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The *Journal of Astronomical History and Heritage* (*JAH^2*) was founded in 1998, and from 2007 will be published three times yearly, in March, July and November. It features review papers, research papers, short communications, correspondence, IAU reports, and book reviews. From 2007, *JAH^2* will be available in paper form, or electronically, on a CD.

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

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The annual subscription rates for Volume 10 (2007) are:

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AU$88:00 for individuals

COVER PHOTOGRAPH

Bruce Slee (1924–) at the control desk of the Australia Telescope Compact Array in September 2002. Since this radio telescope was opened, in 1988, Slee has used it to carry out a variety of Galactic and extra-galactic research projects in conjunction with Australian and overseas colleagues. This is the fourth and final issue in the *JAH^2* series 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 June 2006 issue.

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Journal of Astronomical History and Heritage, 9(2), 138 (2006)

EDITORIAL

This is the fourth issue of the *Journal of Astronomical History and Heritage* (JAH^2) to appear under the banner of the Centre for Astronomy at James Cook University, Townsville, Australia. In the course of the past two years we have published 30 different papers, 5 IAU Reports, 26 book reviews and 1 obituary. We have also seen the size of the journal vary between 68 pages and 136 pages, in part reflecting the volume of copy (much of it unsolicited) that is now crossing my desk. In order to accommodate this increased research output from the international community of historians of astronomy we have decided to produce three issues per year (instead of two) from 2007, with copies scheduled to appear in March, July and November. In addition, we plan to start using spot colour in some of the papers, and we will also offer subscribers a choice of paper copies of the journal or electronic copies on CDs.

These changes will involve significantly increased production and mailing costs (remember, all overseas issues of JAH^2 go out by airmail), and so we will have to increase subscriptions, as from 2007. Nevertheless, JAH^2 will still be one of the best-priced international astronomical journals on the market, and we trust that you will continue to give it your support.

Many of you will be pleased to know that the experiment to launch graduate programs in history of astronomy (HoA) here at James Cook University (JCU) has been an overwhelming success. Currently, there are thirteen students enrolled in HoA doctorates. One is studying full-time, and the rest (from Australia, Lebanon, South Africa and the USA) are part-time, off-campus students. As you can see from the following list, they are researching a variety of topics:

- A History of Research into the Concept of 'Dark Matter'
- Abdul Rahman al-Sufi and *The Book of the Stars*: A Journey of Re-discovery
- Amateur-Professional Collaboration in Astronomy: A History of South Africa's Earliest Astronomical Societies
- Contribution of the Division of Radiophysics Potts Hill Field Station to International Radio Astronomy
- Early Pulsar Research and the Roles of the Molonglo Radio Telescope, Parkes Radio Telescope and the Culgoora Circular Array
- Kepler's War on Mars and the Usurpation of Seventeenth Century Astronomy
- Observations of the Southern Open Cluster NGC 4755 from 1751 to 1980: Changing Perspectives
- Quasi-Stellar Objects, the Owens Valley Radio Telescope, and the Changing Nature of the Caltech-Carnegie Nexus
- The Cosmology of Huacas and Ceques: A Study in Peruvian Archaeoastronomy
- The History of Low Frequency Radio Astronomy in Tasmania
- The Lick Observatory Solar Eclipse Expeditions and the Study of the Solar Corona
- The Published Research Output of the Melbourne Observatory: A Critical Evaluation
- The Tennessee Impact Sites: Changing Perspectives in Meteorite Research

Sharing the supervision of these students with me are JCU Centre for Astronomy staff members, David Blank (Lecturer) and Graeme White (Associate Professor), plus three new JCU Adjunct Professors of Astronomy: Kim Malville (USA), Richard Stephenson (UK) and Brian Warner (South Africa). Others also involved in student supervision are Richard Strom (The Netherlands) and Richard Wielebinski (Germany), and shortly both will also be appointed to JCU Adjunct Chairs in Astronomy (and, along with Brian Warner, will also be able to co-supervise JCU astrophysics doctorates). A number of new part-time HoA doctoral students will begin their studies in 2007, researching topics in archaeoastronomy, the scientific output associated with a large U.S. historic telescope, early Australian solar radio astronomy, and the popularization of U.S. astronomy during the nineteenth century.

The introduction of history of astronomy within the JCU Master of Astronomy degree has also been a resounding success, with students enrolling for both the coursework unit ("Scientific and Technological Developments in Astronomy") and the following final unit of their degree, a semester-long historical research project. To date, students have carried out research projects on aspects of Canada and New Zealand meteor astronomy, Cook voyage instrumentation and astronomical observations at Nootka Sound, the role played by M31 in furthering our understanding of astronomy and astrophysics, and the research accomplished with the Jodrell Bank 218-ft transit radio telescope.

Now to return to the journal: his issue of JAH^2 brings to an end the four radio astronomy numbers dedicated to the pioneering Australian radio astronomer, Bruce Slee. I began my own involvement in radio astronomy back in November 1961, as Bruce's Research Assistant, and it was a pleasure to resume our research collaboration in 2001, when I joined the Australia Telescope National Facility as their Archivist and Historian. Bruce was by then an ATNF Honorary Fellow, and we scored several observing runs with the Australia Telescope Compact Array researching radio emission from chromospherically-active stars. Bruce and Nan have been friends for more than forty years, and it has a pleasure producing these special radio astronomy issues of *JAH2*. Future issues of the journal will continued to include papers on the history of radio astronomy—along with papers on a wide range of other topics.

Finally, I hardly need remind you all that we are always happy to receive unsolicited manuscripts. When preparing your manuscript it is best to begin by consulting the 'Guide for Authors' on our web site: just click the 'History Astro. Journal' box on the following web site: www.jcu. edu.au/astronomy Here, then, is the December 2006 issue of JAH^2. Enjoy …

Wayne Orchiston
Editor

Journal of Astronomical History and Heritage, 9(2), 139-144 (2006).

OLOF RYDBECK AND EARLY SWEDISH RADIO ASTRONOMY: A PERSONAL PERSPECTIVE[1]

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Abstract: The spectacular development of radio astronomy in Europe and Australia in the period soon after WWII was mostly propelled by 'amateur' scientists motivated by a spirit of adventure. Totally untrained in astronomy, these pioneers were necessarily courageous and highly individualistic. Each of the leaders was 'a character', and often larger than life. And among these personalities there was none bigger than Olof Rydbeck of Sweden. He was already well known for his studies of electromagnetic theory and the invention and fabrication of devices for ever higher frequencies. He was one of the pioneers in the study of the ionosphere, and had built powerful sounders and also detectors for meteor trails. The creation of the Onsala Radio Observatory was entirely due to his efforts.

Keywords: Olof Rydbeck, Onsala Radio Observatory, Würzburg antennas, H-line

1 INTRODUCTION

A crude paraphrasing of the development of radio astronomy immediately after the end of WWII could be *'Engineers with instruments of warfare looking for something to do with them.'* In England and Australia the pioneers were all radar specialists who understood how to build directional antennas and sensitive receivers (Lovell, 1983; Sullivan, 1988). When they pointed their gadgets skywards, they discovered extraordinary things and overnight expanded the known Universe, so to speak (Sullivan, 1984). In the United States, the group that was intimately involved in developing electronics for warfare and trying to use it for astronomy later was based at the Naval Research Laboratories (Haddock, 1983). The difference was that unlike the other two groups, they were using the very high frequency end, which was the region of their expertise.

It is an interesting fact that on a visit to NRL the father of Australian radio astronomy, Joe Pawsey, told them they were wasting their time because the important messages were all at low frequencies. They had the good sense to ignore his advice, and went on to make equally interesting and important discoveries. The point I want to make, and which is true in some sense even today, is that one really does not know about the astrophysical mechanisms that may be operating up there, to predict the frequency and the strength of signals we could expect to receive on Earth. Pulsars, discovered twenty-five years after the period I am talking about, and molecular masers even later, are good examples.

2 OLOF RYDBECK

Sweden being a neutral country was not involved in any activity related to World War II. But its radio astronomy pioneer, Olof Rydbeck,[2] had several attributes in common with his counterparts from the warring nations. He was a great expert in electronics, as adept at using radar as any of them, and equally ignorant about astronomy. He had written a definitive paper on the theory of the travelling wave tube, built several of them, and was a celebrated pioneer in the use of radar for studying the ionosphere and aurorae.

Rydbeck was an authority on electron tubes and was always trying to invent better ones. My first job with him was to work on a frequency-multiplying concept he called the rotatron, in which a rotating electron beam went through an anode with a ring of holes and produced pulses on the next anode.[3]

Returning to radio astronomy pioneers, there was one who knew nothing about radio, even less about radar, but everything about astronomy, who had no radio telescope but wanted one to do a specific research project. And unlike all the other pioneers I mentioned, he knew what frequency was the right one for his purpose. Jan Oort wanted to unravel the structure of the Galaxy, with a radio spectral line from interstellar hydrogen. As everybody knows, the line was first detected in 1951 (Ewen and Purcell, 1951), and launched the era of radio spectroscopy in astronomy. But not many know that Rydbeck's first attempt to get funding to build a 21cm receiver was in 1950, a year before the detection of the line. He knew about van de Hulst's 1945 paper and Shklovsky's 1949 paper (presumably translated from the Russian). His proposal, which was rejected, was to use such a receiver on a Würzburg German radar antenna of 7.5 meters diameter, exactly as Oort and company were planning to do.

3 ESTABLISHING THE ONSALA RADIO OBSERVATORY

Rydbeck had seized upon the idea of obtaining abandoned Würzburgs as the way to realize his dream of setting up a first-rate radio observatory despite having limited funds. These ex-radar antennas could work at high frequencies and came complete with mountings, all for little or no money. But where to find them? The determined character that he was, Rydbeck undertook an expedition, and he drove a Chalmers University station wagon all the way from Naples along the coasts of France, Belgium and Holland looking for these antennas. He heard that three had been rescued from the Channel coast and were at Meudon Observatory in Paris. He went to see them and was told that there were none in Belgium, but that two had been rescued in Holland, at least one of which was at Kootwijk (see Figure 1). This photograph was taken in 1950, and shows part of the radio telescope that a few years later did all that spectacular first mapping of the Galactic neutral hydrogen (see Westerhout, 2002). It was at Kootwijk that Rydbeck learned that there were five large Würzburg antennas in very remote places on the Norwegian coast (e.g. see Figure 2), erected with the labour of prisoners of war.

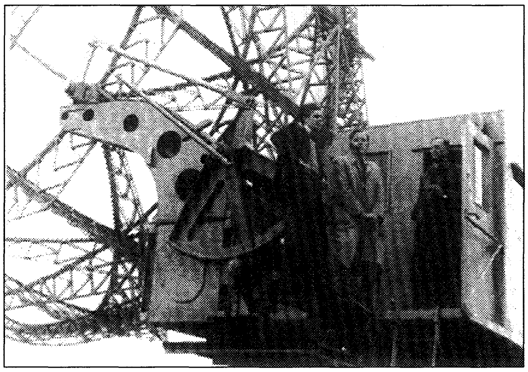
Figure 1: The receiver cabin and part of the Würzburg antenna at Kootwijk, Netherlands (after Rydbeck, 1991).

This was the chance Rydbeck had been looking for, and the political exercise of getting the consent of the authorities in the Norwegian government was the sort of thing he was expert at. He stressed the role that Chalmers University had played in educating Norwegian students, highlighted the good science that could be done with these old antennas which would otherwise only have scrap value, and successfully negotiated the token price of 300 crowns for each of them.

The physical exercise of demounting these 17 ton assemblies (Figure 3), all in relatively inaccessible locations, dragging the pieces down the mountains to the coast, loading them on lighters, and shipping them by sea to Gothenburg, is a saga that Rydbeck actually had nothing to do with. He was away in the U.S.A. in June 1950, when this feat was carried out in less than a month by staff from the laboratory's workshop. These were people I got to know and work with when I came to Sweden several years later. In recording this amazing operation in his autobiography, Rydbeck (1991) graciously notes that the speed and success of the whole exercise in part may have been due to the absence of their Professor breathing down their collective necks!

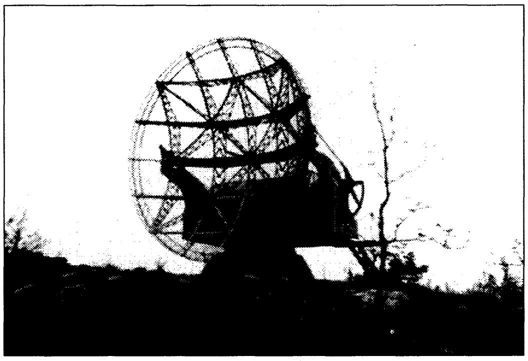
Figure 2: One of the abandoned Würburg antennas located on the remote Norwegian coast (after Rydbeck, 1991).

In a process that began in the late forties and ended only around the mid-fifties, Chalmers University of Technology in Gothenburg acquired a substantial plot of land on the picturesque peninsula of Råö (see Figure 4), the benefactor being Herbert Jacobsson (Figure 5) who had contributed to many such good causes in the course of his career. Unfortunately he did not live to participate in the formal opening there of the Onsala Radio Observatory in 1955.

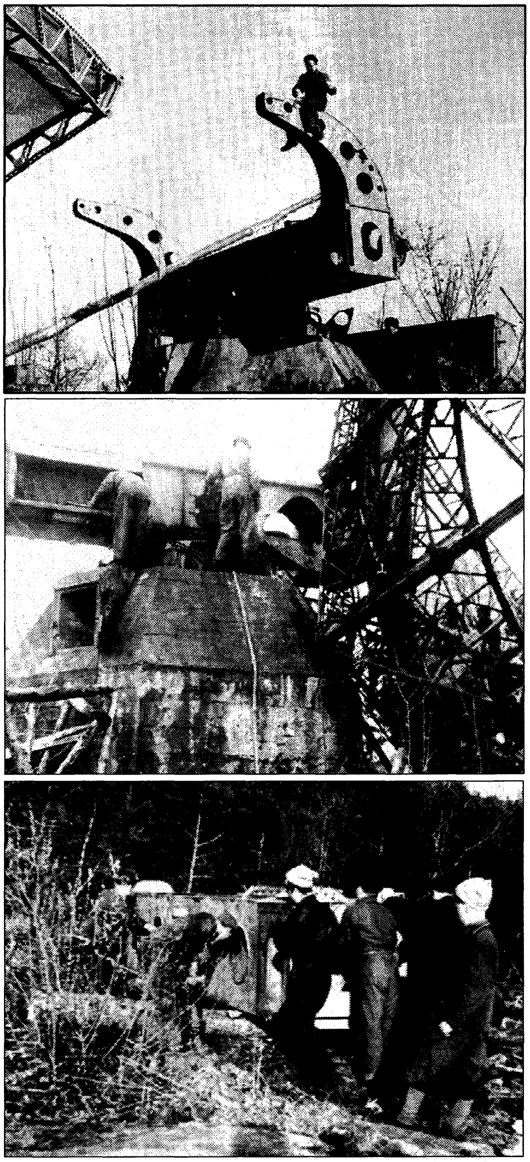
Figure 3: Three photographs showing the dismantling of one of the Norwegian Würzburg antennas (after Rydbeck, 1991).

The intervening years were apparently the worst economically, with no money even to set up the radio telescopes that had been brought from Norway, almost for free. Another benefactor in the form of Axel Wennergren (Figure 6), the inventor of the monorail concept, then came to the rescue and enabled the Würzburgs to be erected (Figure 7).

Not all of the five Würzburg antennas that had been brought in pieces from Norway were restored to original status, or could be, because of the state of the reflectors.

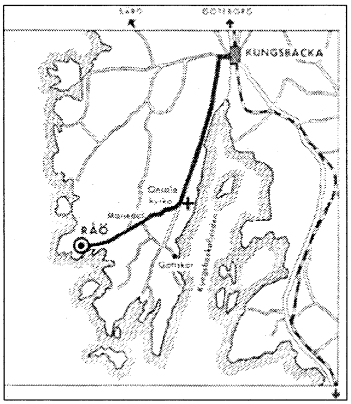

Figure 4: Map showing the location of the Onsala Radio Observatory (cross) and nearby Råö (after Rydbeck, 1991).

Figure 5: Herbert Jacobsson, who donated land near Råö to the University so the Onsala Radio Observatory could be established (after Rydbeck, 1991).

Figure 6: Axel Wennergren (right foreground), the benefactor who funded the erection of the Würzburg antennas at the Onsala Radio Observatory (after Rydbeck, 1991).

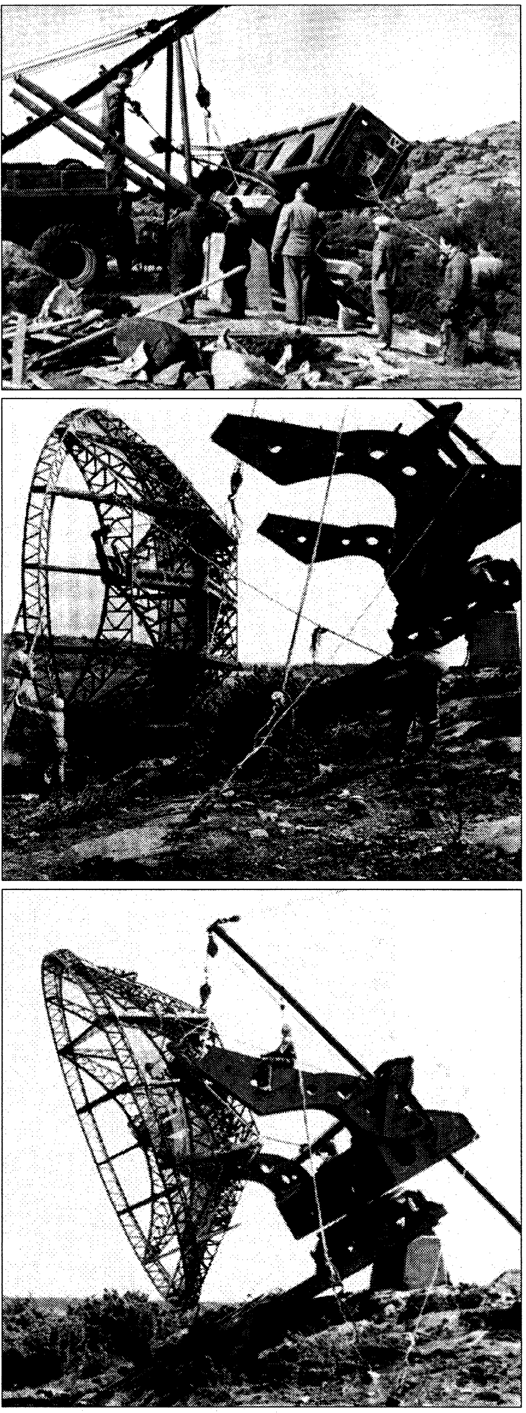

Figure 7: Three photographs showing erection of one of the Würzburg antennas at the Onsala Radio Observatory (after Rydbeck, 1991).

Two of the mounts were erected with a vertical axis as in the original configuration for radar use, but without the paraboloidal reflector. Instead two towers were mounted on the horizontal beam from which hung a gigantic array of dipoles working at 150 MHz (Figure 8). With an impressive area of about 135 square meters and equipped with cascode amplifiers, the low-noise system of those days, this provided good sensitivity for several investigations. A very important early study of ionospheric scintillation using one of these antennas was by Torleiv Orhaug (Figure 9), who observed Cygnus A for several years and discovered new phenomena associated with the ionosphere.

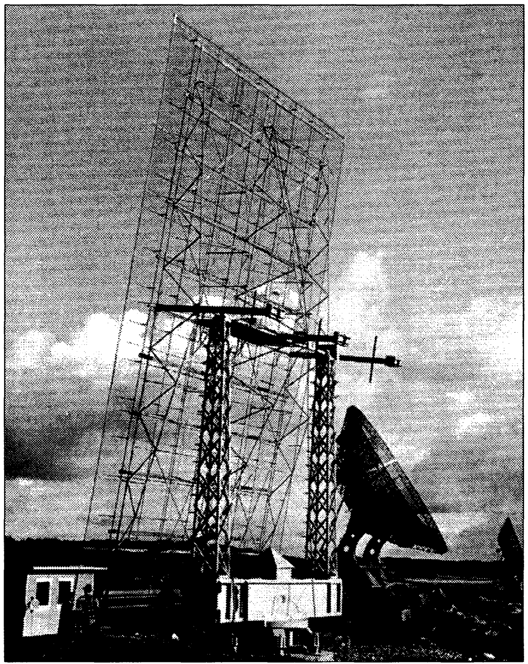
Figure 8: The 150 MHz broadside array (after Rydbeck, 1991).

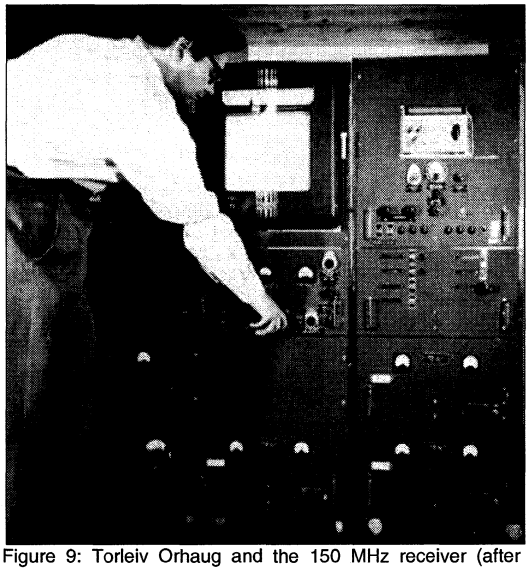
Figure 9: Torleiv Orhaug and the 150 MHz receiver (after Rydbeck, 1991).

The Würzburg antenna with the best reflecting surface was mounted on a tilted (equatorial) axis and just called # 1; it is shown in Figure 10. This was the antenna intended for 21cm line observations and for which Sverre Eng, a Norwegian who had graduated from Chalmers University in 1953, was appointed to build a receiver (Figure 11). The first profiles were obtained in late 1955, and in his autobiography Rydbeck (1991) laments that it was a dream delayed by four years due to limited financial resources. The famous Dutch papers on the spiral structure of the outer part of the Galactic system (van de Hulst, Muller and Oort, 1954) and the rotation of the inner part of the Galaxy (Kwee, Muller and Westerhout, 1954) had appeared in print more than a year earlier. With the altazimuth-mounted Kootwijk telescope that had to be moved by hand every few minutes, the structure of the Galaxy, its differential rotation, and the coordinates of its poles and Centre had been established by observations over the period between when the Würzburgs had landed in Gothenburg and the first spectra were obtained with Eng's receiver. But even worse, the receiver was not stable enough to produce good measurements, and Eng decided to leave Sweden and go to California. Rydbeck also heard that Oort had succeeded in getting money for a larger 25-meter dish, which would be installed in Dwingeloo in the course of the following year.

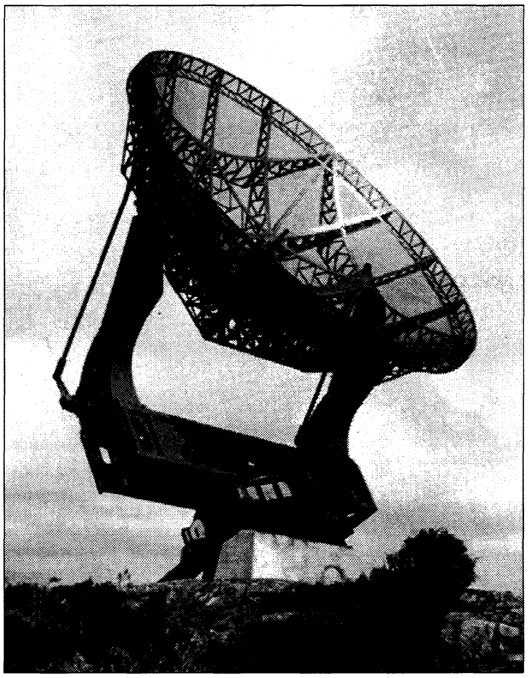
Figure 10: The #1 Würzburg antenna at the Onsala Radio Observatory, which was used for H-line investigations (after (Rydbeck, 1991).

Given these circumstances, one can imagine Rydbeck's depressed state of mind in late 1955 which was when I came to Sweden, ran out of money, and tried to see him to ask if he could give me a temporary job in his laboratory so that I could save money for further travels. In hindsight, it is no surprise that he did not want to see me, and he sent word that the laboratory had no job to offer, and he was reluctant to pay wages to an unqualified Indian to do a job he had no training to do who turned up unexpectedly at his door. At the time, I did not know there was any hydrogen in the sky, and I did not really care ... I was not looking for a career in astronomy, just some money in order to keep travelling. Despite all this, he found a job for me!

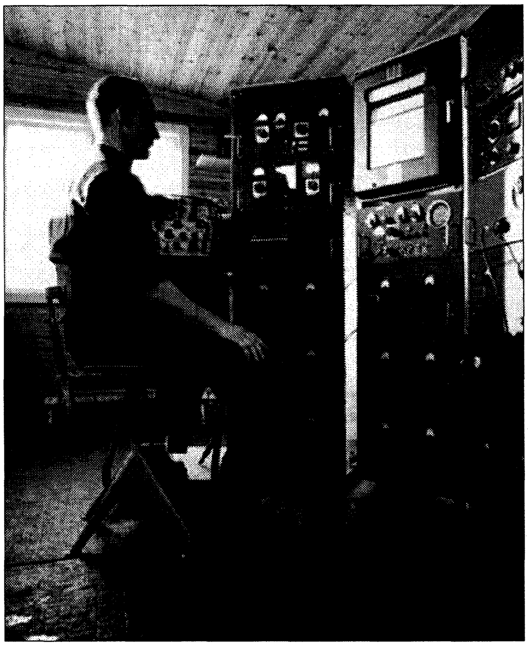

Figure 11: Norwegian-born Sverre Eng and the first H-line receiver at the Onsala Radio Observatory (after Rydbeck, 1991).

4 THE H-LINE RECEIVER

Rydbeck must have been desperate, because after I had spent a month or two in the tube lab he asked me to build a new hydrogen-line receiver. Fortunately there were two others in the radio astronomy team who could help, Ellder, who had made measurements with Eng's receiver, and Höglund, who had been interested in astronomy since childhood.[4] The three of us proceeded to build the receiver (see Höglund and Radhakrishnan, 1959), all five racks of it, which consumed 2 kilowatts of power that helped to keep the hut warm in winter. Today, each of these chassis could be replaced by a chip, but remember this was in the vacuum tube era. I did in fact build one transistor into the machine that I claim was the first in any radio astronomy receiver. As soon as the receiver started to work I decided to follow Eng's lead and go west, as the U.S.A. seemed the only place where I could earn enough money to buy a sailing yacht before I became too old to handle it. Meanwhile, Bertil Höglund, like a good astronomer, justified the effort we had all put in and used the H-line receiver to make thousands of measurements of the Anti-centre region of our Galaxy (Höglund, 1963), interpreting them in terms of the dispersion orbit theory of Lindblad. This was one of several valuable contributions in his doctoral thesis.

5 CONCLUDING REMARKS

In his book, Rydbeck (1991) states that his decision to build a new hydrogen-line receiver was to gain confidence in receiver development that would be essential for the Observatory's future, and that this was a wise decision. As a result of long-term association with Charlie Townes, the search for molecules was something else that was always on his mind, and the need for sensitive receivers to find them (see Figure 12).

Figure 12: Olof Rydbeck with his first ruby crystal, which he used to make a maser in order to look for molecules (after Rydbeck, 1991).

This short paper simply provides a personal perspective on my short stay at Chalmers University of Technology and the Onsala Radio Observatory. New larger radio telescopes were acquired at the Observatory following these early pioneering efforts (Figure 13), and much valuable research was carried out there. An excellent account of all this is contained in Rydbeck's (1991) autobiography.

Figure 13: Olof Rydbeck and the Onsala 25m antenna that was shared with the Skandinaviska telesatellit kommitten.

6 NOTES

1. An earlier version of this paper was presented in one of the Historic Radio Astronomy sessions at the 2003 General Assembly of the IAU in Sydney.

2. Olof Rydbeck was born in Greifswald, Germany, in 1911, and after moving to Sweden completed an electrical engineering degree at the Kungliga Tekniska Högskolan. He then carried out post-graduate research at Harvard, and in 1940 completed a doctoral thesis on the ionospheric reflection of radio waves. He returned to Sweden in 1945, accepting a Chair in 'Radioteknik' at Chalmers University of Technology in Gothenburg. He subsequently held chairs in 'Elektronik' (1948-1963) and 'Teoretisk Elektronfysik' (1963-1979) at Chalmers University. Rydbeck has been described as

> … an engineer, physicist, spectroscopist, geophysicist – he was even interested in cosmology, but above all he was a man who got things done. He will be remembered as a pioneer, a builder of instruments and a man of ideas. He was also a man of great general knowledge which he often liked to demonstrate … Olof was a man with a sense of humour; he was a great character. (Obituary, 1999).

He was the father of Swedish radio astronomy, and died on 27 March 1999.

3. I was paid next to nothing at this time, and concluded that Rydbeck did not hesitate to use cheap labour to get the job done.

4. I remember that at this time there was a clock on the table in the receiver hut that was always showing the wrong time, which irritated me. I was about to reset it one day when Höglund stopped me and explained that it was keeping sidereal time, something else I had not heard about before!

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BABYLONIAN TIMINGS OF ECLIPSE CONTACTS AND THE STUDY OF EARTH'S PAST ROTATION

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Abstract: Long-term changes in the length of the day are investigated using an extensive series of Late Babylonian timings of lunar and solar eclipses. The dates of these observations range between about 700 BC and 50 BC. In recording the times of eclipse contacts, the Babylonian astronomers reported time intervals rather than specific moments. Hence scribal errors tend to be cumulative. To reduce this effect, I have concentrated in this paper almost exclusively on first contact observations. Analysis of these measurements leads to a result for the parameter ΔT of 31.7 ± 0.3 sec/cy/cy and a mean rate of change in the length of the day over the selected interval of 1.74 ± 0.03 ms/cy.

Keywords: Babylon, Earth rotation, eclipses

1 INTRODUCTION

The publication of transliterations, and translations into English, of the extant Late Babylonian eclipse (and other astronomical) records of known date is now essentially complete (Sachs and Hunger, 1988, 1989, 1996, 2001, 2006; Huber and de Meis, 2004). Unless an unexpected archive comes to light, little further progress seems likely in the next few decades. It is thus appropriate to reconsider the eclipse observations in detail to investigate long-term variations in the Earth's rate of rotation.

Over many centuries, the cumulative effect of these variations, termed ΔT, amounts to several hours. It can thus be readily detected from ancient observations, even those of low precision. The parameter ΔT is defined as the difference between Terrestrial Time (TT), as defined by the motion of the Moon and planets, and Universal Time (UT), as measured by the Earth's spin. Throughout the whole of the pre-telescopic period, eclipses have proved to be the only astronomical observations which are of real value in the study of Earth's rotation. By comparison, occultations and planetary conjunctions are of little utility.

2 THE OBSERVATIONS

For several centuries, Babylonian astronomers systematically recorded eclipses of both Moon and Sun. Although only a small percentage of the original records has been recovered, observations of nearly one hundred separate eclipses are preserved. Nearly all of these records range in date from about 700 BC to 50 BC. Steele (2000) identified a single later eclipse which probably dates from 10 BC. The various observations were recorded on clay tablets using a cuneiform script. Practically all of the extant texts are in the British Museum, having been recovered from the site of Babylon (latitude = 32.55 deg N, longitude = 44.42 deg E) during the 1870s and 1880s. Most tablets are very fragmentary, but thanks to the painstaking work of a number of scholars, the various inscriptions are well understood.

Over the period covered by the Late Babylonian texts, very few eclipse observations from any other part of the world are of comparable value for the investigation of ΔT. A few untimed observations of total or near-total solar eclipses are preserved in the histories of ancient China and Europe (Stephenson, 1997). However, the place of observation is frequently in doubt. Several careful Greek measurements of lunar eclipse times (mainly from Alexandria) are recorded in the *Almagest*, but none is earlier than about 200 BC. Prior to about 700 BC, allusions to eclipses (notably from China and Western Asia) are extremely rare and are sometimes of dubious reliability. In this paper I shall concentrate specifically on the set of Babylonian data from 700 BC to 50 BC.

The Late Babylonian observational texts are of three main kinds: (i) astronomical diaries summarising the observations made over a period of several months; (ii) lists of eclipses compiled by the Babylonian astronomers, often extending over many years; and (iii) so-called 'goal-year' texts used in the preparation of almanacs. The data in texts in categories (ii) and (iii) were compiled from the diaries. Occasional duplication of observations can thus be found in the extant texts.

When reporting eclipses, the astronomers of Babylon noted both predictions and observations. In most texts, predictions can be readily distinguished from observations. In the case of predictions, little more than the expected time interval of first contact relative to sunrise or sunset is recorded; there is usually a comment that the observer 'did not watch' (on account of unfavourable weather) or that the eclipse 'passed by'. Use of this latter term indicated either an eclipse occurring when the appropriate luminary was below the horizon or an unsuccessful prediction—e.g. when the shadow of the Moon at a solar eclipse passed completely to the north or south of Babylon. Observations of eclipses frequently contain much more detailed information. This typically includes timings of each phase of the eclipse as well as other data, such as an estimate of the eclipse magnitude and whether the Sun or Moon rose or set whilst eclipsed. When an observation of first contact is reported, the corresponding predicted time is never cited, having been superseded by measurement.

Numerous eclipse (and other astronomical) texts are extensively damaged, but in many cases either the date is preserved or it can be confidently restored. Prior to the Seleucid Era (311 BC), years were counted from the accession of each ruler. However, all subsequent years were numbered continuously. Dates within a year are expressed in terms of the Babylonian luni-solar calendar. Most years had 12 lunar months, each of either 29 or 30 days. However, an occasional

13th month was intercalated, in order to keep the calendar in step with the seasons. The operational rules of the Babylonian calendar have been extensively studied and are well understood. Accurate tables for converting Babylonian dates to the Julian Calendar are available (Parker and Dubberstein, 1956).

Several major tablets contain collections of eclipse records at 18-year intervals. Here the occasional preserved date is usually quite sufficient to identify missing dates in the sequence. Other texts which contain lunar and planetary information enable the date to be computed—independently of the eclipse records. Among the extant texts, lunar eclipse observations far outnumber their solar counterparts. A partial explanation of this anomaly is the relatively higher frequency of lunar eclipses which are visible from any one location on the Earth's surface. However, the date range covered by Babylonian observations of lunar eclipses is about twice as long as for solar obscurations. This appears to be the result of chance—probably less than 10 per cent of the original archive has come to light.

The Babylonians systematically timed—possibly with a water clock, but the actual method is uncertain—the start (first contact) of both lunar and solar eclipses relative to sunrise or sunset, depending on which was nearer. The next phase of the eclipse (second contact in the case of a total eclipse and greatest phase for a partial eclipse) was then timed relative to first contact; other phases were similarly timed in steps. Times were measured in UŜ (time degrees, usually abbreviated to deg), where 1 UŜ was precisely equal to 4 minutes (cf. Stephenson and Fatoohi, 1994); there were 360 UŜ in a combined day and night. Before about 550 BC, measured times were quoted only to the nearest 5 or even 10 UŜ. However, all later measurements are expressed to the nearest 1 UŜ.

To give an example, the total lunar eclipse of 11 November 371 BC is recorded as beginning at 30 deg after sunset; after a further 22 deg it became total; the duration of maximal phase was 20 deg, while the Moon became bright after a further 21 deg. The duration from start to finish is confirmed in the text as 63 deg. We can readily compute the local solar time (LT) of sunset at Babylon as 17.35 h. Hence the LT of the four contacts may deduced as 19.35 h, 20.82 h, 22.15 h and 23.55 h.

3 SELECTION OF DATA

In the investigation of the Earth's past rotation, it is important to use eclipse reports which have been dated without recourse to retrospective calculation based purely on the eclipse observations themselves. Otherwise, there is a danger of circular reasoning: what Robert R. Newton aptly described as "… playing the identification game". In the present analysis I have concentrated on those eclipses for which the date is recorded directly or has been reliably established independently by other means (see Section 2 above).

In analysing the various Babylonian eclipse timings to determine ΔT, I have concentrated almost exclusively on first contact measurements. There are two main reasons for this choice. To begin with, there are many more preserved reports of first contact than for any other single eclipse phase. Furthermore, the way in which the Babylonian astronomers recorded eclipse times other than first contact can lead to systematic errors. As may be seen from the above example of the eclipse of 371 BC (Section 2), the Babylonian astronomers almost invariably expressed the times of all contacts apart from the first relative to the immediately previously measured moment. Hence any mistake in measurement, or scribal error, in recording the time interval between sunset (or sunrise) and first contact—or a damaged reading—will systematically affect the LT of each phase. Similarly, an error in the time-interval between first and second contact would affect the LT of second, third and fourth contacts, and so on. The use of all the available observations of each particular eclipse—rather than first contact alone—can thus lead to a higher proportion of faulty data which are not independent of one another.

The adverse effects of using all contact observations in the derivation of ΔT is displayed by comparing two diagrams in the monograph by Stephenson (1997). Comparison between the ΔT values obtained from single contact measurements (Figure 6.6) and from results derived from other contacts (Figure 6.7) reveals a considerable bias towards high values of ΔT in the latter diagram caused by observations of three total lunar eclipses (and hence twelve measurements in all) between about 300 and 200 BC; the precise dates are 13 December 317, 1 August 226 and 23 December 215 BC.

For some reason, recorded durations of totality are particularly subject to considerable error. The computed durations of lunar eclipses—unlike solar obscurations—are independent of ΔT. Of 18 reported durations in the compilation of Huber and de Meis (2004), 12 (of mean duration some 20 deg) are in fairly good accord with computation (average error 3 deg). However, on the remaining six occasions errors are serious: 14 deg instead of 23.7 deg in 501 BC, 25 deg instead of 16.7 deg in 501 BC, 7 deg instead of 21.6 deg in 327 BC, 5 deg instead of 20.9 deg in 317 BC, 22 deg instead of 11.0 deg in 284 BC, and 10 deg instead of 16.1 deg in 226 BC.

In the present investigation, although I have restricted my attention almost exclusively to first contact observations, I have also included a few observations in which the Moon or Sun rose very near the end of an eclipse. In these instances the observers directly estimated the (short) time interval between moonrise or sunrise and last contact.

In principle, restriction to little more than first contact observations may introduce what might be termed 'contact bias'—due to such factors as delay in catching sight of the start of an eclipse by inattentive observers or (specifically in the case of a lunar eclipse) difficulties in resolving the actual contact due to the 'fuzziness' of the Earth's shadow caused by the terrestrial atmosphere. However, the available evidence indicates that contact bias should not be serious. For instance, the Babylonian astronomers made systematic attempts to predict eclipses. As shown by Steele (2000), the average error in predicting the time of a lunar eclipse was about 2 hours, whereas for a solar eclipse it was about 3 hours. Intending observers thus knew roughly when to watch for first contact.

In my own personal experience, first or last contact for a lunar eclipse can be resolved with the unaided eye with tolerable precision, despite the indistinct edge of the Earth's shadow. For example, at the very small partial eclipse of 7 September 2006 (magnitude only 0.19), my estimate of the UT of last contact—made without taking advantage of advance predictions—proved to be within 2 or 3 minutes of the computed time.

Investigation of the durations of the individual partial eclipse phases as recorded by the Babylonian astronomers also indicates that contact bias may not be serious. In observing a partial eclipse the astronomers normally timed the interval between first contact and the moment when the eclipse appeared to reach its height. Then, a short interval was recognised—usually ranging from about 5 to 10 deg—during which the phase did not sensibly change. Finally, the interval between the end of this stage and last contact was measured.

For example, the record of the partial lunar eclipse of 28 April 239 BC was translated by Sachs and Hunger (1989: 85) as follows:

> At 80 deg after sunset, lunar eclipse; it began on the south and east side; in 15 deg night it made a little over 2/3 (?) of the disk; 10 deg of night maximal phase. When it began to clear, in 15 deg of night it cleared from the east to the west; 40 deg onset, maximal phase and clearing ...

For this eclipse, the computed magnitude was 0.41. The computed semi-duration of 17.8 deg was a little shorter than the measured figure of 20 deg. In examining the extant records of partial lunar eclipses, I note that the measured intervals between first contact and mid-eclipse (which I shall term first phase) and between mid-eclipse and last contact (last phase) averaged only about 0.4 deg (= 100 sec) less than their computed values. Furthermore, although only five sets of measurements of the durations for both the first and last phases are preserved, the systematic bias was only about 0.6 deg (= 140 sec).

In the case of solar eclipses, the Babylonian astronomers did not recognise an intermediate phase; they measured directly the interval from the start of an eclipse to maximal phase and from this latter moment to the end of the eclipse. The durations of both first and last phase are only preserved today for four solar eclipses: in the years 254, 190, 136 and 133 BC. Recorded semi-durations averaged about 16 deg, but the mean bias between the durations of the two phases was only 0.5 deg (= 120 sec). Hence for the purposes of the present study it seems feasible to ignore contact bias.

We have no knowledge of any method used by the Babylonian astronomers to dim the Sun when observing solar eclipses. However, it is interesting to note that the 1st century AD Roman writer Seneca, in his *Naturales Quaestiones*, remarked that in Italy it was the practice to view the eclipsed Sun by reflection in pitch. Owing to its viscosity, this liquid—which also had the advantage of low reflectivity, was not easily disturbed by wind, etc. Bitumen was readily available in Babylon and was regularly used there in building construction; possibly its optical qualities were also appreciated by the astronomers!

Lunar observations of first contact are of two main kinds. More usually, time-intervals were measured relative to sunrise or sunset, depending on which was nearer. However, after about 250 BC, times were also often measured relative to the culmination of any one of about 26 selected stars (or small star groups), known as 'ziqpu' stars. The identities of most of these stars are well established (for details, see Huber and de Meis, 2004: 32). Sometimes a lunar report of this type only states that the eclipse began when a particular star culminated. However, on several occasions it is implied that first contact occurred a few degrees (up to a maximum of about 7) before or after culmination of the reference star. The beginning of a solar eclipse was invariably timed relative to sunrise or sunset.

In compiling the data used in this investigation, I have extracted the various observations as the result of thorough searches through two major sources: the five volumes of transliterations and translations of Babylonian astronomical texts published by Sachs and Hunger (1988, 1989, 1996, 2001 and 2006) and the extensive compilation of Babylonian eclipse records compiled by Huber and de Meis (2004). I have carefully intercompared the two independent sets of translations. In the case of disputed readings, I have requested that other colleagues—notably Dr J.M. Steele—check the appropriate texts. I have rejected any records for which the interpretation—or reading of a key number—is doubtful. There are now many more observations available than was the case even a very few years ago. It is my hope that the present set of observations will prove to be definitive.

4 ANALYSIS OF DATA

In all eclipse computations in this paper, I have assumed a lunar acceleration of –26.0 arcsec/cy/cy, as derived from lunar laser ranging (Williams and Dickey, 2003). When the onset of a lunar eclipse was measured relative to sunrise or sunset, I have initially deduced the observed local apparent time (LT) at Babylon of first contact by calculating the LT of sunrise or sunset. Then I have derived the LT of first contact by adding or subtracting the measured time-interval. Finally, I have subtracted this result from the computed LT, based on the assumption that ΔT was zero. This difference gives the estimated value of ΔT at the date in question. In the case of lunar eclipse measurements based on ziqpu star observations, I have computed the LT of culmination of the appropriate star and then proceeded as previously. On the rare occasions when last contact has been used, I have computed the LT of moonrise or sunrise as necessary.

During a lunar eclipse, the appearance of the eclipsed Moon at any moment is virtually the same from any point on the Earth's surface where the Moon is above the horizon. Hence analysis of a lunar observation is a relatively simple matter. However, the derivation of ΔT from solar eclipse observations is more complex since the lunar shadow crosses the terrestrial surface and the appearance of the Sun is very much dependent on the observer's location. I have derived the observed LT of first contact as for a lunar eclipse, based on the measured interval after sunrise or before sunset. However, in order to deduce the value for ΔT, an iterative technique must be used. In this process, ΔT is progressively refined until the computed LT of contact matches the observed time.

5 ILLUSTRATIVE EXAMPLES

As examples, I have selected the following:

(i) The lunar eclipse of 17 October 537 BC is recorded as commencing 14 deg before sunrise. (Incidentally, it is also reported that when 2/3 of the disk was obscured the Moon set.) The computed LT of sunrise was 6.24 h, implying a measured LT of first contact of 5.31 h. Comparing with the computed LT of 10.54 h, based on a value for ΔT of 0, the result for $\Delta T = 18800$ sec.

(ii) The lunar eclipse of 13 August 105 BC was reported to begin 7 deg (= 0.47 h) after the "bright star of the Old Man" (= α Per) culminated. At the time the R.A. of the star was 1.17 h, while that of the Sun was 9.28 h. Hence it may be derived that the LT of first contact was $12.00 + 1.17 - 0.47 - 9.28 = 3.42$ h. Subtracting this result from the computed LT of 6.76 h (based on $\Delta T = 0$) yields $\Delta T = 12000$ sec.

(iii) The solar eclipse of 31 January 254 BC was observed to begin 56 deg before sunset. The computed LT of sunset on this occasion was 17.26 h, so the LT of first contact was 13.53 h. Using an iterative method, the computed LT of first contact may be deduced as 13.53 h for a value of ΔT of 11400 sec.

6 PRESENTATION OF RESULTS

The various results of the current investigation are summarised in four tables. In each table, years are given as negative integers, differing from one year by their BC equivalent; thus –685 is equivalent to 686 BC, and so on. This difference arises from the fact that there is no year zero on the BC/AD system. If more than one eclipse occurred in a year, tabular years are followed by a, b or c. The other parameters listed are, in order, computed eclipse magnitude (for reference only), measured time-interval in degrees, and derived ΔT result in seconds.

In Table 1, the investigation of 58 lunar first contact measurements relative to sunrise (SR) or sunset (SS) is summarised. For the eclipse of –554 October 6, both Sachs and Hunger (2006) and Huber and de Meis (2004) assume an error in the text; for "55 deg before sunrise" they read "55 deg after sunset"; this interpretation seems reasonable; adoption of the original reading would lead to an impossibly large value for ΔT of some 86000 sec! In several other instances, a text gives a clear measurement, but on account of partial damage it does not record whether the observation was made before sunrise or after sunset. However, similar reasoning to the above readily identifies the only viable alternative in each case.

Table 2 covers 17 measurements relative to the culmination of ziqpu stars. In most cases the text implies that the eclipse commenced at the time of culmination of the appropriate star or star group. However, in other examples the time interval before (indicated by a minus sign) or after (plus sign) culmination is specified in deg.

Table 3 deals with the four lunar observations in which the time interval between moonrise (MR) and fourth contact is estimated directly; first contact occurred when the Moon was still below the horizon.

Finally, Table 4 is restricted to solar first contact measurements, apart from a single preserved instance (in –280) where the time of end of the eclipse after sunrise is specified. There are ten observations in all. Unlike in the case of a lunar eclipse, the magnitude of a solar eclipse at a particular place is a function of the adopted value for ΔT. I have computed magnitudes using the approximate expression $\Delta T = 32t^2$, where t is in Julian centuries from the reference epoch AD 1820.

Table 1: ΔT results from lunar first contact timings measured relative to sunrise or sunset.

Year	Mag	Interval (deg)	ΔT (Sec)
–685	0.55	100 after SS	22500
–684	1.83	20 after SS	19100
–600	0.84	95 after SS	18000
–598	0.75	105 after SS	14400
–587	0.55	20 before SR	15700
–586	1.80	35 before SR	18900
–579	1.82	45 after SS	19100
–576	1.88	105 after SS	19200
–575	1.27	40 before SR	20000
–572	1.73	90 after SS	18000
–561	1.77	90 after SS	16000
–554	1.53	55 after SS	17800
–536	1.50	14 before SR	18800
–525	1.61	60 after SS	19300
–500	1.47	77 after SS	15000
–482	1.47	10 before SR	17300
–420	1.65	19 after SS	15400
–407	0.18	15 after SS	15200
–406	1.39	48 before SR	15000
–405	0.96	14 before SR	16500
–396	0.09	48 after SS	15500
–377	1.32	37 after SS	16000
–370a	0.78	66 after SS	12800
–370b	1.36	30 after SS	16100
–366	1.33	56 before SR	19500
–363b	0.33	40 before SR	16400
–363c	1.48	14 before SR	15300
–362	1.03	64 after SS	15500
–352	1.35	47 before SR	15900
–316a	0.37	10 after SS	15600
–316b	1.34	44 after SS	16600
–307	0.99	10 before SR	14100
–239	1.40	3 before SR	14200
–238	0.41	80 after SS	8800
–225	1.21	52 after SS	17600
–211a	0.62	20 before SR	11800
–211b	0.94	28 after SS	21300
–193	0.92	12 before SR	13600
–189	1.05	30 before SR	10800
–188	1.28	34 before SR	10800
–162	0.12	85 before SR	9500
–159	1.44	48 after SS	14000
–153	0.85	4 after SS	12700
–142	0.88	7 after SS	12600
–135	0.73	30 before SR	4000
–133a	0.25	9 before SR	11000
–133b	0.25	32 after SS	11600
–128	0.63	55 before SR	11900
–119	1.02	66 after SS	12500
–109	1.75	25 after SS	14600
–108	0.51	8 after SS	12100
–105a	1.61	66 after SS	10500
–105b	1.59	50 before SR	12900
–104	0.27	26 before SR	12000
– 95	0.71	57 after SS	13200
– 80	1.70	60 after SS	8900
– 79a	0.60	40 before SR	11500
– 79b	0.39	30 after SS	12100

Table 2. ΔT results from lunar first contact timings measured relative to the culmination of stars.

Year	Mag	Interval (deg)	ΔT (sec)
–225	1.19	0	15500
–214	1.37	0	14200
–193	0.92	0	14000
–177	1.11	0	13300
–162	1.53	–3	10400
–149	1.11	+4	11400
–142	0.88	–5	12900
–135	0.73	0	11800
–134	1.57	0	11900
–122	0.15	–5	14000
–119	1.02	–5	11900
–104	0.27	–7	12000
– 95	0.71	+5	11600
– 93	0.25	0	9800
– 90	0.55	0	10600
– 86	0.41	0	12000
– 79a	0.60	+5	11300

Table 3. ΔT results from lunar last contact timings measured relative to sunrise or sunset.

Year	Mag	Interval (deg)	ΔT (sec)
–562	0.35	6 after MR	19200
–464	1.47	21 after MR	15700
–363a	0.21	10 after MR	14100
– 66	0.81	23 after MR	10100

Table 4. ΔT results from solar first contact timings (all measured relative to sunrise or sunset).

Year	Mag	Interval (deg)	ΔT (sec)
–356	0.90	76 before SS	15600
–321	0.17	3 before SS	14200
–280	0.20	20 after SR	12900
–253	0.26	56 before SS	11500
–248	0.80	90 after SR	13900
–189	0.77	30 after SR	12900
–169	0.44	20 before SS	12300
–135	1.05	24 after SR	12600
–132	0.87	51 before SS	11200
– 88	0.36	45 after SR	9100

In addition to the results listed in Table 4, precise limits to ΔT at the epoch –135 (= 136 BC) are set by the total solar eclipse of 15 April in that year. This is the only solar eclipse recorded in Late Babylonian history which is definitely recorded as total. Visibility of four planets—Mercury, Venus, Mars and Jupiter—as well as several stars is noted during the total phase. Only values of ΔT somewhere between 11200 and 12150 sec would yield totality at Babylon.

7 DISCUSSION OF RESULTS

The individual ΔT results (89 in all) listed in column 4 of each table are shown diagrammatically in Figure 1. Here three separate symbols are used: for lunar eclipses timed relative to sunrise, sunset or moonrise; for lunar eclipses timed relative to the culmination of ziqpu stars; and for solar eclipses. Only a single ΔT result is off the scale covered by Figure 1; a value of 4000 sec derived from the lunar eclipse of –135 (= 136 BC). This is presumably the result of a scribal error.

There is clearly a considerable scatter among the results displayed in Figure 1; scribal errors may well be partly responsible for this feature. The scatter is especially notable among the lunar eclipse timings measured relative to sunrise or sunset. However, these have the advantage over the other types of data of extending over a much longer time-scale (more than 600 years). Although after about the year 550 BC the Babylonian astronomers consistently estimated time-intervals to the nearest UŜ—and thus 240 seconds—they clearly did not achieve anything like this precision. Furthermore, there is little evidence of improvement in the accuracy of timing down the centuries. Evidently, the accuracy which the astronomers did achieve was adequate for their purposes.

Two curves are shown in Figure 1. The upper curve, representing the effect of lunar and solar tides, has the equation $\Delta T = 42t^2$, where t is measured in centuries from the standard reference epoch AD 1820. As is evident from the diagram, with a single exception the various ΔT results lie systematically below the tidal curve. Hence there is clear evidence of a marked non-tidal component tending to increase the Earth's spin rate in opposition to the main tidal term. The lower curve is the best fitting parabola through the set of ΔT values shown in Figure 1; this has the following equation:

$$\Delta T = (31.7 \pm 0.3)t^2 \qquad (1)$$

In deriving this latter curve I have rejected six data points (all lunar timings relative to sunrise or sunset) in the years –598, –366, –238, –225, –211 and –135. These lay more than 2.5 sigma from the mean curve, and may well be attributed to scribal errors. As shown in Figure 1, the mean parabola also intersects the limits fixed by the observation that the solar eclipse in –135 was total in Babylon; only coefficients of t^2 between 29.3 and 31.8 would satisfy this critical observation.

The data are probably not of sufficient accuracy to indicate significant short-term fluctuations about the mean parabola; most of the scatter results from the inaccuracy of the observations. No obvious trends on the centennial time-scale are evident. The data divide fairly well into two groups: before and after 350 BC. For the earlier group, of average date close to 460 BC, the mean deviation from the parabola $\Delta T = 31.7t^2$ is +170 ± 220 sec. For the later group, of average date close to 140 BC, the corresponding mean deviation is –110 ± 190 sec, a difference which is barely statistically significant.

Comparison between equation (1) and the cubic spline fit by Morrison and Stephenson (2004)—based on timings of a variety of eclipse phases—reveals discrepancies of no more than 170 sec between 500 and 100 BC. Before the former date, discrepancies are much larger (500 sec at 600 BC and nearly 900 sec at 700 BC. However (as evident from Figure 1), there are scarcely any useful observations prior to 600 BC, while —as noted in Section 2 above—all measurements before 550 BC are rounded to the nearest 5 or even 10 UŜ. Hence at these earlier dates spline fitting ceases to be a viable option; see also Morrison and Stephenson (2005).

The average increase in the length of the day (LOD) as derived from equation (1) is 1.74 ± 0.03 milliseconds per century (ms/cy). The difference of about 0.55 ms/cy between this result and the tidal figure of 2.3 ms/cy can probably be largely explained by the effect of post-glacial isostatic compensation: the continuing rise of land which was glaciated during the last ice-age. This leads to a gradual diminution in the terrestrial oblateness, with consequent decrease in the LOD. Artificial satellite measurements yield a decrease in the LOD of approximately 0.45 ms/cy (e.g. Cheng et al, 1989), in reasonable accord with the non-tidal result deduced from the historical observations. However, the relatively short period (6 centuries) covered by the Babylonian data is insufficient to enable the rate of change in the Earth's zonal harmonic, J_2, to be estimated. This would require the use of more archaic data, little of which is accessible.

8 CONCLUSION

In summary, equation (1) provides an excellent fit to an extensive set of independent data. In addition to the geophysical implications of this investigation, it is hoped that the ΔT parabola will prove of value in investigation other ancient eclipses of similar date, e.g. as recorded in Greek and Latin writings.

The question of extrapolation into the more remote past almost inevitably arises. For this purpose I recommend cautious use of Equation (1) back to around 1000 BC. However, so few archaic observations are preserved that the earlier history of ΔT may well remain indeterminate.

I would very much welcome unaided eye observation of the contacts of the following future total lunar eclipses: 3 March 2007 and 28 August 2007.

9 ACKNOWLEDGMENT

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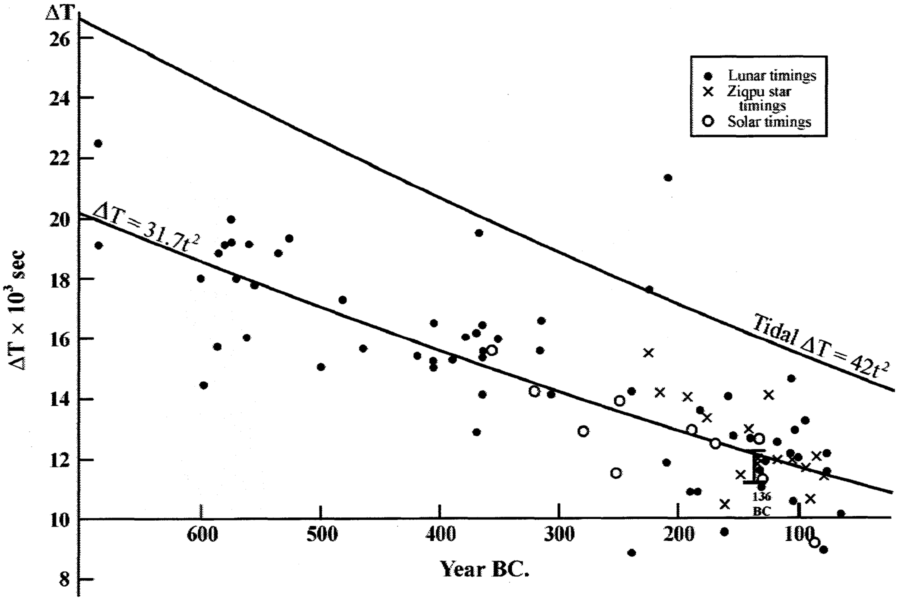

Figure 1: ΔT values and limits derived from late Babylonian eclipse observations.

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JOHN HERSCHEL ON THE DISCOVERY OF NEPTUNE

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Abstract: The letters of John Herschel that concern the discovery of the planet Neptune have not been greatly discussed by historians of science. I have transcribed these in the course of archiving the British Neptune-discovery documents. Herschel tends to be depicted as a background figure in narrations of the story of Neptune's discovery, whereas the present account focuses upon his evolving view of the topic: the rival merits of the two main protagonists, and the startling manner in which an obscure branch of mathematics (perturbation theory) was able to pinpoint the position of a new sphere in the sky. As the son of the man who found Uranus, his views have a special relevance. Also, I suggest that his eloquent prose style may still be enjoyed today.

Keywords: Neptune, Sir John Herschel, U. Le Verrier, J.C. Adams, G.B. Airy

Celebrating the 160th anniversary of the Discovery of Neptune on 23 September 1846

1 INTRODUCTION

Sir John Herschel (Figure 1) played a key role in the turbulent post-discovery Neptune debates of the 1840s. In the process of archiving the British Neptune-discovery papers I have transcribed quite a few of his letters on this topic.[1] These letters remain of interest because of Herschel's eloquent command of the English language, of a quite different order from the other persons concerned with Neptune's discovery, which make his letters a delight to read; but also, because he moved at the centre of the British debate, being President of the British Association for the Advancement of Science, on the Council of the Royal Society and becoming in 1847 President, for the third time, of the Royal Astronomical Society. The letters are mainly stored in Britain's Royal Society Herschel Collection, as well as other libraries: at St John's College, Cambridge, which had the John Couch Adams correspondence; at the former Royal Greenwich Observatory now kept at the Cambridge University library, collated by George Airy the Astronomer Royal over this period, which in 1999 returned from its eventful antipodean journey (see www.ucl.ac.uk/sts/nk/neptune/takes.htm); and at the Paris Observatory, which preserves letters sent by Herschel to Urbain Le Verrier.

Sir John Herschel here appears as a chief philosopher in the discussions, consulted by all parties. His view concerning the significance of the near-synchronous discoveries by John Couch Adams and Le Verrier, has clearly been little appreciated (e.g. see Ronan, 1992, which contains almost nothing on the subject). His best-selling *Outlines of Astronomy* appeared in 1849, and its view on the joint discovery was not its least point of interest. This was the last great, classic, English-language astronomical textbook, and it rolled through twelve editions, as well as being translated into many languages, including Chinese and Arabic. At the period which concerns us, Herschel was no longer making scientific discoveries of his own, his last having been the ascertaining in 1840 of the variability of Betelgeuse. In 1847 his observations on southern-hemisphere stars were published, with his theorising about the structure of the Milky Way. This book brought him the Royal Society's Copley Medal. The reader may wish to consult the author's website concerning the discovery of Neptune (www.ucl.ac.uk./sts/nk/neptune/), or his recent paper on the subject (Kollerstrom, 2006), as a background for appreciating Sir John's remarks. The occasional question-mark in the text indicates that I could not fully read Herschel's handwriting.

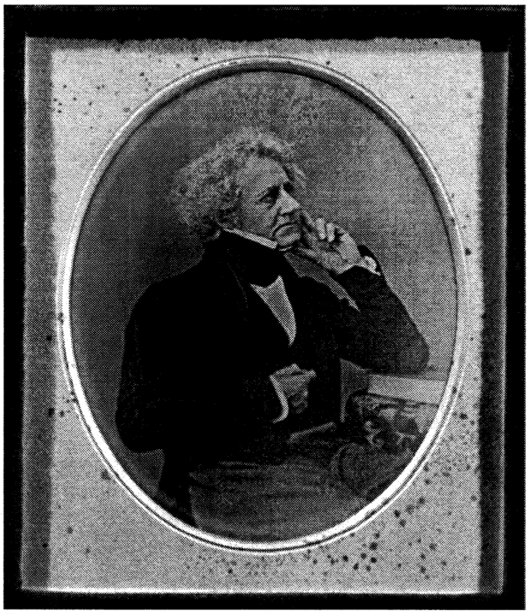

Figure 1: Restored version of a Daguerrotype of Sir John Frederick William Herschel, ca. 1848, taken by J.E.Mayall, © National Portrait Gallery, London (P660).

The letters we here peruse are from a time when, in the words of U.S. astronomer Benjamin Gould (1850: 21), "The remembrance of the enthusiasm excited by this discovery, of the amazement with which the tidings were received, not only by astronomers, but by almost all classes of the community, and of the homage paid to the genius of Le Verrier, is still fresh in the memory of all. Nations vied with one another in expressions of their admiration." The discovery was made on 23 September, 1846. The Neptune-debate was one which, as Sophie de Morgan (1882: 134) commented in her biography of her husband Augustus, threatened the RAS with mere dissolution through the turbulence of the passions which it aroused, and it climaxed around the turn of 1846/1847.

Upon reading news of the planet's discovery, in the form of Hind's letter to *The Times* on 30 September, Herschel (Figure 2) swiftly composed a letter to *The Athenaeum*, the British weekly that carried the best coverage of the debate, sending it off the next day:

> In my address to the British Association assembled at Southampton, on the occasion of my resigning the chair to Sir R. Murchison, I stated, among the remarkable astronomical events of the last twelve months, that it had added a new planet to our list, - adding, "it has done more, - it has given us the probable prospect of the discovery of another. We see it as Columbus saw America from the shores of Spain. Its movements have been felt, trembling along the far-reaching line of our analysis, with a certainty hardly inferior to ocular demonstration." (Herschel 1846c).

The 'new planet' here alluded to was the asteroid Astraea. Widespread correspondence amongst European astronomers from December 1845 onwards concerning a 'new planet' alluded to this—and not anything else! Herschel's prophetic words here remembered the great discovery of his father William, in finding Uranus. This BAAS meeting had been a mere week or so prior to the moment of discovery, yet had no mention or discussion of the expected new planet, on which Le Verrier had by then twice gone into print, except only for these allegedly-spoken words of Herschel. Did they comprise the first British allusion to Adams, the 28-year old Cambridge mathematician, in this context? Herschel was here claiming so. By way of confirming these words, I found a letter by an Irish correspondent (Stevelly, 1846) who states that Herschel had indeed spoken them on the occasion of his valedictory speech to the BAAS in Southampton.

Figure 2: Sketch of John Herschel presiding over an 1846 meeting of the British Association for the Advancement of Science, as depicted in an issue of the *Illustrated London News* (courtesy RAS Archives).

2 THE ENGLISH AND FRENCH CLAIMS

Herschel averred, in his *Athenaeum* letter, that this expression of confidence would hardly have been warranted merely from Le Verrier's calculations, and that it was their corroboration by Adams which "... justified so strong an assurance." This provoked an angry rebuke from Le Verrier, writing to the *London Guardian*, concerning Herschel's want of faith in his predictions: "When he scrupled not to put into print that my calculations were not sufficient to command his confidence, did he not perceive that he was bringing discredit on his own scientific penetration, when he attacked a calculation ...", etc, and then Le Verrier added the commendable sentiment: "Among men of science of different countries, there ought to remain only that friendly rivalry, which, as leading to the benefit of science, so far from hindering, does but cement, the frank and brotherly friendship of those who cultivate it." (Le Verrier, 1846a). In his reply, Herschel (1846d) assured *Guardian* readers that "The prize is his [Le Verrier's] by all the rules of fair adjudication, and there is not a man in England who will grudge him its possession ...", and then suggested that the synchrony of this discovery was *beneficial for science*:

> The history of this grand discovery is that of thought in one of its highest manifestations, of science in one of its most refined applications. So viewed, it offers a deeper interest than any personal question. In proportion to the importance of this step, it is surely interesting to know that more than one mathematician has been found capable of taking it. The fact, thus stated, becomes, so to speak, a measure of the maturity of our science; nor can I conceive anything better calculated to impress the general mind with a respect for the mass of accumulated facts, laws, and methods, as they exist at present, and the reality and efficiency of the forms into which they have been moulded, than such a circumstance. We need some reminder of this kind in England, where a want of faith in the higher theories is still to a certain degree our besetting weakness. (ibid.).

His diary for that day, however, says: "Wrote to the editor of *The Guardian* in reply to M. LeVerrier's savage letter. These Frenchmen fly at one like wildcats." (Herschel, 1846a).

Astronomer James Challis (Director of the Cambridge Observatory) had failed, after a strenuous six-week search, to find a planet which Galle and d'Arrest at Berlin spotted in half an hour. Herschel conveyed his regret at this outcome, to his old friend the British philosopher William Whewell:

> I mourn over the loss to England and to Cambridge of a discovery which ought to have been theirs every inch of it, but I have said enough about it to get heartily abused in France, and I don't want to get hated in England for saying more. Only if you have any influence with Challis for heavens sake exert it to prevent him saying more about it in the papers - or elsewhere. (Herschel, 1846e; in pencil he added to his copy of the letter: "After all it is now quite clear Adams was the prior discoverer.").

After being chastised by a London *Guardian* editorial (21 October, p.404), Challis responded by agreeing that he and Adams had no claim over the discovery of the new planet:

> I beg distinctly to say that I had no intention of putting in any claim to discovery, either for Mr Adams or myself. The facts I stated were, as I thought, sufficient to show that no such claim could be made ... I certainly was desirous of

> proving, for the credit of English science, that Mr Adams's researches were spontaneous and independent: but I am unable to see that the fact of their being so at all diminishes M. Le Verrier's merits, or that the making of the fact public implies an intention of taking in any degree from the honour of the discovery. The very natural wish to show that the University of Cambridge could produce a mathematician capable of handling a problem of so high an order ... (Challis, 1846).

That was far from being the consensus British view, and it provoked Herschel's above-quoted response, as well as a stiff rebuke from Airy.

Concerning the relative merits of the two claims, British and French, Sir John opined to R. Jones:

> It is a shame to make rivals and competitors of two men who ought to be sworn brothers. Adams has the acknowledged priority in point of time that nothing can shake but till the Planet was found it was only a physical hypothesis upon trial, and no one can truly deny also that LeVerrier *shot fair*, and *brought down the bird.* Now my view of the matter is that there is quite enough for both ... (Herschel, 1846f).

Both mathematicians, Adams and LeVerrier, had used the perturbation-theory of Lagrange and Pontecoulant, a French creation:

> Barring Newton's law of gravity (who never meddled with the planetary perturbations), what Englishman ever furnished the smallest tottle of a tool towards rigging out a man for such a struggle? It is all French du fond en comble [?] Clairaut, D Alembert, Laplace, Lagrange, and more recently Poisson and Pontecoulant for the analysis and Bouvard for the tables, which though not *quite* correct were yet correct enough to raise the hue and cry. - The New Planet is as much Laplace's as it is either Leverrier's or Adams's. (ibid.).

A postscript added: "Who made one and all of the formulae by which both have grappled the planet but Frenchmen?" (ibid.). We may note that both Adams and LeVerrier used the same textbook, Pontécoulant's *Théorie Analytique du Systeme du Monde.*

The new planet's discovery had been "... in every way a most spirit-stirring event ..." Herschel (1846n) found, writing to Otto Struve at the Pulkova Observatory. He had nearly found it himself, he realized, during a sky-sweep in 1830 (see Buttman, 1974: 162), however it was better that it had not been found by mere accident (Herschel, 1846n). A couple of days later, he wrote to William Whewell, again weighing up the priority claims:

> ... Galle looked for it and found it on the *sole* ground of Leverrier's place, while Challis cannot shew that he looked for it (when at last he did so) purely and simply by Adams's. When he began to look he had already a knowledge of Leverrier's results, and he did not *find* it till after Galle had done so - for I do not call finding an individual object merely including it in a crowd of others (without knowing that it is there, and rather suspecting it *not* to be) with an *intention* of examining them at leisure to ascertain if it be among them or not - Nobody but Sheepshanks will ever say that Challis *found* it before Galle.
>
> Until the planet was actually seen and shewn to be a planet - there was no discovery. (Herschel, 1846p).

(Challis had observed Neptune, i.e. recorded its transit in his log-book, on both the 4 and 12 of August, amongst the three thousand stars he also noted; but, failed to recognize it.) On the back of his copy of the letter, Herschel had pencilled in anguish:

> God forgive me for writing in this way - The truth lies on the other side & Adams is the 1st theoretical discoverer of Neptune. The whole thing was parried [?] and perverted by Airy's indefensible reticence. On him be the responsibility of the (temporary) transfer of one of the brightest stars in Britain's Scientific fame to France. (ibid.).

Fortunately, he never published this somewhat unbalanced view.

3 NO RAS MEDAL

In December 1846 the full text of Adams' 13 November RAS presentation was published in the *Nautical Almanac*, and this publication of his case awakened great sympathy and appreciation for the strength of his argument. The Royal Society had earlier awarded Le Verrier their prestigious Copley Medal, a relatively unproblematic decision (which Herschel had received on Le Verrier's behalf). December 1846 was stressful for the British astronomical community, because the RAS had its annual Gold Medal to award, which its bye-laws stipulated could only be done in January. They further stipulated that a 3:1 vote was necessary for awarding this medal, and that only one such could be awarded each year. Passions were running high, and there was simply not enough time for the RAS Council to sort out a realistic course of action.

Herschel's first letter on the subject was written on 3 December to the RAS's Secretary, the Reverend Richard Sheepshanks, and it seems to imply that the RAS's medal should be awarded to Le Verrier: "My own opinion is that Adams stands in quite as good perhaps a better position without a medal as with - that if he be medallised it should be most cautiously worded so as not to bear the least allusion to that ugly word priority – and that to medallise Galle and Challis (or even Bremiker) would be decidedly wrong." (Herschel, 1846g). Alas, this advice was not taken, and the RAS medal decision sank into the quicksand of these multiple proposals.

Two weeks later Herschel (1846h) proposed three Gold Medals:

> If the council resolve on medallising Mr Adams, I would by no means *object* or oppose it - but I conceive the way of stating the grounds of proceeding in that case, both in reference to him and to M. Leverrier ought to be more carefully considered so as in the first place neither to state nor to imply anything that all the world will not admit to be true in the most ordinary acceptation of the words (already the word "discovery" begins to break down under the weight of meaning laid upon it) - and 2ndly not to assume to the Astronomical Society as a body a dictatorial power of deciding points of such a nature, which the public mind would rebel against as it tends to do against all decisions *ex cathedra.*

He suggested that three medals be awarded, to Adams, Le Verrier and Hencke. This turned out not to be a very helpful idea (Hencke, at Frankfurt/Oder, had discovered the asteroid Astraea). Still wrestling with the matter, later that same day he penned a second letter to Sheepshanks:

> I really am desirous to say as little as I can about this matter of the Planet. But I must most urgently protest against any official assertion of priority - against any bringing into competition of dates and claims by the wording of our resolutions. What our worthy President may say in his address is his own affair, but I should advise him to keep clear of anything which may tend to stir up a national controversy in the matter as that will be sure to do, and a bitter one. Heaven knows I would not depreciate this if I thought our case *as a whole* were tenable. But though Neptune ought to have been born an Englishman and a Cambridge man every inch of him - *Diis aliter visum* - you will never make "an English discovery" of it no matter what you will. I assure you seriously that the conviction that such is the case has given me more pain and grief than any national event since the expedition to New Orleans or such other *coup manqué* as your military imagination may suggest. It has really made me ill. (Herschel, 1846i; William Smyth was the RAS's President at the time).

A week later Herschel (1846j) wrote again to Sheepshanks, fearful that the impending decision "... may prove a more fatal apple of discord then any that has been thrown down among us for years."

On Christmas Day he wrote a letter marked 'confidential' to Sheepshanks, concluding 'burn this.' By that time six candidates had been advanced for the Gold Medal:

> I see Airy proposes LeVerrier, Adams, Challis and Argelander - Bishop, Hencke - and Johnson, Galle ... I must very candidly tell you that I think this one of the most disastrous combinations of circumstances the A.S. has ever had before it, and that it comes in a most portentious form for the peace of the scientific community of England ... I know you have much influence with Airy, and I am convinced that no other man than yourself has any chance of inducing him to reconsider his judgement in the form he has cast it - and if you can get Challis and Galle left out, all will be well. Probably if he would withdraw Challis and Argelander, Johnson would withdraw Galle – and trusting that this may be the upshot, I remain ... (Herschel 1846m; Bishop and Johnson were RAS Council members).

If the number of candidates could be reduced to merely two, Herschel hoped that there might be a slim chance of the Council deciding to award one extra Gold Medal that year – a view championed by Charles Babbage. Sheepshanks, however, may not have had quite so much influence as Herschel here credits him with. He replied by return concerning "... our good friend Smyth ... [who] had this bitter cup impending over him ... the whole evidence as to Leverrier was out, understood and believed, before anything was known of Adams ... LeV's merits too are of such an order that every one feels anxious to shew his liberality in a case so clear and free from danger." However, "I scarcely expect that half will agree to apply for a suspension of the Bye Laws to present the additional medals." (Sheepshanks, 1846). This was, it turned out, a correct apprehension.

But could a vote for Le Verrier reach the necessary 3:1 majority to award the medal? "Le Verrier's medal will be voted unanimously unless, perhaps, Airy may object to it without some condition, this I think however he will waive ..." Sheepshanks' letter continued with this dire, futile logic: after describing the various voting postures of key Council members, he stated: "Some (I for one) think that in granting a medal to LeVerrier alone we do in fact & to all the world deny any merit in Adams & even the necessity of Airy's memoir. I am certain that in France (where fairness seems not understood) it would be impossible by any language in our Report to prevent this conclusion. I believe moreover that in England the same conclusion would be generally drawn." He concluded by saying that he wished the Society did not have to award Gold Medals.

On 8 January, a motion proposed by Augustus de Morgan prohibited any alteration in the Society's bye-laws for the vote (RAS, 1847),[2] and this motion was carried. Then the six separate candidates were voted for, one by one, and, inevitably, no single name received the necessary 3:1 majority. Airy voted *against* awarding a medal to Le Verrier, and it is not unfair to say that he thereby exerted the casting vote in preventing any such medal being awarded. After this debacle, Herschel wrote next month to Sheepshanks about the course adopted by the Council, "... for I think it a wrong one - or rather a *sheer mistake & nobody's doing*." (Herschel, 1847d). It was De Morgan's doing! Could Council members find some way of extricating themselves, Herschel wondered, from "... the hard knot in which they have got themselves tied up?" (ibid.). From more than one correspondent, he had gathered that the no-medal resolution "... is productive of very great dissatisfaction among the body of the Society & indeed generally among the scientific world." (ibid.). He was perplexed over "... what reasons influenced the rejection of the proposal to admit more than one medal... ":

> The actual state of the subject is therefore an uncontemplated result & the work of nobody; & probably *as a result* and as the *final* and *only* result of the discussion, disapproved by all present. (Herschel, 1847e).

No RAS medals were awarded that year.

4 PUBLIC ESTEEM FOR SCIENCE

The year 1847 began with the arrival of Le Verrier's Memoir on the new planet, and Herschel (1847c) enthused to Airy about it:

> I have within these 2 days got Le Verrier's Book - and I must say my impression is one of unbounded admiration. There is no part of the subject shied or slurred over - a *tabula rasa* - and a total reconstruction with a view from the beginning to the crowning pinnacle of the whole edifice. It is an Epic Poem complete in beginning middle & end with a catastrophe[3] such as *could not possibly be heightened* by any additional circumstances. I am sorry for Adams & for England, but it would really have been a *pity* that so superb a struggle should not have been crowned with victory as a spectacle for Gods & men. (Herschel, 1847c).

Within a day or so he also received Adams's tract, *Explanation of the observed irregularities of the motion of Uranus*,[4] and wrote to the latter:

> Though it is now long since I entered at all into the Planetary theory and can do little more than seize the spirit of the methods & practices and yet I see enough in both to excite my unbounded admiration of the skill and power displayed in grappling with so difficult a problem, and I cannot say that the triple coincidence of your results *with each other* and *with the fact,* considering the minute amount of the quantities to be dealt with seems to me by far the most wonderful gave [?] of the whole affair and gives an idea of the firmness of grasp which theory has obtained of the Planetary perturbations infinitely beyond what the most sanguine could have dared to hope would *ever* be obtained. In this point of view (and setting aside all question of rivalry and competition between two men whose names will go down indisputably linked together to the latest posterity and between whom, if even, there ought to be a brotherhood of mutual admiration and regard) I cannot help considering it as fortunate for science that this should have happened. All idea of a lucky guess - a mutual destruction of conflicting errors - of a right result got at by wrong means is precluded - and the most reluctant to accord any merit to theories must be bound to admit that in this matter at least theories are facts. (Herschel, 1847b).

These two documents confirmed his view that this synchronous discovery had been beneficial to the public's appreciation of science. Writing to Fitten, after admitting that he had an unanswerably large pile of letters from the RAS's no-medal debacle, he admitted rather too late in the day that

> … it will be the *right* course to give two medals, making however such a distinction in the tenor of the award as shall secede to Leverrier the intact possession of the first honours of the achievement - upon the grounds that he *shot fair* and *brought down the bird* - while at the same time every possible justice shall be done that words can do to Adams' merit. (Herschel, 1847f).

Concerning Adams's claim:

> It is the correctness of the mathematical conduct, & the perfect independence of Adams' researches, and not their priority, which in my opinion constitutes his claim to a reward & a proposing of our gratitude as astronomers. As a competition to Leverrier I never will consider to regard him. But I think it is precisely one of the finest, most interesting & most admirable points in this discovery that it can be satisfactorily shown by evidence that whether published or not, the same result has been arrived at independently by two different Geometers both starting from the ordinary recognised formulae of the planetary perturbations. It is an infinitely greater part - infinitely more creditable to the state of Science, infinitely more illustrative of the reality of its grasp in the planetary theory that two shared have done this than one only. I am not aware that this view of the subject has been taken, but I pray you to give it your serious consideration. (ibid.).

5 THE NAMING OF URANUS AND NEPTUNE

Concerning the name of the planet, François Arago before the Paris Academy had impetuously pledged himself not to call it anything other than 'planete Le Verrier,' a mere week after its discovery—possibly not realizing that Le Verrier had already written to various European observatories suggesting the name 'Neptune.' Subsequently Le Verrier came to adopt Arago's suggestion, leaving European astronomers in perplexity. There turned out to be an implication to Arago's proposal, a kind of corollary, namely that the planet Uranus had to be called 'Herschel.' When Le Verrier (1846b) wrote to Herschel on 28 November 1846 and sent him a copy of his Memoir, he pointed out that he had altered its title, 'Researches on the Movements of Uranus', by changing 'Uranus' to 'Herschel'. However, he had not altered it within the text, which produced some confusion. This Memoir did not arrive until the beginning of January, when Herschel politely declined the nomenclatural dedication to the memory of his father, explaining: "I have personally committed myself to a mythological name, a few years ago …" (Herschel, 1847a).

European astronomers became immersed in a debate over what should be the name of the new sphere, and gradually came to agree that national sentiment should be excluded from heavenly nomenclature, whereby Uranus received that name and not *Georgium Sidus* or Herschel, and at around the same time Neptune ceased to be called Planète Le Verrier. In May 1847 Herschel (1847g) wrote:

> As regards *Uranus* I have for a long time used that name and intend to do so. Of course I cannot possibly object to its being used in the N.A. or in any other publication. I thought I had expressed as much at the time of the "Reform" of the Nautical Almanac.[5]
>
> My full impression is that the name Uranus has taken too deep a root to be displaced.
>
> As to the name of the new Planet - As Adams acquiesces in Neptune - As Neptune is a name of French origin (which I think *very* important) and as it is a mythological name, I give my adhesion to it as an admirable *mezzo-termine* to avoid bringing its two discoverers into needless opposition.
>
> I say I consider it as very important that the name Neptune is of French origin (and also that it had at one time the acquiescence if not the implied sanction of Leverrier himself). I regard the discovery whether made by Leverrier or Adams or both as in the main of French origin. The analytical theory of the Planetary Perturbation which alone render it *possible* is almost exclusively French. Clauraut, Laplace, Lagrange Pontecoulant and Poisson are the authors of those formulae which, used as tools or as *telescopes of the intellect* have done the thing and we owe them this national recognition.

He would personally have preferred "1st. Minerva as having sprung fully armed from the head of Jupiter – or 2nd Hyperion (the transcender) the offspring of Uranus and Terra." (ibid.). As regards Arago's pledge, he diplomatically suggested that 'Le Verrier's planet' was more of a description than a name: "Those who think it 'LeVs Planet' may yet *call* it Neptune without compromise and may also if they like *speak of it* as LeV's P[lanet]." (ibid.).

6 AMERICAN SCEPTICISM

American astronomers emphasized how different was the actual planet's orbit compared to the two models of Adams and Le Verrier, because the latter's orbit radii, eccentricities and apse positions had all been so wrong.

Benjamin Peirce was using the phrase 'happy accident' to describe its discovery (Hubbell and Smith, 1992: 269), a view also associated with the American astronomer Sears Cook Walker. Edward Everett, the President of Harvard University, wrote to Herschel in some concern upon this matter, saying he wished for a confidential opinion. He first thanked Herschel for an early, pre-publication copy of the *Outlines*: "I should regard the volume – however it had come into my possession – as one of the most valuable in my library. The letter of the duke accompanying it, with the inscription on the blank-page, makes it truly inestimable." (Everett, 1847). He then described the controversy stirred up by "… Prof. Peirce of this University …" (ibid.). At the American Academy of Arts and Sciences, "… he holds that the real elements of Neptune as observed, are so different from the predicted elements of Adams and Le Verrier, that the discovery must be considered as accidental." (ibid.). Everett distrusted these views, as having an "… extravagant and improbable cast …", yet had to admit that Peirce was regarded as "… one of our very first mathematicians …" and seemed confident enough when propounding his views. Everett feared that they might bring discredit to the University, and asked: "I wish you would impart to me your view of the subject, as freely & candidly as if we were talking over the matter quietly at Trinity Lodge, with no-one but Dr Whewell to listen." (ibid.). Sir John's reply is, alas, lost.

In the summer of 1848, the Paris Archive librarian, Jacques Babinet, began advancing the argument associated with Peirce whereby the new sphere's discovery was a mere 'happy accident.' The two Neptunes, as predicted, had radii far too large (38-35 AU as compared to 30 AU), and their masses were also too large to compensate for this. Here is Sir John explaining the matter, to his old friend, William Whewell:

> By the way what a fuss is raising about the identity of Neptune - The case is as clear as daylight - Neptune (the real Nep.) - comported himself all the time he was within pull of U. very nearly indeed as the hypothetical N of Leverrier and Adams would do. - Their Nep. was a respectable counterfeit - he put on a mass to hide the excess of his distance - an excentricity to get him within reach in spite of his huge axis - and a place of perihelion near conjunction to spur up his sluggish angular motion and enable him to keep tolerably in the right direction. But what can have set Babinet (who is a good mathematician) at sea about it? (Herschel 1848b).

The hypothetical planet had been placed by both parties near to an imagined perihelion of an orbit with hugely exaggerated eccentricity, so that—over the time of its discovery and for some decades earlier (i.e. the period containing the most accurate observations)—it could be seen as keeping "… tolerably in the right direction." (ibid.).

Le Verrier had been obliged to defend his case against Babinet. The latter's view he summarized as: "That Galle's planet had nothing to do with the one which Adams and I had searched for; and that the coincidence was fortuitous." (Le Verrier, 1848). Babinet argued that the predicted planet still awaited discovery—and he named it Hyperion! Even Le Verrier found himself coming round to accept Herschel's argument, that the synchrony and concordance of the two predictions was the best argument against Peirce's 'happy accident' thesis. Herschel (1848a) reassured him: "My faith in Neptune being the real planet which has perturbed Uranus has never for an instant been disturbed …", and he entered into a discussion of the perturbation-theory involved, so as to reassure Le Verrier. When Uranus and Neptune became conjunct in 1820, the two imaginary orbits were then, he noted, both near their perihelia. On his somewhat simplified version of how-to-find-the-planet he wrote:

> The perturbation of an interior by an exterior planet in the longer planetary orbits becomes large only when the bodies approach conjunction. The disturbing force of N. on U. in conjunction is 10 or 12 times greater than in opposition or in quadratures. - Now, the first and only conjunction of N & U which has taken place since 1690 has been that of 1820, and the period of disturbance may, I suppose be taken at about 20 years on either side. (ibid.).

The perturbation of Uranus increased until somewhere around 1817 and then started to decrease. The perturbations should be centred around conjunction, as Airy had explained in his book *Gravitation, an Elementary Explanation of the Principal Perturbations in the Solar System* (1834). This gives a general indication of when the meeting with the unseen new sphere must have been, from which its present position could be roughly inferred, and "This in great measure indicates the direction in which the new planet must lie." (Herschel, 1849b: 513).[7]

In his *Outlines of Astronomy*, Herschel (ibid.) added that he had described the new planet's discovery, "… and I hope also to put the salient points of the present discussion in a light intelligible to all the world." One must surely agree that he did so. Herschel (1848a) also wrote Le Verrier an encouraging letter regarding the validity of the calculation he had performed:

> The actual longitude of your and Mr Adams's perihelia of N. is nearly that of the two planets in conjunction - hence the angular motion of the hypothetical planet being, by reason of the large excentricity, much greater at perihelion than at its mean distance, would approach nearly to the angular motion of the true Neptune - and in fact the hypothetical Neptune appears to have been a *very fair imitation* of the real one at that epoch.

Replying to RAS Secretary, Richard Sheepshanks, by way of excusing himself from contributing anything to the *Monthly Notices* immediately prior to the publication of his *Outlines of Astronomy*, Sir John explained: "… what little I have to say on the subject of Neptune will be said *very quietly and guardedly* in Chap. 14 of the 'Outlines' (whenever they shall appear) for to say the truth there are points in the matter of the perturbations which I do not quite see my way through by the light of common sense and dynamics." (Herschel, 1849a).

On 1 October 1846, the same day that Herschel penned his decisive letter to *The Athenaeum*, he had also written a letter to William Lassell (near Liverpool), whose large equatorially-mounted reflecting telescope could easily track the stars. "Look out for satellites with all possible expedition!" was the President's injunction (Herschel 1846b). Lassell did so, becoming the first to espy Triton, the large moon of Neptune,[8] and he announced the existence of this satellite to *The Times* in several letters between July and September of 1847. Lassell achieved priority over several other European and American astronomers with large telescopes who were likewise searching for any such moon. From its orbit Neptune's mass was found, and thereby key questions concerning the manner of its prediction could be resolved. Herschel had written to the right person.

On 10 July, 1847 Le Verrier and John Couch Adams finally met, at Herschel's home 'Collingwood', in Kent, on the occasion of an Oxford meeting of the British Association for the Advancement of Science. Despite a language barrier to their communication, the two were reported to have got on together. Later that year, Le Verrier (1847) asked for an extra copy of Sir John's new book, *Results of Astronomical Observations at the Cape of Good Hope*, so that he could present it to the King of France. Herschel was a representative figure of British science to the extent that, indeed, "In his own day, the name 'Herschel' *meant* 'science' …" (Ruskin, 2004: 202), having become "… England's most influential philosopher of science in the 1830s …" (Buttmann, 1974: 162) following the publication of his *Preliminary Discourse on the Study of Natural Philosophy* in 1831. His contributions helped to guide British science through the stormy drama of Neptune's discovery, and elucidated the key scientific concepts of prediction, discovery and priority.

7 NOTES

1. Thirty letters are cited here, both to and from Herschel, of which only twenty-two are archived within Crowe's collection of 14,815 Herschel letters (see Crowe *et. al.*, 1998); six of those cited here (i.e. Herschel, 1846f, 1847a, 1847b, 1847c, 1848a, and Le Verrier, 1848, in Section 8) were, for whatever reason, omitted. My Neptune-discovery archive (www.ucl.ac.uk/sts/nk/neptune-corr.htm) has forty-eight letters from Herschel, including twenty-eight in the Royal Society Library; seven in John's College, Cambridge; three in the RAS's 'Neptune file' in the Cambridge University Library; two in Trinity College Library, Cambridge; and two in the Observatoire de Paris Archives.
2. The Minutes of the RAS Council (1847) record Augustus de Morgan's motion: "It is not expedient to recommend a General Meeting to depart from the course laid down in the bye laws as to the award of the medal." Sheepshanks and Main proposed a motion to omit 'not' from this text, but it was refused. Herschel was not present at the meeting but Airy was.
3. This is a technical literary term used to describe that moment in Greek tragedy *katastrophe*, from *katastrephein* to overturn, in which the final event of the dramatic action of a tragedy occurs (my thanks to W. Sheehan).
4. Adams' manuscript, 'On the Perturbations of Uranus,' was published as an appendix to *The Nautical Almanac and Astronomical Ephemeris for the Year, 1851* (see Adams, 1847).
5. *The Nautical Almanac* only changed this planet's name from 'The Georgian' to 'Uranus' in 1851.
6. On 24 December, Herschel (1846k) had written to Augustus de Morgan, advocating these two names. In this letter he states that Heinrich Schumacher, Editor of the *Astronomische Nachrichten,* had written asking his view concerning a name for the new sphere, and that he had advocated these two names, but these letters are lost.
7. See Herschel's (1849) *Outlines of Astronomy*, page 513, section 773. Sampson (1904: 149) argued against this attractively simple view: "The conclusion is drawn [by Herschel] that Uranus arrived at conjunction with the disturbing planet about 1822; and this was the case. Plausible as this argument seems, it is entirely baseless." For more recent comments on this theme, see Kollerstrom, 2006 (Appendix III).
8. A response to Herschel's letter came from the astronomer William Dawes who was staying with Lassell. On 6 October 1846 he wrote to Herschel that, "Lassell has described its [Neptune's] appearance as 'a neat pale small bluish disc' and believes he may have detected a ring around it." (Dawes, 1846). This is probably the first astronomical allusion to Neptune's colour.
9. After Herschel's death his son, Colonel John Herschel, collected and copied his father's correspondence, and these copies, and the originals, are now in the Royal Society's library in London. After John Couch Adams' death, Douglas McAlister transcribed many letters relevant to his life, and these included some Herschel letters. The McAlister Collection is now stored in the St John's College library in Cambridge. Transcription of the Herschel letters has been done primarily using copies of the letters, as being more legible, while any originals have been used for checking the text. Letters here cited are originals unless listed as copies.

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RS:HS = The Royal Society's Herschel Collection

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A GLIMPSE AT THE ASTRONOMY HERITAGE OF THE SCIENCE MUSEUM, LONDON[1]

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Abstract: The astronomy collections at the Science Museum, London, are probably the richest in the world with respect to their diversity and size. Amassed over the last 150 years, they contain items as diverse as a model exhibited at the London Great Exhibition of 1851 to instruments used on today's robot space probes. To give a glimpse of the wealth of the collections, this account will focus upon a number of objects under four themes. These are early telescopes, people associated with astronomy, scientific expeditions and models in astronomy.

Keywords: astronomical collections, Science Museum, London

1 EARLY TELESCOPES

Although priority for the invention of the telescope is uncertain and may never be resolved (see van Helden, 1977), definite landmarks in the instrument's evolution are known. Key objects in our collections illustrate the twin track development of both reflecting and refracting telescopes. This is epitomized by Christopher Cock's drawtube telescope dated 1673 (Baxandall *et al*, 1926; NMSI 1926-419). Made in London, this is the oldest complete telescope at the Science Museum (Figure 1).

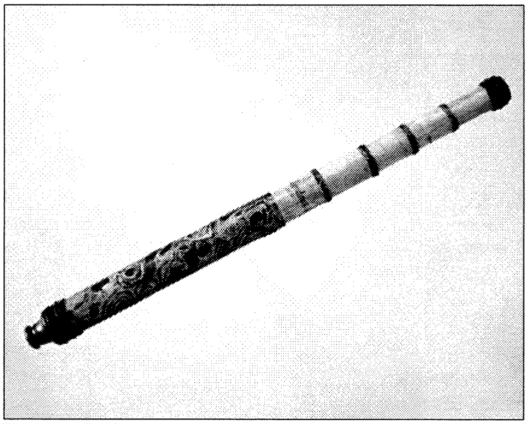

Figure 1: Early draw tube telescope signed Christopher Cock, London, and dated 1673.

The evolution of the non-achromatic refracting telescope is further illustrated by Huygen's aerial telescope (Smith, 1738: 354-362 and Plate 52). With a focal length of 150 feet (46m), the telescope dispensed with the need for a tube; instead the object lens was mounted aloft a high pole connected to the eyepiece by means of a taut cord (NMSI 1932-461; see Figure 2). At night objects were located using the image of a candle flame that reflected from the reverse side of the main object lens via a lantern adjacent to the observer. The poor quality glass used in early telescopes can be judged from an early lens in the collections (Howse, 1975) that Pierre Boreal (1629–1689) of the French Academy of Science is thought to have made (NMSI 1932-460). Like the Huygens' telescope, it also originates from the Royal Society of London. John Flamsteed (1646–1719), the first Astronomer Royal, is believed to have used it with the 90-ft (27.4m) Well Telescope at the Royal Observatory, Greenwich, in his search for stellar parallax (Laurie, 1956).

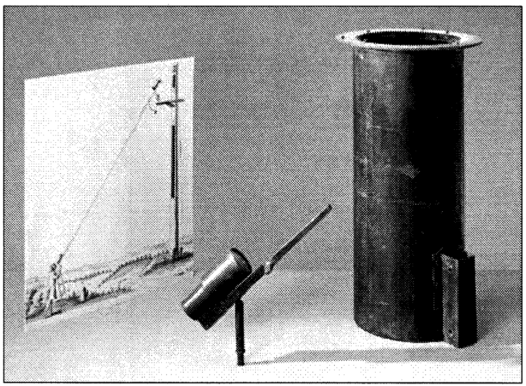

Figure 2: An eyepiece and objective lens mounting for a 210-foot aerial telescope. The lens for the instrument was given to the Royal Society, London, by Christiaan Huygens.

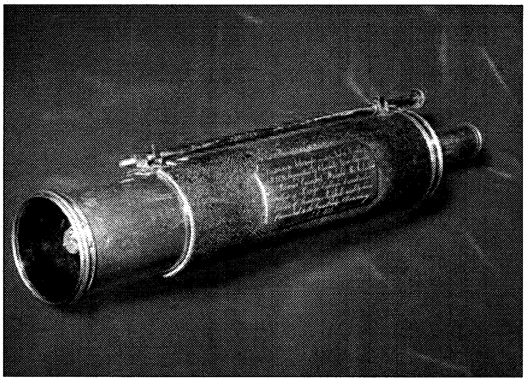

Figure 3: Early Gregorian telescope made by John Hadley, with a plaque that dates it to 1726

The collections hold similar icons relating to the early development of the reflecting telescope. While Sir Isaac Newton's first telescope might be seen as the prototype, it was John Hadley (1682–1744) who was the first to make useful reflecting telescopes. Preserved at the Science Museum are two telescopes built by Hadley that illustrate these changes. The first (NMSI 1932-459) consists of the optical components of Hadley's first Newtonian reflecting telescope that he made in 1723. Hadley (1723) first demonstrated and later donated this instrument to the Royal Society. The other is a Gregorian reflecting telescope (NMSI 1937-601) that has been credited as being the oldest surviving example (Figure 3). Although James Gregory (1638–1675) first proposed this optical design in 1633, the difficult optical surfaces could not be made by the opticians of the period. Today there is some doubt as to this claim, as stylistic details suggest a later date. It seems more likely that this relic was made at the end of Hadley's life, perhaps as a representation of his first Gregorian instrument. This historical object from the early history of the telescope has a documented provenance (Rigaud, 1835). It owes its survival to having been kept within the Hadley family circle before being donated, in 1874, to the University Observatory, Cambridge, in England.

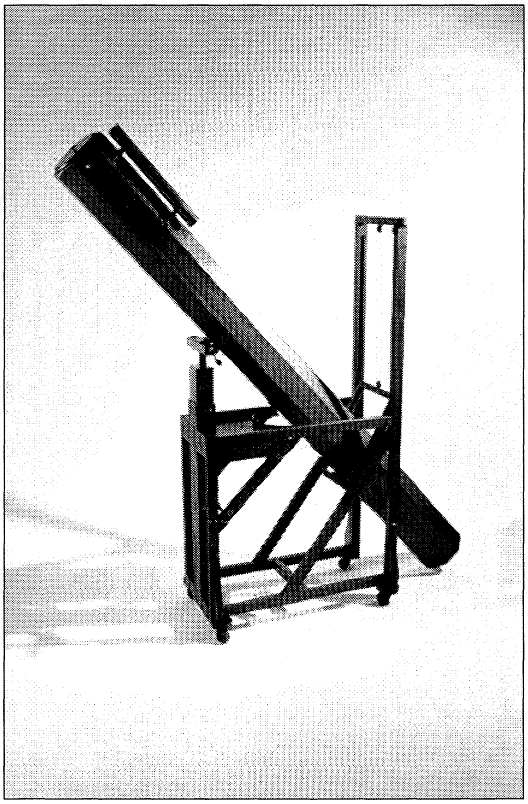

Figure 4: Dated to about 1783-1785, this 7-foot telescope was made by William Herschel for his good friend Dr Watson whom he first met in Bath whilst observing the heavens.

2 PEOPLE IN ASTRONOMY

For much of its history, the discipline of astronomy has been the domain of the amateur. Prior to the emergence of professional astronomers during the nineteenth century, individuals of independent financial means funded most astronomical research. Employment in this field was limited to a small band of people in the service of a rich sponsor. These were more likely to be influential aristocrats rather than the government itself. The national observatories of both France and Britain, established in the seventeenth century, are rare examples of state-funded astronomical institutions. Their function was strictly utilitarian, to help commerce through maritime trade, by developing a solution to finding longitude at sea. Pure research tended to lie in the realm of the amateur scientists, who could follow their own programmes of investigation, free from interference or accountability to government (see Chapman, 1998).

Within the ranks of the amateur astronomy there are many examples, both from the eighteenth and nineteenth century, of amateurs having instruments preserved at the Science Museum. By far the best known is Sir William Herschel (1738–1822), a trained musician from Germany, who came to England as a refugee to escape the French occupation of his native Hanover. His subsequent achievements, which placed him at the forefront of the developing discipline of astronomy, have been well documented by Hoskins (1963) and Schaffer (1981). The Science Museum is fortunate to possess an unrivalled selection of his telescopes, along with a range of associated material. At present, the majority of these can be seen on display at the Museum in the 'Science in the Eighteenth Century' and 'Making of the Modern World' galleries. Preeminent amongst these Herschel relics are two of his, so-called, '7-foot telescopes'. The first (NMSI 1876-1000), with a mahogany tube and stand (Figure 4), was made around 1784 (Lubbock, 1933: 138) for Sir William Watson the younger (1744–1825?). Longstanding friends, Watson first met William Herschel outside his house where he was observing with a telescope (Lubbock, 1933: 73). Through Watson's influence, William Herschel published his first research paper and was introduced into the circle of Britain's scientific elite. The second 7-foot telescope (NMSI 1908-160 and Dreyer *et al*, 1923), which is made of black painted deal (pine), was once the property of Caroline Herschel (1750–1848). Acting as amanuensis, Caroline recorded William's observations at the telescope and was an able observer in her own right, discovering eight comets during her lifetime (Herschel, 1876; Hoskin, 2003). The telescope, made after 1795, is a copy of the one William used to discover the planet Uranus (Herschel, 1876: 313). Caroline is thought to have taken the telescope to Hanover after William Herschel's death; it was later given to the Royal Astronomical Society.

Other Herschel material (Steavenson, 1925: 210-220) includes a large selection of eyepieces (NMSI 1925-466 and 467; see Figure 5), a mirror grinding/polishing machine (NMSI 1876-1019) and a selection of mirrors (1925-464 and 1971-465; see Figure 6). Though most of Herschel's mirrors are made of speculum metal, an arsenic-rich bronze alloy, the Museum has a rare example of a glass mirror (NMSI 1925-463) that he made, and another made of the white ceramic known as Tassies compound (Steavenson, 1925: 221-238). The largest Herschel item currently on display is the original 48-inch mirror (Figure 7) that was cast for his Forty Foot Telescope in 1785 (NMSI 1932-567 and Dreyer 1912).

In stark contrast to William Herschel, Dr James Lind (1736–1812) is almost unknown outside historical circles. As a medical doctor and gentleman scientist he was familiar with most of the prominent scientist of his day. Later in life he retired to Windsor, England, where he was doctor to the Royal household. By chance his telescope (Figure 8), which he used to observe the 1769 transit of Venus (Lind 1769), is preserved in the collections of the Science Museum (NMSI 1906-71). The instrument has recently gained more significance as it has been suggested that this

Scottish physician was the role model for the figure of Dr Frankenstein in Mary Shelley's famous novel. It is argued that Mary Shelley (1797–1851) drew her inspiration for the character from recollections of Dr Lind by her husband, the poet, Percy Bysshe Shelly (1792–1822) (Goulding, 2002). During his education, Shelly attended Eton College School near Windsor, where Dr Lind was his science mentor (as the school did not teach the subject).

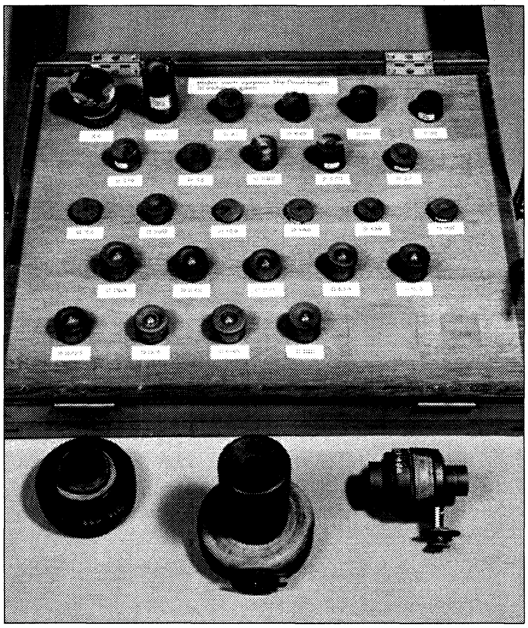
Figure 5: Selection of twenty-six eyepieces and two filar micrometers made and used by Sir William Herschel with his telescopes.

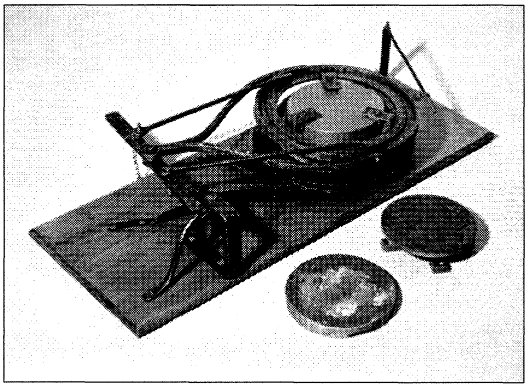
Figure 6: Hand-operated polishing and grinding machine used by Sir William Hershel to make 6-inch telescope mirrors.

Whilst the instruments of Herschel and Lind are famous by their associations, the Groombridge Circle (NMSI 1918-169) is significant because of the observations made with it. Acquired by the Science Museum in 1918, the transit circle (see Figure 9) was the first large instrument of its type to be used in England (Pearson, 1829: 402-405). Completed in 1806, it was ordered by Stephen Groombridge (1755–1832), a successful London merchant, from the instrument-maker Edward Troughton (1753–1835). Housed in a small observatory within his home, Groombridge used this transit circle to undertake an exhaustive survey of the positions of the north polar stars (Ashbrook, 1974). He would frequently excuse himself from the dinner table to make a vital observ-ation, only to return soon after and resume where he had left off. Such was the accuracy of the resulting star catalogue (Groombridge, 1838), published after Groombridge's death, that it was still of value well into the twentieth century.

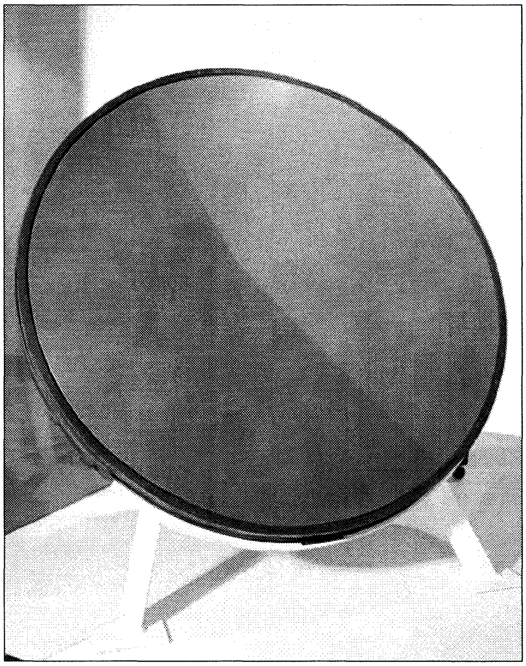
Figure 7: The first of the two speculum mirrors that Sir William Herschel made for his great Forty Foot Telescope, erected at his home at Slough, England.

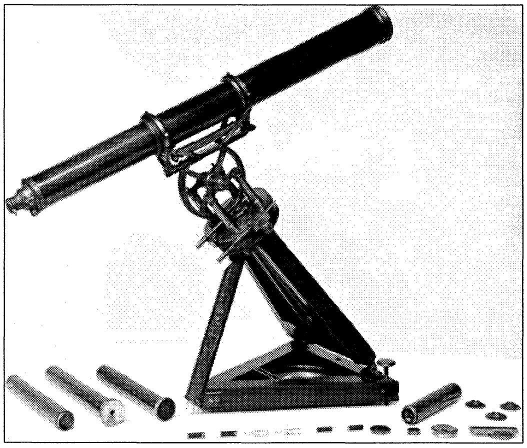
Figure 8: Dr Lind's 2-foot refracting telescope made by Jesse Ramsden with a lens by Peter Dollond and mounting by John Miller. It was used by Lind to view the 1769 transit of Venus.

3 SCIENTIFIC EXPEDITIONS

When Edmond Halley proposed that the transits of Venus could be used to measure the vital Earth-Sun distance, he set in train the first large-scale scientific expeditions. These rare astronomical events, occurring at century-long intervals, were therefore a great spur for scientific co-operation between nations. Due to the Seven Years War (1756–1763) the 1761 transit of

Venus was poorly observed, but eight years later, in 1769, matters were entirely different. From both these events the Science Museum has a rich collection of instruments that were sent around the world to try and gauge the size of the Solar System.

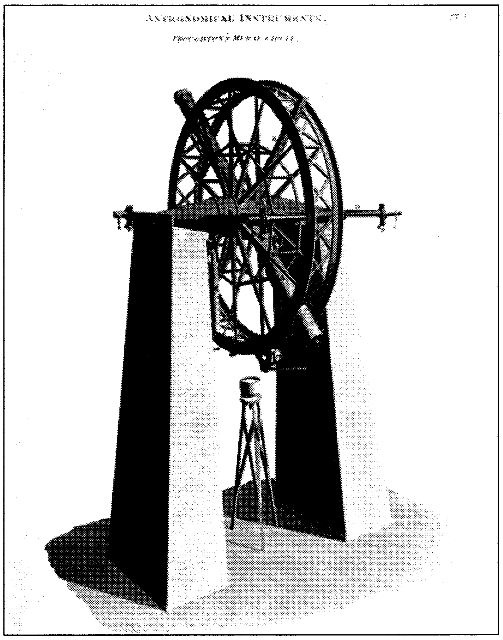

Figure 9: Print showing the Groombridge Circle, a four foot transit circle made by Edward Troughton in 1806.

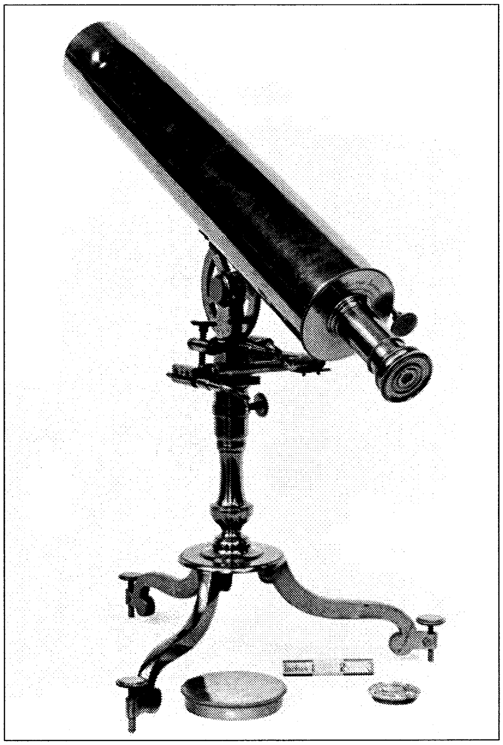

Figure 10: Gregorian telescope by Benjamin Martin, London, used by John Winthrop to observe the 1761 transit of Venus from Newfoundland.

From the 1761 transit the Museum has the Gregorian telescope (NMSI 1911-283) used by John Winthrop (1714/15–1779) to view the event from St John's, Newfoundland, in Canada (Figure 10). Originally the property of Harvard College (Wheatland, 1968: 13-14), it survived a disastrous fire in 1764, but was lost during the American revolutionary war.

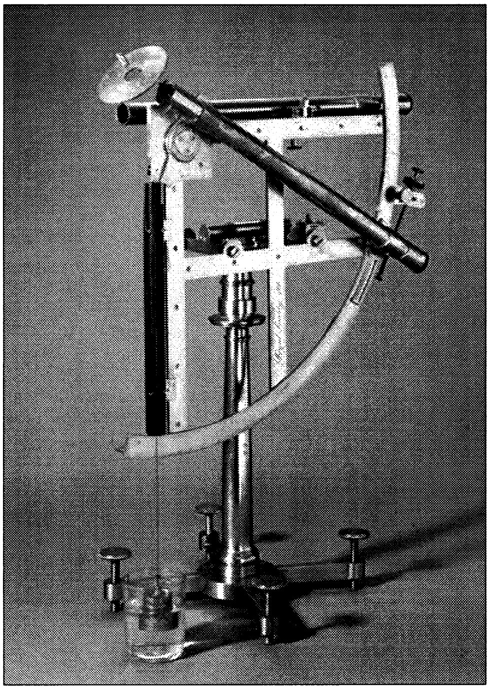

Figure 11: Two foot quadrant with a stand by John Bird, ca.1760s. Acquired by the Royal Society of London for the observation of the 1769 transit of Venus.

Figure 12: Two foot Gregorian telescope and stand by Sir James Short, with split-lens micrometer, ca.1763. Made for the Royal Society of London for the observation of the 1769 transit of Venus.

The Museum also has a selection of instruments that were dispatched to several sites around the world by the Royal Society of London (e.g. see Bayly, 1769; Dixon, 1769). These instruments include a pair of portable quadrants NMSI 1900-138 and 139; see Figure 11), a regulator clock (NMSI 1914-591), along with a reflecting telescope by James Short (NMSI 1900-136; Figure 12) and a refracting telescope by John Bird (NMSI 1900-133).[2] It is now impossible to distinguish the exact history of each instrument with respect to specific expeditions (Howse, 1979), but it is likely that some were used on Captain Cook's first voyage of discovery (1768-1771) to Tahiti to view the transit (see Beaglehole, 1968; Orchiston, 2005). Another item from the same source is a model to demonstrate the basis for a transit of Venus across the Sun's disk (NMSI 1900-150; cf. Calvert, 1967: item No. 17). Used to popularise the approaching event, the precious model is probably the only surviving example from this period. On loan from the Royal Society, this de-vice was recently returned, and can now be seen on display at the Society's headquarters in Carlton Terrace, London.

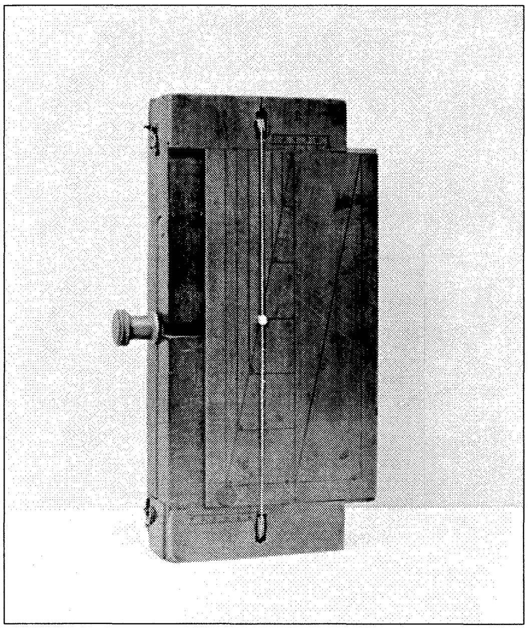

Figure 13: Boxwood model made by James Bradley (Savilian Professor of Astronomy at Oxford) to illustrate his discovery of the aberration of light. It was used by him during his lecture on the subject at the Ashmolean Museum, ca.1729.

4 ASTRONOMICAL MODELS

The impression that the astronomy collections of the Science Museum mainly consists of telescopes and associated instruments is misleading. A significant percentage of the objects are in fact astronomical models that either represent or demonstrate astronomical principles or, in some cases, accomplish both. An example of the latter category is a diminutive boxwood device (NMSI 1876-1029) created by James Bradley (1693–1762) to demonstrate the aberration of starlight (Figure 13). Made around 1729, when Bradley was the Savilian Professor of Astronomy at Oxford, it was used by him in his lectures. In operation the apparent displacement of starlight is achieved using a laterally-moving plate driven by a pulley-driven string carrying a glass bead representing a corpuscle of light. As a handle is turned both the bead and the inscribed plate moved at right angles to each other. Over a century later, in 1845, the model was described and illustrated by a later Savilian Professor, the Reverend Baden Powell (1796–1860) (see Powell, 1846).

Figure 14: Planetary model called an orrery to show the motions of the Sun, Moon and Earth, ca.1715. This example is the one to which the title of orrery was originally conferred after the Earl of Orrery who commissioned it.

In comparison to Bradley's simple device, the Science Museum's original orrery (NMSI 1952-73) is an ostentatious and intricate device (Figure 14). Built by John Rowley (d. 1728), a London instrument-maker, it was made for Charles Boyle (1676–1731) the fourth Earl of Orrery around 1713 (see King, 1978: 150-167). It was not the first planetary model to incorporate gearing to show the Copernican Sun-centred system, as in 1657 a fine example was made in the Dutch city of Leiden (see Dekker, 1986). Models of this type became popular as a means to portray the clockwork Universe that Sir Isaac Newton's new theory of gravity could now predict in precise detail. Copied from an earlier example and designed by the clock-maker George Graham (1673–1751), the machine's fine qualities were soon described in print by the London wit and essayist, Sir Richard Steele (1672–1729) (see Cudworth, 1883: 81-82). Such was the publicity that it was soon being called 'The Orrery', a term that was subsequently applied to all similar devices in English-speaking countries.

The astronomy collections of the Science Museum have a broad selection of models that convey representations of the heavens and our nearest celestial neighbours. Most cherished amongst these is a celestial globe (NMSI 1910-249) thought to be the oldest known example that uses printed star maps on paper gores (Figure 15). Attributed to Johann Schöner (1477 –1547) (Zinner, 1956: 171), a German globe-maker from Nuremberg, it has been dated to around 1532 (Dekker and Krogt, 1993: 23-24). A near identical globe can be found in the composition 'The Ambassadors', painted by Hans Holbein, which currently hangs in the National Gallery, London. By contrast, John Russell's Moon globe (NMSI 1949-117) has a complex stand to demonstrate lunar libration and parallax (Figure 16). The complete assembly named Selenographia by Russell was based on his own lunar observations, which he started in 1785 using a small telescope fitted with an eyepiece micrometer. An established portrait painter, John Russell (1745–1806) believed he could create a more realistic and accurate portrayal of our lunar neighbour (Ryan, 1966). Produced in only small numbers through direct subscription, the Moon map was published in 1797. This particular example came to the Museum in 1949, being a gift from the British monarch King George VI (1895–1952).

Figure 15: Celestial globe made by Johann Schöner in Nuremberg, Germany, ca.1532. This is thought to be the oldest surviving printed celestial globe.

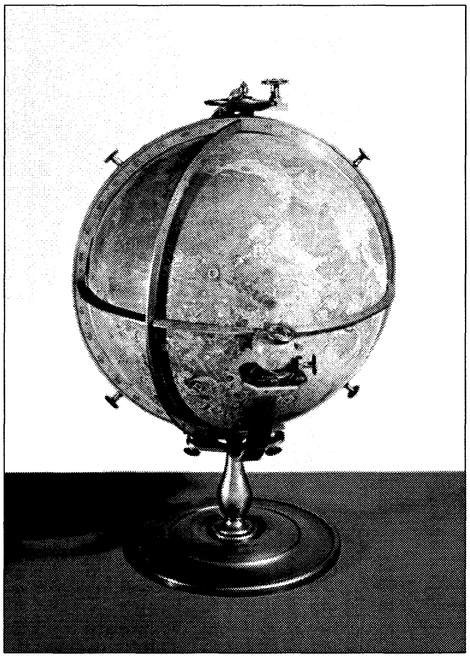

Figure 16: Moon globe with elaborate stand to demonstrate libration and lunar parallax, made by the artist John Russell.

Also under the lunar theme is an electrotype model of the lunar crater Eratosthenes (NMSI 1858-43) made by Henry Blunt (d. 1853) from Shrewsbury, England (Figure 17). Given to the Museum by the Commissioners of the Great Exhibition in 1858, it is one of the earliest acquisitions in the collection. The model was exhibited at the 1851 World Exposition in London and was awarded a prize by Exhibition jurors (Bennett, 1983: 12). The original plaster model from which the electrotype model was made now resides in the archives of the Royal Astronomical Society in London (see Royal Astronomical Society, 2000).

5 SUMMARY

This brief resume of the astronomical heritage of the Science Museum, London, offers a unique insight into the wealth and diversity of its collections. This account is intended as a stimulus to open people's eyes to the narrative of these objects and their significance to the history of astronomy. Often hidden from public view, these items need to be more widely seen, a process that is now becoming possible through rapidly-expanding multi-media avenues, including the Internet, and through changes in how museums now interpret their collections. Already an inventory of our astronomy holdings can be found at the Online Registry of Scientific Instruments website (http:// www. isin.org/) hosted by the Museum for the History of Science in Oxford, England. Likewise, the London Science Museum has launched a new narrative website called 'Ingenuity'. Amongst the topics tackled is Sky Watching, which globally explores the subject through the Museum's astronomy collections.

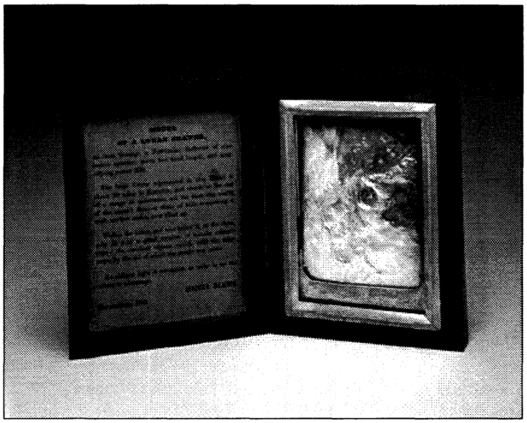

Figure 17: Electrotype copy of a plaster model showing the lunar crater, Eratosthenes. Made by Henry Blunt using lunar observations made from his home in Shrewsbury, England.

6 NOTES

1. This paper was presented in the Historical Instruments Working Group meeting at the 2003 General Assembly of the IAU in Sydney.
2. All of these instruments were used on Royal Society sponsored expeditions to observe the 1769 transit of Venus. The Royal Society's 1834 catalogue of instruments states that the Bird refracting telescope was used for transit of Venus observations, but it does not appear on the list of instruments lent to expeditions specially undertaken by the Royal Society.

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THE 1874 TRANSIT OF VENUS OBSERVED IN JAPAN BY THE FRENCH, AND ASSOCIATED RELICS[1]

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Abstract: In 1874, Janssen, Tisserand and others went to Japan to observe the transit of Venus. Most of the members of the team set up their instruments in Nagasaki, while two of them observed at Kobe. Details of the expedition are mentioned. In 1998, on the occasion of an international astronomical conference held in Japan, the participants had the opportunity to visit the place in Nagasaki where the 1874 observations were performed. A few relics were preserved there, and these are discussed in this paper. They consist of a pyramid erected by Janssen and two pillars. At Kobe, the column built by the Governor is also preserved.

In 2004, a transit of Venus was observed from Europe. Many observing sites were organized in many countries, including Great Britain and France, with many places for the public, students and amateurs. The event was an opportunity for teachers to give an unusual observing experience to their students.

Keywords: 1874 transit of Venus, Janssen, Tisserand

1 INTRODUCTION

After Edmond Halley (1656–1742), French astronomers were apparently the first to take an interest in his proposal to use transits of Venus to determine the solar parallax. The accuracy could be improved in comparison with the value deduced by Jean Dominique Cassini (1625–1712) from observations he made on the Paris Observatory meridian line, while Jean Richer (1630–1696) went to Guyana. After Halley's death, Joseph Nicolas Delisle (1688–1768) called on his colleagues to observe the 1761 and 1769 transits, and he collected their observations under the leadership of the 'Académie des Sciences'. After his retirement, Jérôme Lalande (1732–1807) took over, in the same way (Dumont, 2004). However, astronomers were disappointed by the results and they decided to leave it to their successors to investigate later transits. The next one to come along was in 1874.

2 THE 1874 TRANSIT OF VENUS

The differences between the values obtained from the eighteenth century transits being of the order of 1.5%, i.e. ±0.13″ (Toulmonde, 2004), the 1874 observations had to be organized carefully, all the more as photography and electrical telegraph did allow some real improvements. In France, the Government asked the Académie des Sciences to set up a Commission to decide on observing sites, and which instruments to use. All the members of both the astronomy and the geography and navigation sections of the Académie composed the Commission (Dumas, 1874). Among the astronomers were Charles Delaunay (1816–1872), as first President, Hervé Faye (1814–1902), who became President after Delaunay's death, Urbain Le Verrier (1811–1877), and Jules Janssen (1824–1907) in 1873, when he was elected a member of the Académie. By that time, Faye had left the group (Canales, 2002), and the new President of the Commission was one of the two so-called 'perpetual secretaries' of the Académie, the chemist Jean-Baptiste Dumas (1800–1884).

Among several possibilities for the location of the stations, the Commission eventually decided to consider only six, all of them being in the appropriate part of the world in Eastern Asia and in the islands north of Australia and to the south of New Zealand. The chosen locations were Campbell Island, Saint-Paul Island, and Noumea in the southern hemisphere and Peking, Yokohama and Saigon in the northern hemisphere (Dumas, 1874).

Figure 1: Portrait of Jules Janssen (1824–1907) from a photograph by Paul Berthier (after *L'Univers Illustré*, 1874: 509; Launay Collection).

3 JANSSEN'S PARTIES IN JAPAN

Jules Janssen (Figure 1), who was in charge of the station at Yokohama in Japan, was also among the fifty people sent out of France for the event, the only member of the Académie, which he thus represented officially (Dumas, 1874). The Académie, which had just fully recognized all the achievements made by Janssen during all his previous scientific expeditions, was of course quite confident in his capacities for achieving his new mission.

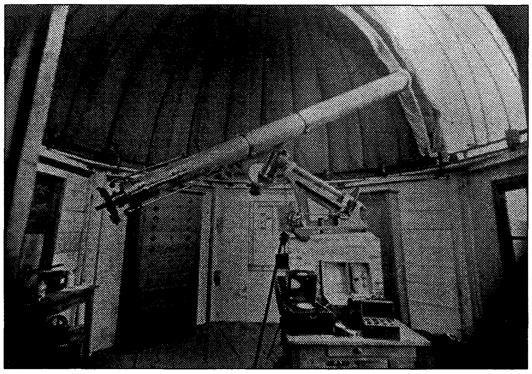

Figure 2: The 8" equatorial used by Janssen at Nagasaki (courtesy Paris Observatory Library).

As usual, Janssen did not know in advance where he would set up his instruments. He was used to arriving at least two months in advance in the country of observation in order to be able to consider several possible sites, taking into account both the probability of good weather and the help he could obtain from the locals. When he arrived at Yokohama, he realized that the weather would certainly be better towards the west coast of Honshu and decided to go to Kobe. Unfortunately, information he received there was not very promising, and he decided to settle further south, at Nagasaki. The location eventually chosen was Kompira Mountain (Kompira-Yama), a hill well "... above the vapours of the town." (Janssen, 1875a: 343) but reachable by road and close to everything he might need. The only difficulty was to carry up to this place the 250 boxes full of instruments and equipment, but it did not take too much time for Janssen to find the five hundred people needed to take on this task (Janssen, 1875a).

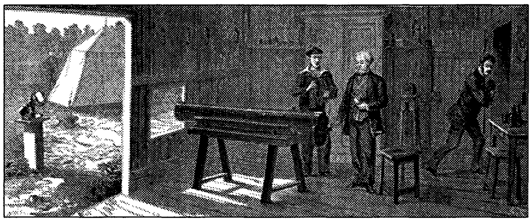

Figure 3: Janssen's photographic revolver in operation (after *L'Illustration*, 1875: 28; courtesy Patrick Fuentes).

As far as the instruments were concerned, Dumas (1874) emphasized in his report that Janssen would use not only the instruments of the Commission, two refractors of 8" (Figure 2) and 6" aperture and a photographic equatorial using daguerreotypes, but also some other photographic cameras and the 'photographic revolver' (Figure 3) that he had specially designed for the event (Janssen, 1876a). This device, which was more widely used by the British (Launay and Hingley, 2005), is now recognized as the precursor of the movie camera. The principle of the instrument (Janssen, 1873) was to record, for each of the four contacts, a series of images taken at regular and short intervals on a circular plate (Figure 4).

The first version of the instrument built by Deschiens did not satisfy Janssen because its clockwork mechanism was causing too much vibration. A new version, a copy of which is preserved at Paris Observatory (Figure 5), was then built by Redier and his son. The device allowed 48 images to be recorded in 72 seconds, the plate being stationary during each exposure. Of course, the precise time of each shot was automatically recorded, and the instrument was automatically driven as soon as the rotation began. The result is shown on the enlarged photo of the practice plate, which is also preserved at Paris Observatory (Figure 6).

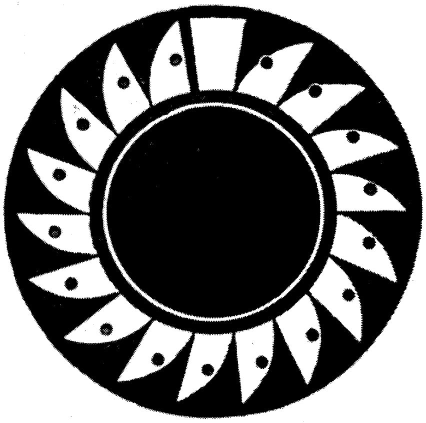

Figure 4: Sketch of the chronographical recording after a drawing by Janssen (after *L'Illustration*, 1896: 446; Launay Collection).

In order to secure the observations on the day of the transit, Janssen sent two members of his team (Delacroix and Chimizou) back to Kobe, while the main part of the group (Tisserand, Picard, Arents, d'Almeida and a few others) remained with him in Nagasaki.

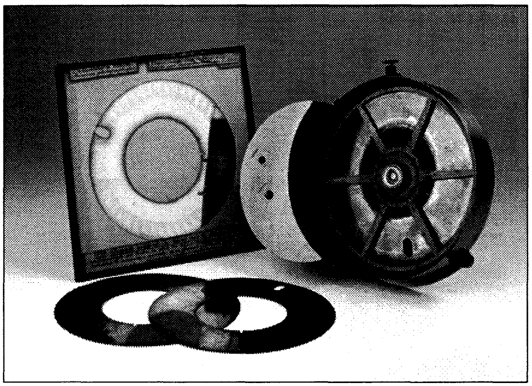

Figure 5: The photographic revolver built by Redier preserved at Paris Observatory (courtesy Paris Observatory Library).

To obtain the value of the solar parallax, an accurate value of the local coordinates of the stations was needed. To do so, Janssen was helped by Félix Tisserand (1845–1896). A graduate of the 'École Normale Supérieure' with a rank of number one, he was recruited by Le Verrier in 1866 at the Paris Observatory and was asked to accept the Directorship of the Toulouse Observatory in 1873. Through his duties at the Paris Observatory, he was well trained in meridian observations and in geodesy. At Nagasaki, he had to take care of the clocks and chronometers, and to determine the longitude and latitude from his observations made with a portable meridian refractor (Tisserand, 1880). The longitude of Kobe was determined thanks to chronometers telegraphically adjusted to those of Nagasaki, while Janssen (1875b) went back to Kobe after the transit in order to determine the latitude.

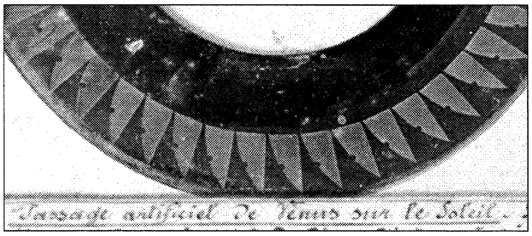

Figure 6: Part of the revolver practice plate preserved at Paris Observatory (courtesy Paris Observatory Library).

4 THE RESULTS

At Nagasaki, the internal contacts were observed by both Janssen and Tisserand, using respectively the 8" and 6" refractors, while Delacroix observed them in Kobe where the weather, incidentally, was better. Both teams, at Nagasaki and Kobe, recorded about 80 photographic plates of the transit, among which were 60 daguerreotypes now preserved at the 'Conservatoire National des Arts et Métiers' in Paris. A plate of the first internal contact was taken at Nagasaki with the revolver, but unfortunately can no longer be found.

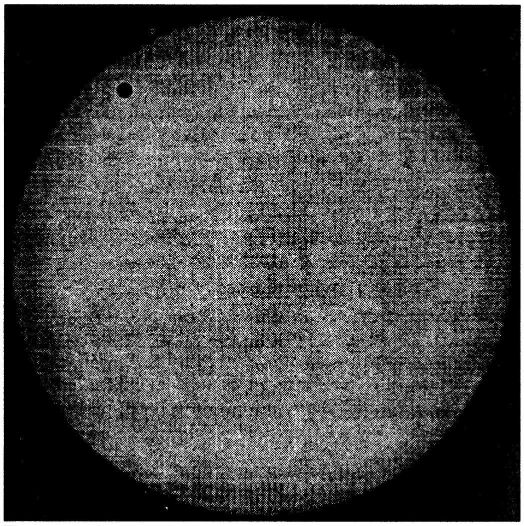

Figure 7: Venus, with luminous aureole, shown on the solar disk on 9 December 1874. This photograph was taken in Japan (after Guillemin, 1877: 946; Launay collection).

The results of the French were no better than those of other nations, and it is well known that, as far as the solar parallax was concerned, the results obtained by all parties all around the world were not very successful (e.g. see Dick, et al., 1998). Astronomers were disappointed: the error of ±0.06″ obtained for all stations was less than that of the eighteenth century, but the astronomers had expected only about ±0.01″. Anyway, as a physicist, Janssen (1874) was very proud to claim that he was able to observe the solar corona eclipsed by Venus before the first contact, using a blue violet filter and, even better (Janssen, 1875c), that Venus' atmosphere was also recorded on the plates taken during the transit (see Figure 7)!

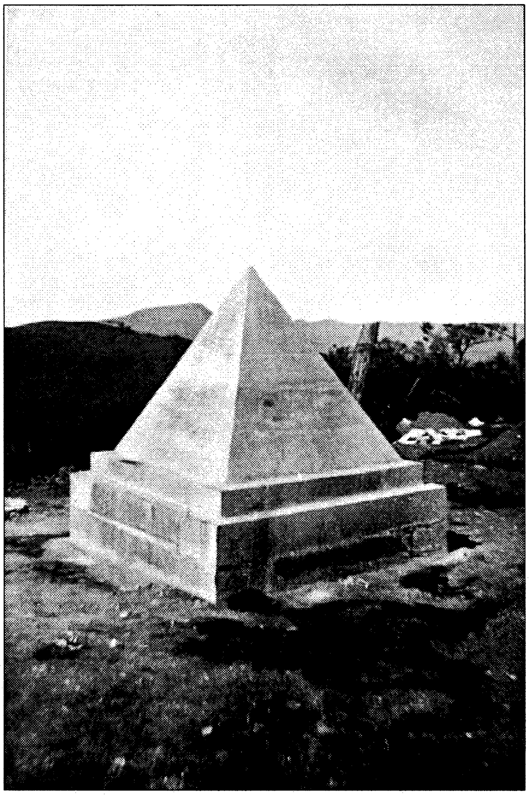

Figure 8: The pyramid installed by Janssen at Nagasaki (after Janssen, 1929: Plate '1876 Fig. 1'; Launay Collection).

5 THE RELICS OF THE 1874 FRENCH EXPEDITION TO JAPAN

In memory of the 1874 French expedition, two monuments were soon erected (Janssen, 1975b). Janssen arranged for a pyramid to be built at Kompira-Yama (Figure 8), and the Governor of Kobe erected a column at Kobe (Figure 9). Both monuments still exist, surviving testimony to the transit of Venus observations, and despite the tragic events that happened at both sites in the twentieth century. At Kobe, one can still admire the column, which is situated in a nice area (see Figure 10).

On the occasion of the 'Third International Conference on Oriental Astronomy', held in Fukuoka (Japan) from 27 to 30 October 1998, Masanori Hirai organized a tour to the Nagasaki area. The participants, including one of us (S.D.), then had an opportunity to visit Kompira-Yama Mountain, 124 years after the French observed the transit there.

The site is well sign-posted both in Japanese and in English, and the pyramid installed by the French is still there (Figure 11). Of course, the inscriptions, which are written in French and in Japanese, are not as legible as they were in the past. Nearby, was a small pillar (Figure 12) where Tisserand is assumed to have used his portable meridian refractor, and a larger pillar (Figure 13), which probably supported either an equatorial or a coelostat. It was quite impressive to see these 1874 expedition relics so close to the town of Nagasaki.

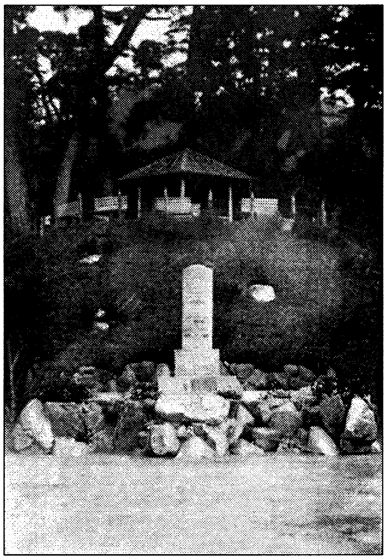

Figure 9: The column erected at Kobe by the Governor (after Janssen, 1929: Plate '1876 Fig.2'; Launay Collection).

Figure 10: Photo of the column taken at Kobe in 2000 (courtesy Marcus Durand, Hubert Durt and Philippe Papin).

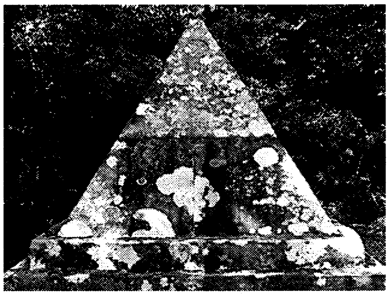

Figure 11: Photo of the pyramid taken at Kompira-Yama in 1998 (courtesy Masanori Hirai).

6 THE 2004 TRANSIT OF VENUS

One of the two twenty-first century Venus transits occurred on 8 June 2004, as predicted long ago by astronomers. On this occasion the chance to observe from Paris Observatory was offered to the general public. This proved a great success, with a great many people and several different instruments available in the garden at the Observatory. An image of the transit could also be seen in the 'Cassini Room', where Janssen's revolver was on display. Many teachers had organized, with their pupils or students, measurements from which they had in mind to determine the solar parallax as an academic experiment.

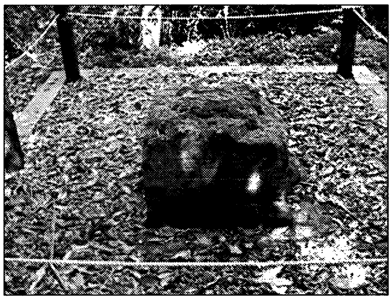

Figure 12: Small pillar at Kompira-Yama (courtesy Kiitiro Hurukawa).

Figure 13: Large pillar at Kompira Yama (courtesy Kiitiro Hurukawa).

At an international level, an IAU Colloquium (No. 196) was organized in Preston (Lancashire, U.K.) by staff from the Centre for Astrophysics. The title of the meeting was 'Transits of Venus: New views of the Solar System and Galaxy', with a part devoted to historical presentations under the title 'Transits of Venus: History, Results and Legacy' and a modern part concerned with the astronomical unit, parallaxes, other planetary transits, distances in galaxies, programmes, etc.

A special day was devoted to the observation of the 2004 transit of Venus, and this included a tour to the place where Horrocks was the first to observe such an event, in 1639. It was also possible to visit the house where Horrocks is thought to have made these observations, thanks to the kindness of the owners.

7 ACKNOWLEDGEMENTS

We are grateful to Patrick Fuentes for supplying Figure 3; Marcus Durand, Hubert Durt and Philippe Papin for Figure 10; Masanori Hirai for Figure 11; Kiitiro Hurukawa for Figures 12 and 13; and the Paris Observatory Library for Figures 2, 5 and 6.

8 NOTES

1. This paper was presented at the meeting of the 'Transits of Venus Working Group' held at the General Assembly of the IAU in Prague on 17 August 2006.

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THE INTRODUCTION OF ABSOLUTE MAGNITUDE (1902–1922)

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Abstract: The absolute magnitude of a star, this being the apparent magnitude that a star would have if it was moved to a distance of 10 parsecs from the observer, is a ubiquitous concept and is commonly used by today's astronomers to represent the luminosity of a star. This short paper traces the history of the expression 'absolute magnitude' from the time of its introduction in 1902 by its originator J.C. Kapteyn, up to the ratification of its acceptance by the International Astronomical Union at its first meeting in 1922.

Key words: *stellar luminosity, parallax, absolute magnitude, H-R diagram*

1 INTRODUCTION

As soon as astronomers could measure both the brightness of a star and its distance, they could then calculate the star's total energy output. As with all physical quantities this had to be expressed as a number. In the MKS (Metre Kilogram Second) system the energy output of the Sun, the solar luminosity $L_\odot$, could be quoted as 3.827×10^{26} W. But this number is very large and one of the golden rules when it comes to quoting physical and astronomical quantities is that the units that are used should give the quantity as a handy number, usually somewhere between 1 and 1,000. So why not choose the solar luminosity itself, as a unit. Well the snag here is that stars have luminosities that typically range from about 0.001 $L_\odot$ to 30,000 $L_\odot$, so, like the MKS energy output, this is not very 'handy' either. The answer was to move to a quantity that was logarithmic. The *absolute magnitude* was ideal. This is the apparent magnitude a star would have if it were seen from a distance of 10 parsecs. Stars typically have absolute visual magnitudes that range from about −6.5, for the most luminous supergiants, to +12 for the feeblest main sequence stars of M5 spectral class (although it must be pointed out that the full extent of this range was not know at the time of the introduction of the absolute magnitude). The Sun has an absolute visual magnitude of 4.82 (see Cox, 2000). Absolute magnitudes are thus numerically extremely 'handy'.

The history of astronomy often produces intriguing questions. Three have fascinated me for many years, and these concern the afore-mentioned absolute magnitude:

(a) Who introduced the concept and when?
(b) Who decided that the reference distance should be 10 pc?
(c) When was this commonly accepted?

These questions are answered in this short paper.

Let us start with the modern definition. The absolute magnitude, M, of a star is equal to the apparent magnitude, m, that the star would have if it were placed at a distance, d, equal to 10 pc, from the Earth-bound observer, and thus had a parallax, π, of 0.1 seconds of arc. We can thus write

$$M = m + 5 - 5 \log d, \text{ or} \quad (1)$$
$$M = m + 5 + 5 \log \pi. \quad (2)$$

The 'apparent magnitude' system, in which the brightness of a star was expressed as an 'importance', appears in the *Almagest* or *Syntaxis*, an astronomical treatise written in Alexandria by Claudius Ptolemy around AD 145 (see, for example, Graßhoff, 1990; Hutchins, 1952; Toomer, 1984). Many historians of science think that both the star catalogue in the *Almagest,* and its associated magnitude (brightness) system, were probably first produced by Hipparchus, the famous Greek astronomer and mathematician, in around 134 BC, and not by Ptolemy some 280 years later. For those wishing to enter the debate I refer them, for example, to Evans (1987), Graßhoff (1990), Newton (1982) and Rawlins (1982).

Hipparchus was observing the sky from the Greek island of Rhodes, at latitude 36° N. Supposedly encouraged by the appearance of a nova in the constellation of Scorpius, he decided to produce a new catalogue of stars. Not only did he list the positional coordinates of each of 1,028 stars (1,025 plus three duplicates), grouped into 48 constellations (12 zodiacal, 21 in the northern sky and 15 in the southern sky), but he also is thought to have introduced a grading system representing the relative 'importance' of each of his catalogued stars. This started at 1 for the brightest fifteen stars visible in 'his' sky, and increased, in unit steps, to 6, the latter grade containing all those stars that were barely visible to the naked eye. According to François Arago (1854: 333), Hipparchus/Ptolemy recorded 15 stars as being of first magnitude stars, 45 second, 208 third, 474 fourth, 217 fifth and 49 sixth, plus 9 obscure and 5 nebulous.

The logarithmic relationship between apparent magnitude and stellar brightness was first placed on a firm footing by the Oxford astronomer, Norman R. Pogson (1856). He suggested that a scale of apparent magnitudes should be introduced such that a star of magnitude m was *exactly* $10^{2/5}$ brighter than one of magnitude (m +1). Hints as to the logarithmic nature of the relationship between brightness and magnitude had been made well before the time of the formalisation by Pogson. Among others, Halley (1720) and Herschel (1829) mention it (see Hearnshaw, 1996: 76). Pogson tacitly assumed that the human eye and brain responded to light such that the sensation was proportional to the logarithm of the stimulus, a relationship that was formalised by G.T. Fechner (1858, 1860). Logarithms were much in vogue in the seventeenth, eighteenth and nineteenth centuries having facilitated numerical calculations greatly since their invention by Baron Napier of Merchistoun in 1614 (see Bell, 1945). Pogson's ideas became generally accepted among the astronomical community when Pickering et al. (1887) used them as the photometric basis of the work at the Harvard College Observatory and Müller adopted them for the Potsdamer Durchmusterung (see Müller, 1897: 446). Jones (1968) notes, however, that they were only universally accepted after 1905.

Returning to Equations 1 and 2, it can be seen that any thoughts about absolute magnitude only become realistic when a reasonable number of stellar distances are known. Historically the astronomer's concept of the cosmos changed drastically with the general acceptance (about the middle of the seventeenth century) of the heliocentric model put forward by Nicolaus Copernicus (1473–1543) in *De revolutionibus Orbium Coelestium* (see, for example, Hutchins, 1952: 510-838). The previous paradigm, that stars were all the same distance from Earth, was replaced by the realisation that the visible world of the fixed stars was immeasurably large (see Koyré, 1957) and that differing distances as well as differing luminosities affected stellar brightness.

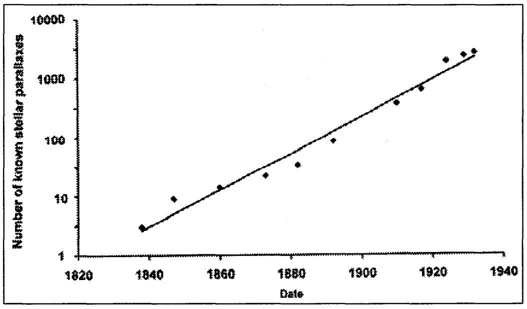

Figure 1: The rate of development of the astronomical knowledge of stellar distance is shown by plotting the way in which the number of stars with reasonably accurate parallaxes (shown logarithmically) varies as a function of date. These data have been taken from Lundmark (1932).

The orbiting Earth presented astronomers with a possible trigonometric mechanism for measuring the distance to stars. In six months our planet moves to a place that is two astronomical units (300,000,000 km) on the other side of the Solar System. Nearby stars thus change their celestial position with respect to more distant stars, and a measurement of this parallactic shift (plus knowledge of the astronomical unit) gives the distances. Astronomers had, however, to wait from 1543 to 1838 before their instruments became sufficiently sensitive to enable this parallactic angle to become measurable.

October 1838 saw the first announcement of a measured parallax. This was for the 5.2 magnitude star 61 Cygni, a so-called 'flying star' with a huge proper motion of 5,260 sec arc per millennium. Friedrich Wilhelm Bessel had been observing this star using the Königsberg Observatory's 6.25-inch Fraunhofer heliometer. Nearly simultaneously Thomas Henderson (1839), working in South Africa, reported the parallax of α Centauri, another 'flying star'. Other parallaxes were reported in steady succession over the next fifty years, and Charles Young (1895), in his famous *Text Book of General Astronomy*, was able to publish a list of 28 known stellar parallaxes (stellar distances were given in light-years).

The stars on this list were fairly eclectic. They had been selected because they were thought to be close to Earth and thus to have large and easily-measurable parallaxes. The two main selection criteria were a large proper motion, and a large brightness (the latter making the stars easily discernible using the visual telescopes of the day). The resulting 28 parallaxes ranged from 0.187″ to 0.054″, this being equivalent to a distance range of 5.4 to 18.5 pc. The median parallax of the group was 0.176″ (d = 5.7 pc), and 64% of the group lay in the range $0.12 < \pi < 0.28''$ ($3.6 < d < 8.3$ pc). As to brightness, the median apparent magnitude was 4.5 and the range was $1 < m < 9$. The data showed no obvious relationship between parallax and apparent magnitude.

The way in which the number of known stellar distances varied as a function of date is shown in Figure 1, these data coming from Young (1895) and Lundmark (1932). The fact that this number was increasing by a factor of ten about every 32 ± 2 years clearly had an influence on the date around which absolute magnitude-spectral type diagrams could be drawn for stars in general.

Many of the parallax results presented at the end of the nineteenth century were of dubious quality and the errors in individual values were large. It was only in the first decade of the twentieth century that accuracy improved, mainly due to the endeavours of the American astronomers H.N. Russell (1905) and F. Schlesinger (1904), and the Cambridge astronomer A.R. Hinks (1906).[1] Only then did astronomers start to become very interested in the importance of the fact that stellar luminosity varied drastically from one star to another. Crommelin (1893) selected 14 stars which had been estimated to be within 4 pc of the Sun, and recorded that their luminosities varied from 83 $L_{\odot}$ to 0.01 $L_{\odot}$. Interestingly he did not comment on this difference. By the first decade of the twentieth century the interest in stellar luminosity had blossomed, and the possible units of measurement became of great interest too.

Agnes Clerke (1905: 383) listed 70 stellar parallaxes, these ranging from 0.75″ to 0.015″ (1.3 to 67 pc). The median parallax was 0.11″ (d = 9.1 pc) and 64% of the group lay in the range $0.04 < \pi < 0.26''$, ($3.9 < d < 25$ pc). The stars had a very similar brightness range to the Young's set (1895: 536). Their median apparent magnitude was 4.5, the range being $-1.6 < m < 9.0$. (64%- of the group lay in the range $1.15 < m < 7.5$).

Parallax measurement at that time was far from easy. Eddington (1914: 40) noted:

> … for a parallax-determination of the highest order of accuracy, the probable error is usually about 0″.01. Thus the position of a star in space is subject to a comparatively large uncertainly, unless its parallax amounts to at least a tenth of a second of arc.

At the time Eddington was clearly thinking in terms of certain standard stellar distances. He was also trying to estimate the actual spatial densities of stars in the local region of the Galaxy. Realising that the stellar tally became less and less complete as their distance increases, he chose 5 pc as a standard distance and recorded (1914: 41) that there were 19 known stars closer to the Sun than 5 pc.[2]

Eddington (1914: 47) also recorded that there were 27 known stars with distances between 5 and 10 pc. The use of 5 pc and 10 pc as significant 'celestial boundaries' clearly echoed the previous use of 'standard distances' at the time when 'absolute magnitude' was first introduced. Interestingly, Eddington did not use the term 'absolute magnitude' in his 1914 book, *Stellar Movements and the Structure of the Universe*. As a measure of energy output he listed stellar luminosities as a ratio of the solar luminosity.

Eddington realised that both the numbers given for the star counts, i.e. 19, and 27, were very much lower limits. When it came to the 19 stars closer than 5 pc he noted that none had a luminosity less than 0.006 (i.e. 1/200) that of the Sun. He was convinced (quite correctly) that "... numerous fainter stars exist." (Eddington, 1914: 42). Also, the volume of the 5 to 10 pc region is seven times greater than the volume of the sphere of 5 pc radius. So if measurements were being made to the same luminosity limit in both regions the outer region should contain 133 stars, not 27.

By the second decade of the twentieth century, parallax studies had become more formalised, mainly due to both the efforts of the northern European astronomers J.C. Kapteyn and H.A. Weersma at the University of Groningen (see Kapteyn and Weersma, 1910) and the English Astronomer Royal, F.W. Dyson (see Dyson, 1909).

The use of the standard distance of 10 parsec, a distance that is now used to define absolute magnitudes, must clearly post-date the introduction of the parallax units of stellar distance, as opposed to the light year. According to Waterfield (1938: 133), the name of the major 'parallax unit', the parsec, is simply a portmanteau word (**par**allax of one arc **sec**ond), this word being introduced by the Oxford Savilian Professor of Astronomer, Herbert Hall Turner (1861–1930). Eddington (1914) was apparently fairly quick off the mark. The 'parsec' as a new basic stellar distance unit (i.e. the distance of a star at which the radius of the Earth's orbit subtends one second of arc) was first mentioned (according to the *Oxford English Dictionary*) in 1913 by the then English Astronomer Royal, Frank Watson Dyson (1868–1939). Quoting from Dyson (1913: 342):

> There is need for a name for this unit of distance. Mr Charlier[3] has suggested Siriometer, but if the violence to the Greek language can be overlooked, the word *Astron* might be adopted. Professor Turner suggests *Parsec*, which may be taken as an abbreviated form of "a distance corresponding to a parallax of one second."

Early astronomical distance terminology was also discussed by Lundmark. He noted (1932: 430) that

> ... the light-year is the one most used. The parsec is also comparatively much used and would be more so also if it were not for its awful name.

Other stellar distance units were mentioned by Lundmark (1932), these being the Herschel (66,890 au), Siriusweite (1,031,324 au), and the metron and Sternweite (both, like the parsec, 206,265 au).

2 ABSOLUTE MAGNITUDE AND THE HERTZPRUNG AND RUSSELL DIAGRAMS

Today the most easily encountered early uses of the concept of stellar absolute magnitude is in two historic and extremely famous graphs, these being the 1911 'Hertzsprung' diagram and the slightly later 1913 'Russell' diagram.

A redrafted version of the original 'Hertzsprung' diagram is reproduced as Figure 2, taken from his first illustrated paper on the relationship between stellar luminosity and surface temperature (see Hertzsprung, 1911, and also, for example, Struve and Zebergs, 1962). Notice, in passing, that figures and graphs in research papers were much less common in those days than they are today. Hertzsprung's previous two papers on stellar physical characteristics (see Hertzsprung, 1905; 1907) were without diagrams. In these papers Hertzsprung was investigating the characteristics of the stars in the Hyades open cluster. As all the Hyades stars are approximately the same distance away from the Earth-bound observer (a distance now known to be about 46 pc) there is a constant difference between their apparent magnitudes and absolute magnitudes (this being $m - M = 3.3$). If Hertzsprung were one of today's university students he would have lost marks for not labelling the axes of his graph, and I have taken the liberty of adding these.

In Figure 2, the ordinate is colour index, i.e. the apparent photographic magnitude of the star minus the apparent visual magnitude. Hertzsprung used the Draper Catalogue G-band magnitude for the former (this being obtained using photographic telescopes equipped with objective prisms and blue filters isolating the 0.4215 – 0.4325 μ region around the CH line; see Hearnshaw, 1986: 372) and the Harvard Photometry for the later (see, for example, Pickering (1913) and Hearnshaw, 1996: 91). Approximately main-sequence stars of spectral class A0, F0, G0, K0 and M0 have colour indices of −0.05, +0.3, +0.6, +0.8 and +1.4. For comparison with a modern Hertzsprung-Russell diagram, Figure 2 needs to be rotated clockwise through 90°.

The upper abscissa is the stellar apparent photographic magnitude. The full dots represent stars that, at the time, were thought to be members of the Hyades, and the open circles are stars in the same region of the sky, so some of these are Hyades members and some are not. Hertzsprung's brightest star in Figure 2, $m_{pg} = 4.2$ mag, is probably θ^2 Tau ($m_V = 3.4$).

The lower abscissa in Figure 2 is the first known visual representation of the absolute magnitude. As a 'standard distance', Hertzsprung has used a standard parallax of 1 arcsec (a distance later known as 1 pc). His absolute magnitudes have values that are thus five less than the ones used today, with the today's accepted 'standard distance' of 10 pc.

Hertzsprung (1911) also produced a similar diagram for about 62 stars in the Pleiades, but as this diagram had no absolute magnitude numbers on its abscissa axis it is of less importance in the context of this paper. The Pleiades was investigated in a very similar way by Rosenberg (1911).

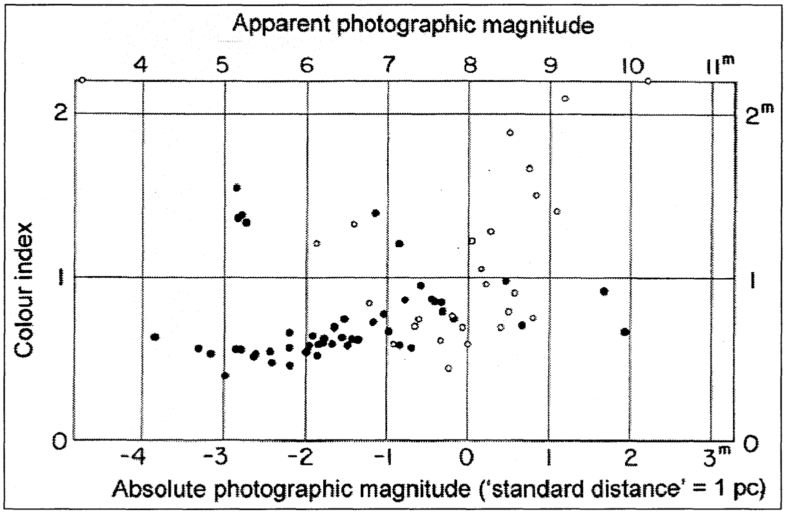

Figure 2: This redrafted original 1911 'Hertzsprung' diagram has been taken from his first illustrated paper on the relationship between stellar surface colour and luminosity. The full dots represent stars that, at the time, were thought to be members of the Hyades open cluster, and the open circles are stars in the same region of the sky. As all the Hyades cluster stars are assumed to be the same distance away from the observer, there is a constant numerical difference between apparent magnitude and absolute magnitude.

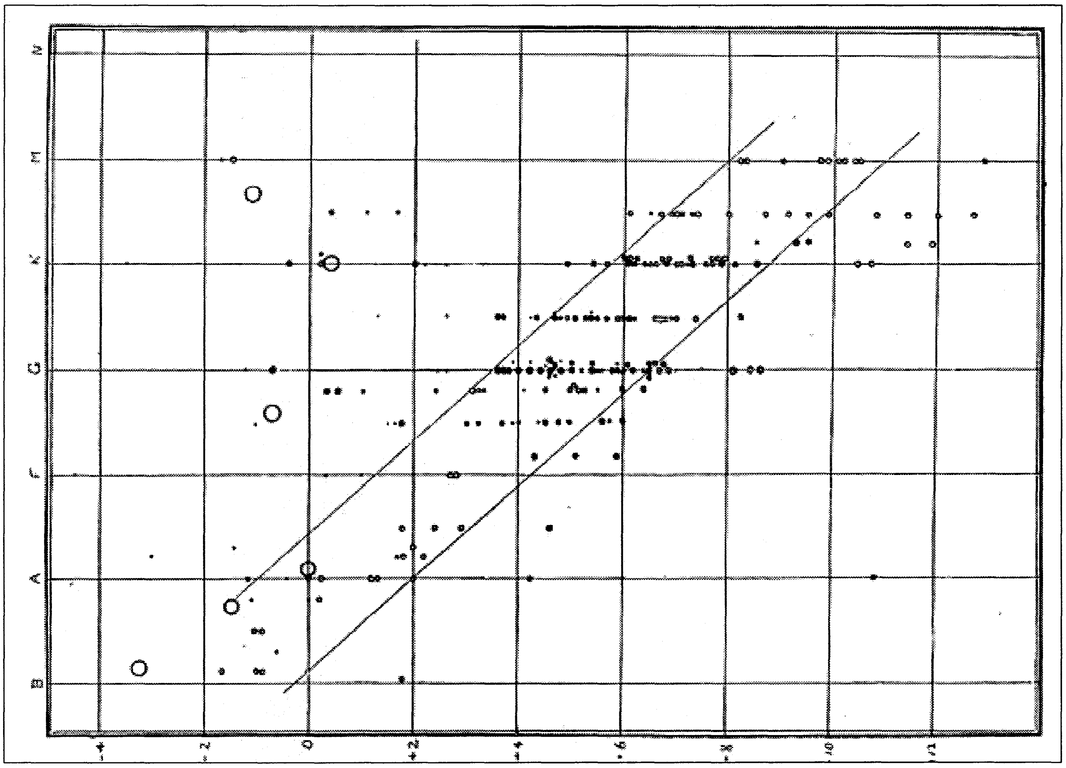

Figure 3: The original 'Russell' diagram (see Russell, 1914a and 1914b). The abscissa shows seven stellar spectral types and the ordinate is the absolute magnitude ("according to Kapteyn's definition", with a standard distance of 10 pc, corresponding to a parallax of 0.1″), the range being $-5 < M < 14$. Four types of data points have been used for the 220 stars represented. The filled circles are for stars which have had their parallaxes measured at least twice. Small filled circles indicate an absolute magnitude error of greater than ± 1.0, and the large filled circles are for stars with an absolute magnitude error of less than ± 1.0. The small open circles are for stars with single parallax determinations. The large open circles at the top of the diagram represent mean values for collections of stars (about 120 altogether) with small proper motions and parallaxes which hardly exceed their probable errors.

The two diagonal lines delineate the main sequence of 'dwarf' stars. The lone point in the bottom left portion of the diagram was regarded at the time as being very strange (the observation of its spectrum was hindered by the proximity of a bright primary). This star, Omicron Eridani B, was later found to be a white dwarf.

Finally, note that the original version of this diagram has been rotated through ninety degrees so that it has the same orientation as the 'Hertzsprung' diagram in Figure 2, above.

The original 'Russell' diagram, Figure 3, first saw the light of day in the spring of 1913, in London, at the 13 June meeting of the Royal Astronomical Society. The Princeton University astronomer, Henry Norris Russell (1877–1957), was giving a lecture on *"Giant" and "Dwarf" Stars* during a short stop-over on his journey, with a small group of American astronomers, to the summer meeting of the International Solar Union, in Bonn, Germany. The slide that Russell showed illustrated the physical characteristics of some 220 stars with known parallaxes. This graph, to quote Russell (1913: 324), plotted "... the relation between the spectral types of the stars and their real brightness."

Russell's RAS lecture was subsequently published twice as a research paper (see Russell, 1914a and 1914b), in *Nature* and *Popular Astronomy*, both papers being identical. Fortunately in the written version Russell had changed the expression 'real brightness' into the much more acceptable (and longer lasting) 'absolute magnitude'. In these papers the 'Russell' diagram was plotted as a figure in which

> ... the spectral class appears as the horizontal coordinate, while the vertical one is the absolute magnitude, according to Kapteyn's definition, – that is, the visual magnitude which each star would appear to have if it should be brought up to a standard distance corresponding to a parallax of 0″.1

Russell had no idea at the time he drew this historic and iconic diagram, that Hertzsprung had essentially 'pipped him to the post' three years previously. In passing, David Leverington (1995: 131) notes that Russell's graph was first given its present appellation 'Hertzsprung-Russell diagram' in a paper by Strömgren (1933), this being the written version of a lecture Strömgren gave at a meeting of the Astronomische Gesellschaft in Göttingen.[4] Actually the laurels for the appellation introduction should go to the Swiss-American astronomer, Robert Julius Trumpler (1886–1956) who, like Hertzsprung, was interested in the colour-magnitude diagrams of open clusters. Writing about the Wild Duck cluster in Scutum (M11) Trumpler (1924: 13) referred to the "... well known Russell diagram of giant and dwarf stars ..." A year later Trumpler (1925) reviewed the brightness and spectral characteristics of 52 clusters. In his 1925 paper he rather arbitrarily and alternatively used the expressions 'magnitude-spectral class diagram' and 'Hertzsprung-Russell diagram'. Trumpler (1925: 311) never gave any indication that he was pioneering the use of the later title.

The expression 'Hertzsprung-Russell diagram' entered into common usage after Chandrasekhar (1939) published his book, *An Introduction to Stellar Structure*. For more details about the history of the Hertzsprung-Russell diagram refer to DeVorkin (1977; 1978; 1984), Gingerich (1982) Nielsen (1963) and Sitterly (1970).

3 J.C. KAPTEYN AND ABSOLUTE MAGNITUDE

The Kapteyn mentioned above by Russell was the famous Dutch astronomer Jacobus Cornelius Kapteyn (1851–1922) who, after studying mathematics and physics at the University of Utrecht had become the Professor of Astronomy and Theoretical Mechanics at the University of Groningen (see Hertzsprung-Kapteyn, 1993). He remained at Groningen until his retirement in 1921. Kapteyn was interested in the proper motion of stars and their distribution in the vicinity of the Sun. To help in this investigation, Kapteyn and H.A. Weersma (1910) published a list of stellar parallax determinations. Much care was taken with error evaluation and the assessment of the accuracy of the different values obtained by different observers. It was clear that this list was one of the major foundation stones of the subsequent work by H.N. Russell. Each star was catalogued according to the normal characteristics, such as name, position, spectral type, apparent magnitude, proper motion and so on. What is important in the context of the present paper is the fact that the final two columns of the catalogue table (columns seventeen and eighteen) contained the stellar absolute magnitude and luminosity. To quote Kapteyn and Weersma (1910):

> The seventeenth column gives the absolute magnitude (= apparent magnitude at a distance corresponding to parallax 0″.1), the eighteenth gives the luminosities (unit = luminosity of the sun). These quantities have been computed by means of the formulae (see Gron. Publ. 11, page 12):
>
> Abs. mag = appar. mag + 5 + 5 log π .
> Log Lum. = 0.200 – 0.4 app. mag - 2 log π .
>
> These quantities have not been computed in the case, that the parallax is + 0″.030 or smaller. It is considered that no reliable values can be obtained in these cases.

It is clear that Kapteyn found the absolute magnitude a very interesting and useful concept. In 1910 he discussed the fact that stars of different spectral classes have different values of average absolute magnitude (Kapteyn, 1910). This, needless to say, is the basis of the main sequence of the early Russell H-R diagram where it can be seen that, for example, stars of spectral class B0, A0, F0, G0, K0 and M0 have average absolute magnitudes of about −2.0, 0.0, 2.8, 5.2, 6.8 and 10.2 respectively.

But let us go back to the earlier paper mentioned in the Kapteyn quotation, i.e. *Publications of the Astronomical Laboratory at Groningen*, No. 11. In this 1902 paper, (i.e. Kapteyn, 1902), we find the very first definition of the term absolute magnitude. Kapteyn introduces the concept in terms of stellar luminosity, and adopts as the unit of luminosity the total luminosity of the Sun. Equation (11) in Kapteyn (1902) is

$$\text{Log}\, L = 0.2000 - 0.4\, m - 2 \log \pi, \tag{3}$$

where L is the stellar luminosity of a star of apparent magnitude m and parallax π. Kapteyn (1902: 12) writes

> We further define the *absolute magnitude* (*M*) of a star, of which the parallax is π and the distance r, as the apparent magnitude which that star would have if it was transferred to a distance from the sun corresponding to a parallax of 0.1″. It is easily seen that
> $M = m - 5 \log r + 5 = m + 5 \log \pi + 5 = 5.5 - 2.5 \log L$ [his Equation 12]

For the Sun, L = 1; the formula thus gives for the absolute magnitude of the Sun M = 5.5, in accordance with what has been said above.

So the 'father and founder' of the absolute magnitude system is the great Dutch astronomer Jacobus

Cornelius Kapteyn, and the concept was first introduced in 1902.

4 CONCLUSIONS

Even though Kapteyn defined absolute magnitude for the first time, in 1902, and chose the standard parallax of 0″.1 (i.e. a distance later referred to as 10 pc), its universal acceptance owes much to the work of Hertzsprung. To quote Waterfield (1938: 133):

> The importance of the conception of the real or "absolute brightness" of stars was first urged by Professor Hertzsprung, the great Danish astronomer … By absolute brightness we mean that brightness a star would have if placed at a certain standard distance from us.

Historically, the role of Hertzsprung has been somewhat confused. Some (e.g. Abbott, 1984: 72) have suggested that Hertzsprung actually pioneered the usage of absolute magnitude in 1905. This is not so, as Kapteyn preceded him by three years. Also, when Hertzsprung used absolute magnitudes he had a standard parallax of 1 sec arc, and not the 0.1 sec arc suggested by Kapteyn in 1902.

In the first two decades of the use of the term absolute magnitude, this being the period 1902–1922, the choice of standard distance was left to the individual. This clearly presented ample opportunity for confusion. Things were regularised in 1922 at the first meeting of the General Assembly of the International Astronomical Union in Rome. The Commission des Notations, des Unités et de l'Économie des Publications accepted an American suggestion: Quoting from Volume 1 of the *Transactions of the International Astronomical Union* (see Fowler, 1922: 23):

> UNITÉS.
>
> En ce qui concerne les unites on pourrait adopter les propositions du comité américain (Report on the organisation of the International Astronomical Union, *Proceedings of the National Academy of Sciences*, 6, 1920, p. 360 …
> (b) *Magnitude absolue*. Magnitude d'une étoile, ramenée à la distance de 10 parsecs. (Fowler, 1922: 23).

By 1922 the word parsec was in common usage, and everyone had adopted the same standard distance for the absolute magnitude.

5 NOTES

1. Russell worked with Hinks as a Carnegie Institution funded research assistant when he was at King's College, Cambridge, during 1902–1905 (see DeVorkin, 2000: 54).
2. Moving to the present, on 1 July 2005, Henry et al. (2005) recorded that there were 48 stellar systems inside a sphere of radius 5 pc. Five were triple stars, 11 were doubles and 32 were single stars, making 69 stars in all.
3. The Swedish astronomer, Carl Vilhelm Ludwig Charlier (1862–1934) was the Director of the Lund Observatory.
4. Note, however, that in a rather Germanic fashion Strömgren hyphenates the whole expression trying to make Hertzprung-Russell-diagram one word.

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EARLY PHOTOGRAPHS OF THE DISTANT SIERRA NEVADA MOUNTAINS TAKEN FROM LICK OBSERVATORY

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Abstract: During World War I, a group of American chemists, physicists and astronomers developed processes for greatly increasing the infrared sensitivity of photographic emulsions, for long-distance reconnaissance from airplanes or the ground. After the war Lick Observatory astronomers, beginning with C.D. Shane and Mary Lea Heger, used long-focal-length astronomical cameras and these hypersensitization methods to photograph the distant Sierra Nevada range, including Yosemite Valley and Half Dome, nearly one hundred miles away across the Central Valley of California. These pictures, widely exhibited and admired, strengthened links between astronomers, the Eastman Kodak Company and the public.

Keywords: Lick Observatory, infrared photography, J. Fred Chappell, C.D. Shane, Mary Lea Heger Shane, Sierra Nevada Mountains, W.H. Wright

Figure 1: Panoramic photograph of the Sierra Nevada Mountains taken in December 1931 by Lick Observatory photographer, J. Fred Chappell. This photograph is prominently displayed at the Lick Observatory (Lick Observatory photograph).

Public visitors to Lick Observatory on Mount Hamilton, California, see daily a large, striking photograph displayed there of the snow-covered Sierra Nevada mountain range, over 100 miles (160 km) away (see Figure 1, above). It was taken on 16 December 1931, by J. Fred Chappell (Figure 2), long-time Lick Observatory photographer, using a special long-focus lens, an infrared filter, and a glass astronomical photographic plate hypersensitized for infrared 'light'. Very probably, the camera he used was built around a 2.25-inch (6-cm) diameter Voigtlander lens (stopped down), with 60-inch (1.5-m) focal length, that had been used to photograph the Sun and its corona on Lick Observatory eclipse expeditions, and for other special purposes (Chappell, 1933). This photograph was the culmination of a series of experiments going back to 1920 at Lick Observatory, where the distant Sierra Nevada mountains can be seen several times each winter, especially after storms which bring heavy rain to the Central Valley, washing out the haze and dust in the atmosphere for a few days, and depositing snow on the mountain peaks beyond.

Keivin Burns, a former Lick graduate student and later postdoctoral fellow, who was a research physicist at the National Bureau of Standards from 1913 to 1919, had brought the process used to take all these successive photographs to Mount Hamilton in 1919. During World War I he had worked with other physicists, chemists and astronomers in a group which developed the process for long-range photography. Infrared radiation penetrates haze and dust-laden air much farther than ordinary blue light, to which the early photographic emulsions then in use were most sensitive. In addition, infrared radiation is considerably less affected by the twinkling, blurring or 'poor seeing' that result from atmospheric turbulence. The scientists working on this project had developed several dicyanin-type dyes and a method for using them to hypersensitize photographic plates to infrared radiation. Burns brought samples of the best hypersensitizing chemical they had found, kryptocyanin, to Mount Hamilton to use in his post-war research program of obtaining infrared spectrograms of the Sun (Burns, 1920). He described these concepts to the astronomers and graduate students there, and showed them how to use the dye to hypersensitize their photographic plates for the infrared (Shane, 1980).

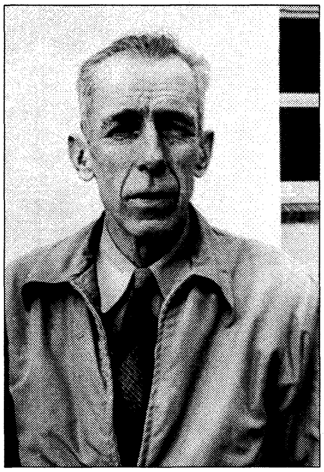

Figure 2: J. Fred Chappell, Lick Observatory photographer (Mary Lea Shane Archives of the Lick Observatory, UCSC Library).

C. Donald Shane and Mary Lea Heger, then both graduate students (but later both Ph.D.'s and husband and wife), took the first successful photographs of the Sierras from Mount Hamilton on 25 January 1920, using a camera variously described as "... of 20-inch focal-length ..." or "... a 21-inch [focal-length] Goetz lens." No doubt it was one of the many Lick Observatory photographic telescopes used on eclipse expeditions. Shane and Heger probably took their photograph from the flat area, now a parking lot, south of the large dome of the 36-inch (0.9-m) refractor. This is the site from which a photograph was taken in 1920, which was included in an early list of Sierra negatives in the Shane Archives. This photograph was published as the frontispiece of the February 1920 issue of the *Publications of the Astronomical Society of the Pacific* (Publications Committee, 1920). Shane also took several infrared photographs of individual snow-capped peaks in the Sierras, using the 12-inch (30-cm) Clark refractor as a long-focus camera, but they are not as spectacular as the panoramic pictures of the range, and their definition is only fair, because of the effects of seeing at this very great enlargement.

Figure 3: W.H. Wright, Lick Observatory astronomer and later Director (Mary Lea Shane Archives of the Lick Observatory, UCSC Library).

William H. Wright (Figure 3) of the Lick Observatory staff was an outstanding observational astronomer (and later Lick Director from 1935 to 1942), and he then decided to try his hand at this long-range terrestrial photography. He was an active member (and an honorary Vice-president for 18 years) of the Sierra Club, then a small, elite outdoor hiking, camping and mountaineering organization. After "... a considerable number ..." of trial exposures over a period of two years, Wright took his best picture of the Sierras from Mount Hamilton on 12 March 1922. He used a different camera, also with a 60-inch (1.5-m) focal length, a four-element Ross lens, one of a pair specially designed to have a wide field and made for the upcoming solar eclipse of 22 September 1922. W.W. Campbell and Robert J. Trumpler of the Lick staff took these two photographic telescopes and others to a site in remote Australia to obtain the direct photographs (at totality) which confirmed Albert Einstein's General Theory of Relativity and its prediction of the gravitational deflection of light by the Sun to high accuracy. Wright's Sierra photograph has better definition than the earlier one by Shane and Heger, and is also better pictorially, because it was taken from a different site, looking over more of the foreground ridges of the Mount Hamilton range, which block part of the view of the Sierras in the earlier photograph. We have no record of just where on Mount Hamilton Wright set up his camera for this picture, but a partial list of sites he tried includes the saddle near Galileo Peak, and another through an open window on the second floor of the 'old' dormitory just below the 36-inch (0.9-m) dome. Wright (1923) published a section of his photograph in an article in the *Sierra Club Bulletin*, in which he stated that the earlier photograph had been taken by Shane and Heger. Carl A. Bergmann, the Lick Observatory photographer at that time, prepared the print of Wright's photograph for it. Bergmann, a part-time employee, operated a photography studio in the San Francisco Bay area, but came up to Mount Hamilton for a week's work whenever a sufficient backlog of orders had built up for him, or when an important set of illustrations was needed for a scientific publication.

These spectacular photographs of the snow-covered mountains and the Yosemite Valley, with the romantic names that Bret Harte, Mark Twain, John Muir and Theodore Roosevelt had made famous in their writings, excited admiration and respect, particularly at the Eastman Kodak Co. in Rochester, New York. Campbell (1921), the Lick Director, had sent negatives of Shane and Heger's best photographs to Frank E. Ross, the astronomer trained at Berkeley and Lick, who was then working as a research physicist at Kodak. Ross passed them on to the heads of the Laboratory. Soon a Kodak official suggested to Campbell that these photographs be entered in an exhibition of the Royal Photographic Society in England (Newton, 1922). Campbell (1922) replied that Wright's photographs, very recently taken, were even better, and should be used. The Kodak Laboratory technicians produced an excellent panorama print, which received a medal at the exhibition (Newton, 1924; Wright, 1924a).

This episode brought Wright into close personal contact with C.E. Kenneth Mees, the founder and Director of the Kodak Research Laboratory, and with George Eastman himself, the founder and President of the company. Both congratulated him and promised to help him with their best new emulsions for his projected series of photographs of Mars (Eastman, 1924; Mees, 1924). Wright and Ross, who left Kodak in 1924 for a faculty position at Yerkes Observatory, became the pioneers in this newly-available field of monochromatic photography of planets with filters and various orthochromatic, panchromatic and infrared plates (Wright, 1924b).

Wright turned the negatives that Kodak had made for him and their copyrights over to the Sierra Club, which then ordered several prints for him and one for their own use (Webber, 1924). This was probably the source for the two additional photographs published in the *Sierra Club Bulletin* in 1925, credited to Wright (1925). They are in a long fold-out, one above the other, and are even more professional looking. There is no text or information about the exposure, camera, etc., but they are the best of his published photographs.

By 1925 Campbell, who had become President of the University of California but retained the Lick Directorship though he lived in Berkeley, and Robert G. Aitken, his Assistant Director in immediate charge on the mountain, decided they needed a full-time photographer on Mount Hamilton. Chappell, who had been working in a studio in La Crosse, Wisconsin, since 1919, got the position and came west (Aitken, 1925; Chappell, 1925a, 1925b). He was an excellent photographer and photographic technician, and was highly interested in astronomy and in advancing the photographic techniques then so important to it. After Chappell had proved his skills and abilities, Wright encouraged him to try to take an even better photograph of the Sierras. Chappell took several pictures over a period of years before he got the 'best' one (still sold today) from Copernicus, the peak where the Mount Hamilton fire tower is located, just east of the 120-inch (3-m) telescope dome. Chappell worked long and hard on this picture, preparing a master negative with instructions for dodging it, and printing and assembling the sections of the very large print (100 inches = 2.5 m long) which now hangs in the Main Building on Mount Hamilton (Chappell, 1943).

Chappell continued his photographic work at Lick Observatory for thirty-one years, and is probably best known for his excellent direct photographs of the Moon, taken with the 36-inch (0.9-m) refractor under the supervision of Joseph H. Moore. Several of these pictures were included in a large-format folio of *Photographs from the Lick Observatory* (Moore, Mayall and Chappell, 1940). Chappell retired in 1956 and moved to San Jose with his wife Dorothy, who long survived him (Shane, 1956). She was the person who inspired young Bill Unruh, now W.J. Shiloh Unruh, to become a dedicated Lick buff and amateur historian.

Most recently, Professor Don Olson, the 'Celestial Sleuth' of Texas State University, informed Lick Observatory that he, a colleague, and a group of their students, had studied the Chappell photograph and had noted (among other minor errors) two incorrect identifications of peaks on the old key to it (Olson, 2005). Robert Gargett, of the Lick Publications Office, then carefully studied all the old identifications, found a few more misidentifications, and updated all the elevations to reflect the values on newer maps. All have now been corrected on the prints and on the Lick Observatory web page. Olson et al. (2005) published a good reproduction of the Chappell Sierra photograph in a longer article, primarily on the Yosemite photographs of Ansel Adams.

I am most grateful to Dorothy Schaumberg, Curator of the Mary Lea Shane Archives of the Lick Observatory, University of California Santa Cruz Library, and to Arnold Klemola, Remington P.S. Stone, Anthony A. Misch, Robin Witmore and Robert Gargett of the UCO/Lick Observatory staff for helping run down the facts of these Sierra photographs in the Shane Archives. We are all especially indebted to the first two named for saving several original negatives, especially Chappell's negatives, from being discarded and lost.

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The following abbreviation is used:

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Don Osterbrock is a Professor Emeritus of Astronomy and Astrophysics at Lick Observatory of the University of California Santa Cruz. He was Director of the Observatory from 1973 to 1981, and after that remained an active research worker until he formally retired at the end of 1992. He specialized mostly in observational research on gaseous nebulae in our Galaxy, and in other nearby ones and in active galactic nuclei, after his theoretical Ph.D. work on gravitational interactions between stars and giant molecular clouds and his postdoc work on the internal structure of red dwarf stars.

He wrote and published the research monograph and graduate-level textbook *Astrophysics of Gaseous Nebulae* (1974), and the later, updated and expanded *Astrophysics of Gaseous Nebulae and Active Galactic Nuclei* (1989). Most recently, he and Gary J. Ferland wrote the still further updated and greatly expanded *Second Edition* (2006) of the latter book.

In his later years at the Lick Observatory Osterbrock became interested in the history of astronomy in the 'big-telescope era' in the United States, beginning about 1888, and has written and published five books on various topics in that subject. One of them, with John Gustafson and Shiloh Unruh as co-authors, is *Eye on the Sky: Lick Observatory's First Century* (1988), and his most recent one is *Walter Baade: A Life in Astrophysics* (2001). He has also written more than fifty historical papers or articles on many subjects in this same era of big-telescope research.

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THE WISCONSIN EXPERIMENT PACKAGE (WEP) ABOARD THE ORBITING ASTRONOMICAL OBSERVATORY (OAO-2)

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Abstract: On 7 December 1968, NASA's Orbiting Astronomical Observatory (OAO-2) was launched into space. Roughly ten years in development, the OAO carried two sets of experiments, each designed to conduct the first extended observations of the sky at ultraviolet wavelengths. One experiment package was designed by the University of Wisconsin; the other by the Smithsonian Astrophysical Observatory.

Remote operation of the OAO, especially the WEP's narrow-field photometric instruments, demanded a complex stabilization and control system that could point the spacecraft towards any desired object with an accuracy of better than one arc-minute. A host of other calculations were performed to ensure that the instruments were never pointed within a fixed number of degrees of the Sun, Moon, or even the Earth.

During its 50 months of operation, WEP successfully observed more than a thousand celestial objects. It was the first true stellar space observatory, whose operations represented a greater technological leap forward in its day than did the Hubble Space Telescope (HST), launched in 1990.

At the same time, OAO-2 marked a significant turning point in the way astrophysical research was conducted. OAO scientists' dependence upon high-speed, digital techniques of data acquisition, storage, transmission, and reduction, not only presaged but also influenced the universal adoption of such techniques throughout the astronomical community. The OAO spacecraft was a significant bellwether of the transition to an era of digital data manipulation that occurred well *before* the impact of the personal computer and the charge-coupled device (CCD).

Keywords: Orbiting Astronomical Observatory, Wisconsin Experiment Package, Space Astronomy Laboratory, University of Wisconsin-Madison, Arthur D. Code, ultraviolet astronomy

1 INTRODUCTION

Creation of the Space Science Board (SSB) of the National Academy of Sciences (NAS) in 1958 proved a decisive step towards opening the space frontier for future astronomical as well as astronautical exploration. The SSB's first meeting (27 June) was held before the International Geophysical Year (or IGY, stretching from 1 July 1957 to 31 December 1958) had officially ended. Geophysicist Lloyd V. Berkner (1905–1967), formerly of the Department of Terrestrial Magnetism at the Carnegie Institution of Washington, was elected Chairman of its fifteen-member panel. Berkner's appointment to the SSB stemmed from the guiding roles he had played in creation of the IGY itself, where he served as President of the International Council of Scientific Unions (ICSU) and Vice-President of the Comité Spécial de l'Année Géophysique Internationale (CSAGI), which coordinated the scientific efforts of more than sixty nations. Historian Allan A. Needell has demonstrated Berkner's central role behind the 1954 decisions by the International Union of Geodesy and Geophysics (IUGG), and the CSAGI, to request the launch of one or more scientific satellites (Needell, 2000: 325).[1] Berkner also served as IGY reporter for rockets and satellites, which put him in close touch with planned spaceflight developments, particularly the American Vanguard project (Bulkeley, 1991; Chapman, 1959; Green and Lomask, 1971; Sullivan, 1961; Wilson, 1961).

One of the principal motivations that led to the SSB's creation was its anticipated advisory capacity to "… provide help and advice … [to the] possible new civilian space agency ..." (the future National Aeronautics and Space Administration, or NASA) then being established by the U.S. Congress (SSB, 1958a: 3), whose transformation from the prior National Advisory Committee on Aeronautics, or NACA, became official on 1 October 1958 (McDougall, 1985).[2] But over the next two years, tensions frequently arose between NASA and the SSB over matters of authority in establishing future directions of the emerging space science program. By 1960, however, NASA was well-positioned "… to direct American achievements in space during the ensuing decade." (Hetherington, 1975: 107; Newell, 1980, 205-214).

After the first SSB meeting, Berkner sent telegrams (on 3 July) to a number of prominent U.S. astronomers, asking for their suggestions regarding future satellite experiments. Among those receiving these requests was Arthur D. Code (b. 1923), newly-appointed Director of the Washburn Observatory at the University of Wisconsin-Madison.[3] This was an opportunity for which Code was well prepared, and in which he would distinguish himself and his institution in coming decades.

Code, a native of Brooklyn, New York, entered the University of Chicago in 1940, but his education was interrupted by the Second World War. Widely experienced in radio communications and their associated technologies, Code enlisted in the U.S. Navy, where he received advanced training in electronics in the Chicago area before being stationed as an instructor at the Naval Research Laboratory in Washington, D.C. During the War, he gained extensive practical experience with the design and construction of technical equipment that served him well in years ahead. He also took coursework in physics at George Washington University, receiving instruction from Russian émigré physicist George Gamow (Code, 1982). Though he never received a formal bachelor's degree, Code was admitted to Chicago's graduate program in 1945. At the Yerkes Observatory, he design-ed and constructed an improved amplifier for the tracings of coude spectrograms obtained at McDonald Observatory (Osterbrock, 2003). In 1950, he was awarded his Ph.D. for a theoretical study of radiative transfer in O- and B-type stars, directed by Subrahmanyan Chandrasekhar (Code, 1950). Following a one-year appointment at the University of Virginia, Code was hired onto the faculty of the Department of Astronomy at the University of Wisconsin-Madison (1951-1956), where he specialized in photoelectric photometry. There, he and Albert E. Whitford (1905–2002) built a one-channel scanning spectrophotometer that was used at the Mount Wilson Observatory. In 1956, however, Code was hired away by Caltech's Department of Astronomy, then chaired by Jesse Greenstein (1909–2002).

In the autumn of 1957, Code and Caltech colleague Donald E. Osterbrock (b. 1924) both observed the passage of *Sputnik I* over Pasadena—an event that left a lasting impression upon the former. As Code later related to Osterbrock, the sight of *Sputnik* brought to him the realization that humanity would at last be able to measure the ultraviolet fluxes of stars (and other celestial objects) from above the Earth's atmosphere. Thereupon, Code privately resolved that he would attempt to conduct those measurements from an orbiting spacecraft (Osterbrock, 2003).

In his 1960 chapter on "Stellar Energy Distribution", published within the volume *Stellar Atmospheres* (edited by Greenstein), Code reiterated the problem that had always thwarted ground-based astronomical observations, namely, that the atmosphere's opacity to short-wavelength radiation represented "... the greatest single limitation on our knowledge of the spectral energy distribution of stars." (Code, 1960a: 50). In that same year, he published a cogent essay, "Stellar Astronomy from a Space Vehicle", that presented a host of critical observations that could be made from beyond the atmosphere, and likewise offered the prediction, strongly borne out in coming decades, that "... astrophysical investigations [conducted] throughout the entire electromagnetic spectrum, ... cannot fail to have a tremendous impact on the future course of stellar astronomy." (Code, 1960b: 278).

2 OPPOSITION TO SPACE ASTRONOMY

Yet, among many senior members of the American astronomical community, notable opposition arose to the growth of Federal support being awarded to the advent of space astronomy. Although difficult to understand today, this attitude was especially prevalent among the more elite institutions and personnel of west-coast observatories. Cost considerations stood among the principal sources of contention; some tens of smaller, ground-based telescopes could be built, it was argued, for the cost of a single, large space telescope (Smith, 1989: 44-48).

In the immediate post-war period, only a handful of American astronomers foresaw the potentials of spaceflight upon their discipline and pursued an active research interest (slanted toward measurements of the solar ultraviolet spectrum) through the launch of specially-instrumented V-2 rockets at White Sands, New Mexico (DeVorkin, 1989; 1992). But growing awareness of the very limited returns in useable data, combined with the inherent risks of failure, significantly dampened the spirits of this group, and soon spread to colleagues elsewhere.[4] At the same time, completion of the 200-inch reflector on Palomar Mountain was eagerly anticipated, and was viewed as the primary tool with which significant new discoveries in the coming decade (and beyond) would likely be made. Despite the limitations of slower photographic plates and long exposure times necessary, such traditional observing techniques were well-understood and accepted among the community as defining what it meant to be an astronomer (McCray, 2004).[5]

Opposition to space astronomy also reflected a generational issue, especially among researchers who had established their institutional homes and careers before the Second World War. The creation and continued support of leading ground-based observatories still highlighted the pinnacles of pre-war astronomical patronage, wherein many of the discipline's leading discoveries about the nature of the Galaxy and the expansion of the Universe had been accomplished. As a result, elder practitioners of the discipline exhibited a reluctance to support expenditures for newer types of research that strayed away from traditional problems that were to be investigated through continued usage of large ground-based optical telescopes.

An example of this type of thinking is displayed in the 1960 response of Mount Wilson and Palomar Observatory astronomer Horace W. Babcock to a request for ideas regarding possible development and launch of astronomical satellites, posed to him by University of Michigan astronomer Leo Goldberg (1913–1987), then Chairman of the ad-hoc Committee on Optical and Radio Astronomy that reported to the Space Science Board. Strongly evident among Babcock's argument was the feeling that events of the Space Age were moving far too rapidly to permit a proper assessment of pending and future research needs and opportunities. He judged that "The greatly condensed time schedules and the 'crash' nature of many of the developments ..." would likely prove quite inefficient, and "... can hardly be justified ..." by standards of traditional astronomical research. Babcock lamented that "... the immense expenditures being made and planned for 'space research' ..." had arisen not from scientific motives, but instead seemed "... in large part the result of popular demands for [improved] national prestige ..." in the aftermath of *Sputnik* and other Soviet space initiatives. He objected that such funding decisions retained a strong political component, and questioned whether such initiatives would remain "... dependent on the varying trends of mass psychology." Nonetheless, and speaking more directly as a scientist, Babcock admitted that "... if large satellite vehicles are to be launched in the coming years, [then] astronomers have a distinct obligation to exploit the opportunities [presented] for the advancement of science." (Babcock, 1960). He did not seem to realize, however, that such a moment was already at hand.

3 CODE'S RETURN TO WISCONSIN

What happened next, with regard to Code's subsequent career trajectory, must be regarded as a mixture of fortunate timing, opportunity, and reward; but at the same time, reflected the somewhat unusual and potentially risk-filled decision to leave a tenured position at Caltech and return to the University of Wisconsin-Madison (effective 1 July 1958) as a full Professor and Director of the University's Washburn Observatory. This relocation had been made possible by the concurrent appointment of former Washburn Director, Whitford, to the Directorship of the Lick Observatory of the University of California (Osterbrock, 1976; 2004). As a strong incentive to bringing Code back to their institution, the University of Wisconsin administration expressed its commitment to the rapid enlargement of its astronomy department and the establishment of a full graduate-level program—steps previously recommended by Whitford. As evidence of that commitment, the Department of Astronomy (on 30 June 1958) dedicated its state-of-the-art 36-inch (0.9 m) reflector at the Pine Bluff Observatory located west of Madison, during the 100th Meeting of the American Astronomical Society. Superseding the historic 15.6-inch (0.4 m) Clark refractor of the original Washburn Observatory, the new reflector was optimized for research in photoelectric photometry and spectrophotometry (Bless and Lattis, 2000).

At the same time, it was no secret that Code's Caltech supervisor, Jesse Greenstein, had no fondness for the possibilities of spaceborne astronomical observations (McCray, 2004). Indeed, Greenstein continued to champion the superiority of ground-based optical telescopes, especially Caltech's 200-inch reflector, that made his facility the world's premiere astronomical research institution. The prestige of a Caltech appointment, and assurance of future research opportunities that it afforded, were no small matter for Code to walk away from. But the Wisconsin position offered him the autonomy and the opportunity to pursue research in space astronomy that would almost certainly be lacking, were he to remain at Caltech under Greenstein (Code, 1982). Those crucial factors undoubtedly cast the difference in favor of Code's return to the smaller Washburn Observatory, which nonetheless possessed a distinguished history of scientific and technical innovation in the development of photoelectric photometry, achieved under the tenures of Joel Stebbins (1878–1966) and Whitford (DeVorkin, 1985; Hearnshaw, 1996; Leibl and Fluke, 2004).

By October 1958 Code was invited by Berkner to become a member of the ad hoc SSB Committee on 'Optical and Radio Astronomy' chaired by Leo Goldberg, which first met at Ann Arbor, Michigan, on 6 October 1958 (Berkner, 1958; SSB, 1958b).[6] There, Code presented one of ten informal 'proposals' which described possible measurements of radiation densities (chiefly in the UV) that might be accomplished from a 100 pound satellite. Code was subsequently named a coordinator of such proposals in stellar astronomy (SSB, 1958b: 5, 6, 9). Along with other proposals made in areas of fundamental physics, solar, and radio astronomy, these were utilized by the Goldberg Sub-Committee in its drafting of a "... primer of astronomical research from space vehicles ..." that was to be delivered to the Space Science Board early in 1959 (Goldberg, 1958: 1).

Berkner's solicitations of suggestions for satellite experiments netted roughly two hundred responses from the astronomical community (Berkner and Odishaw, 1961; Hetherington, 1975; Smith, 1989).[7] Yet, the number of core groups wishing to pursue these matters quickly dropped to four or five, once it was learned that more formal proposals would be requested. Those who were most seriously committed formed the basis of NASA's Space Science Working Group (SSWG), whose meetings commenced in February 1959 (DeVorkin, 2005). With a decided emphasis upon UV astronomy, SSWG members began to develop their proposals for the first 'Orbiting Astronomical Observatories'. The institutions, principal investigators, and names of the experiment packages eventually flown by these core groups are listed in Table 1.

Table 1. Original NASA SSWG

Institution	Principal Investigator	Instrument Package
Wisconsin	Arthur D. Code	WEP, on OAO-1 and OAO-2
SAO[a]	Fred L. Whipple	Celescope, on OAO-2
Goddard	Albert Boggess III	GEP[b] (OAO-3; launch failure)
Princeton	Lyman Spitzer, Jr.	PEP[c] (OAO-4)
Michigan/ Harvard	Leo Goldberg	OSOs[d]

[a] Smithsonian Astrophysical Observatory

[b] Goddard Experiment Package. This group began as the astronomical branch of the Naval Research Laboratory, Washington, D.C., but was moved to the Naval Ordnance Laboratory, Anacostia, Maryland (Code, 2003), and later became reorganized as the Goddard Space Flight Center (GSFC).

[c] Princeton Experiment Package. Renamed Copernicus.

[d] Orbiting Solar Observatories. Partly due to the radically different thermal requirements of the OSOs, solar astronomers removed themselves from the SSWG (Roman, 1972).

4 FOUNDING OF THE SPACE ASTRONOMY LABORATORY (1959)

In 1959, Code and then Assistant Professor Theodore E. Houck (1926–1974) established the Space Astronomy Laboratory (SAL) within the Department of Astronomy at the University of Wisconsin-Madison (Code, 1982). SAL was the first of several notable startups in Code's career, which came to include the OAO spacecraft and the Space Telescope Science Institute (Code, 1997). Houck had earned his Ph.D. in 1956 under Whitford (only the third Wisconsin doctorate awarded in astronomy) with a thesis on early-type stars (Houck, 1956). He was originally appointed an Instructor (1956-1959) and had chosen the site for the Department's new Pine Bluff Observatory.

Also joining SAL was Robert C. Bless (b. 1927), a recent Ph.D. from the University of Michigan, who had completed a thesis under William C. Liller on the photoelectric spectrophotometry of A-type stars (Bless, 1959). While still at Michigan, Bless and several other students, along with Leo Goldberg, had observed *Sputnik* in the fall of 1957, which whetted both men's appetites for space astronomy. Originally, Bless came to Wisconsin on a temporary two-year research appointment that was extended to three years (1958-

1961); he then became a permanent faculty member, achieving Assistant- (1961), Associate- (1963), and full Professorships (1969). The fourth original member of SAL was John F. McNall (1930–1978), who had earned bachelor's (1953), master's (1956), and doctoral degrees (1960) in electrical engineering (computer science) from the University of Wisconsin. Starting in 1960, McNall became a Project Associate with SAL and later was appointed an Assistant Professor of Computer Science (1963).

The Space Astronomy Laboratory was first housed in Sterling Hall on the Wisconsin campus, but with the reassignment of astronomy faculty (from offices in the overcrowded Washburn Observatory), SAL was moved to the basement of a house owned by the University, and then to a vacant warehouse on North Park Street in Madison, before acquiring a permanent home in Chamberlin Hall. Despite these somewhat adverse operating conditions, the four researchers nonetheless developed a camaraderie that possessed a number of advantages. In contrast to the larger team assembled by the Smithsonian Astrophysical Observatory (SAO), the smaller SAL team proceeded without much evidence of a hierarchy. The relative absence of bureaucratic 'red tape' (in those early days) contributed to a free flow of scientific ideas. Ironically, the loose-knit organization of SAL proved somewhat suspect to NASA officials, who urged them to be more hierarchical and to emulate the Smithsonian (Bless, 2003).

Well before the OAO satellites were constructed, SAL personnel (along with other SSWG members) requested financial support from NASA to fabricate and test several smaller, less sophisticated devices as a means of demonstrating their capabilities under near-spaceflight conditions (Smith, 1989: 40). In 1961, a weather balloon carrying a small UV-sensitive photometer (for measuring sky brightness) was launched from the Madison campus and recovered by an Illinois farmer. Prototypes of the photometer assemblies being prepared for the OAOs were flown by SAL scientists aboard Aerobee sounding rockets, starting in 1962. Additional instruments, including a camera, photometer, and spectrograph, were tested aboard several flights of NASA's X-15 rocket plane (Code, 1982; Bless and Lattis, 2000).

Code has described the initial excitement, the team spirit, and 'can-do' attitude that dominated the early days of SAL. Great satisfaction came from reliance upon the self, rather than an outside agency. Also characteristic of the startup was the newness, the curiosity, which accompanied the dawning Space Age. Awareness and opportunity of setting an important scientific precedent furnished additional rewards to the team. Finally, there was the attendant joy of communicating their findings to peers. All of these things Code has likened to cross-country skiing on a field of virgin snow (Code, 2003).

5 NASA'S ORBITING ASTRONOMICAL OBSERVATORY (OAO) SERIES

Due to launch vehicle capabilities extant at the end of the IGY, SSWG teams were initially restricted in the expected weights of their payloads to approximately 100 pounds. But from rapid developments in rocket booster technology, created in response to the coming era of manned spaceflight, that weight restriction was soon substantially raised. By the third meeting of Goldberg's Optical and Radio Astronomy Committee (on 26 February 1960), there was foreseen a "... jump in payload from about 150 lbs. to about 5000 lbs. in the not too distant future." (SSB, 1960: 2). This anticipated outcome bore significant consequences for the creation and operation of much larger scientific payloads, and led NASA to attempt a much bolder step in spacecraft design than was first envisioned by the SSWG teams.

Seemingly the most important decision made regarding these future astronomical satellites was NASA's adoption of a 'modular' approach, or 'standardized platform', for containment of the individualized experiment packages. As Nancy G. Roman, former Chief of Astronomy and Relativity Programs at NASA from 1959 to 1979, has explained, "Because of the common pointing requirements, it was decided early on that a standard spacecraft design would serve each experiment ..." with only minor modifications expected for individual payloads (Roman, 2001: 523).[8] At the first meeting of SSWG members in February 1959, the notion of a standardized platform was announced as being applicable to a wide variety of scientific payloads (DeVorkin, 2005: 245). Not all SSWG members, however, were enthusiastic about NASA's mission-oriented approach, which seemingly threatened their individual autonomies.

The standardized platform that NASA envisioned was to consist of a single, ten-foot-long tube that was approximately forty inches in diameter. Outside of this framework would be attached the arrays of solar panels and other instruments. Thus, for the smaller experiment packages (those designed by the University of Wisconsin and the Smithsonian Astrophysical Observatory), it was recognized that the expansive payload container would "... require the efficient combination into one large package of compatible experiments." (SSB, 1960: 2). But how was this to be accomplished? When management officials at the GSFC first proposed to cluster the Wisconsin and Smithsonian packages within the same tube, objections were immediately raised. In its place, Code and Houck suggested that the two packages be separated and placed at opposite ends of the tube, allowing views in different directions. Reportedly, Goddard engineers at once declared such an idea to be impossible to execute from the operations standpoint. But only two weeks later, the idea of a double-ended spacecraft had become an accomplished fact (Code, 1982; Code, 2006). Such a turnaround in thinking evidently recognized that Code and Houck's proposal represented the *optimal* solution to the problem of combining the two experiment packages (Figure 1). Today, a full-scale engineering model of the OAO spacecraft is displayed at the University of Wisconsin's Space Place at 2300 South Park Street, Madison, Wisconsin.

NASA's OAO series comprised more than just a scientific showpiece; it was also a political tool selected to enhance the nation's prestige. Still smarting from the blow to U.S. pride felt in the wake of *Sputnik*, President Eisenhower welcomed the counsel of his Scientific Advisor, George B. Kistiakowski, who informed him about the "... potential gains in national prestige if we establish the first astro-observatory on a satellite ..." (quoted in Smith, 1989: 37; see also

Dickson, 2001; Divine, 1993; Killian, 1977; Launius, Logsdon, and Smith, 2000). Representing an enormous technical advance over previous IGY experiments, these instruments would become the largest, heaviest, and most sophisticated unmanned satellites yet launched. Through its demonstration that the U.S. had acquired both launch and scientific capabilities comparable to those of the rival Soviet Union, NASA hoped that the public's image of the American space program would itself be strongly boosted. Yet, by the time of their earliest successful launch (almost ten years later), the OAOs were largely overshadowed by the achievements and goals of the manned spacecraft program.

5.1 Challenges of Remotely Operating an Orbiting Telescope

Accompanying NASA's development of the OAO spacecraft as a full-fledged astronomical observatory in space were a number of critical operations capabilities that would determine its ultimate success or failure as a scientific instrument; most importantly, the ability to move efficiently from one target star to another. As GSFC scientist Paul B. Davenport wrote, the OAO demanded a "… complex stabilization and control system …" that could point the spacecraft toward any desired object "… with a high degree of accuracy …" and maintain that orientation for the duration of the observations (Davenport, 1963: 1). This overall task was envisioned by NASA as comprising at least four principal subtasks: (a) maximization of sunlight on the arrays of solar cells that provided the spacecraft's power; (b) generation of slewing commands that enabled the spacecraft's attitude (i.e., orientation) to be changed, usually by the most efficient route (unless dictated by other constraints); (c) determination and maintenance of the satellite's orientation by means of six gimbaled star trackers; and (d) calculating when an object would be observed near to, or occulted by, the Sun, Moon, and (most frequently) the Earth (Davenport, 1963: i; see also Jenkins, 1970; Lynn, 1970; Purcell, 1970).

To ensure the safety of its on-board equipment, the optical axis of the OAO could never be pointed within 45° of the Sun. Light reflecting from the Moon, and especially from the fully-illuminated Earth, could (and in one case did) cause serious damage to one of the SAO's UV-sensitive cameras (vidicons). Sunshades were designed to automatically close if the optical axis came too near the Sun's position. A further observing restraint was that imposed by the so-called South Atlantic Anomaly (SAA), a region over the South American continent and southern Atlantic Ocean where the Van Allen radiation belts descend relatively closely to the Earth's surface. Passage of the spacecraft through this region sometimes caused the production of secondary electrons that interfered with operation of its photomultiplier tubes. The necessary calculations concerning these restraints were first performed on the ground and then relayed in advance to the orbiting satellite. Such "… computer-assisted advanced planning [was] mandatory …" for conducting these operations efficiently (Purcell, 1970: 43).

The mathematical theory behind those subtasks of OAO operations was furnished by Davenport (1963). One fundamental coordinate system, centered on the Earth and defined by its equatorial plane, provided the basis for specifying the positions of all celestial objects, including the Sun, Moon, and stars, along with the spacecraft's location in its orbit. A second fundamental coordinate system was that centered upon the OAO itself, whose optical axis coincided with the positive x-axis, and around which the rotational motion known as 'roll' was defined. Two remaining orthogonal axes (y-axis and z-axis) corresponded to the rotational motions known as 'pitch' and 'yaw', respectively. The ever-changing relationships between these two coordinate systems, and the transformations that were necessary whenever the spacecraft was slewed from one target object to another, demanded solutions employing matrix multiplications (and a high number of logical decisions). Before additional constraints were imposed, as many as twenty-four possible solutions (though in practice only twelve principal solutions) existed for each desired slewing sequence. All spacecraft slews were restricted to angles of less than 30 degrees. This condition was imposed by the relatively limited fields of view of its six gimbaled star trackers (two for each axis). Determination of the gimbal angles for each star tracker was another of the tasks to be performed by the OAO operating system. Related calculations and logical decisions were necessary in repositioning the OAO's solar panels at the end of each slewing sequence. Unless the spacecraft were kept in observing mode on a round-the-clock basis, it would eventually lose its stability and pointing capability (Code, 1982). The OAO's best observing time usually occurred during its passage through the Earth's shadow, where it spent roughly 30 minutes out of each 100-minute orbit.

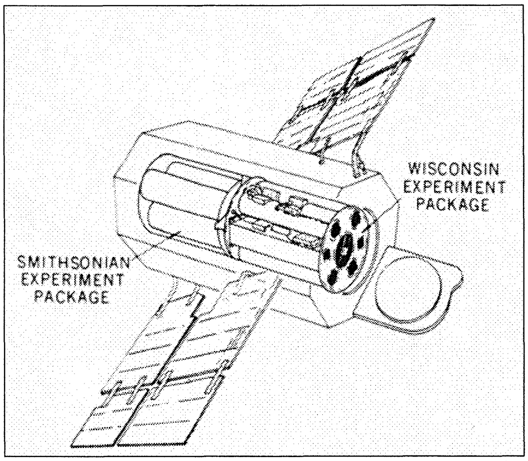

Figure 1: Phantom view of OAO-2 spacecraft, showing the Wisconsin and Smithsonian experiment packages. (Reproduced from *Publications of the Astronomical Society of the Pacific*, 81, p. 477 (1969), by permission of the author and the Editor, Astronomical Society of the Pacific).

To support those calculations used in operating the Wisconsin package, a computer program was written by University of Wisconsin-Madison graduate student Harry C. Heacox, Jr., and which earned him an M.S. degree from the Department of Astronomy (Heacox, 1970; Heacox and McNall, 1972). Heacox's FORTRAN program, dubbed HARUSPEX (Heacox's Answer to a Request for Unparalleled Superiority in Programmed EXperimentation), was run on the IBM

360/65 mainframe computer at the Goddard Space Flight Center. Heacox's principal advisor was SAL scientist John F. McNall.

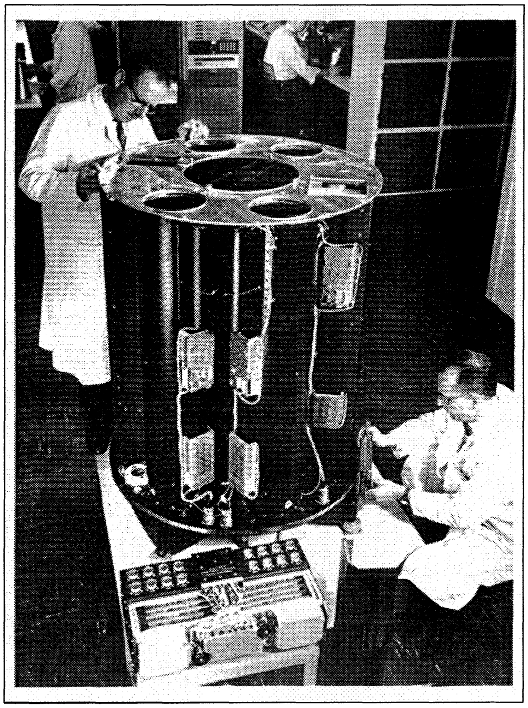

Figure 2: Wisconsin Experiment Package (WEP), undergoing assembly at Cook Technological Center. Courtesy, Space Astronomy Laboratory (SAL) Archives, University of Wisconsin-Madison.

Heacox described HARUSPEX as a "... large, complex computer program intended for use by personnel who are not computer-oriented." In attempting to maximize the "... ease, simplicity, and convenience of use ..." of the program by SAL personnel, both "Extra programming effort and some degree of machine inefficiency ... " were deliberately built into the programming structure. Wherever possible, the machine's output was presented in 'graphic form', which then meant using a line printer as a plotter, although tabular output was frequently employed. One of the principal tasks of HARUSPEX was the preparation of a tentative WEP observing schedule that might typically extend for a week. The proposed schedule was then subjected to more rigorous testing before its commands were folded into the OAO's own command-and-control system (overseen by the GSFC) and communicated to the spacecraft. In Heacox's judgment, HARUSPEX functioned like a "... very simple monitor system ..." in which the mainline program played the part of an executive, while 'major tasks' were accomplished "... by means of large subroutines." An 'extensive library' of sub-routines performed the necessary 'trigonometric manipulations' and other 'utility functions' (Heacox, 1970: 2, 3, 4).

HARUSPEX also provided an interface to the Wisconsin team's Ground Operating Equipment (GOE) station, structured around a smaller Digital Equipment Corporation (DEC) PDP-8 minicomputer containing 32K of storage and operated by teletype. This system routinely displayed the status of the Wisconsin equipment including filter positions, gains, voltages, and temperatures of all seven telescopes (described below). If an emergency arose, a command generator within the GOE could issue instructions directly to the OAO during an interval when the ground station was in contact with the spacecraft.

An important distinction, however, must be drawn between the SAO and WEP packages aboard the OAO-2. The SAO experiment package (termed Project Celescope) consisted of four UV television cameras used in conjunction with relatively wide-angle Schwarzschild reflecting telescopes. Its mission was to capture two-by-two degree UV images of celestial objects, whose photometric characteristics were later analyzed (Anonymous, 1962; Davis, 1972; Davis, et al., 1972; Rogerson, 1963; Watts, 1968). The SAO package required far less precision in the spacecraft's pointing system for capturing those images. By contrast, the WEP consisted of seven, narrow-angle reflecting telescopes that fed five filter photometers and two scanning spectrometers. To succeed, these required a much higher pointing capability—better than one arc minute accuracy—that necessitated a high-performance and -flexibility operating system that was furnished by HARUSPEX and Goddard's command-and-control system. No operating system of this sophistication had previously been developed for remotely operating an orbiting satellite.

For this reason, the OAO spacecraft represented a greater technological leap forward (in its day) than even the Hubble Space Telescope, when launched more than two decades later. The bulk of this accomplishment was centered around the remote operating systems that were necessary to point, slew, and control the spacecraft while in orbit. This small-scale astronomical observatory could neither achieve the same light gathering power nor angular resolution as the HST's larger 94-in. (2.4 m) primary mirror. But for the first time, it enabled extended observations to be made across the ultraviolet spectrum, for periods of time far in excess of those glimpsed from sounding rockets. Whether used for purposes of imagery (in the SAO/Celescope experiment), or to obtain photometric and spectrophotometric data (in the WEP), the OAO represented a milestone in the advent of space astronomy.

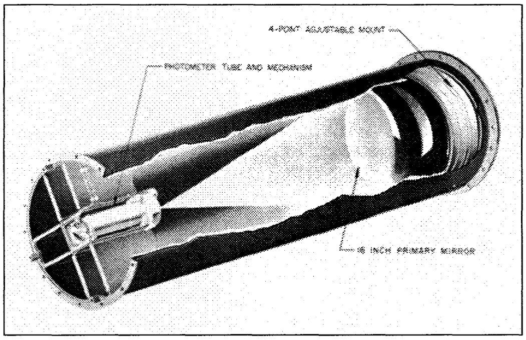

Figure 3: Sixteen-inch aperture nebular photometer, that occupies the central portion of the Wisconsin Experiment Package (WEP). Courtesy, Space Astronomy Laboratory (SAL) Archives, University of Wisconsin-Madison.

6 THE WISCONSIN EXPERIMENT PACKAGE (WEP)

The adoption of a standardized platform, around which the OAO spacecraft was constructed, led to other important decisions (plus a division of labor) within NASA and its various centers that became operational during the 1960s. The OAO spacecraft was managed by the Goddard Space Flight Center (GSFC), Greenbelt, Maryland. The Grumman Aircraft Engineering Corporation (later renamed the Grumman Aerospace Corporation), Bethpage, Long Island, New York, was chosen as prime contractor for the platform that housed the twin experiment packages. The launch vehicle, a two-stage Atlas rocket, was managed by NASA's Lewis Research Center, Cleveland, Ohio, though actual launch operations were conducted from the Kennedy Space Center, Cape Canaveral, Florida. Five data acquisition stations were united under the Satellite Tracking and Data Acquisition Network (GSFC, 1969).

Minus the twin scientific packages, the OAO spacecraft had a weight of roughly 3,400 pounds. An additional one thousand pounds was allotted to scientific experiments; each team's payload was thus restricted to half that amount. Although the spacecraft itself was operated on some 420 watts of power, the WEP was restricted to no more than thirteen watts! In addition, the OAO data storage units were extremely limited; maximum memory capacity was 4,096 25-bit words (in redundant mode), or twice that amount in non-redundant mode (ibid.).

Neither the Wisconsin nor the Smithsonian teams possessed in-house resources needed to design and construct the experiment packages themselves. Each solicited and accepted bids from outside professional contractors. But here again, substantial differences are apparent in the teams' organizational styles and resulting choice of contractors. Nine contractors submitted bids to construct the WEP; these included a joint proposal from the Grumman Corporation (builders of the OAO 'platform') and the Perkin-Elmer Corporation (future contractor of the optical system for the Hubble Space Telescope).[9] But the Wisconsin team was reluctant to place its investment into a large, bureaucratic organization that was located perhaps one or two thousand miles away. Instead, it selected the much smaller and nearer Cook Technological Center located at Morton Grove, Illinois (a division of the Cook Electric Company, Chicago) because, according to Bless, "... we thought we'd have more control ... and you could also drive down there in two hours." (Bless, 2003). The principal electronic engineer at Cook was Curtis B. Bendell. As a result, the Wisconsin team chose a smaller, regional company that more nearly matched its own, less-formal organizational style. While Cook designed and tested all of the electronic circuitry that was used in the WEP (Figure 2), actual construction of the electronics was performed by the SAL team at Madison (Code, 1982).

For the twin reasons of redundancy (to minimize potential losses in the event of equipment failure) and to maximize coverage over the broadest UV spectral ranges (without constructing a single, all-purpose instrument), the WEP consisted of seven co-aligned scientific instruments of three different types. Yet, all reflected the long tradition of photoelectric photometry conducted at the Washburn Observatory. The largest instrument, which occupied the central portion of the five foot-long cylinder, was a sixteen-inch aperture, f/2 reflecting telescope equipped with a photoelectric photometer at the prime focus (Figure 3). This 74-pound telescope was designed to examine nebulae and other extended objects over a wavelength range from 2000 to 3300 angstroms. Four different filters, each with a bandpass of roughly 300 angstroms, could be selected by ground command. Two diaphragms provided viewing fields of 30 or 10 arc minutes. Because none of the UV interference filters could then be obtained from commercial suppliers, they were all fabricated at SAL, according to techniques developed by Daniel J. Schroeder (b. 1933) and implemented by Timothy Fairchild (Code, 2006).

Surrounding the sixteen-inch reflector were four identical eight-inch aperture eccentric-pupil (off-axis) telescopes, each equipped with a photoelectric photometer at their f/4 prime focus (Figure 4). These 28-pound instruments, used for stellar observations, were equipped with four filters covering bandpasses between 1000 and 4250 angstroms. Having narrower fields of view (either 2 or 10 arc minutes), these telescopes were capable of observing sources as faint as visual magnitude 12 or 13. Similar photometers were employed on both the eight- and sixteen-inch telescopes, which by design of the amplification system (or gain), enabled a range in sensitivity of between four and six orders of magnitude. SAL scientists also undertook the absolute calibrations of all photometers, using the synchrotron radiation storage ring at the UW Physical Sciences Laboratory, Stoughton, Wisconsin (Code, 1982).

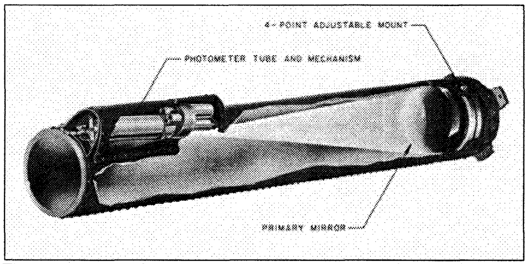

Figure 4: One of the four eight-inch aperture (off-axis) stellar photometers aboard the Wisconsin Experiment Package (WEP). Courtesy, Space Astronomy Laboratory (SAL) Archives, University of Wisconsin-Madison.

The final instruments aboard the WEP were a pair of nearly identical scanning spectrometers (Figure 5). These devices resembled the technique of objective-prism spectroscopy pioneered by Harvard College Observatory Director, Edward C. Pickering (1846–1919). Each spectrometer contained a six-by-eight-inch plane epoxy replica grating held on a pyrex base. Light from a star first struck this objective grating and was dispersed into its component colors. A parabolic mirror, with a 32-inch focus, reflected the light back through a small hole at the center of the grating and onto a slit at the entrance to the spectrometer. One of these devices examined the UV spectral range from 1000 to 2000 angstroms, while the other performed measurements across the 2000 to 4000 angstrom range. During the course of operation, a drive motor tilted the grating in order to scan the entire spectrum in 100 steps of 10 or 20 angstroms each, respectively. These instruments produced a highly accurate series of measurements of the source's intensity across its entire spectral range—the same result as if its UV spectrum had been recorded 'photographically' and later scanned to produce an intensity profile. More than any other OAO instruments, these scanning spectrometers (technically speaking, spectrophotometers) enabled Code and his colleagues to measure the UV fluxes of stars that he had first envisioned in the wake of *Sputnik* (Code, 1969; Code et al., 1969; Code et al., 1970; Code, 1972; Watts, 1964).

Nonetheless, inclusion of the scanning spectrometers on the WEP became an item of controversy. Earlier, the team responsible for constructing the larger Goddard Experiment Package (GEP), which was renamed OAO-3, intended to equip its own instrument, a 36-inch (0.9 m) UV telescope, with higher-resolution scanning spectrometers. The Wisconsin team had to convince NASA of the desirability of installing its lower-resolution spectrometers aboard the WEP, under the argument that they would provide a backup data collection system to the four eight-inch WEP filter photometers (Bless, personal communication). A final irony was realized, however, because the Goddard package suffered a complete failure when its protective shroud did not detach during the 30 November 1970 launch of OAO-3. Thus, data obtained by the WEP spectrometers was not superseded until the successful deployment of the Princeton Experiment Package (PEP) and its 32-inch (0.8-m) UV telescope as OAO-4, renamed Copernicus, launched 21 August 1972.

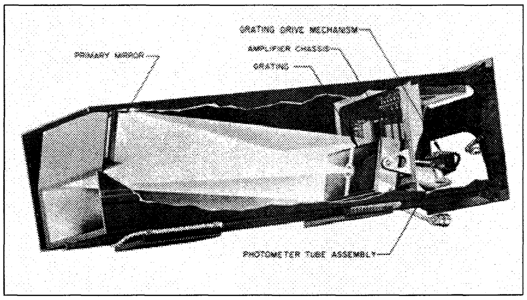

Figure 5: One of the two eight-inch aperture scanning spectrometers aboard the Wisconsin Experiment Package (WEP). Courtesy, Space Astronomy Laboratory (SAL) Archives, University of Wisconsin-Madison.

7 OAO FAILURE; THEN SUCCESS

By 1966, the WEP had passed all pre-flight inspections and was readied for launch; the SAO's Project Celescope, however, remained incomplete (from holdup of the vidicons). On account of this "... delay of the Whipple experiment ..." NASA was forced to come up with a "... hasty substitution for the SAO experiment." (Roman, 2001: 524). MIT physicists, William L. Kraushaar (b. 1920), who had previously worked on the OSO-3 and Explorer 11 satellites, along with George W. Clark (b. 1928), were appointed co-principal investigators behind a "... 50-100 MeV gamma ray experiment ...", while Lockheed Missiles and Space Systems physicist Philip C. Fisher (b. 1926) and GSFC physicist Kenneth J. Frost, were named principal investigators on "... two soft (2-150 KeV) X-ray experiments." (Leverington, 2000: 292; Code, personal communication). Kraushaar later joined the University of Wisconsin-Madison Physics Department.

As the launch date of the first (OAO-1) spacecraft drew near, members of the Wisconsin team assisted with pre-launch operations from the GSFC, but were forced to go on virtually no sleep, as the first five or six launch attempts were all scrubbed. Bless (2003) later recalled, "As soon as one launch [date] was canceled we had to start working on the next." At last, on 8 April 1966, the spacecraft was finally placed into orbit by an Atlas-Agena D launch vehicle. But major difficulties soon beset the spacecraft's power and guidance systems; one of its batteries may have exploded, and after two days, contact with the spacecraft was permanently lost. It was a devastating blow to the Wisconsin team, whose particular timing, Bless later realized, was marked by a perverse symbolic coincidence: "It turned out that OAO-1 ascended on Good Friday and died on Easter Sunday." (Bless, 2003). In retrospect, NASA's Associate Administrator, Homer E. Newell, remarked that the OAO "... was a good example of the kinds of trouble one could get into by trying to force too big a technological step." (Newell, 1980: 165).

Following an overhaul of the OAO project within NASA's management structure, a newer and more conservative objective was at first announced: instead of one year in space, only one hundred hours of successful observations would be attempted. But now Goddard scientists, who nominally were 'next-in-line' to launch their own experiment package, fought against such a measure, and argued that cumulative expenditures for the OAOs demanded a far greater return (in data acquisition) than that permitted by the one-hundred-hour goal (Bless, 1983). As a result, NASA reversed its decision concerning the one-hundred-hour restriction. But more importantly, and in light of the availability of another full-scale prototype model of the OAO spacecraft, NASA resorted to its original flight schedule (as if OAO-1 had never happened) and determined to make a second launch effort (to be known as OAO-2) that would finally unite the Smithsonian and Wisconsin experiment packages (Code, 2006; Bless, personal communication). That decision likewise kept the Goddard and Princeton experiments (to be known as OAO-3 and OAO-4) in the usual pipeline. Accordingly, the SAL team was asked to construct a duplicate experiment package, which occupied them for the next two-and-one-half years. By the end of 1968, they were poised to try their payload again. And this time, the WEP was accompanied by the SAO's now-completed Project Celescope (Figure 6).

The OAO-2 spacecraft was successfully launched by an Atlas-Centaur rocket on 7 December 1968, and placed into a roughly circular orbit 480 nautical miles high, inclined 35 degrees to the Earth's equator (GSFC, 1969; Code et al., 1970). Therein, the WEP began a remarkably successful 50-month period of gathering photometric and spectrophotometric data on stars, nebulae, galaxies, and even planets and comets.[10] The OAO's early accomplishments were summarized in a letter (3 July 1970) from Code to Jesse Mitchell, Director of NASA's Astronomy and Physics Branch, wherein Code proclaimed that "OAO II is a fantastically successful spacecraft from both the scientific and engineering standpoints. No other observatory on the ground or in space, to my knowledge, has contributed so much to astronomy in its first 18 months of operation." (Code, 1970). Unlike its SAO counterpart, whose cameras were shut down in April 1970 after only sixteen months of operation, the WEP spectrometers showed "… no significant degradation over a 3-year period." (Code and Savage, 1972: 213). Yet, a notable failure did occur (after only 2.5 months) in the circuitry that controlled the stepper motor which drove the filter wheel on the 16-inch nebular photometer; this action permanently positioned the calibration source in front of the photomultiplier tube and eliminated all further data acquisition with the WEP's largest instrument.

Over the course of its mission, political rather than technical problems posed the most significant threat to the WEP's longevity. Fears that the OAO-2 spacecraft could be prematurely terminated first arose among the Wisconsin astronomers during Christmas break of 1970 (Bless, personal communication), and grew to a climax in the spring of 1971. As the manned Apollo program was beginning to wind down, and prospects for the coming era of the Space Shuttle were growing, NASA's priorities were shifting rapidly (and its Congressionally-approved budget was facing some of its first-ever declines). Termination of the OAO-2 spacecraft, on 30 June 1971, was seemingly viewed as a possible 'cost-saving venture' on NASA's part for fiscal year 1972 (Code, 1971a).[11]

In April 1971, as Congressional debate over NASA's budget for the upcoming fiscal year approached, the Wisconsin astronomers launched an intensive lobbying effort that sought immediate help (in the form of letters and phone calls) from colleagues. On behalf of the OAO satellite, these combined appeals were directed to various U.S. senators and congressmen, and especially toward the NASA administration (specifically, John Naugle, Associate Administrator for Space Sciences and Applications, with copies to be sent to Homer Newell, Jesse Mitchell, and also to Joseph Karth, Chairman of the House of Representatives Subcommittee on Space Science and Applications) (Code, 1971a; 1971c). In addition, Code (1971b) forwarded a copy of the planned OAO-2 observing program for the latter half of 1971 to Naugle. The effects of this lobbying effort were swift and decisive. On 30 April 1971, and again on 10 May, Naugle sent (combined) telegrams to at least 33 individuals, affirming that "… funds have been reprogrammed and NASA will continue to provide support for the OAO-2 mission through December 1971." Naugle admitted that the lobbyists' effort "… expressed by letter ... was a consideration in making this decision." (Naugle, 1971a; 1971b). Never again was funding for the OAO-2's mission threatened.

On 14 February 1973, the WEP's power supply failed, and the mission was finally terminated. Having exceeded its anticipated lifetime of one year by *more than four times*, WEP completed observations of more than a thousand celestial objects. This feat was deemed a "… superior engineering accomplishment …" by Cook Technological Center engineer, Curtis B. Bendell (1972: 23).

8 SCIENTIFIC RESULTS FROM WEP

Roughly six months after OAO-2 was launched, Wisconsin astronomers Code, Bless, and Blair D. Savage (b. 1941) gave presentations on their preliminary results at International Astronomical Union (IAU) Symposium No. 36, held at Lunteren, The Netherlands, between 24 and 27 June 1969 (Houziaux and Butler, 1970). Yet, only a brief synopsis of WEP measurements and discoveries can be recounted below. Apart from the principal monograph and review paper already cited (Code, 1972; Code and Savage, 1972), findings from the OAO spacecraft have mainly appeared as a series of forty-one successively-numbered research papers, published chiefly in the *Astrophysical Journal*, extending from that of Code et al. (1970)[12] to that of Davis et al. (1982). Various catalogues of OAO data have likewise been issued; most recently that of Meade (1999). At NASA's request, Bless also delivered a series of lectures (1969-1970) at eighteen institutions (Bless, 1969; 1970; 1983), which doubled as a public relations and polling opportunity for the Agency (Anonymous, n.d.).

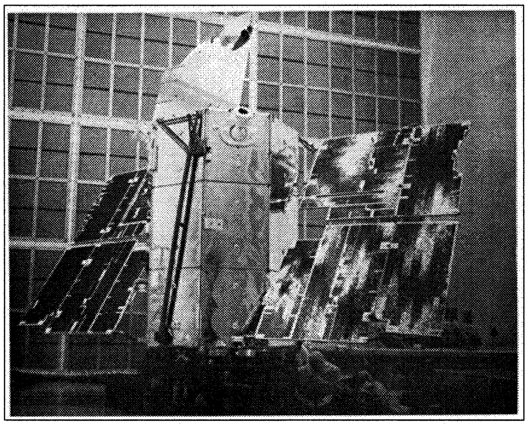

Figure 6. OAO-2 spacecraft undergoing preparations in a clean room at Cape Kennedy before launch. (Reproduced from *The Astrophysical Journal*, 161, Plate 1, f.388 (1970), by permission of the author and the American Astronomical Society).

The principal scientific mission of the WEP was to measure the spectral energy distributions of stars in the ultraviolet, using both filter photometers and spectrometers. Data obtained from a wide variety of stars led to more precise determinations of stellar effective temperatures and chemical compositions. Among O- and B-type stars, as was expected, strong absorption features were associated with the Lyman-alpha line (1216 angstroms), while the presence of silicon and carbon (as resonance lines) was readily confirmed. One fundamental result was a better match to theoretical predictions than could be obtained from the visual spectral region alone; the previous temperature scale was found to be too low. By correcting for the scanner's sensitivity across the UV spectral range, absolute luminosities of roughly 150 stars could be derived to an accuracy of ten percent.

Roughly a quarter of the O- and B-type stars observed by WEP proved suitable for the derivation of interstellar extinction in the Galaxy, based upon measurements from the filter photometers. Although difficulties were encountered in attempting to separate interstellar from stellar absorption features, overall results offered independent confirmation that the derived densities of interstellar hydrogen were not at variance with those obtained from 21-cm wavelength measurements.

Ultraviolet emission lines were observed in the spectra of a number of peculiar stars. Those lines supported the notion of expanding shells of material surrounding these objects, which has aided our understanding of the processes of mass-loss in highly-evolved stars.

Observations of Nova Serpentis (1970) revealed that its UV brightness continued to increase, even as its visual brightness decreased. This finding argued for the rapid evolution of an optically thick shell surrounding the star, that behaved somewhat analogously to the evolution of planetary nebulae seen around lower-mass stars.

A majority of 'dark' nebulae appeared brighter, at wavelengths below 1,600 angstroms, than do many diffuse nebulae when examined in visible light. This finding suggested that interstellar dust grains have significantly higher UV albedos than was expected. The measured peak on the interstellar extinction curve, at 2,200 angstroms and possibly signifying graphite, was judged "... perhaps the most interesting ..." result from the OAO-2's extensive data collection (Roman, 2001: 524). Such a discovery stood to revise understandings of the interstellar medium and the processes of star formation.

WEP provided unambiguous evidence that galaxies on average are systematically brighter in the UV than was expected from the known main sequence stars that comprise them. Disk populations of O- and B-type stars are thought to contribute those added fluxes, which are modified through processes of interstellar absorption.

Within our own Solar System, the Wisconsin package discovered a vast hydrogen cloud surrounding the nucleus of Comet Tago-Sato-Kosaka (1969g). Originally predicted by astronomers L. Biermann and E. Trefftz, the cloud was detected through its strong Lyman-alpha emission. The feature was confirmed around Comet Bennett (1969i), whose cloud extended roughly 100,000 kilometers from the nucleus. These and other observations provided strong support that water ice is a dominant component of the material in a comet's nucleus, which undergoes photo-dissociation when it becomes active.

The presence of ozone in the Martian atmosphere was another unexpected discovery made by the WEP. Subsequent reanalysis of data collected by *Mariner* spacecraft confirmed this finding, which offered important clues to the chemistry and evolution of that planet's atmosphere.

9 THE WISCONSIN APT: A GROUND-BASED ROBOTIC TELESCOPE

Between launches of the OAO-1 and OAO-2 spacecraft, SAL scientists Houck, McNall, Terrell L. Miedaner, and Donald E. Michalski also demonstrated the capabilities of the first computer-controlled telescope at the department's Pine Bluff Observatory. This instrument was later dubbed the Wisconsin Automatic Photoelectric Telescope, or APT (Code, 1992). Houck first obtained the surplus mounting, designed for an aircraft guidance system, and suggested its use in collecting atmospheric extinction data. The device carried a WEP 8-inch off-axis reflecting telescope and photoelectric photometer, whose operations were essentially identical to those employed on the OAOs. The original analog control system was found to possess serious mechanical difficulties, but McNall added shaft encoders and stepper motors as a means of providing digital pointing and control functions. A PDP-8 minicomputer, having a memory of only 4,096 12-bit words, and similar to that used by the Wisconsin team's Ground Operating Equipment station, was reprogrammed by Miedaner. Even the small shed-like observatory, which sported a roll-off roof, was automated. Published accounts of the telescope's capabilities noted that the instrument represented an 'offshoot' or 'spinoff' from the nation's space program that stood to benefit contemporary ground-based astronomy—a tremendous understatement, in retrospect (Miedaner and McNall, 1967; NcNall, Miedaner, and Code, 1968).

Code (1992) later noted three levels or operative criteria which have emerged and that roughly characterize the enormous diversity of telescope control systems employed by today's professional astronomers. (a) The simplest of these is called *remote observing* (and was originally called 'remote control' —Maran, 1967). Here, commands are issued (usually in real time) from a control center, located some distance away from the observatory, to direct the telescope's functions. Remote observing, by definition, requires active input from a human being, who monitors and adjusts the subsequent operations. (b) *Automatic operation* consists of a set of pre-programmed instructions that enables a device (such as a telescope) to perform a set of operations without direct assistance or intervention from a human being. However, no deviation from the pre-programmed sequence of commands is allowed. (c) Finally, a *robotic* (or intelligent) system performs a set of logical decisions, based upon all forms of input to the system, and then executes a series of automatic commands, ranging from the mundane to the highly sophisticated (e.g., star acquisition and subsequent collection of photometric data). The 8-inch ground-based telescope fashioned by McNall and his colleagues satisfied all three of these functionality criteria, and thus "... the Wisconsin APT can quite properly be called a robotic telescope." (Code, 1992: 8). Later cannibalized for its parts, the robotic telescope no longer exists.

Might some (or all) of these criteria perhaps be applied to the OAO-2 spacecraft itself? By observing from above the Earth's atmosphere, OAO-2 was unquestionably a "... remotely operated observatory ..." (Code, 1970: 381). Further, its principal operations, such as slewing from one star to another and automatic collection of photometric data, were performed under the controls of computer programs such as HARUSPEX. However, OAO-2 (and WEP) could do nothing without those pre-programmed instructions that were supplied on a continuing basis from the Ground Operating Equipment station. Moreover, all of the safety checks (i.e., logical decisions) that accompanied each spacecraft maneuver and observing sequence were programmed in advance before they were transmitted to the satellite (Bless, personal communication). As a result, the third and final criteria associated with the label, 'robotic telescope', cannot be assigned to the OAO-2.

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10 CONCLUSIONS

A number of important 'firsts' may be attributed to the OAO-2. It was the first true stellar space observatory that embodied both remote and automatic modes of operation, before those attributes were widely applied to the control of large and small ground-based optical telescopes. While the OAO-2 spacecraft cannot be called a robotic telescope, the concurrent development of a ground-based, automatic photoelectric telescope (or APT) was successfully undertaken by members of the Wisconsin team and described as a 'spinoff' from the nation's space program.

OAO-2 (and especially WEP) played an important role in the field of space astronomy; its large collection of UV data (acquired by filter photometers and scanning spectrometers) marked the first significant opening of the electromagnetic spectrum in wavelengths shorter than visible light—a trend that has characterized much of astrophysics in the latter half of the twentieth century. Perhaps the best analogy of that accomplishment is one supplied by Bless, who has likened the whole of the electromagnetic spectrum to the sounds produced by a symphony orchestra. Having long been restricted to observing through the narrow window represented by visible light, ground-based optical astronomers were in effect only able to hear but a few notes of the entire symphony (Bless, 2003).[13] Space-based astronomy has removed that limitation and contributed to a full investigation of the electromagnetic spectrum, ranging from high-energy gamma rays to long-wavelength radio waves. OAO-2 observations enabled data to catch up with, and in some cases to supersede, existing theories of stellar atmospheres and the ISM.

The design, construction, and operation of OAO-2 appears to have marked another, and more significant, transition in the development of late-twentieth century astrophysical research. That trend has been the growing reliance placed upon digital means of data acquisition, storage, transmission, and reduction that is now practiced throughout the discipline. While the tremendous advantages conferred by such techniques are unquestioned assumptions today, that was far from the case, prior to the launch of OAO-2. From the demands imposed by remote, automatic operations above the Earth's atmosphere, both Wisconsin and Smithsonian scientists were forced to adopt the (mainframe) digital computer as the only means of achieving the necessary speed, accuracy, and control over the OAO-2 spacecraft. As Code (1969: 292) presciently observed, "… high-speed computers are essential to the astronomer …" for carrying out these multiple tasks.

That is not to say that digital computers had not gained widespread usage in astronomical applications; just the opposite was true. For example, three of the leading U.S. centers for astronomical computations, namely, (a) the United States Naval Observatory, Washington, D.C.; (b) the IAU's Central Bureau for Astronomical Telegrams, Smithsonian Astrophysical Observatory, Cambridge, Massachusetts; and (c) the Minor Planet Center, Cincinnati Observatory, Cincinnati, Ohio, had all pioneered the use of digital computing techniques, some even predating the Second World War (employing analog means). Such facilities were routinely used in the production of the *American Ephemeris and Nautical Almanac* (*AENA*), and the calculation of orbital elements and ephemerides of newly-discovered comets and minor planets, respectively. The point, however, is that these digital tools were largely restricted to the solution of routine, computational problems in practical astronomy and were not employed in the *collection* and *analysis* of observational data, which remained chiefly optical and analog in nature (i.e., long-exposure images recorded on photographic plates).[14]

Electronic devices themselves were nothing new to astronomers of this era. Photoelectric cells and photomultiplier tubes saw widespread application to the study of point-source phenomena, especially after the Second World War (DeVorkin, 1985). Related devices were applied to the measurement of intensity profiles of stellar and galactic spectra. Wisconsin astronomer, John F. McNall, and his colleagues at the Lick Observatory, Mount Hamilton, California, developed a new electronic detector and amplifier system; namely, the image-intensifier image-dissector scanner (McNall, Robinson, and Wampler, 1970; Robinson and Wampler, 1972). More broadly, steps were taken toward fabrication of an electronic camera, first envisioned by French astronomer André Lallemand (1936), whose development was intensified by wartime research (Wlérick, 1987). And yet, all of these systems, which displayed increased sensitivity and decreased exposure times (in comparison to photographic plates), were to pale in comparison to the forthcoming universal adoption of the charge-coupled device (CCD)—itself a product of industrial-scale research-and-development (Smith and Tatarewicz, 1985; 1995).

It is no surprise, therefore, that the WEP arose from an institution that had *avoided* photographic research methods, and which instead had pioneered the techniques of photoelectric photometry to analyze the properties of stars and the interstellar medium (DeVorkin, 1985; Hearnshaw, 1996; Liebl and Fluke, 2004). In that regard, the Wisconsin package may be seen as a natural extension of those earlier techniques, once the technology of spaceflight was achieved and the long-standing barriers to investigation of the electromagnetic spectrum beyond the Earth's atmosphere were at last removed.

As demonstrated by the WEP and OAO-2 spacecraft, a transition to digital data acquisition and analysis, which occurred within the context of mainframe (and mini) computing environments, nonetheless took place well *before* the 1970s advent of the personal computer and the charge-coupled device (CCD). Personal computers and their accompanying software programs eventually brought more sophisticated and flexible computing powers to researchers, along with graphical interfaces, plus the added convenience of the individual user's desktop. Superior networking technologies have enabled personal computers to remotely operate even the largest and most sophisticated of today's astronomical instruments and observatories (McCray, 2004). Succeeding generations of astronomers have adopted the computer as perhaps the single most important tool in their arsenal, after the telescope or spacecraft itself. Along with opening the field of UV space astronomy, OAO-2 was a bellwether of the equally-important transition to digital data acquisition and analysis strategies employed universally today.

11 NOTES

1. Needell (2000: 319-320) has argued that the satellite proposals 'transformed' the IGY from a scientific program "... into a watershed in the relations between science, technology, and the American federal government."
2. The SSB was also expected to provide advice to the National Science Foundation (NSF) and the Advanced Research Projects Agency (ARPA) (SSB, 1958a).
3. Neither the original telegram from Berkner, nor Code's response, has been preserved, although Code's response was acknowledged by R.C. Peavey, Secretary, SSB (Telegram, Peavey to Code, 5 August 1958, ADC Papers, SSB Folder). Code (2005; 2006) has further recalled that he received a 'letter' from Berkner, evidently prior to the SSB's first meeting, soliciting suggestions for "... a 100 pound satellite." This opportunity proved influential in Code's decision to return to the University of Wisconsin-Madison. Berkner's letter has not been located; nor has it been described by Needell (2000).
4. DeVorkin (1989: 73) writes: "Most astronomers were simply not comfortable with the degree of funding and manpower commitments required to pursue active experimental programs with rockets, nor were they comfortable with the style of research which required a high dedication to building devices that were likely to be destroyed upon use." For a broader assessment of astronomers' responses to the advent of Federal support after World War II, see DeVorkin (2000).
5. McCray's first chapter, "Leo and Jesse's Changing World" (pp. 13-49), examines the profound differences in viewpoint between astronomers Leo Goldberg and Jesse Greenstein over the approaching Space Age and the expected role of Government patronage in the support of public vs. private research institutions.
6. Goldberg's Committee was originally titled, "Astronomy and Radio Astronomy," but afterwards its name was changed to the less redundant, "Optical and Radio Astronomy."
7. Berkner and Odishaw (1961: 432) report that some 200 proposals were assessed, whereas Hetherington (1975: 104) states that, from over 150 telegrams sent out in July 1958, "... some one hundred replies ..." were received by the SSB. Smith (1989: 37) notes that "... approximately two hundred replies ..." came to the Academy. This higher number evidently reflects the additional responses made to Berkner's earlier letter.
8. Roman does not say with whom the modular approach originated. Homer E. Newell, NASA's former Associate Administrator, has written that Abe Silverstein, then Director of Space Flight Development and (Acting) Director of the Beltsville Space Center (later renamed GSFC), "... favored the development of large, observatory-class spacecraft ..." under the assumption that "... larger spacecraft would probably give more science per dollar than smaller ones." (Newell, 1980: 207). Rosenthal (1968) offers no direct guidance on this question.
9. In response to the University of Wisconsin Request for Quotation 31-1360-0, dated 10 May 1961, bids were received from the following: Grumman Aircraft Engineering Corporation and Perkin-Elmer Corporation; McDonnell Aircraft Corporation; Space Technology Laboratories, Inc.; Texas Instruments, Inc.; Control Technology Corporation; AC Spark Plug Division of General Motors Corporation; Bendix Corporation; Astronautics Corporation of America; and Cook Technological Center (Box 21/84, "Univ. of Wisconsin/Space Astronomy Lab/WEP Bids," SAL Archives).
10. For the most detailed recollections of actual day-to-day operations of OAO-2, and the numerous problems encountered by the Wisconsin team, see Bless (1984).
11. Code (1971a) stated that, as Principal Investigator on OAO-2, he had "... received no formal notification of the proposed shutdown of the satellite ...", nor had he "... been invited to participate in any NASA consideration of the OAO-2 termination."
12. A draft of the Code et al. (1970) paper is preserved in ADC Papers, OAO - Ap.J. Paper 1 Folder.
13. Of course, this claim unfairly excludes the accomplishments of more than a generation of radio astronomers, particularly after the Second World War, and similarly neglects the achievements of early infrared observations conducted with ground-based instruments.
14. From the beginning, radio astronomy likewise demanded the use of electronic equipment (originally analog, and later digital) for the reception, recording, and analysis of all incoming signals. Such observations were made with single, fixed or steerable antennas and later employed the signal-combining capabilities of very-long-baseline interferometry (VLBI).

12 ACKNOWLEDGEMENTS

Anyone attempting to write a well-balanced account of the OAO-2 spacecraft will encounter a nearly-complete lack of documentary materials available at the Goddard Space Flight Center (GSFC), which nonetheless managed the project (R.W. Smith, personal communication). As a result, the story must be pieced together from a variety of other sources, including published and unpublished accounts, meeting minutes, conference presentations, and especially oral history interviews with the leading participants.

The author wishes to express his thanks to Drs Arthur D. Code, and Robert C. Bless, both Emeritus Professors at the University of Wisconsin-Madison, for sharing their recollections of the Orbiting Astronomical Observatory and early years of the Space Astronomy Laboratory. In addition, Bless offered critical commentaries on earlier drafts of this paper. Thanks are also due to the Department of Astronomy at UW-Madison for allowing me to examine those documents in the SAL Archives (presently not arranged) that preserve the history of the Wisconsin Experiment Package. The Memorial Library Archives, UW-Madison, granted access to the Arthur D. Code Papers. Transcripts of prior oral history interviews with Code and Bless, conducted by David H. DeVorkin and Robert W. Smith, respectively, were furnished by the Oral History Project, Department of Space History, National Air and Space Museum, Washington, D.C. Donald E. Osterbrock kindly supplied a copy of his remarks delivered at the banquet honoring Code's 80th birthday. The National Space Science Data Center (NSSDC) provided little-known facts about OAO-1.

An important influence on the development of this paper was the author's experience in working with UW Astronomy 100 honors student Adam J. Walsh (2003), whose own research and writing on this topic proved insightful.

The paper's two referees provided wise counsel on the correction of omissions and deficiencies of its historical resources and interpretations; any remaining errors are the author's own.

He also wishes to thank David H. DeVorkin, National Air and Space Museum, Smithsonian Institution, Washington, D.C., whose concurrent research on the SAO's Project Celescope inspired the arrangement of a special session devoted to the OAO-2 at the Historical Astronomy Division of the American Astronomical Society's 207th meeting on 8 January 2006.

13 REFERENCES

The following abbreviations are used:

AAS = American Astronomical Society
ADC = Arthur D. Code Papers, 1958-1985, Memorial Library Archives, University of Wisconsin-Madison
GSFC = Goddard Space Flight Center, Greenbelt, Maryland
SAL = Space Astronomy Laboratory Archives, Department of Astronomy, University of Wisconsin-Madison
SSB = Space Science Board, National Academy of Sciences

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HISTORICAL ARCHIVES IN ITALIAN ASTRONOMICAL OBSERVATORIES: THE "SPECOLA 2000" PROJECT[1]

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Abstract: Italy's well-consolidated tradition in astronomy is fully witnessed by its rich archival heritage. Astronomical records are stored in many observatories and universities, as well as in libraries and in private institutions. In 2000 a project was promoted to arrange and produce inventories of all material kept in Italian observatory archives. The project was planned by the Società Astronomica Italiana, and financial support was provided by the Italian Ministero per i Beni e le Attività Culturali. In this paper, the results obtained thus far are presented and commented on.

Keywords: Astronomical archives, Italian observatories, "Specola 2000" Project

1 INTRODUCTION

Italy's well-established tradition in astronomy is fully witnessed by its impressive astronomical heritage, which consists of archival and bibliographic material, as well as historical instruments. About seventy-five percent of this material is located in astronomical observatory archives.

There are twelve astronomical observatories in Italy that are supported by Government funding, not by privates or by foundations. This abundance of observatories is due to the political history of Italy: for many centuries the country was divided into small states, until political unity was achieved in 1870, after the wars of independence and the Risorgimento events.

Italy's first 'institutional' observatories were established in the eighteenth century (as in the majority of the other European nations), and at this time Italy was composed of several different states. Between 1711 and 1819 each of them established one or two observatories.

After political unity, the Government at first decided to preserve the *status quo*, but this implied a great financial commitment. To maintain and fund twelve different astronomical observatories was an unrealistic target for the new national Government whose finances needed to be invested in more urgent areas (such as education) rather than in astronomical research.

In 1874 a reform project was presented by the astronomer Pietro Tacchini (1838–1905), proposing a classification of Italian observatories into research observatories, university observatories and meteorological observatories. The Government almost completely accepted the proposal, and in 1876 the Minister, Ruggiero Bonghi (1826–1895), signed the decree that reformed the observatories, but this reform was never fully applied, for various political reasons.

Perhaps because this reform was a partial failure, three more observatories were established at the end of the nineteenth century. As a result, after the Second World War and the annexation of the ex-Austrian territories, the total number of the Italian observatories was twelve—which is the current number.

Currently, astronomical archives are found in many institutions (not just the twelve 'official' observatories), including universities (e.g. public institutions, like Bologna University, and private institutions, like the Papal Gregorian University), prestigious libraries (e.g. the Biblioteca Estense in Modena and the Istituto e Museo di Storia della Scienza in Florence) and private collections (e.g. family archives of certain astronomers).

2 EARLY ATTEMPTS AT PRESERVATION IN ITALIAN OBSERVATORIES AND IAU RESOLUTIONS

Concerning observatories, an important effort for the conservation of historical materials has been made since the 1980s, thanks to a growing interest in the field of history of science.

The first Italian observatory to pay attention to its archival heritage was Brera Observatory in Milan, which started a program of preservation and inventorying in 1983. Following this example, other observatories decided to arrange their archives, and the prominent role of Edoardo Proverbio in this regard is worth mentioning. Through the History Working Group of the Società Astronomica Italiana, in 1989 and 1993 he arranged two national meetings devoted to the conservation of astronomical archives, books and instruments.

In an international context, it is important to mention that the following Commission 41 (History of Astronomy) resolution was passed at the 1991 General Assembly of the IAU in Buenos Aires:

> *(1) to establish a register of the whereabouts of all extant astronomical archives of historical interest; (2) to impress on observatories and other institutions their responsibility for the preservation, conservation, and where possible, cataloguing of such archives; (3) to search for an institution that will allocate space and funds for maintaining such a register and publishing it.* (Resolution C41 ..., 1991).

Commission 41 then created an Archives Working Group to further the objectives of this Resolution, and in the following three years some progress was made in compiling national inventories of astronomical archives.

Two further archival resolutions proposed by Commission 41 were adopted at the 1994 General Assembly of the IAU in The Hague; a number of members presented papers on their archival researches; and there were also discussions regarding IAU archives. A further archival resolution was discussed at the Commission 41 Business Meeting and adopted at the 1997 General Assembly of the IAU in Kyoto; a half-day Special Session on "Inventory and Preservation of Astronomical Archives, Records and Artifacts" was held at the 2000 General Assembly of the IAU in Manchester; and the 2003 General Assembly of the IAU in Sydney included a half-day meeting of the Archives Working Group. Many of the papers from the Manchester and Sydney meetings were subsequently published in a special 'heritage' issue of the *Journal of Astronomical Data* (Volume 10) in 2004.[2]

3 THE "SPECOLA 2000" PROJECT

In July 1999 the Ufficio Centrale Beni Archivistici of the Italian Ministero per i Beni e le Attività Culturali expressed to the Società Astronomica Italiana its interest in starting a project to preserve the archives in the official Italian observatories.

A joint Commission was then formed to prepare a report on the current situation of the archives, and to propose steps for the development and the completion of the project (Fodera et al., 2000; Pastura, 2005). At the end of 1999 the "Specola 2000" Project for the inventorying of Italian observatory archives was ready to begin.[3]

The following steps were identified:

- Preliminary Phase: to survey all archival material more than 40 years old kept in the observatories. [This Phase was concluded in March 2000.]
- Phase 1: to arrange and produce inventories of the archives a) not yet inventoried, or b) partially inventoried; the digital descriptions of the items were compiled following the rules laid down by the International Standard Archival Description (ISAD).
- Phase 2: to complete or to start the cataloguing of the astronomers' correspondence.
- Phase 3: to catalogue the photographic material (with the help of experts).
- Phase 4 (final): the merging of the observatory archival documents with astronomical archives held in other institutions.

From the start it was agreed that archives which in the course of time have lost their 'order' should be brought back to the organizations from which they originated.

The project was initially supported by the Ministero per i Beni e le Attività Culturali and by the Consorzio Nazionale per l'Astronomia e l'Astrofisica. The latter institution was established in 1996, in order to coordinate the activities at all of the 'official' Italian observatories.

The Ufficio Centrale Beni Archivistici asked the District Superintendents to send one or two archivists to each observatory in order to carry out the work, while the observatories designated one person (generally the librarian) to supervise this work. The observatories also assisted the archivists by providing them with adequate work space, as well as stationery and technical support.

In 2002 all the Italian observatories merged into the National Institute for Astrophysics (INAF) and, since then the Specola 2000 Project has been carried out within the INAF Libraries and Historical Archives Working Group.

The Specola 2000 Project was born under the patronage of the Società Astronomica Italiana, but it was conceived and coordinated by Giorgia Foderà and Agnese Mandrino respectively, as Scientific and Technical Coordinators. In 2003, Giorgia Foderà retired, and was replaced by one of the authors of this paper (F.B.).

As Table 1 indicates, currently the archival collections in seven different observatories have been either partially or totally inventoried, and five of these are now entirely or partially on line (Mandrino et al., 2007). Meanwhile, work on an eighth observatory collection has just started, so overall there has been excellent progress with Phase 1. Note, also, that the archival collections at two of the observatories were inventoried prior to the commencement of the Specola 2000 Project.

Table 1: Status of the Specola 2000 Project in 2006

Status	Observatory
Complete	Bologna (on line)
Partially complete	Brera-Milan (on line), Rome, Palermo, Catania [each about 80% complete], Arcetri-Florence (Phases 1-2) [about 60% complete] (on line) Padua [about 50% complete]
Just Started	Trieste
Not Yet Started	Collurania-Teramo, Cagliari
Completed Before 2000 (Pre-'Specola 2000')	Capodimonte-Naples (on line), Turin (on line)

The next steps of the Project are the completion of the Phases 1 and 2.

4 FUTURE DIRECTIONS

Following the early success of the Specola 2000 Project, the INAF Museums Working Group intends to implement a similar project (MuSA 2009—Museo della Strumentaria Astronomica) in order to preserve and catalogue the historical instruments kept in museums and collections of the different Italian astronomical observatories. The aim of MuSA 2009 is to build on the start made by the Specola 2000 Project and focus on the conservation of the historical astronomical heritage of Italy's observatories (see Chinnici et al., 2006). This will be the main focus during the next three years, and it is hoped that the Italian experience will stimulate analogous initiatives in other countries.

5 NOTES

1. This paper was presented in the Archives Working Group meeting at the 2006 General Assembly of the IAU in Prague.
2. The contents are available online at http://www.vub.ac.be/STER/JAD/JAD10/jad10.htm
3. This project is described on the following web site: www.archivi.beniculturali.it/divisione_III/progspecola.html

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IAU HISTORIC RADIO ASTRONOMY WORKING GROUP. TRIENNIAL REPORT (2003–2006)

1 Introduction

This WG was formed at the 2003 General Assembly of the IAU as a joint initiative of Commissions 40 (Radio Astronomy) and 41 (History of Astronomy), in order to:

- assemble a master list of surviving historically-significant radio telescopes and associated instrumentation found worldwide;
- document the technical specifications and scientific achievements of these instruments;
- maintain an on-going bibliography of publications on the history of radio astronomy; and
- monitor other developments relating to the history of radio astronomy (including the deaths of pioneering radio astronomers).

The membership list of the WG contains the names of about one hundred astronomers who are active in the history of radio astronomy field or sympathetic to it.

2 Progress Reports of the Working Group

Annual Progress Reports were prepared in 2004 and 2005 and were published in the newsletter of Commission 41 and in the June 2004 and 2005 issues of the *Journal of Astronomical History and Heritage*. Copies also were submitted to Commission 40 and to the Presidents of Divisions X and XII.

3 National Masterlists of Surviving Historically Significant Radio Telescopes

WG members actively worked on national masterlists for Australia, Germany, the Netherlands, the United Kingdom and the USA, and a number of research papers were prepared documenting individual instruments or instruments and research associated with specific radio astronomy field stations.

4 The Preservation and Destruction of Historically-Significant Radio Telescopes

WG members lobbied Stanford University to rescind its decision to demolish the array of five 60-ft antennas at the field station off Highway 280 (California), and reported—with some dismay—the demolition of the twelve surviving Chris Cross dishes from the solar grating array at Fleurs, near Sydney.

5 Research Projects by Working Group Members

WG members who actively researched aspects of radio astronomical history during the triennium included *Bruce Balick* (USA), *Ron Bracewell* (USA), *Jessica Chapman* (Australia), *Marshall Cohen* (USA), *Rod Davies* (United Kingdom), *Bob Duncan* (Australia), *Dave Green* (United Kingdom), *Miller Goss* (USA), *Alastair Gunn* (United Kingdom), *Richard Jarrell* (Canada), *Dave Jauncey* (Australia), *Ken Kellermann* (USA), *Bruce McAdam* (Australia), *Dick McGee* (Australia), *Doug Milne* (Australia), *Masaki Morimoto* (Japan), *Wayne Orchiston* (Australia), *Brian Robinson* (Australia), *Bruce Slee* (Australia), *Slava Slysh* (Russia), *Richard Strom* (The Netherlands) *Woody Sullivan* (USA), *Govind Swarup* (India), *Hugo Van Woerden* (The Netherlands), *John Whiteoak* (Australia), and *Richard Wielebinski* (Germany).

6 Bibliography of Publications on the History of Radio Astronomy

An on-going list of publications in the history of astronomy field was maintained (and included in the WG Annual Reports). Arrangements were made to publish a succession of history of astronomy papers in 2005 and 2006 issues of the *Journal of Astronomical History and Heritage*.

7 Conferences

Since the 2003 General Assembly sessions on historic radio astronomy, the following conferences have featured sessions on the history of radio astronomy:

(1) "The New Astronomy: Opening the Electromagnetic Window and Expanding our View of Planet Earth. A Meeting to Honor Woody Sullivan on His 60th Birthday" (Seattle, June 2004).
(2) "Radio Astronomy at 70: From Karl Jansky to Microjansky" (Budapest, August 2004).
(3) Fifth International Conference on Oriental Astronomy (Chiang Mai, Thailand, October 2004).
(4) AAS Historical Astronomy Division Meeting (Cambridge, UK, September 2005).

Members of the WG were instrumental in organizing all but the second of these.

8 Planning for the Prague General Assembly

The WG Committee has applied to hold four quarter-day meetings, so that members can discuss their latest research, with emphasis on the development of radio astronomy in Europe, and the status of radio astronomy worldwide fifty years ago when 'big science' first began to impact on radio astronomy.

10 The End of an Era

With sadness WG members noted the passing of the following pioneering radio astronomers: *Semion Braude*, *Robert Hanbury Brown*, *Bob Duncan*, *Frank Gardner*, *Victor Hughes*, *Vladimir Kotelnikov*, *John D. Kraus*, *Harry Minnett*, *Christiaan Alexander (Lex) Muller*, *Grote Reber*, *Brian Robinson*, *Gordon Stanley*, *Hendrik Christoffel (Henk) van de Hulst*, *Kevin Westfold* and *Don Yabsley*. Where these existed, relevant obituaries were included in the WG Annual Reports, and biographical details were provided for several of these individuals.

11 Further Information

For further details of Working Group activities during 2003-2005 see:

Orchiston, W. et al., 2004. The IAU Historic Radio Astronomy Working Group. 1. Progress Report. *Journal of Astronomical History and Heritage*, 7, 53-56.
Orchiston, W. et al., 2005. The IAU Historic Radio Astronomy Working Group. 2. Progress Report. *Journal of Astronomical History and Heritage*, 8, 65-69.

Wayne Orchiston, Chair (Australia)
Rod Davies (United Kingdom)

Ken Kellermann (USA)
Alain Lecacheux (France)
Masaki Morimoto (Japan)
Slava Slysh (Russia)
Govind Swarup (India)
Hugo Van Woerden (The Netherlands)
Jasper Wall (Canada)
Richard Wielebinski (Germany)

IAU HISTORICAL INSTRUMENTS WORKING GROUP. TRIENNIAL REPORT (2003–2006)

1 Introduction

The Organizing Committee of the Working Group for 2003-2006 comprised: Nha Il-Seong (Korea, Chair), Juergen Hamel (Germany), Kevin Johnson (UK), Tsuko Nakamura (Japan), Wayne Orchiston (Australia) and Sara Schechner (USA).

Activities of this WG can be summarized in yearly base as below.

2 Activities in the 2003-2004

1. At the IAU General Assembly in Sydney, the following papers were presented and some of them have been published in the issues of the *Journal of the Antique Telescope Society*:

Johnson, K. "A glimpse at the astronomy heritage of the Science Museum, London."
Kaptueg, V.B., Chubey, M.S., Vereshchagin, S.A., and Sokolov, Y.A. "On recovery and research work at the Russian Struve station in Gogland."
Lomb, N. "Historically significant astronomical instruments at Sydney Observatory."
Orchiston, W. "History of the 'Catts Telescope': a nineteenth century 20-inch Grubb reflector."
Pigatto, L., Tomasella, L., and Zanini, V. "Telescopes at the Astronomical Observatory of Padova, Italy. From the last refractor to the first reflector."
Shankland, P.D., and Orchiston, W. "Lost and found: saga of the historic Clark refractor at the U.S. Naval Academy."
Watson, F. "James Gregory and the invention of the Cassegrain telescope."

2. The following ten papers have been published in *Astronomical Instruments and Archives from the Asia-Pacific Region*. Orchiston, W., Stephenson, R., Débarbat, S., and Nha, I.-S. (eds.). Seoul, Yonsei University Press and IAU C41, 2004:

Nha, I.-S. "King Sejong's sundial, Anbu Ilgui." Pp. 21-26.
Nha, Sarah L. "A progress report on the C41/ICHA Historical Instruments Working Group web site." Pp. 29-34.
Debarbat, S. "Korean instruments preserved in the Paris Observatory collections." Pp. 121-124.
Ohashi, Y. "Medieval Indian astronomical instruments and archives." Pp. 125-128.
Bandyopadhay, A. "The famous sun-temple of Konarak and Maharaja Jai Singh's Observatory at Jaipur: outstanding historic astronomical instruments in India." Pp. 129-133.
Setyanto, H. "Rubu al-MUJAYYAB: concept and practice in Indonesia." Pp. 135-140.
Fountain, J., and Abt, H.A. "Chinese jade serrated Bi discs as astronomical instruments." Pp. 141-144.
Allen, C. "The Frisius-Arsenius astrolabe in the National History Museum, Mexico." Pp. 145-148.
Batten, A.H. "The 72-inch Plaskett Telescope in Victoria, B.C." Pp. 151-156.
Orchiston, W. "The rise and fall of the Chris Cross: a pioneering Australian radio telescope." Pp. 157-162.

3 Activities in the 2004-2005

1. A list of references dealing with historic astronomical instruments was published in the *Journal of Astronomical History and Heritage*, Volume 7, pp. 57-58, 2004.

2. For four days in October, 2004, the Fifth International Conference on Oriental Astronomy was held in Chiang Mai, Thailand. The following three papers about instruments were presented:

Guan, Z. "The historical and cultural value of Dengfeng Observatory."
Nha, S.L. "Progress report on the IAU Historical Instruments Working Group web site: the classification of Korean and Chinese sundials."
Nha, I.-S., Oh, G., and Chen, K. "Xin'an Xiuyu, a city of Chinese sundials and compasses."

4 Activities in the 2005-2006

Three conference proceedings of the International Conference on Oriental Astronomy are currently in press:

Highlights of Oriental Astronomy, Proceedings of the Second International Conference on Oriental Astronomy (eds. Chen, Kwan-Yu, Bo Shuren and Sun, Xiaochun).
Proceedings of the Fourth International Conference on Oriental Astronomy (eds. Strom, Richard and Liu, Yongping).
Proceedings of the Fifth International Conference on Oriental Astronomy (eds. Chen, K.-Y., Orchiston, W., Soonthornthum, B., and Strom, R.).

5 Overall in the 2003-2006

In addition to lists of publications in each year-term above, there is one other major achievement relating to our WG. This is the web site set up by Sarah Nha to inventory historically-significant astronomical instruments world-wide. As is reported in the *Journal of Astronomical History and Heritage*, the URL is: http://www.nhamuseum.org/WG However, this web site is still being developed and the final classification of instruments it will contain has yet to be finalized. One of those working on a suitable thesaurus for this is WG Committee member, Juergen Hamel.

Nha Il-Seong (Chairman)

IAU TRANSITS OF VENUS WORKING GROUP. TRIENNIAL REPORT (2003–2006)

Since the last General Assembly the Transit of Venus Working Group has published Progress Reports # 3 and # 4 in the *Journal of Astronomical History and Heritage*, Volume 7 (June, 2004), pp. 50-52, and Volume 8 (June, 2005), pp. 70-71. Reports # 1 and # 2 were published in the same journal, Volume 5 (December, 2002), pp. 185-188 and Volume 6 (June, 2003), p. 64. Readers are referred to these publications for details of the activities of the Working Group.

The most important event since the last General Assembly was the occurrence of the transit itself on 8 June 2004. Gordon Bromage and D.W. Kurtz organized IAU Colloquium 196 centered around this event. The meeting was held at the University of Central Lancashire, Preston, UK, near the site where Jeremiah Horrocks first observed a transit of Venus in 1639. To the delight of all present, on the day of the rare event almost the entire transit was observed from the tiny Lancashire village of Much Hoole, where Horrocks lived, and several other locations. Members of Commission 41 who presented historical papers included Allan Chapman, Suzanne Débarbat, Wayne Orchiston, Luisa Pigatto, Steven Dick, Brian Warner and Mary T. Brück. There was also an excellent set of scientific papers. The Proceedings were published as *Transits of Venus: New Views of the Solar System and Galaxy*, ed. D W. Kurtz (Cambridge University Press, 2005).

Historical markers continue to be erected to commemorate sites where transit of Venus observations were made. In addition to those listed in WG Report # 3, among the latest are two markers in San Antonio, Texas, one at Bullis House Inn commemorating the Belgian transit expedition, and another at Fort Sam Houston commemorating the American transit of Venus expedition, both in 1882.

The Transit of Venus Working Group is organizing reports on the transit of Venus observations at the Prague General Assembly in 2006.

Steven J. Dick (Chairman)

Journal of Astronomical History and Heritage, 9(2), 206-208 (2006)

BOOK REVIEWS

***JENAM 2003. Radio Astronomy From Karl Jansky to Mictrojansky,* edited by L.I. Gurvits, S. Frey and S. Rawlings (PA de Courtaboeuf, EDP Sciences, 2005; EAS Publication Series, Volume 15), pp. x + 489, ISBN 2-86883-735-2 (hardback), €72.**

Radio astronomy has made enormous strides since Karl Jansky dramatically expanded our multiwavelength horizons seventy-five years ago. In August 2003 JENAM (the annual Joint European National Astronomy Meeting) held a symposium on "Radio Astronomy at 70: from Karl Jansky to microjansky" in Budapest to highlight advances in modern radio astronomy, and this book contains a set of invited review papers presented at that meeting (other papers having already been published in *Baltic Astronomy*, Vol. 14, No. 3, 2005).

This book is primarily of interest to astrophysicists, and contains excellent reviews of the CMB, extragalactic radio sources, deep field surveys, AGNs, extragalactic radio supernovae, Galactic and extragalactic magnetic fields, Galactic and extragalactic neutral hydrogen, radio emission from stars, pulsars, recombination lines, the ISM and Galactic masers; chapters on radio astrometry and on twenty-first century developments in instrument (including the Planck Mission, ALMA, LOFAR, the SKA, and space VLBI); accounts of the interface between radio astronomy and X-ray and gamma-ray astronomy; Gilmore's short yet illuminating 'outsider's' view on radio astronomy; and a final chapter where Parijskij indulges in a little crystal ball-gazing in his "Radio astronomy: the next 70-year step".

However, three historical chapters launch this book, and these will be of immediate interest to readers of this journal. In the first, F. Graham-Smith discusses "The early history of radio astronomy in Europe", and although his canvass spans England, France, Germany, Hungary, the Netherlands, Norway and Russia, a mere thirteen pages is far too short a space to paint a detailed picture. Although it was pleasing to see photographs of some of the pioneers of European radio astronomy (e.g. Hachenberg, Hanbury Brown, Hey, Lovell, Ryle), I found the text rather superficial, based as it was (in large part) on data drawn from a small number of relatively well-known books. I have to admit that I came away feeling frustrated—Graham-Smith is a famous figure in British radio astronomy, and I was expecting much more.

Fortunately, the two following chapters provided better fare. In the first of these, Alastair Gunn discusses how the study of high-energy cosmic rays "... led to the establishment of Jodrell Bank as one of radio astronomy's founding institutions." In his text, Gunn uses published and archival sources to weave an intriguing tale of science, personalities and politics, extending from wartime radar research to early meteor work at Jodrell Bank, the development of the 218-ft transit instrument, and eventually the 250-ft radio telescope.

Bernard Burke's 30-page chapter on "Early years of radio astronomy in the U.S." provides further, welcome, intellectual sustenance. Jansky and Reber are well-documented by others (Sullivan, 1984, and Kellermann, 2005, respectively), so after quickly disposing of them, Burke introduces us to his early years in radio astronomy at the Carnegie Institution's Department of Terrestrial Magnetism, well-known for the 22 MHz 'Mills Cross' that he and Franklin used to discover Jovian decametric emission. Drawing in his personal knowledge of the U.S. 'scene', Burke then discusses the January 1954 'Washington Conference', which ultimately led to the formation of the NRAO. Along the way, personalities and politics entered the fray, including the power struggle between Merle Tuve and Lloyd Berkner that is deemed to have delayed the establishment of the NRAO by up to a year. Burke then highlights developments by the early radio astronomy groups at Caltech, Harvard and the Naval Research Laboratory, before returning once more to the NRAO and the sagas surrounding the design and construction of the 140-ft and 300-ft radio telescopes. At the time, Burke was serving on the NRAO Advisory Committee, and he found the experience "... both painful and educational." (page 41)! After a diversionary tale about the discovery of quasars (involving both Palomar and Parkes observations), Burke returns to his main theme and summarizes the Lincoln Lab's development of its Haystack 120-ft dish, before discussing early research into the CMB by Penzias, Wilson and Dicke, and the lost Washington opportunity: had fate played a different hand, Burke believes that Hagen's NRL group would have discovered the CBM back in the 1950s. Burke then brings his chapter to an end by discussing the concept of aperture synthesis, the torturous steps that led ultimately to the construction of the VLA, and early attempts at VLBI. All in all, I found this a masterful chapter, and it is a 'must' for anyone seeking a thumbnail sketch of early developments in U.S. radio astronomy. It covers considerable territory, and is enriched throughout by anecdotes and quotes that reveal Burke's personal knowledge of—and, in many cases, his direct involvement in—the various topics that he discusses.

In addition to the three foregoing contributions, some of the astrophysics chapters include valuable historical perspectives. For instance, Bignall, de Bruyn and Jauncey reach back to the 1960s in their discussion of variable extragalactic radio sources; Wielebinski reminds us that the concept of magnetic fields can be traced back more than 3,000 years to the Chinese; Taylor summarizes early Galactic H-line studies; Konovalenko & Stepkin, and Booth, respectively, provide valuable overviews of early work on recombination lines and Galactic masers; Wilson and Batrla discuss the pioneering days of 'radio astrochemistry'; and in his chapter on "Next generation space VLBI" Hirobayashi takes us back to early terrestrial VLBI experiments, and introduces the Radioastron and VSOP projects.

Radio Astronomy from Karl Jansky to Microjanksy is an attractive book and a credit to the editors. It is well laid out and very readable (notwithstanding the technical nature of some of the content). Another notable feature of the volume is the large number of illustrations, many of them in colour. The only obvious limitation I noticed was the absence of an index, yet this is a minor quibble and in no way diminishes the overall value of this volume. It is an excellent reference work for astrophysicists and for historians of radio astronomy who wish to measure their own studies against more recent developments, and at €72 will be an affordable and valuable addition to many libraries.

References

Kellermann, K.I., 2005. Grote Reber (1911-2002): a radio astronomy pioneer. In Orchiston, W. (ed.). *The New Astronomy: Opening the Electromagnetic Window and Expanding our View of Planet Earth.* Dordrecht, Springer. Pp. 43-70.

Sullivan, W.T. III (ed.), 1984. *The Early Years of Radio Astronomy. Reflections Fifty Years after Janksy's Discovery.* Cambridge, Cambridge University Press.

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In Synchrony with the Heavens. Studies in Astronomical Timekeeping and Instrumentation in Medieval Islamic Civilization. Volume 1: The Call of the Muezzin (Studies I-IX). Volume 2: Instruments of Mass Calculation (Studies

***X-XVIII)*, by David A. King (Leiden, Brill, 2004 & 2005), pp. 930 & 1,068, ISBN 9004122338 (Volume 1) & 900414188X (Volume 2) (Hardback), €380(2 volumes).**

The five prayer times in Islam are based on the astronomical position of the Sun in the sky. They are calculated based on the length of the shadow and the start and the end of the twilight during the day. No Western scholar knows more about the history of regulating the time schedules for these prayers, or the determination of the direction of Mecca, than David A. King, the Professor of History of Science at Goethe University in Frankfurt.

His book *In Synchrony with the Heavens*, which is in two volumes, contains a series of studies that was written by the author over a period of thirty years. Many of the papers in this book have been published before in various journals. However, several chapters of this work are published here for the very first time. They are based on more than five hundred Arabic manuscripts unearthed by the author in libraries around the world that had never been studied before. Dr King confirms that most of the material in this book will be new to many western readers. Surprisingly, he also mentions that some of this material will also by new to many Muslim readers who are unfamiliar with Western writings on the history of Islamic science.

The first volume of this book is titled *The Call of the Muezzin*, and it is divided into several parts. The first and second parts of this volume are surveys of tables for timekeeping by the Sun and stars and the regulation of astronomically-defined times of Muslim prayer for the period between ninth to the nineteenth centuries. The third and fourth sections describe the arithmetical shadow-schemes for time-reckoning, as well as the definition by legal scholars of the times of prayer in Islam. The role of the *Muezzin* and the *Muwaqqit* in medieval Islamic societies is described in part five of this volume. In part six, Dr King writes about the universal solutions to problems of spherical astronomy in Islamic astronomy, and provides examples of universal solutions from *Mamluk* Syria and Egypt. Another aspect of Dr King's work has been in explaining the orientation of medieval Islamic architecture and cities. This combination of architecture and astronomy is revealed in the orientation of the ventilators of medieval Cairo and in the *Safavid* world-maps which were centered on Mecca. These topics are discussed in detail in part seven of this book, where, as in part eight, the author highlights aspects of practical astronomy in mosques and monasteries. inally the last part of this volume, which is titled "When the night sky over Qandahar was lit only by stars ...", is a study of several tables that were found in an astronomical handbook (*Zij*) dating from around AD 1000 written by the astronomer ibn Labban.

The second volume of this book is tilted *Instruments of Mass Calculation*. It opens in part ten with a survey of the astronomical instruments used by Muslim astronomers for over a millennium. The next section of this volume, part eleven, explains the approximate formula for timekeeping which was used for many instruments from the eighth century until the nineteenth century. The author goes on to describe in the next part of this book the use of the universal horary quadrant for timekeeping by the Sun and stars. In the following parts of this volume Dr King conducts several studies on early selected Islamic astrolabes. He gives detailed descriptions of many instruments dating between the eighth and tenth centuries which were found in Baghdad as well as many others which are still preserved in museums and private collections around the world. This second volume then concludes with a detailed checklist of medieval Islamic and European astronomical instruments pre-dating AD 1500 ordered chronologically by region.

Several years ago Dr King coined the term "Astronomy in the Service of Islam", although it is a philosophical debate as to whether it is more accurate to consider Islam‘s service to astronomers which might better describe the significance of this religion in opening up new branches of astronomical activity in the Islamic civilization. The message that the author always tries to convey throughout his work is summarized in his words: "... the material presented here makes nonsense of the popular modern notion that religion inevitably impedes scientific progress, for in this case, the requirements of the former actually inspired the progress of the latter for centuries."

Finally, I would like to recommend this book to those who are really involved in the study of the history of religious Islamic astronomy. This book is a purely scholarly endeavour, and is by no means a light read. As the nature of this work is a collection of studies, the information is sometimes repeated in a number of different papers. However, Dr King's works are always a delight to read. His knowledge in his field is unequaled today.

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***The Cosmic Century: A History of Astrophysics and Cosmology*, by Malcolm Longair (Cambridge, Cambridge University Press, 2006), pp. xvi + 545, ISBN 978-0-521-47436-8 (Hardback), AU$90.**

The Cosmic Century is unusual in that it is really two books in one. The first book, while focussing on the development of astrophysics and cosmology in the twentieth century, starts by discussing key nineteenth century developments in photography and spectroscopy. The photographs of thousands of stellar spectra led to a classification scheme that, when connected with stellar colour, directly led to the HR Diagram and subsequently to a basic understanding of stellar physics by the time of the Second World War.

This book divides the historical developments broadly into those that occurred before the Second World War, and those that occurred afterwards. Longair makes clear that while the discoveries made before the War depended on nineteenth century technology, those afterwards often depended on new technologies operating at wavelengths other than in the visible region of the electromagnetic spectrum. Radio astronomy is probably the best example. While Jansky and Reber did pioneering work in the 1930s, it was mostly ignored by the astronomical community, and radio astronomy did not 'take off' until the War created both the trained people and equipment that could be used in this new science.

The 'second book', so to speak, is the detailed and clear explanation of the technical developments which, by themselves, could almost make an upper-level undergraduate astrophysics textbook. Longair also includes about fifty pages of explanatory notes where derivations or further details are given to concepts discussed in the text. Fifty-six pages are given to references, so if needs be the interested reader can go to the literature for more information.

This book is more than the sum of the two above-mentioned parts: it is an opportunity to learn astrophysics and cosmology from the point-of-view of what astrophysicists and cosmologists were thinking about as the science developed. The writing is always clear, and this book would make an excellent supplement for an upper-level astrophysics course. Even a less-prepared reader would get a lot out of it if they skipped the more mathematical sections.

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***Journal of the Antique Telescope Society*, #27-#28, 2006 [Special Alvan Clark Issue], pp. 44, US$20:00. Copies of this issue of the *Journal* can be obtained from the Executive Secretary of the Society, Dr Walter Breyer (for details e-mail him at: whbreyer@alltel.net).**

The Antique Telescope Society was founded in 1990 to "... unite colleagues interested in antique telescopes, binoculars, books, and related items; and to promote the membership's interests in astronomical history and discovery, the history of optics, and the preservation and use of these instruments through stewardship and education."

One of the most valued benefits of Society membership is the *Journal*, and the latest issue deserves special mention. This is a 44-page double number devoted solely to that distinguished American telescope-maker, Alvan Clark.

While Alvan Clark's principal telescopes are well-known and have been brilliantly documented by Warner and Ariail (1995), remarkably little has been written about his early years. In a bid to remedy this, the special 2006 Alvan Clark issue of the *Journal of the Antique Society* contains an introductory paper (by *Journal* Editor, Trudy E. Bell), and the following seven contributions:

- Early Clark I: Alvan Clark's Letters to Boston Newspaper Editors, 1847- 1851 (by Craig B. Waff)
- Table of Alvan Clark's Known Pre-Factory Refracting Telescopes (by Craig B. Waff and Robert B. Ariail)
- Early Clark II: *Scientific American* Coverage of Clark's Pre-Factory Career, 1849–1860 (by Trudy E. Bell)
- Alvan Clark Bicentennial at Mount Auburn Cemetery (by Richard Koolish and Kenneth J. Launie)
- Early Clark III: The Loomis and Clark Connection, 1850–1855 (by Ian R. Bartky and Robert B. Ariail)
- Early Clark IV: William Leitch's 1861 Visit to Alvan Clark's Workshop (by Robert A. Garfinkle)
- Early Clark V: Maria Mitchell's 1872 Notes on Alvan Clark and Telescope Making (by Trudy E. Bell and Robert B. Ariail)

Between them, these well-illustrated papers provide a wealth of new information about Alvan Clark, and throw new light on the early-Clark era. They are essential reading for anyone interested in the history of telescope-making in the U.S.A.

References

Warner, D.J., and Ariail, R.B., 1995. *Alvan Clark & Sons: Artists in Optics*. Richmond, Willmann-Bell.

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