryon inventory, and then it is a matter of explaining the concept of the critical density.

From here the discussion centers on MACHO astronomy in Chapter 9. This chapter opens with a review of the history of the final involvement of the Mount Stromlo Observatory, with a little discussion on the Great Melbourne Telescope. Freeman traces the beginning of Stromlo's MACHO Project to numerous discussions he had with Bohdan Paczynski, who deserves the credit for suggesting the use of microlensing to search for MACHO's (Massive Astronomical Compact Halo Objects). Paczynski had developed a very simple and direct analysis of how microlensing would look during an event and the expected difference of what would be recorded based on different mass/distance combinations. He had determined that to carry out such a search it would be necessary to measure several million stars every night. When asked, Freeman suggested that this could be done by photography, which had been developed by a group of French astronomers involved in the EROS project.

The idea lay dormant until 1990 when Freeman was contacted by a microlensing team from Princeton asking whether Stromlo would be interested in a collaboration. Coincidently, there was similar interest at the Lawrence Livermore National Laboratory and the University of California in Berkley, and they had a young research physicist, Chris Stubbs, who could built a large CCD array that could photograph 500,000 stars simultaneously.

Observations began in mid-1992 and finished in 1999, the most significant result being that the total mass of the dark matter objects in the Galaxy was only somewhere in the range 0.3–0.8 solar masses. The final chapters of the book discuss what else dark matter could be.

This book paints a very interesting picture of the search for the elusive dark matter, and I would recommend it to anyone with an interest in modern astrophysics or the history of Australian astronomy.

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Einsteins Kosmos. Untersuchungen zur Geschichte der Kosmologie, Relativitätstheorie und zu Einsteins Wirken und Nachwirken (Acta Historica Astronomiae, Volume 27). (_Einstein's Universe. Studies of the History of Cosmology, Relativity Theory, and Einstein's Effect and After-effects._ In German), edited by Hilmar W. Duerbeck and Wolfgang R. Dick (Frankfurt/M, Verlag Harri Deutsch, 2005), pp. 313, ISBN 3-8171-1770-1, Euro 26.80.

The Einstein year, 2005, attracted a great deal of interest from scholars and public alike, and another book on this famous scientist hardly surprises. But the astounding thing about the present volume is that it is really captivating and offers new and unexpected findings. The reason is that emphasis is laid not so much on the work of Einstein—that has already been presented elsewhere at length—but on the environment and its contribution, which usually, and unjustly, remains in the shadow of Einstein's popularity.

The volume starts with a presentation of the discussions about the gravitational redshift which at present is understood as an obvious consequence of the equivalence of inertial and gravitational mass and implemented in any theory of gravitation. The question of the observation of this redshift was considered so important at the time of the search for a general theory of relativity that it was the impulse to construct the famous tower telescope at Potsdam. In this context, Klaus Hentschel's paper is well worth reading. An equally-appealing paper by Peter Brosche follows about the deflection of light near the Sun, where arguments associated with alternative explanations are presented as well. Without acquaintance with these arguments we would remain in the dark about why the question has raised so much controversy. Then Matthias Schemmel shows in his paper that it is overly simple to recognize Einstein as the inventor of a curved space Universe.

An extensive description of Einstein's attitude with respect to cosmology is given by Tobias Jung. It shows (implicitly of course, and backed in the papers about Friedmann and Lemaître that follow) how wrong and misleading the often-used slogan about Einstein as the 'engineer of the Universe' really is. Jung presents the person and work of F. Selety, together with the often-ignored classical model to solve Newton's paradox. Hans-J. Schmidt takes up the topic of the cosmological parameter and the deSitter models. Georg Singer, the German editor and translator of Friedmann's book, *The World as Space and Time*, treats the often-cited relat-ion between Friedmann and Einstein. As E.A. Poe wrote: "Nil sapientiae odiosius est acumine nimio." Kurt Roessler presents G. Lemaître and his ideas which are accepted by present cosmologists in different respects. Hilmar Duerbeck and Piotr Flin treat the impact of Ludwik Silberstein. Silber-stein gives an interesting perspective on the relation between the theory of relativity and cosmology during the first half of the last century. He recalls that there were not only scientifically-irrelevant and politically-motivated attacks on Einstein, but also honorable *advocati diaboli* who essentially established the magnificence of relativity theory. Jürgen Renn and Tilman Sauer describe the history of gravitational lensing and the role played by the little-known Rudi W. Mandl, which is a tragic-comical story of a person from outside academia who had a brilliant idea.

Dieter B. Herrmann publishes his short but interesting correspondence with E.G. Straus, with comments by Siegfried Grundmann, an expert in history. The volume ends with a presentation of Einstein's relations with the Archenhold Observatory in Berlin, a corresponding address of H.-J. Treder dating back to 1979, and an attempt to list various Einstein memorial sites by Wolfgang Dick and Arno Langkavel—which seems rather limited when one takes into account the present eagerness to decorate things and places with the name of Einstein. A list of related publications in the *Acta Historica Astronomiae* is added.

When time is short, history, and particularly the history of science, is often put aside as being less urgent. The reader is encouraged not to do so with the present volume. It is entertaining and captivating, and those who are prepared to spend the time will discover many new facts and now and then will hear the penny drop.

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The Evolution of Harmonic Analysis: Models of the Real World, by Elena Prestini (Boston, Birkhäuser, 2004), pp. 349, ISBN 0-8176-4125-4 (softback), 235 x 155 mm. $59.95.

Harmonic analysis, closely associated with the name of Jean-Baptiste-Joseph-Fourier (1768–1830), has had an intimate relationship with astronomy. One immediately thinks of Albert Michelson (1852–1931), discoverer of the Gibbs overshoot phenomenon, and his analysis of the diffraction patterns of starlight as he presented them to the eyepiece of the 100-inch Mt Wilson telescope, thereby making possible the measurement of the angular diameters of the red giants Betelgeuse and Arcturus, something that was not possible from a photographic image made with the same telescope. (Mind you, he could reach an interferometric baseline of 196 inches using the 100-inch telescope—smart!) Here we see the essence of the Fourier transformation, an operation that relates two distinct physical entities, an image on the one hand and a spectrum on the other. Later (1954), the Brown and Twiss interferometer permitted extension of the technique to Sirius and other main-sequence stars. Later still, and in current use today in radio astronomy, two-dimensional images $f(x,y)$ of the radio sky are derived from interferometer observation of a two-dimensional spectrum $F(u,v)$. Despite the hundred-thousand-fold handicap in wavelength, radio astronomical images are now superior in angular resolution to the best optical images. But watch out! Optical interferometry is alive and well as it moves to large spatial scales.

Fourier's theorem was first encountered by many of us in a mathematics course on the theory of real functions, where physical application was not the point. Fourier's essay submitted to the Institut in 1811 for the prize in mathematics seemed indefensible to the great French mathematicians of the day (Laplace, Lagrange, Biot, Poisson, no less), but led to the epoch-making flowering of mathematical analysis in the nineteenth and twentieth centuries. Others of us encountered Fourier in elementary physics demonstrations of vibrating-string waveforms and their audible spectra. These developments are already far removed from Fourier's original motivation, the study of heat conduction, where one entity might be the temperature distribution $f(x)$ on a bar of metal and the other entity is the spatial spectrum $F(s)$. In order to find out how an initial temperature distribution will evolve as heat conducts from the hotter to the cooler we analyse into spatial harmonics, follow the appropriate exponential time-decay of each as time elapses, and reassemble the attenuated harmonics to get the temperature distribution at a later time. How unpredictable that thermal studies for direct engineering application should prove to be of use in astronomy. But wait. We now know from his collected works that Carl Friedrich Gauss (1777–1855) was using harmonic analysis *before* Fourier's announcement. In 1805 Gauss had Fourier-analysed a time series of observations of a comet into its sinusoidal components in order to predict an orbit. The astronomy connection thus proved to be deeper than we were taught. Not only that, but the Fast Fourier Transform algorithm that is now ubiquitous in computer modelling, was used by Gauss. In 1805 he wrote (in Latin) "… illam vero methodum calculi mechanici tae dium magis minuere, praxis tentatem docebit." (trial will teach the user that this method truly lessens greatly the tedium of mechanical calculation).

Just as music in its origins would not fall within the denotation of music today, antedating even the drum, so harmonic analysis has roots antedating the sine wave. Claudius Ptolemaeus (c. AD 150), was well acquainted with the sine wave (witness his table, which is more precise than today's high-school four-figure sine table). He nevertheless used epicycles and equants to generate corrections to circular orbital motion, whereas our ephemerides today correct by adding fully equivalent first- and second-order sinusoids. But even before that, the Assyrians around 300 BC were predicting lunar positions (using the sexagesimal system of arithmetic inherited from the Babylonians that survives to this day), from a circular fundamental plus added saw-tooth shaped overtones to account for the deviations due to eccentricity and inclination. Harmonic analysis thus has very deep roots in astronomy. The same can be said of spherical harmonics.

In a real sense, the algebraic corrections used in today's ephemerides mimic the small geometrical corrections introduced by Ptolemy to gain agreement with observation; in both cases these correction terms belong to a *mathematical model* of reality, not to dynamics. To ridicule Ptolemy for believing in epicycles in the sky is equivalent to criticising the *Explanatory Supplement of the Nautical Almanac* for positing polynomials in the sky. The ellipse is rarely utilised in the tables presenting the positions of the Moon, planets and Sun.

Elena Prestini's book is not only historically interesting but is also useful. She relates developments in apparently disparate fields. Topics from engineering include telecommunication, wireless telephony, noise, sampling, space propulsion, and space exploration. The fascinating thing about Fourier analysis is that the symbols and terms that become familiar in some particular field translate into analogous but different symbols and terms in many other fields. The mathematical basics remain the same.

The spectral sensitivity function, as introduced in radio astronomical mapping, is identifiable as the familiar transfer function of electrical network theory. Fringe visibility, introduced by Michelson in his stellar diameter work, identifies with the modulus of the complex visibility of radio astronomy. An observer at an eyepiece could estimate the maximum/minimum ratio of fringes seen sliding by under atmospheric influence but spatial phase was then beyond reach. As deteriorating atmospheric conditions cause coherence between star waveforms incident on two spaced mirrors to fall off, Michelson's instrument and the original Brown and Twiss instrument fail.

So the book moves on to 4. Sound, Music, and Computers, 5. Fourier Optics and the Synchrotron Light, 6. X-ray Crystallography, 7. The Radon Transform and Computerised Tomography, 8. Nuclear Magnetic Resonance, and 9. Radio Astronomy and Modern Cosmology.

Some linkages between fields illustrate the timelessness of the basics. Reconstruction of medical X-ray images from line scans borrows its algorithm from a 1967 publication in the *Astrophysical Journal* dealing with reconstruction of celestial images from fan-beam scans made with a unidimensional radio telescope array! The word 'reconstruction' in this usage derives from 1958, when one needed to distinguish the mathematical inversion (which is unique) from 'restoration' for beamwidth smoothing (which is not).

Because of early entry by electrical engineering into academic coursework (due to the pioneers of radio astronomy having emerged from wartime radar and electronics) the courses now available to radiologists, geophysicists, fibreoptic and wireless communicators, statisticians, and computer scientists are in electrical engineering departments. Books on all these subjects are challenged by the annual rates of advance, but Fourier texts are durable.

As William Thompson (1824–1907, later Lord Kelvin) wrote, "Fourier's theorem is not only one of the most beautiful results of modern analysis, but it may be said to furnish an indispensable instrument in the treatment of nearly every recondite question in modern physics." The Prestini book is your guide to a good part of this beauty and universality.

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***The Transits of Venus*, by William Sheehan and John Westfall (New York, Prometheus Books, 2004), pp. 407, ISBN 1-59102-175-8 (hardback), 235 x 160 mm, US$28.**

The 1769 transit of Venus has a special place in the hearts of the Centre for Astronomy at James Cook University (JCU) where I am the Director. The principal campus of JCU, where the Centre for Astronomy is located, is situated in Townsville on the coast in the north-eastern corner of Australia. We live here now as a consequence of Cook's first voyage that took him to Tahiti for the 1769 transit of Venus. It was from Tahiti that Cook sailed westward to ultimately discover Australia's east coast, and then northward along the coast and past where Townsville now stands. If it were not for that voyage this part of the world would have developed in a completely different way. Perhaps we would now be speaking French. Who knows?

In this part of Australia today we have localities with names like '1770', 'Cooktown' and 'Magnetic Island', all named by or as a consequence of Cook's first voyage. Indeed, as I write this review on a perfectly clear winter's day I can see Cook's 'magnetical island or peninsular' from my window. In 1770 this land mass was wrongly accused of deviating the compass of the *Endeavour*. Today Magnetic Island is a popular tropical paradise for tourists.

The Centre for Astronomy at JCU is proud of the connection with the 1769 transit of Venus, and we use images of Cook and his work as part of our promotional material.

I have been a fan of the writings of William Sheehan after reading his *Epic Moon* (this is because I have always been a closet lunar observer, although I don't often admit to it for professional reasons!). I have also read numerous articles of his that have appeared in *Sky and Telescope* and *Mercury*. I am not sure what I like the most about such material. Perhaps it is that it highlights the integrity of the earlier astronomers and the quality of the work that has laid the foundations for our modern cosmological view of the Universe, or perhaps, it just exposes the idiosyncrasies of their personalities. I wonder if history will be kind to us?

For the *Transits of Venus* Sheehan has combined forces with John Westfall from San Francisco University to produce a scholarly work of about 400 pages on the history of the transits and technical data for the (then) future transits of 2004 and 2012.

This book is authoritative. It is not a book for someone with a passing interest in the subject, and indeed among the flurry of books that appeared prior to the 2004 transit this is perhaps the best. There are fourteen chapters which cover the subject in depth—perhaps a little too much depth—as is seen by the fact that Chapter 2 starts with planetary observation where we meet irrelevant personalities such as Ra and Tutankhamen. This book certainly starts at the beginning, and all non-technical details are introduced in this comprehensive tome.

Chapter 3 moves on slowly with subheadings such as "Venus in Antiquity" and "Astronomic Models in History", which is again too much detail in my opinion. And so the book progresses. We meet "Halley's Grand Proposal" (one of the driving forces for the 1769 voyage of Cook) as late as Chapter 7, and "Cook's Tour" is covered in Chapter 8.

The latter Chapters 11 to 13 at last get to the point as far as I am concerned. Here we see that the science of transits of Venus has reached its zenith. For the transits of 1874 and 1882 the power of 'modern' science is applied, and this makes fascinating reading. We learn about the teams of observers, the expedition, the methods of observation (for example photography was used in 1874 and 1882), the flaws that are found in techniques and the ways that these were rectified. We see in Table 4 that there were about one hundred observers from ten countries participating in the 1874 campaigns and that (Table 6) twenty-four individual determinations of the AU were made and that the precisions derived was approaching one third of one percent.

However—and this is a criticism of much historic science writing—there is a lot of detail about the collection of the observations but no detail of the numeric analysis that was applied to it. One can only imagine the countless months/years of methodical work, the countless pages of calculations that went into corrections for clock errors, solar limb darkening, planet orbit computations, etc., that must have followed the observations and which had to be done before the final value of the AU could be determined. Volumes such as this are unfortunately silent on these matters and I, for one, would like to look into the historic magnifying-glass at such detail.

The last chapter, Chapter 14, moves away from the history and is a *Compleat* (sic) *Guide to Our Transits: 2004 and 2012*. This is popular stuff, 'historic' in part now, and probably irrelevant as better popular material will no doubt appear prior to the 2012 event.

Appendices (Appendix A) list transits from 2970 BCE to AD 7464, (B) data on the transits from 1639 to 2012, (C) data for the 2004 transit (now obsolete) (D) data for the 2012 Transit, and (F) sunshine probability maps. Chapter notes and the bibliography are comprehensive, as one would expect from these authors.

This is a very comprehensive piece of work that could have been improved by a little more care with presentation. A colleague of mine has noted that the plotting of locations of observation on the world maps (e.g. Figure 77) could have been done with greater precision, and I am very disappointed with the fact that the review copy supplied to me broke its spine at page 142!

The book is well illustrated with about one hundred figures. Maintaining the authenticity of content is obviously important in any historic writing, however, I do believe that for a book like this many of the images could have been improved with a little work with Photoshop and presented in a larger format. In addition there are no colour plates, and the images presented lack the visual impact of, for example, the work of Lomb which I reviewed in the previous issue of this Journal, where the colourful cover of H.C. Russell's *Observations of the Transit of Venus, 9 December 1874* from Sydney Observatory was reproduced with spectacular and superior effect.

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***Hōkūloa. The British 1874 Transit of Venus Expedition to Hawai'i*, by Michael Chauvin (Honolulu, Bishop Museum Press, 2004), pp. xiv + 262, ISBN 1-58178-023-0 (hardback), 235 x 160 mm, US$26:95.**

The thought of yet another book on the historic transits of Venus—its appearance timed to coincide with the 2004 event—would under normal circumstances hardly fill me with joy. But in the case Michael Chauvin's tome this was a different story. A skilled astronomical historian (e.g. see his masterly chapter in Selin's *Astronomy Across Cultures*, 2000), Chauvin promised much with his detailed account of the British expedition to Hawai'i in 1874, and he has not disappointed.

Hōkūloa is a carefully-research and copiously-illustrated exposé on the 1874 transit, as seen through the eyes and minds of the visiting British astronomers and their Tahitian hosts. But it is more than this. Chauvin also explains what a transit of Venus is, and he introduces us to earlier transits via Horrocks (1639) and Cook and Green (1769). There are also chapters on finding longitude, and on the extensive preparations undertaken for the various British 1874 expeditions—including the paramount role played by the colourful Astronomer Royal, George Biddell Airy.

And so to Hawai'i, where the seven astronomers in the British party (to whom we have already been introduced) are treated royally. The main observatory is set up at 'Āpua, two ancillary stations are established at Kailua-Kona and Waimea (on the islands of Hawai'i and Kaua'i respectively), instruments are readied, and practice observations are made in anticipation of great day. But one thing the waiting astronomers could not control was the weather:

> … Mother Nature was ... simply biding her time and refitting her arsenal. Before October was out, she would produce enough wet weather to transform a hot town into a steamy one. And in the weeks and months to follow, she would regularly give Tupman [head of the British party] good cause for his private—and frequently repeated—lamentations over her moist outpourings. In November she was especially generous in this respect, unleashing at Nu'uanu a cumulative rainfall of 23.99 inches. It was the wettest month of 1874 at that location … On one occasion, more than a foot of rain pummeled the 'Āpua site within a twenty-four-hour period, and Tupman's sagging disposition grew increasingly droopy under the dependable inundations. (page 85).

This lengthy quote also reveals something of Chauvin's marvelous writing style which, when combined with the shortness of most chapters, made the reading of this book a pleasure.

As it turned out, Tupman need not have worried, for 8 December 1874 dawned warm and clear, and all three observing teams saw the transit. After detailing their successful observations, and providing an account of the public interest in the spectacle, Chauvin recounts the post-transit preparations for the long voyage home.

The final chapters deal with "Reducing the Data", "Publishing and Retiring", "Reaffirmations" and "A Meeting in Siberia", and bring the number of pages to 157, leaving a goodly number—105 pages to be exact—for four appendices, background details relating to many of the diagrams in the book and extensive notes and references for all thirty-five chapters. This supplementary material, and the eleven-page Bibliography which follows it, are an invaluable resource for anyone wishing to chase up further information about the transit, or life in Hawai'i in 1874.

All in all this is an excellent book. It is sheer entertainment as well as an astronomical *tour de force*, and I thoroughly recommend it to anyone with an interest in nineteenth century astronomy. It will be a valuable addition to your library.

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***Revolutionaries of the Cosmos. The Astro-physicists*, by Ian Glass (Oxford, Oxford University Press, 2005), pp. xiv + 317, ISBN 0-19-857099-6 (hardback), 234 x 156 mm, £35.**

Biographical sketches of eight astronomers from Galileo to Hubble show what it takes to become a 'cosmic revolutionary' in this interesting book by the well-known South African astronomer, Ian Glass. Through the careers of these scientists certain themes emerge in this quest. Getting one's work published first has always been important. Galileo was not the only one to use the newly-invented telescope to look at the heavens, but his name is far better known than the others. His pamphlet, *The Starry Messenger*, detailed his telescopic discoveries, and became known throughout Europe only weeks after publication.

Recognizing that the new may also be important is a factor. This may seem obvious to us today, but it was not always so. Glass points out that Uranus was observed at least thirty times before its discovery by Sir William Herschel, and he attributes this to the fact that previous observers thought that for the most part objects that could only be seen in a telescope were somehow less important than those that could be seen by the unaided eye.

Sometimes *not* believing the observations prevents a breakthrough. For example, Glass relates an anecdote from Alan Sandage, that in 1920 Milton Humason showed Harlow Shapley photographic plates of M31 with the cepheid variables in the images marked off. Shapley did not believe that these variables could be so faint. Had he followed up the observations then he, and not Hubble, would be famous today for confirming that we live in a Universe of galaxies.

How astronomy is financed has in some respects not changed over the centuries. Most of the astronomers profiled in this book were employed by universities for at least a significant part of their careers. What has changed has been the decline of the self-financed 'gentleman-scientist' as personified by Sir William Huggins in the nineteenth century, and the rise of private foundations in financing observatories. Glass rightly focuses on the latter aspect in his chapter about George Ellery Hale.

Since each biographic sketch is given approximately thirty pages, naturally not everything can be fully presented. I therefore found particularly intriguing a letter Glass quotes from the poet Thomas Campbell to an unknown correspondent detailing his dinner with Sir William Herschel in 1813. Herschel is quoted as stating "... I have looked further into space than ever human being did before me. I have observed stars of which the light, it can be proved, must take two million years to reach the earth ..." This sounds to me as though Herschel was over a century ahead of his time in his estimate for the size of the Universe, and I also wonder if he was the first to understand the concept of 'look back time'.

In addition to the afore-mentioned dedicated observers, two other astronomers who are primarily recognised as theoreticians—Newton and Eddington—are also assigned chapters in this book. They, too, were 'cosmic revolutionaries'.

This book has many references to primary and secondary sources. I recommend it for anyone who has a general interest in astronomical history and it may be particularly useful as a supplement for an undergraduate course on the topic.

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***English Mechanic (Facsimile series)*, published by E.S. Hutton (bookman@rmplc.co.uk, 2006), 14 DVDs, £22 (individual DVD), £308 (set of 14 DVDs), £50 (site license, individual DVD), £650 (site license set of 14 DVDs).**

The *English Mechanic* was a popular 24-page weekly magazine that was published in London between 1865 and 1926. Advertised as a "Mirror of Science and Art", it contained articles and letters on a very wide variety of science-technology-medical topics, which provide us with "... a fascinating picture of the social and cultural attitude of the time ..."

Of special interest to us are the numerous items relating to astronomy and astrophysics, telescopes and other astronomical instrumentation, contributed by a world-wide network of well-known amateur and professional astronomers.

Copies of the original magazines are now rather hard to find (unless you happen to live near a library that carries issues), so Eric Hutton has done us all a great service by producing a full set of facsimiles, available on fourteen DVDs. As he says in the publicity brochure, you can now "Search 72,269 pages, an estimated 78,000 letters, and 160,000 queries (in seconds)", and chase up elusive details about those nineteenth century telescope-makers and the celestial observations they carried out. And if you are only interested in a sub-period within 1865-1926, you only need buy those DVDs that are relevant.

Finally, don't forget that extra bonus: the innumerable non-astronomical entries that are bound to provide you with many hours of light-hearted entertainment along the way. For further information about this facsimile edition you can e-mail Eric Hutton on bookman@rmplc.com.uk, or consult the following web site: www.englishmechanic.com

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