JOURNAL OF ASTRONOMICAL HISTORY AND HERITAGE

VOL. 10 NO. 1 March 2007

JOURNAL OF ASTRONOMICAL HISTORY AND HERITAGE
ISSN 1440-2807

The *Journal of Astronomical History and Heritage* (*JAH*^{2}) was founded in 1998, and from 2007 is 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} is 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.

A 'Guide for Authors' is on the *JAH*^{2} web site ('History Astro. Journal') at

www.jcu.edu.au/astronomy

and should be followed carefully when preparing manuscripts. Papers and book reviews should be e-mailed to the Editor, Dr Wayne Orchiston (Wayne.Orchiston@jcu.edu.au), or posted to him at

Centre for Astronomy,
James Cook University,
Townsville,
Queensland 4811,
Australia.

Enquiries concerning subscriptions, review copies of books, advertising space, back numbers or missing issues of the *Journal* also should be directed to Dr Orchiston.

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

AU$200:00 for institutions
AU$88:00 for individuals

COVER PHOTOGRAPH

Portrait of William Doberck (1852–1941), the Danish-born astronomer who, after graduating with a Ph.D. in astronomy from the University of Jena, served as Superintendent of the Markree Observatory in Ireland before accepting the founding Directorship of the Hong Kong Observatory. A committed double star observer, Doberck continued his micrometric measures of selected pairs from his private 'Kowloon Observatory' in Surrey (England) after his retirement in 1907. Between 1872 and 1935, Doberck published 223 papers and reports, and apart from double stars, these discussed cometary orbital elements and variable star observations. For details of Doberck's long life and his contributions to astronomy see pages 49-64 in this issue of the journal.

JOURNAL OF ASTRONOMICAL HISTORY AND HERITAGE
ISSN 1440-2807

VOLUME 10 NUMBER 1 MARCH 2007

CONTENTS

Published by the
Centre for Astronomy, James Cook University, Townsville, Queensland 4811, Australia.
(www.jcu.edu.au/astronomy)

Journal of Astronomical History and Heritage, 10(1), 3-10 (2007).

HIGHLIGHTING THE HISTORY OF FRENCH RADIO ASTRONOMY. 1: NORDMANN'S ATTEMPT TO OBSERVE SOLAR RADIO EMISSION IN 1901

Suzanne Débarbat
SYRTE, Paris Observatory, 61 av de l'Observatoire, 75014 Paris, France.
E-mail: suzanne.debarbat@obspm.fr

James Lequeux
LERMA, Paris Observatory, 61 av de l'Observatoire, 75014 Paris, France.
E-mail: james.lequeux@obspm.fr

and

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

Abstract: Soon after the discovery of radio waves by Hertz in 1886 the idea that the Sun must emit this radiation was suggested. A number of scientists from different nations then attempted to detect this emission, and one of these was the French astronomer, Charles Nordmann. This paper provides biographical information on Nordmann before discussing his attempt to detect solar emisson in 1901 and the reasons he was unsuccessful.

Keywords: solar radio emission, Charles Nordmann, Johannes Wilsing, Julius Scheiner, Sir Oliver Lodge.

1. INTRODUCTION

The founding of radio astronomy is conventionally traced back to the pioneering efforts of Jansky and Reber during the 1930s (see Kellermann, 2005; Sullivan, 1984), but Woody Sullivan (1982: 141) is quick to remind us that the idea that the Sun emits radio waves emerged soon after 'hertzian waves' were discovered. During the critical decade from 1891 to 1901 a number of different scientists attempted to detect solar radio emission. One of these was the French astronomer, Charles Nordmann, and this short paper provides biographical material about him before critically examining the ambitious research project that he mounted in 1901.[1]

2. CHARLES NORDMANN: A BRIEF BIOGRAPHICAL SKETCH

Charles Nordmann (Figure 1) was born in Saint-Imier, Switzerland, on 18 May 1881 (Esclangon, 1941), but moved to France early in life, both of his parents being of French extraction (for localities mentioned in the text see Figure 2). We know nothing about his schooling, but in 1899 he received his 'Licence ès sciences',[2] and the following year he accepted an honorary position at Meudon Observatory in Paris (Nordmann, 1911).

Obviously Nordmann was totally committed to astronomy, for June 1902 saw him appointed as an astronomer at Nice Observatory, heading the Magnetic Service. Being interested in solar astronomy, he was able to carry out a variety of investigations in this field (e.g. on the periodicity of sunspots, the solar corona, geomagnetism, possible solar effects on the compass, and the *aurora borealis*). In 1903, soon after turning 22 years of age, Nordmann was awarded the title of Docteur ès Sciences for his thesis *Essay on the Role of Hertzian [=Radio] Waves in Physical Astronomy and on Various Related Issues* (Nordmann, 1903). This also reflected his solar focus, but went even further by announcing his interest in the possibility of radio emission from celestial bodies. We will return to this topic in Section 3.

While there is no definite evidence of this, Nordmann may not have been happy at Nice Observatory, for in July 1903 we find him based at Paris Observatory working in an honorary capacity whilst retaining his Nice appointment (Loewy, 1904). He continued in this same vein the following year (Loewy, 1905), but must have subsequently severed links with Nice for in 1905 he was appointed by the Bureau of Longitudes to lead a solar eclipse expedition to northern Africa. He then showed that he was only too willing to expand his solar horizons by carrying out geomagnetic mapping of Algeria and Tunisia (Nordmann, 1911).

Figure 1: Charles Nordmann (1881–1940), (after Berget and Rudaux, 1923: 242; Françoise Launay Collection).

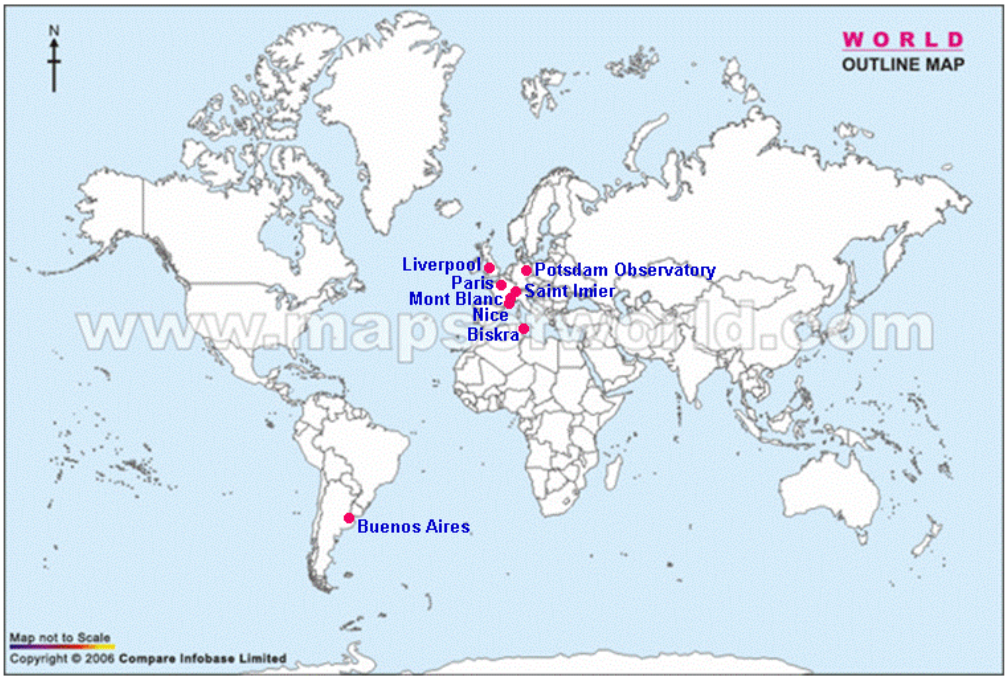


Figure 2: Localities mentioned in the text (outline map courtesy of www.theodora.com/maps, used with permission).

Later in 1905, after returning from Africa, Nordmann finally joined the staff of Paris Observatory in an official capacity, as an astronomer (Loewy, 1906), and he continued to work there until 1940 (Esclangon, 1941). Soon after starting at Paris Observatory he had to quickly broaden his research portfolio, so that he could head a mission to Biskra (Algeria) in 1907 and carry out stellar photometry (Nordmann, 1911). His publications over the next five years included papers on such diverse topics as atmospheric physics, terrestrial magnetism, Comet 1P/Halley, variable star research, stellar photometry, stellar physics, stellar parallaxes and even the dispersion of light in interstellar space, and these were published in a variety of journals, including *Astronomische Nachrichten*, *Comptes Rendus de l'Académie des Sciences*, *Revue Générale des Sciences* and *Terrestrial Magnetism and Atmospheric Electricity* (Nordmann, 1911). The lists of research papers by staff included in the various Paris Observatory *Annual Reports* confirm that Nordmann was an active researcher and a prodigious publisher.

Nordmann served with distinction during the First World War, and at the end of hostilities returned to Paris Observatory, where he then proceeded to devote much of his time to research in stellar photometry. For this he assembled his own 3-colour liquid filters,[3] which he attached to a modified Zöllner photometer. This instrument was used with the Observatory's 27-cm 'Petit coudé' telescope (which was destroyed at the beginning of the 1970s).[4] It is interesting that Nordmann's work is discussed on no fewer than seven different pages in Hearnshaw's (1996) authoritative history of astronomical photometry.

During his lifetime, Nordmann organised various conferences, and he received a variety of honours and awards. In 1907 and 1908 he was a laureate of the French Academy of Sciences, and in 1912 was appointed *Chevalier de la Légion d'honneur*. The previous year he had become a Professor at the School of Clockmaking and Mechanical Precision. In 1920 he was promoted to the post of *Astronome titulaire* (Senior Astronomer) at Paris Observatory, and in December of that year received the Prize of the Academy of Sciences for his research on stellar photometry. In 1928 he ventured abroad to deliver a course on astrophysics at the University of Buenos Aires in Argentina (Nordmann, 1928).

Charles Nordmann died prematurely on 28 August 1940 after a long and difficult illness (Esclangon, 1941); he was just 59 years of age.

Figure 3: Henri Deslandres, 1853–1948 (after Berget and Rudaux, 1923: 63; Françoise Launay Collection).

3. NORDMANN'S ATTEMPT TO DETECT SOLAR RADIO EMISSION

Henri Deslandres (Figure 3) was probably the first French astronomer to think about the emission of radio waves from the Sun. In 1889 Deslandres joined the staff of Paris Observatory specifically in order to develop astrophysics, which was a rather new field of research in France at the time (see Débarbat et al., 1990; Véron, 2005). He worked at Paris Observatory until 1897, when he transferred to Meudon Observatory (Michard, 1971). In about 1900, Deslandres became aware that the Sun could emit radio waves (see Deslandres and Décombe, 1902), and it is not unreasonable to suppose that he discussed this matter with Nordmann when they met.

Be that as it may, on 19 September 1901 Nordmann carried out a carefully-planned attempt to detect radio emission at hectometric wavelengths (i.e. at a frequency in the range 0.3–3 MHz) from a 3,100 m site at Grands-Mulets, on the slopes of Mont Blanc, in the Alps (see Figure 2). His reasoning in selecting this site is interesting. Although atmospheric absorption is actually negligible at this wavelength,

> The choice of an elevated site for this research was definitely indicated since it eliminated to the largest extent possible the absorbing action of the atmosphere and above all water vapour on the hypothetical [radio] waves ... (Nordmann, 1902a: 273; our translation).

Nordmann and his assistant, an electrical engineer by the name of F. Haberkorn (Nordmann, 1902a: 275),[5] set up their 175 m long antenna (which was mostly sensitive to wavelengths between 100 and 1,000 m) on the surface of the Bossons Glacier, supported at intervals by wooden posts. The antenna was obviously oriented N-S, so that "… towards midday the solar rays were normal to it." (Nordmann, 1902a: 273). Furthermore,

> The choice of a glacier to support the antenna was a very important one ... The glacier can, in effect, be considered a near-perfect isolator… which at the same time is transparent to radio waves; furthermore, another reason is that the thickness of the ice at the place where we erected the antenna (on the basis of crevasses that we found) was estimated to be at least 25 m and the solar rays were, at the time of our experiments (the summer equinox), very inclined from the vertical, so there would be little error caused by interference between solar rays received directly and those reflected by the underlying ground surface onto the aerial. (Nordmann, 1902a: 273-274; our translation).

However the statement that the glacier can be considered as a 'near-perfect isolator' is wrong, and the reflector was only a small fraction of a wavelength below the antenna wire. In this condition the beam maximum was approximately at right angle to the wire, a favorable position since the Bossons Glacier was roughly perpendicular to the Sun's elevation at transit at the date of the observation. The antenna was not tapped to a tuned circuit and accepted a broad range of frequencies, with the sensitivity and to some extent the beam direction dependent upon frequency.

The receiver developed by Nordmann and Haberkorn consisted of a Branly 'radioconductor',[6] immersed in a vessel containing mercury in order to protect this detector from 'external Hertzian waves' (see Figure 4). The antenna was connected to the radioconductor, and an insulated wire, F_1, led from the radioconductor to a galvanometer and a battery. A non-insulated wire, F_2, completed the circuit by linking the battery to the mercury. Two different equally-sensitive radioconductors were used for the solar experiments: one consisted of nickel filings and the other of 30 small steel balls in mutual contract. It was expected that the resistance of these radioconductors would change if solar radio emission was detected, and this would be revealed by deflections of the galvanometer needle.

On 19 September Nordmann and Haberkorn observed the Sun throughout the day, and although the weather was beautiful and the sky was cloudless they did not detect any solar radio emission. Nordmann (1902a: 275; our translation) concluded that

> ... the Sun does not emit electromagnetic radiation at long wavelengths that are capable of making an impression on our radio receivers, or that if it does emit such radiation this is completely absorbed by the solar atmosphere and the upper regions of the Earth's atmosphere.

Nordmann (1902a) reported the results of this investigation in a 3-page paper titled "Recherche des ondes hertziennes émanées du Soleil." (i.e. "Research on radio waves emanating from the Sun"), which was published in *Comptes Rendus de l'Académie des Sciences* in 1902, and this immediately inspired Deslandres and Décombe to assemble a paper on the topic, which appeared in the same journal later that same year.

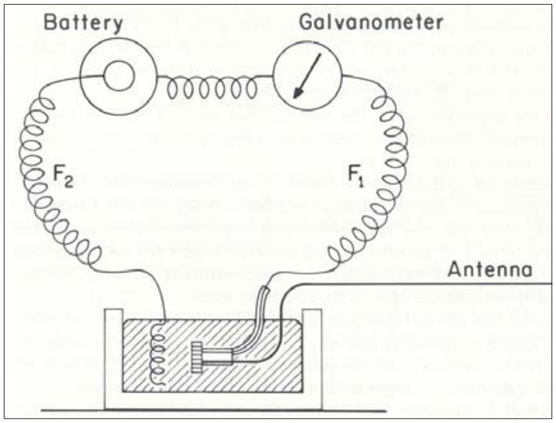


Figure 4: The radio receiver used by Nordmann and Haberkorn at the Bossons Glacier, Mont Blanc, in September 1901 (after Sullivan, 1982: 159; cf. Nordmann, 1902a: 274).

The Deslandres and Décombe paper starts by pointing out that the search for solar radio emission has been in progress since 1895, and that "The Earth does not continuously receive detectable [solar] radio emission, at wavelengths similar to those used in telegraphy (i.e. between 10m and 1000m)." (Deslandres and Décombe, 1902: 528; our translation). The authors then mention attempts made by Wilsing and Scheiner to detect solar radio emission between 1896 and 1899 and Nordmann's recent paper, and make the point that "This negative result is less surprising if one notes that, here on Earth, incandescent substances that emit light and heat do not normally emit radio waves." (ibid). However, the authors suggest that the chromosphere and prominences emit radio waves through a mechanism that is comparable to the electrical discharges that occur in the Earth's atmosphere, and although much of this emission is absorbed by the solar and terrestrial atmospheres it is quite likely that a small percentage of this radiation does in fact reach the surface of the Earth. Deslandres and Décombe (1902: 529) then discuss eruptive prominences and postulate that they are associated with long-wave radio emission that causes storms here on Earth and disrupts telegraphic communication. And with considerable optimism—we might add—they suggest that the study of solar radio emission will eventually become the domain of regular solar astronomers. Finally, they conclude with the prophetic statement: "… a long series of observations will be necessary in order to finally decide if the surface of the Earth does receive radio emission from the Sun." (Deslandres and Décombe, 1902: 530; our translation).

Immediately following this paper by Deslandres and Décombe is a second paper by Nordmann (1902b) which elaborates on his earlier contribution and—as the title suggests—discusses a variety of celestial phenomena that may be explained by invoking radio emission.[7] But first, Nordmann (1902b: 530; our translation) begins by discussing his Mont Blanc result:

> The negative nature of the result that I obtained in the course of carrying out the experiments on Mont Blanc and which I outlined in a recent note to the Academy can be explained by the fact that the solar electromagnetic radiation was entirely absorbed by the upper rarified regions of the Earth's atmosphere.

This is a particularly perceptive comment, for it anticipates by several decades our current thinking on solar radiation and its penetration of the terrestrial atmosphere at different wavelengths.

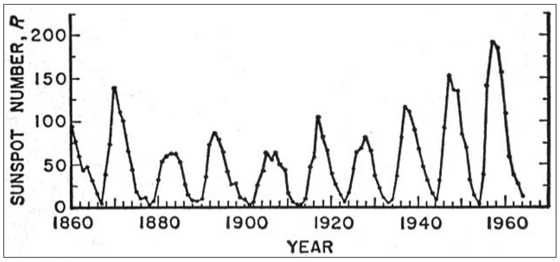


Figure 5: Zurich sunspot numbers between 1860 and 1960 (adapted from Smith, 1967: 28). Note that Nordmann's 1901 observations were made at sunspot minimum.

Nordmann then discusses spectral studies of the chromosphere and prominences, and suggests that the extremely intense electrical discharges with which they are associated undoubtedly also generate radio waves. More specifically:

> The surface of the Sun must emit radio waves, and this emission must be particularly intense from those regions where violent eruptions occur and at periods when these eruptions are at a maximum, that is to say from regions with sunspots and faculae at times of maximum solar activity. (Nordmann, 1902b: 531; our translation).

From here, Nordmann proceeds to discuss the solar corona, the way in which its form changes in the course of the solar cycle, and its association with solar radio emission:

> The physical agent which makes the coronal gases incandescent is of electrical origin: these gases are illuminated by solar radio waves that conform the to known properties of such emission, and the [coronal] illumination is most intense during sunspot maximum, precisely at the time when this emission is at its greatest intensity. (Nordmann, 1902b: 532; our translation).

Nordmann then briefly turns his attention to comets, and suggests that radio waves are responsible for the luminescence of gases in the tails of different comets. This brings his 3-page paper to a end.

In 1903 Nordmann submitted his Doctoral thesis to the University of Paris, and its title—*Essai Sur le Rôle des Ondes Hertziennnes en Astronomie Physique et sur Diverses Questions qui s'y Rattachent*—suggests that we might expect new material on solar radio emission to be included. However, this proves not to be the case: unfortunately, only the contents of his two 1902 papers are reproduced.

From this point, Nordmann's solar investigations remained pretty much forgotten until the early 1950s when they were noted by the Institute of Astrophysics astronomer, M. Laffineur (1952), in his doctoral thesis, but it was only in 1967 that they received international exposure when Alex Smith (1967) mentioned them in his book, *Radio Exploration of the Sun*. Woody Sullivan took Nordmann's work to an even wider audience in 1982 when he included English translations of Nordmann's first 1902 paper and the follow-up paper by Deslandres and Décombe in his *Classics in Radio Astronomy*. Sullivan (1982: 145) described Nordmann's Mont Blanc project as "… a remarkable experiment …"

In spite of Sullivan's publicity, Nordmann's work is little known to present-day astronomers.

4. DISCUSSION

Why was Nordmann's 1901 experiment unsuccessful? There are a number of factors to consider. Even at the very long wavelengths at which he chose to operate, solar bursts of spectral types III and occasionally II do occur (and can be observed from space), but they are rare during the solar minimum, which just happened to coincide with when Nordmann made his observations (see Figure 5). On the other hand, 0.3-3 MHz radio waves are reflected by the Earth's ionosphere at intermediate latitudes, and only stand a chance of penetrating through to the Earth's surface when solar activity is minimal and at special locations (such as Tasmania). Despite his own misgivings, the receiver that Nordmann (1902b) used probably did have the sensitivity to detect energetic solar bursts. Sullivan (1982: 146) believes—perhaps somewhat optimistically—that Nordmann was unlucky: "If it had not been a time of solar minimum or if he had been persistent enough to observe for more than one day, he might well have succeeded and thereby drastically changed the history of astronomy." To the contrary, we feel that Nordmann had no chance of detecting solar radio bursts, because of the inappropriate wavelength range that he selected.

As we have seen, Nordmann's Mont Blanc investigation was partly inspired by Wilsing and Scheiner's unsuccessful attempt to detect solar radio emission in 1896. Johannes Wilsing (1856–1943) and Julius Scheiner (1858–1913) were two well-known astrophysicists from the Potsdam Observatory, and they attempted to observe solar radiation using the simple 'receiver' shown in Figure 6. Wilsing and Scheiner describe their equipment which, contrary to that of Nordmann, was only sensitive at centimetric and decimetric wavelengths:

> … when choosing a method for the detection of electric solar radiation, the highest possible sensitivity was important. We considered as particularly suitable the method … based on the changes in galvanic resistance, discovered by Herr Lodge [in Liverpool, England], which are initiated by electric oscillations incident on two metals loosely in contact.
>
> … For these experiments we inserted into the circuit of a cell both a multiplier [an old type of galvanometer], whose pair of 6-cm long needles had an oscillation time constant of 10 sec, and a "bridge" sensitive to electric oscillations. The bridge consisted of a steel wire, a few millimeters thick and several centimeters long, which had been loosely laid over two other steel wires of similar dimensions, thus closing the circuit ...
>
> We achieved a complete isolation against … [local interference] only when the bridge, the galvanometer, the cell, and the conducting wires were all enclosed in a box covered with tin foil.
>
> … we had to keep the [radio] waves away from the contact points of the wires without enclosing the galvanometer and the cell in the box … and we achieved this in the following manner. On the upper side of a cube-shaped sheet metal box we cut an opening 100 cm^2 in area through which we could bring the bridge into the box. The opening was then closed again by means of a tight fitting metal lid. From the bridge a conducting wire led to the inside wall of the box at which point on the outside a wire was soldered which led to the cell. The second wire, which connected the bridge with the cell, led from the cell first to the center of a metal plate 25 cm^2 in size and then was insulated as it went to the bridge through a small opening in the side of the box. That side of the box was covered with a thin layer of paper which prevented the passage of current from the above mentioned, tightly fitting metal plate to the box. (Wilsing and Scheiner, 1896; cited in Sullivan, 1982: 148-150).

On eight different days between 23 June and 11 July 1896 Wilsing and Scheiner (Sullivan, 1982: 156) used this device to try and detect solar radio emission:

> … we directed solar rays reflected from the metal mirror of a heliostat towards the box. With the lid of the box removed, the rays then struck the bridge. When the heliostat mirror was covered with black paper, a highly sensitive thermopile at the same location as the bridge exhibited only a small heating effect. This effect could be made entirely imperceptible by inserting a paper screen [between the mirror and the thermopile] ...
>
> First of all, a strong effect, in the sense of a decrease in resistance, was exhibited with the mirror uncovered … [but] these changes continued for a long time after the radiation was stopped ...
>
> In order to measure the resistance changes, we used a Wheatstone bridge connected with a Siemens galvanometer whose bell-shaped magnets had been replaced by a lighter system … The movement of the reflected image of the scale, 2 m distant from the galvanometer, was monitored with a telescope. After determining the resistance of the bridge, we measured the sensitivity … by inserting a known small resistance into the same section [of the circuit]. The box was then opened and the effect of the radiation on the resistance observed. Finally, we checked the sensitivity of the bridge by excitation of the spark gap. (Wilsing and Scheiner, 1896; cited in Sullivan, 1982: 154-155).

Wilsing and Scheiner presented their results in a table (see Sullivan, 1982: 156), and concluded that

> These experiments led to no positive results. If we separate the domain of the investigated oscillations from that of the heat radiation by requiring that the oscillations have the ability to an appreciable extent to penetrate a non-conductor, then it has not been possible to measure in these experiments the amount of energy from any such solar radiation. (Wilsing and Scheiner, 1896; cited in Sullivan, 1982: 155).

Having said that, Wilsing and Scheiner (ibid.) caution that "Due to the possible screening effect of our atmosphere … this does not mean that we can deduce the absence [of electrodynamic oscillations] in the original complex of rays emitted by the Sun." In this case, the terrestrial atmosphere does not absorb radio waves, but the small size of the heliostat mirror and of the antenna enormously limited the sensitivity.

At about the same time Wilsing and Scheiner were researching solar radio emission, Sir Oliver Lodge (1851–1940) was carrying out parallel investigations in England. Sir Oliver was a multi-talented scientist, and he writes that some time between 1897 and 1900

> I [hoped] to try for long-wave radiation from the sun, filtering out the ordinary well-known waves by a blackboard or other sufficiently opaque substance. I did not succeed in this, for a sensitive coherer in an outside shed unprotected by the thick walls of a substantial building cannot be kept quiet for long. I found its spot of light liable to frequent weak and occasionally violent excursions, and I could not trace any of these to the influence of the sun. There were evidently too many terrestrial sources of disturbance in a city like Liverpool to make the experiment feasible. I don't know that it might not possibly be successful in some isolated country place; but clearly the arrangement must be highly sensitive in order to succeed. (Lodge, 1900: 33; cited in Sullivan, 1982: 141).

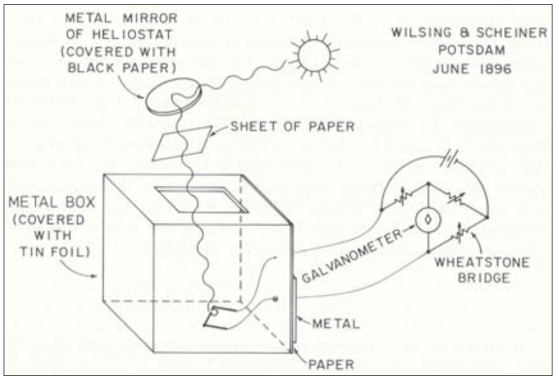


Figure 6: The 'radio receiver' used by Wilsing and Scheiner in 1896 (after Sullivan, 1982: 145).

Let us now return to Nordmann. Despite his unsuccessful attempt to detect solar radio emission, he was convinced that such radiation existed, and on this basis over the next two years he proceeded to write four other research papers about what we would now term 'radio astronomy.' The first of these examined the relevance of radio emission to astrophysics (Nordmann, 1902c), while another paper published in 1902 briefly examined the possible relevance of solar radio emission to aurorae and the magnetic field of the Earth (Nordmann, 1902e). Then in two later papers, Nordmann (1904a; 1904b) expanded considerably on material first presented in some of his 1902 papers and in his 1903 doctoral thesis. One of his conclusions, although plainly wrong, is particularly interesting:

> I think that the aurora borealis is a phenomenon produced in the [Earth's] atmosphere by radio waves emanating from the Sun … (Nordmann, 1902e: 592; our translation).

Nordmann was aware that his 1901 investigation was conducted during sunspot minimum, and he planned to carry out further observations in 1904—when the Sun would be more active. However, this was not to be. From comments he makes in a 1902 paper on nebulae (Nordmann, 1902d) and in his doctoral thesis (Nordmann, 1903), it is obvious that Nordmann did not support some of Deslandres' scientific conclusions, and it is equally clear that this senior French scientist did not enjoy the controversy generated by the young 'upstart' (e.g. see Deslandres, 1902: 1486). The friction between these two men and Deslandres' special interest in solar radio emission might explain why Nordmann never carried out the mooted 1904 investigation, and why he turned to the totally 'neutral' research field of stellar photometry when he joined the staff of Paris Observatory in 1905.

5. CONCLUDING REMARKS

In 1901, Charles Nordmann was the first French astronomer, and one among only a handful of international scientists, to search unsuccessfully for radio emission from the Sun. Although various factors contributed to this negative result, the primary causes were the rarity of burst activity at this time of the solar cycle, his decision to only make observations on just the one day, and above all the very long wavelength at which he chose to search. As it was, it took three more decades before developments in instrumentation saw the launch of radio astronomy, but even then another decade would pass before scientists would detect solar radio emission for the first time. When this did eventually occur, separate independent wartime discoveries were made in Australia, Britain, New Zealand, Norway and the USA (Duerbeck, 1996; Orchiston, 2005; Orchiston and Slee, 2002; and Sullivan, 1984), and the early pioneering efforts of Nordmann and his contemporaries were long forgotten. France, meanwhile, would only begin to make an international contribution to solar radio astronomy in the late 1940s, after WWII (see Denisse, 1984; Orchiston and Steinberg, 2007).

Nordmann was a remarkable scientist, who contributed in many ways to astronomy and geomagnetism (Esclangon, 1940). While his premature foray into solar radio astronomy in 1901 is what primarily concerns us here, others will remember him for the important contributions he made to stellar photometry, while those who research Algol variables will undoubtedly recall the Nordmann-Tikhov Effect (Hearnshaw, 1996: 371-373; Kulikovsky, 1976: 408-409).

6. NOTES

1. This is the first in a series of research papers documenting the early development of French radio astronomy. The second paper in this series is in this same issue of *JAH*^{2}, and deals with radio observations made during a series of solar eclipses in the 1940s and 1950s (see Orchiston and Steinberg, 2007).
2. In today's terminology, this would probably be the equivalent of a Bachelor of Science degree.
3. For a description of the Zöllner photometer see Hearnshaw (2000) and Staubermann et. al. (2000). For a diagram of Nordmann's photometer see Hearnshaw (1996: 102).
4. The 'Petit coudé', which was built in 1882, should not be confused with Paris Observatory's much better-known 'Grand coudé' (completed in 1890), which had an aperture of 60 cm. 'Coudé' comes from the word *coude* (elbow, in English). This type of instrument is mounted equatorially, with the tube comprising two components at right angles to one another.
5. Unfortunately, we were unable to assemble any biographical material about the mysterious Mr Haberkorn.
6. Édouard Branly (1844–1940) discovered in 1890 that an imperfect contact between metallic substances covered by a very thin oxide layer strongly loses its resistance to electricity when submitted to radio waves (Branly, 1890). The resistance is restored by a shock. He called this imperfect contact *radioconducteur*, but in 1894 Sir Oliver Lodge coined the term *coherer* for it. The corresponding set-up, made generally of iron filings and completed by a mechanical striker to restore its resistance after captation of the radio signal, was used for several years for wireless telecommunication experiments in Morse language by Lodge, Alexander Popov (1859–1906) and Branly himself. The theory of the Branly coherer is only now beginning to be understood (see Falcon and Castaing, 2005).
7. Given that the two radio receivers were critical to the whole experiment, it is interesting that Nordmann chose not to include Haberkorn as a co-author—at least of the initial paper.

7. ACKNOWLEDGEMENTS

We are grateful to Francoise Launay (Paris Observatory) for providing Figures 1 and 3 and for searching unsuccessfully through Janssen archives for material relating to Nordmann; and to Laurence Bobis, Josette Alexandre, Danièle Destombes, Sandrine Marchal, Dominique Monseigny and Robert Zeganadin (Paris Observatory Library), and Graeme White (James Cook University), for their assistance.

In particular, one of us (WO) would like to acknowledge the Australia-France Co-operation Fund in Astronomy (Australia), Program of International Collaboration in Science (France), Paris Observatory and James Cook University, for funding this project.

Finally, we wish to thank Monique Pick and Jean-Louis Steinberg (Paris Observatory), Bruce Slee (Australia Telescope National Facility), Hilmar Duerbeck (Muenster University) and Richard Wielebinski (Max Planck Institut für Radioastronomie) for reading and commenting on the manuscript.

8. REFERENCES

Berget A. and Rudaux L., 1923. *Le Ciel*. Paris, Larousse.

Branly, E., 1890. Variations de conductibilité sous diverses influences électriques. *Comptes Rendus de l'Académie des Sciences*, 111, 785-787.

Débarbat, S., Grillot, S., and Lévy, J., 1990. *Observatoire de Paris – Son Histoire 1667-1963*. Paris, Observatoire de Paris.

Denisse, J-F., 1984. The early years of radio astronomy in France. In Sullivan, W.T. (ed.). *The Early Years of Radio Astronomy. Reflections Fifty Years after Jansky*. Cambridge, Cambridge University Press. Pp. 303-315.

Deslandres, H., 1902. Extension de l'hypothèse cathodique aux nébuleuse. *Comptes Rendus de l'Académie des Sciences*, 134, 1486-1487.

Deslandres, H., and Décombe, H., 1902. Sur la recherche d'un rayonnement hertzien émané du Soleil. *Comptes Rendus de l'Académie des Sciences*, 134, 527-530. See, also, Sullivan, 1982: 161-163.

Duerbeck, H., 1996. 100 Jahre solare Radioastronomie. *Die Sterne*, 72, 314-323.

Esclangon, E., 1941. *Rapport Annuel sur l'État de l'Observatoire de Paris Pour l'Année 1940*. Paris, Imprimerie Nationale.

Falcon, É., and Castaing, B., 2005. Electrical conductivity in granular media and Branly's coherer: a simple experiment. *American Journal of Physics,* 73, 302-307.

Hearnshaw, J., 1996. *The Measurement of Starlight. Two Centuries of Astronomical Photometry*. Cambridge, Cambridge University Press.

Hearnshaw, J., 2000. Nineteenth century visual photometers and their achievements. In Sterken, C., and Staubermann, K. (eds.). *Karl Friedrich Zöllner and the Historical Dimension of Astronomical Photometry*. Brussels, VUB Univeresity Press. Pp. 19-37.

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.

Kulikovsky, P.G., 1976. Tikhov, Gavril Adrianovich ... In Gillespie, C.C., (ed.). *Dictionary of Scientific Biography. Volume XIII*. New York, Scribners. Pp. 408-409.

Laffineur, M., 1952. *Contribution à l'Étude du Rayonnement Électromagnétique du Soleil sur Ondes Décimétriques*. Thesis presented to the Faculty of Sciences, University of Paris [copy in Paris Observatory Library].

Lequeux, J., 2005. *L'Univers Dévoilé*. Les Ulis, EDP Sciences.

Lodge, O.J., 1900. *Signalling Across Space Without Wires*. Third Edition. London, The Electrician Publishing Co.

Loewy, M., 1904. *Rapport Annuel sur l'État de l'Observatoire de Paris pour l'Année 1903* … Paris, Imprimerie Nationale.

Loewy, M., 1905. *Rapport Annuel sur l'État de l'Observatoire de Paris pour l'Année 1904* … Paris, Imprimerie Nationale.

Loewy, M., 1906. *Rapport Annuel sur l'État de l'Observatoire de Paris pour l'Année 1905* … Paris, Imprimerie Nationale.

Michard, R., 1971. Deslandres Henri ... In Gillespie, C.C., (ed.). *Dictionary of Scientific Biography. Volume IV*. New York, Scribners. Pp. 68-70.

Nordmann, C., 1902a. Recherche des ondes hertziennes émanées du Soleil. *Comptes Rendus de l'Académie des Sciences*, 134, 273-275. See, also, Sullivan, 1982: 158-160.

Nordmann, C., 1902b. Explication de divers phénomènes célestes par les ondes hertziennes. *Comptes Rendus de l'Académie des Sciences*, 134, 530-533.

Nordmann, C., 1902c. Recherche sur le rôle des ondes hertziennnes en astrophysique. *Revue Générale des Sciences*, 13, 379-388.

Nordmann, C., 1902d. Sur la constitution des nébuleuses. *Comptes Rendus de l'Académie des Sciences*, 134, 1282-1285.

Nordmann, C., 1902e. Théorie électromagnétique des aurorae boréales et des variations et perturbations de magnétisme terrestre. *Comptes Rendus de l'Académie des Sciences*, 134, 591-594.

Nordmann, C., 1903. *Essai sur le Rôle des Ondes Hertziennes en Astronomie Physique et sur Diverses Questions qui s'y Rattachent*. [Thèse présentée à la Faculté des Sciences de Paris.] Paris, Gauthier-Villars.

Nordmann, C., 1904a. Le rayonnement hertzien du Soleil et les aurores boréales. *Journal de Physique Theoretique et Appliquee*, Avril, 281-316.

Nordmann, C., 1904b. Le rayonnement hertzien du Soleil et l'influence de l'activité solaire sur le magnétisme terrestre. *Journal de Physique Theoretique et Appliquee*, Fevrier, 97-121.

Nordmann, C., 1911. *Notice sur les Travaux Scientifiques*. Paris, Gauthier-Villars.

Nordmann, C., 1928. *Notice sur les Travaux Scientifiques*. Paris, Philippe Renouard.

Orchiston, W., 2005. Dr Elizabeth Alexander: first female radio astronomer. In Orchiston, W. (ed.). *The New Astronomy: Opening the Electromagnetic Window and Expanding our View of Planet Earth*. New York, Springer. Pp. 71-92.

Orchiston, W., and Slee, B., 2002. The Australasian discovery of solar radio emission. *AAO Newsletter*, 101, 25-27.

Orchiston, W., and Steinberg, J-L., 2007. Highlighting the history of French radio astronomy. 2: The solar eclipse observations of 1949-1954. *Journal of Astronomical History and Heritage*, 11, 11-19.

Smith, A.G., 1967. *Radio Exploration of the Sun*. Princeton, Van Nostrand.

Staubermann, K., Berger, L., Boettcher, H., Borchardt-Ott, W., Buzzoni, B., Franza, F., Frercks, J. Griffin, R., Herrmann, D.B., Heyer, H., Nair, A., Pettersson, B., Ruland, H., Sterken, C., and Wolfschmidt, G., 2000. The replication of an original Zöllner-photometer. In Sterken, C., and Staubermann, K. (eds.). *Karl Friedrich Zöllner and the Historical Dimension of Astronomical Photometry*. Brussels, VUB Univeredsity Press. Pp. 59-70.

Sullivan, W.T., 1982. *Classics in Radio Astronomy*. Dordrecht, Reidel.

Sullivan, W.T. III (ed.), 1984. *The Early Years of Radio Astronomy. Reflections Fifty Years after Jansky*. Cambridge, Cambridge University Press.

Véron, P., 2005. Préhistoire de l'Observatoire de Haute Provence. In Boistel, G (ed.). *Observatoires et Patrimoine Astronomique Français*. Lyon, SFHST, ENS Editions (Cahiers d'Histoire et de Philosophie des Sciences, No. 54). Pp. 117-134.

Wilsing, J., and Scheiner, J., 1896. Ueber einen Versuch, eine electrodynamische Sonnenstrahlung nachzuweisen, und über die Aenderung des Uebergangswiderstandes bei Berührung zweier Leiter durch electrische Bestrahlung. *Annalen der Physik und Chemie*, 59, 782-792. See, also, Sullivan, 1982: 147-157.

Dr Suzanne Débarbat began working in fundamental astronomy at Paris Observatory in 1953, but changed her research focus to the history of astronomy in 1975. Now retired, she continues her historical work as an astronomical associate of the Department SYRTE at the Observatory. Her main research interests relate to post-seventeenth century European (but particularly French) astronomy. She has more than 200 publications. A long-time member of the IAU, she was President of Commission 41 from 1991 to 1994. She is a committee member of the IAU Working Groups on Astronomical Archives and Historic Radio Astronomy, and is on the Editorial Board of the *Journal of Astronomical History and Heritage*.

Dr James Lequeux belongs to the second generation of radio astronomers, who were physicists rather than radio engineers, but still lacked training in astronomy. He started research in radio astronomy in 1954, and, together with Dr Jean-Louis Steinberg, in 1960 produced the first text book written in French on radio astronomy (which was soon translated into English and Russian). Lequeux founded the first infrared astronomy group in France in 1966. After a career in radio astronomy and in various fields of astrophysics, mostly at Paris-Meudon Observatory, his post-retirement interests turned to the history of astronomy, as an associate of the Department LERMA at the Observatory. His book *l'Univers Dévoilé* (2005) is a history of astronomy from 1910 to the present, and is one of the few that mentions Nordmann's pioneering experiments.

Dr Wayne Orchiston is a Senior Lecturer in Astronomy at James Cook University, Townsville. His main research interests relate to Cook voyage, Australian and New Zealand astronomical history, with emphasis on the history of radio astronomy, comets, historically-significant telescopes, early astronomical groups and societies, and transits of Venus. He has published extensively, and has edited the book *The New Astronomy. Opening the Electromagnetic Window and Expanding our View of Planet Earth* (2005). He also has a book on early Australian radio astronomy, co-authored by Woody Sullivan and Jessica Chapman, which will be published by Springer in 2007. He is the Chair of the IAU Working Group on Historic Radio Astronomy.

Journal of Astronomical History and Heritage, 10 (1), 11-19 (2007).

HIGHLIGHTING THE HISTORY OF FRENCH RADIO ASTRONOMY.
2: THE SOLAR ECLIPSE OBSERVATIONS OF 1949-1954

Wayne Orchiston
Centre for Astronomy, James Cook University, Townsville,
Queensland 4811, Australia.
E-mail: Wayne.Orchiston@jcu.edu.au
and
Jean-Louis Steinberg
Paris Observatory (Meudon), Place Jules Janssen,
F-92 195 Meudon Cedex, France.

Abstract: During the 1940s and early 1950s radio astronomers from a number of nations used observations of total and partial solar eclipses to investigate the positions of radio-emitting regions and to determine the distribution of radio emission across the solar disk. Between 1949 and 1954 French radio astronomers from the Ecole Normale Supérieure and the Institute of Astrophysics between them mounted four successful eclipse expeditions to Africa and northern Europe. This short paper lists the personnel involved, discusses their instrumentation, describes the observations made, and evaluates the significance of these observations in an international context.

Keywords: French solar eclipse expeditions, Ecole Normale Supérieure, Institute of Astrophysics, Paris, Markala, Dakar, Khartoum, Högby, solar corona.

1. INTRODUCTION

In the decade following World War II solar radio astronomy took great strides, as important research was carried out in Australia, England, France, Holland, Japan and Russia (e.g. see Edge and Mulkay, 1976; Orchiston et al., 2006; Strom, 2004; and Sullivan, 1984).

Arguably the most important early solar research conducted by the fledgling French radio astronomers at the Ecole Normale Supérieure (henceforth ENS) and the Institute of Astrophysics (IA) in Paris during the late-1940s and early-1950s was associated with a series of solar eclipses.[1]

At this time the angular resolution of radio telescopes was poor, and observations of total and partial solar eclipses offered a particularly elegant way of pinpointing the positions of localised regions responsible for solar radio emission. The reasoning was that as the Moon's limb moved across the Sun's disk and successively occulted and then unmasked different radio-emitting regions there would be associated dips and rises in the chart record. More than one observing site was desirable in that any dip in the chart record obtained at a single site would simply indicate that the emitting region was located *somewhere* along the arc subtended by the lunar limb *at that particular moment*. In contrast, by using several widely-spaced observing sites the intersections of the different limb profiles allowed the precise positions of the radio-emitting regions to be determined (e.g. see Christiansen et al., 1949a). As an added bonus, from observations of solar eclipses radio astronomers could also determine the distribution of radio brightness across the disk of the Sun and the shape of the corona at radio wavelengths. Dicke and Beringer (1946) were the first to pioneer the use of this technique in radio astronomy when they carried out observations of a partial solar eclipse on 9 July 1945.

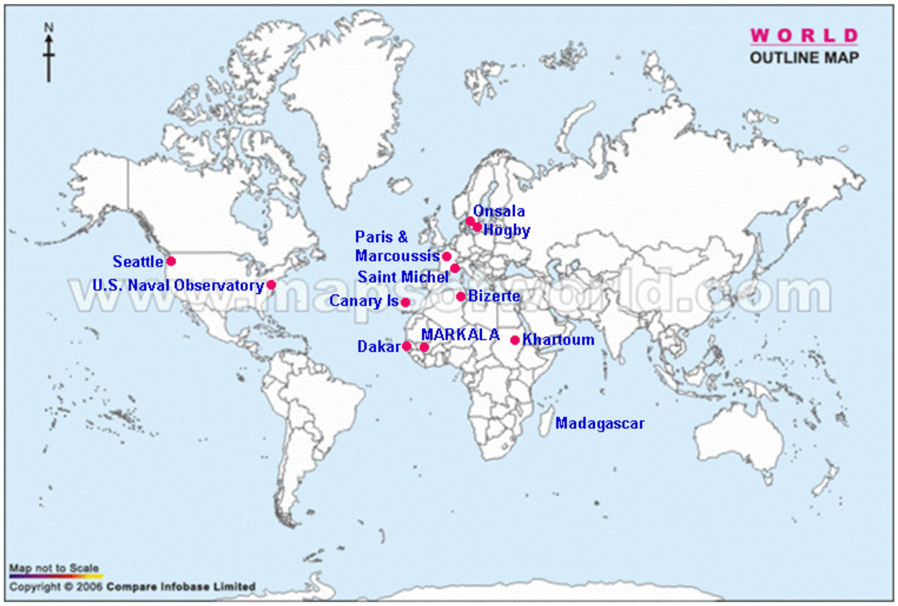


Figure 1: Map of localities mentioned in the text (outline map courtesy of www.theodora.com/maps, used with permission).

Figure 2: The 7.5m Würzburg antenna at Marcoussis used for the solar eclipse observations (courtesy: Observatoire de Paris, Meudon).

French interest in solar eclipses at this time was part of a world-wide phenomenon: other nations that mounted expeditions in the late 1940s and early 1950s to monitor variations in radio emission were Australia, Canada, England, Japan, Russia and the USA (see Hey, 1955 for a full list, and associated references).[2]

This paper focusses on four different solar eclipses that attracted French radio astronomers between 1949 and 1954. Observations were made from France and from different sites in Europe and Africa. For localities mentioned in the text see Figure 1.

Figure 3: The 7.5m Würzburg antenna at Meudon used for the solar eclipse observations (courtesy: Observatoire de Paris, Meudon).

2. THE DIFFERENT ECLIPSE EXPEDITIONS

2.1 The April 1949 Eclipse

On 28 April 1949 a partial solar eclipse was visible from Paris, with just 26% of the disk masked at mid-eclipse, and this event was observed by staff from the ENS and the IA (see Laffineur et al., 1949, 1950; Steinberg, 1953). Three different radio telescopes and frequencies were used. Steinberg and Zisler from the ENS used an equatorially-mounted 3m dish on the roof of the Physics Laboratory and a 7.5m Würzburg antenna located at Marcoussis (Figure 2) near Paris, which operated at 1,200 MHz and 158 MHz, respectively, while Laffineur from the IA accessed the 7.5m Würzburg antenna sited at Meudon Observatory in Paris (Figure 3), which was tuned to a frequency of 555 MHz. All three radio telescopes were equipped with specially-developed low-noise receivers, but this instrumentation "… was better adapted to the study of energetic solar emission [i.e. bursts] rather than precise continuum measurements. " (Laffineur et al., 1949: 1636; our translation). During the eclipse, Hα spectroscopic observations were also made at Meudon Observatory.

Despite the comparatively 'primitive' nature of the equipment, successful eclipse observations were made at all three frequencies, with the solar flux levels reducing by about 50%, 20% and 21% at 158, 555 and 1,200 MHz respectively (Laffineur et al., 1949). However, at 158 MHz fluctuations in the noise levels of ~20% were recorded both before and after the eclipse, so it was impossible to distinguish these variations during the eclipse from those that were associated with the masking and unmasking of features on the solar surface. For this reason, the radio astronomers decided not to subject the Marcoussis data to detailed analysis.

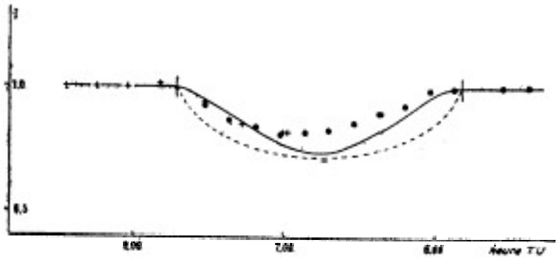


Figure 4: The 28 April 1949 eclipse curve. Dots represent measurements at 555 MHz and crosses at 1,200 MHz. The solid curve indicates the profile expected from a disk of uniform brightness, while the dashed line shows the expected profile if the radio emission derived from an annular ring (after Laffineur et al., 1950: 338).

Instead, Laffineur et al. (1950) published an eclipse curve that combined the results obtained at 555 and 1,200 MHz, and this is reproduced here in Figure 4. While several small sunspots were present at the time of the eclipse and were masked and unmasked by the lunar limb, the eclipse curve is far too crude to reveal any obvious variations in flux density levels; however, Laffineur et al (ibid.) did not note any such deviations during the eclipse observations.

Michard, from the IA, was responsible for the analysis of the eclipse curve, and this proved illuminating. He noted that the start and end times of the radio event did not differ markedly from those of the optical eclipse, suggesting that the radio Sun at these two frequencies was not appreciably larger than its optical counterpart. We now know this finding to be anomalous, and it would soon be challenged by subsequent French eclipse expeditions.

Meanwhile the shape of the Paris eclipse curve also was "… incompatible with the hypothesis of a [radio] Sun of uniform brightness." (Laffineur et al, 1950: 339; our translation) or an annular disk of uniform brightness. Rather the eclipse curve suggested that "It is necessary to suppose that at least a part of the solar radio emission derived from non-uniform sources distributed over the Solar disk." (ibid.). As we have noted, Michard was not able to associate this with the distribution of sunspots, so he proceeded to consider chromospheric plages, as observed in Hα with a spectroheliograph at Meudon. Upon reviewing the relative areas and intensities of the various plages present at the time of the 1949 eclipse, Servajean was able to generate a 'plage eclipse curve', and this was found to provide a better—but by no means precise—fit to the radio eclipse curve, as shown in Figure 5. It was noted that this finding matched that of the Soviet radio astronomers, Khaikin and Chikhachev, when they observed the 20 May 1947 solar eclipse. Michard found that the 1949 eclipse demonstrated that "… an important fraction of the solar emission at decimeter wavelengths is generated by chromospheric plages. Note however that this conclusion rests on features that are at the very limits of possible detection ..." (Laffineur et al, 1950: 341; our translation). The authors concluded their paper by cautioning that the interpretation of radio data from relatively small-phase partial solar eclipses like the Paris one raises notable difficulties, so any results should be viewed as interesting, but no more than provisional. These would prove to be prophetic words.

2.2 The September 1951 Eclipse

In 1951 radio astronomers from the ENS observed an annular solar eclipse from a remote site in Africa (Arsac et al., 1953; Blum et al, 1952a, 1952b; Bosson et al., 1951; Denisse et al., 1952). The previous year Denisse had received a 1951 astronomical ephemeris which listed an annular solar eclipse on 1 September 1951, with the zone of totality extending from the Canary Islands to Madagascar and traversing the African continent. Along the path of totality was the small town of Markala, on the Niger River in French Sudan, 1,500km to the east of Dakar (see Figure 1). From a scientific viewpoint this was an ideal observing site: nearby there was a dam with locks, so the town was an industrial centre and included a metal-working shop.

A successful funding application[3] was made to the Bureau of Longitudes, but additional funds were required so Y. Rocard (Director of the Physics Laboratory at the ENS) proceeded to obtain Naval support. As a result, 6 tons of equipment were transferred by the Navy to Dakar, and then taken by train and truck to Markala. Meanwhile, the eclipse team of Blum, Denisse, Le Roux and Steinberg, plus two Naval personnel, flew directly from Paris to Dakar, and then transferred to Markala by train and road.[4]

The instruments used at Markala to observe this eclipse were an equatorially-mounted 1.5m diameter searchlight mirror attached to a 9,350 MHz receiver (Figure 6) and a 169 MHz ex-US radar antenna (Figure 7). Blum et al (1952a: 186; our translation) provide a useful description of the latter instrument: "… This equatorially-mounted antenna comprises an array of 16 half-wave dipoles placed in front of two flat reflectors: the support comprises the main component of a suitably inclined old American SCR 268 radar. The antenna has a half-power beamwidth of 9º in declination and 25º in right ascension. This low directivity allows for a manual pointing of the antenna."

While both antennas were purchased from the US Army after the War,[5] the radio telescopes of which they formed a part were totally new instruments that were developed for this eclipse expedition; they were not existing instruments that were used in Paris or at Marcoussis for regular solar monitoring at this time.

Markala was just north of the central line of totality, and on 1 September 1951 the eclipse lasted from 11h 20m to 14h 39m UT. At mid-eclipse, 97.5% of the solar disk was masked by the Moon.

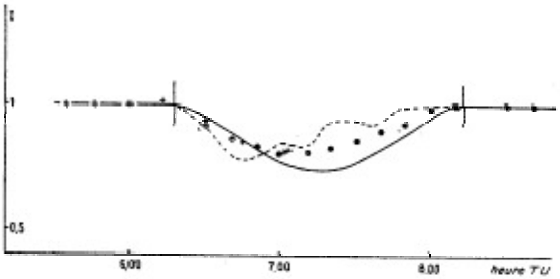


Figure 5: The 28 April 1949 eclipse curve and the expected profile if the emission was directly associated with Hα plages (the dashed line) (after Laffineur et al., 1950: 340).

The primary aim of the radio observations was to "… obtain eclipse curves of the solar emission at 169 MHz (1.78 m wavelength) and at 9 350 MHz (3.20 cm wavelength) and then to deduce the brightness distribution of solar emission at these wavelengths as well as the positions and strengths of possible more-or-less localised sources of emission." (Blum et al., 1952a: 184; our translation). There was also an added interest: "… to compare the total measurements made by Hagen, Haddock and Reber in 1950 with annular eclipse observations … [as] such a comparison may prove to be interesting for limb-brightening studies." (Denisse et al., 1952: 191).

Figure 6: Setting up the Markala 'searchlight antenna' (courtesy: Observatoire de Paris, Meudon).

Thanks to excellent meteorological conditions and an absence of solar burst activity[6] at the time (Bosson et al., 1951), successful observations of the eclipse were made at Markala, but instrumentation problems meant that some of the observations at 9,350 MHz made after the mid-phase of the eclipse could not be used in the subsequent analysis. The resulting eclipse curves are reproduced in Figures 8 and 9. Denisse et al. (1952: 192) note that the 169 MHz result eclipse curve "… is the first published on a metre wave-length in a period of solar radio quietness."

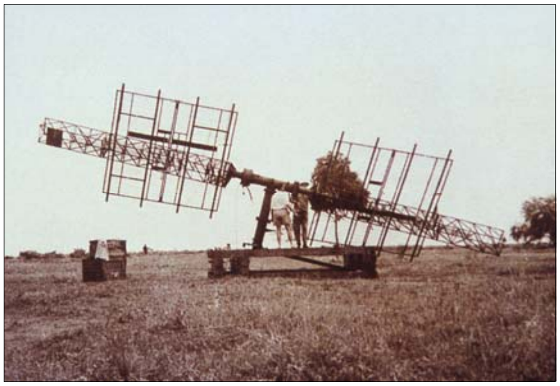


Figure 7: The Markala 169 MHz radar antenna (courtesy: Observatoire de Paris, Meudon – Archives de Nançay).

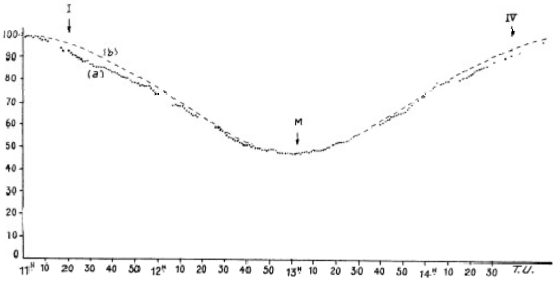


Figure 8: The 1 September 1951 169 MHz eclipse curve (a), and the profile expected for a uniformly bright disk of 1.35 solar radii. The ordinate shows relative intensity of the emission and abscissa Universal Time (after Blum et al., 1952a: 190).

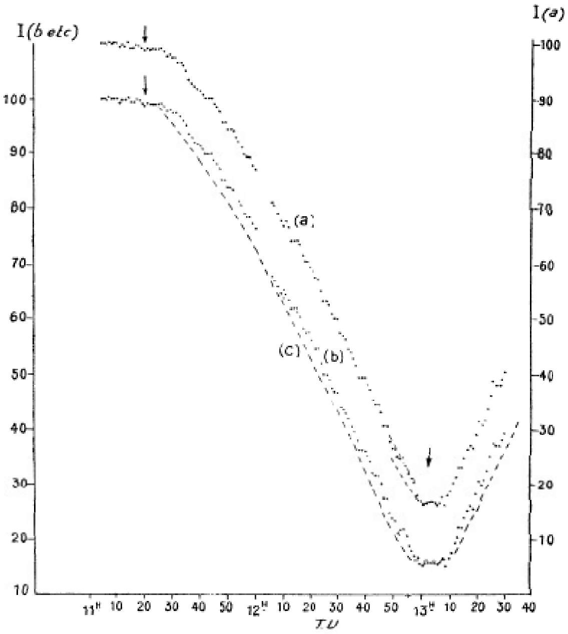


Figure 9: The 1 September 1951 9,350 MHz eclipse curves. (a) shows the uncorrected curve, and (b) after correction for the presence of sunspots. The dashed curve, (c), is the profile expected for a uniformly bright disk of 1.07 solar radii. The ordinate shows relative intensity of the emission and abscissa Universal Time (after Blum et al., 1952a: 190).

Optical observations made at the Schauinsland Observatory and at the U.S. Naval Observatory at the time of the eclipse revealed the existence of two small groups of sunspots on the solar disk, near the eastern limb (see Figure 8 in Blum et al., 1952a: 193). Interestingly, the 169 MHz eclipse curve shows a slight decrease in emission at about the time these two spots would have been covered by the lunar limb. Blum et al. (1952a) explain this fluctuation and others in the eclipse curve as due to receiver noise or interference, but measurements made at 169 MHz between 10 and 12 hrs UT on September 1-3 (inclusive) indicated that the intrinsic level of solar emission did not vary by >1% (Blum et al., 1952a: 191). This implies that some of the fluctuations that exceed 2% in the Figure 8 eclipse curve may be genuine and not artifacts, and could have been associated with radio-emitting regions that had no photospheric correlates at the time. In this context it is interesting to note that when they observed the 1 November 1948 partial solar eclipse, Australian radio astronomers found several localised sources of 600 MHz emission that had no optical counterparts but were associated with sites that had featured sunspot activity on the previous solar rotation.

The 9,350 MHz (b) eclipse curve was also interpreted in the light of the two small sunspot groups, with Blum et al. concluding that

> The decrease of 3% in the signal, which coincides with the occultation of the group of sunspots in question corresponds rather well to the preceding estimations [provided to the authors by J. Pawsey and F. Haddock].
>
> One can state that this occultation [of the sunspots] was very rapid and occurred in the short time of just one minute (from 12 h 58 to 12 h 59); this corresponded to the position of the Moon on the solar disk indicated in Figure 5, during which sunspot B was being occulted.
>
> While more important the occultation of sunspot A did not lead to any decrease in [radio] intensity. (Blum et al., 1952a: 192-193; our translation).

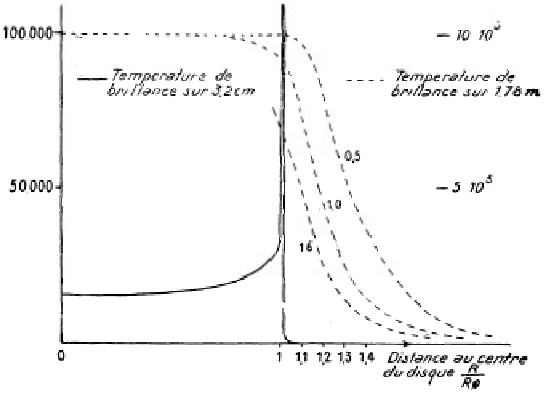


Figure 10: Calculated solar brightness temperature curves for 169 MHz (1.78m) and 9,350 MHz (3.2 cm) based on the eclipse curves in Figures 8 and 9 (after Blum et al., 1952a: 195).

The eclipse curves in Figures 8 and 9 were also used to investigate the distribution of radio brightness across the solar disk and the areal extent of the radio corona. At 169 MHz the radio event began 16 minutes before the first optical contact and ended well after the latter, confirming that emission at this frequency derives from the solar corona. Meanwhile, from the deviation between curves (b) and (c) in Figure 9, Blum et al. (1952a) were able to demonstrate that the radio Sun exhibited significant limb-brightening at 9,350 MHz (see Figure 10). This was a significant result (see Bosson et al., 1951), and built on D.F. Martyn's important paper of 1948.

When it comes to interpreting the 169 MHz eclipse curve, the distribution of radio brightness depends on the temperature of the corona, hence the three different values (1.6, 1.0 and 0.5 million degrees) represented in Figure 10.[7] The 169 MHz results showed the radio Sun at this frequency to be asymmetrical and in the form of a flattened ellipsoid with a radio diameter 1.4 times the equatorial diameter of the optical Sun (Bosson et al., 1951). To elaborate:

> An approximate model which takes account of our observations is indicated in Figure 8 [Figure 12, here]. It is evident that our observational curve does not allow us to be specific about the detailed distribution of the radiation: the proposed model only aims to bring out the most significant features of the asymmetry that are likely to represent the true distribution. (Blum et al., 1952a: 196; our translation).

As Blum et al. (1952a: 197) note in their concluding remarks, this is an entirely new result which alone justified this study.

2.3 The February 1952 Eclipse

On 25 February 1952 a solar eclipse was visible in Africa and Europe, and this was observed by ENS radio astronomers from Onsala, Paris, Bizerte and Dakar (Arsac et al., 1953; Blum, pers. comm., 2007; Blum et al., 1952b; Denisse et al., 1952), and by an IA team from Khartoum and Paris (Laffineur et al., 1952; Laffineur et al., 1954). The eclipse was seen as total in Khartoum, and was partial in Onsala, Paris, Bizerte and Dakar.

The equipment used at Dakar (French West Africa, see Figure 1) comprised the same 169 MHz radio telescope that was based at Markala the previous year (Denisse et al., 1952), while identical 169 MHz antennas were set up at Marcoussis (Blum et al., 1952b), and at Bizerte in Tunisia. Meanwhile, an ex-WWII Würzburg antenna was used at Onsala in Sweden (Blum, pers. comm., 2007).

Successful observations were made from all of these sites, and the resulting eclipse curves for Dakar and Paris are shown in Figure 11 (along with the 1951 169 MHz curve, for comparison). These 1952 results wholly confirmed the initial 1951 finding that at 169 MHz the radio Sun was an asymmetrical flattened ellipsoid (Figure 12). To elaborate, this figure shows that coronal emission from the equatorial regions was relatively more important than emission from the polar regions (Blum et al., 1952b) and

> … at the time of optical contacts, a decrease of 0-1 per cent was observed in Paris in 1952, 8.5 per cent in Markala in 1951 and 12-13 per cent in Dakar in 1952 … [Figure 12 presents] A tentative model of the radio-sun observed on a wavelength of 1.78 m … (Denisse et al., 1952: 192).

In his review of early radio eclipse observations, Hey (1955: 529) regards these combined 1951-1952 eclipse results as important, adding that: "Optical data have previously shown that the coronal density may be expected to vary with heliographic latitude, but the radio eclipse observations offer a useful means of studying the coronal distribution and its departure from spherical symmetry."

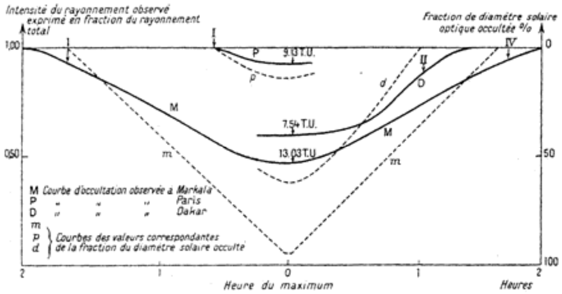


Figure 11: The 25 February 1952 169 MHz Paris (P) and Dakar (D) eclipse curves, plus the 169 MHz Markala curve (M) obtained in 1951 (after Blum et al., 1952b: 1597).

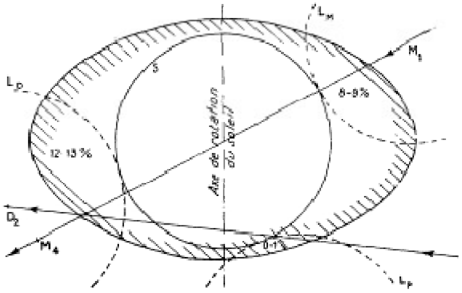


Figure 12: Form of the radio Sun at 169 MHz, based on a combination of the 1951 and 1952 solar eclipse curves (after Blum et al., 1952b: 1598).

In contrast to the modest Dakar exercise, the French mission to Khartoum under Laffineur's direction was a grand affair, and was liberally funded by the Bureau of Longitudes. The expedition involved optical and radio astronomy, and had three primary objectives: (1) to record solar radio emission at 255 and 555 MHz during the eclipse; (2) to photograph the solar corona at 5,303 Å and 6,374 Å with a Lyot coronograph; and (3) to carry out photometric, polarimetric and spectroscopic observations of the corona (see Laffineur, Michard, Pecker, Dollfus, Vauquois and d'Azambuja, 1954).

Figure 13: The 6m radio telescope at the Khartoum observing station (courtesy Dr A. Dollfus).

The radio observations at Khartoum (see Figure 1) were conducted by Laffineur, with occasional assistance from Michard and Pecker. The radio telescope used was a 6m diameter equatorially-mounted parabola (Figure 13) with twin dipoles for simultaneous operation at 255 and 550 MHz.

The eclipse was successfully observed, and at mid-eclipse the intensity of emission from that part of the corona not masked by the Moon's disk was 30.5 ± 1% of the total emission received from the non-eclipsed Sun at 255 MHz and 19.5 ± 1% of that normally received at 550 MHz (Laffineur et al., 1952). The derived eclipse curves are reproduced in Figure 14. Observations of coronal intensity at 5,303 Å were made at the time (see Figure 15), and when Laffineur et al. incorporated these into their analysis, the theoretical eclipse curve and the actual values obtained at 550 MHz were in remarkable conformity, as shown in Figure 16. Similar corrections for variations in coronal intensity were incorporated into the analysis of the 255 MHz data, and again there was an excellent correspondence between the observed eclipse curve and the theoretical curve (see Figure 17).

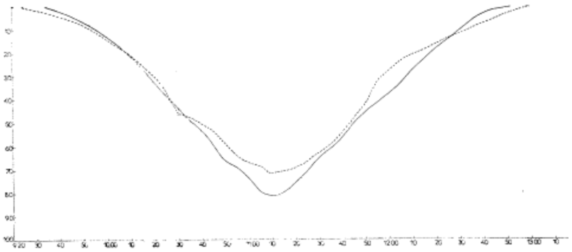


Figure 14: The 25 February 1952 Khartoum 255 MHz (dotted line) and 555 MHz (solid line) eclipse curves (after Laffineur, Michard, Pecker and Vauquois, 1954: 362).

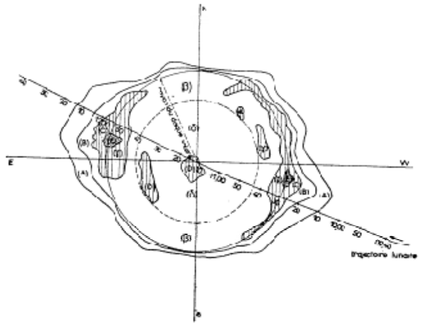


Figure 15: Isophotes of coronal intensity at 5,303Å derived from observations made at Pic du Midi and at Khartoum (after Laffineur, Michard, Pecker and Vauquois, 1954: 366).

While the Khartoum observations were in progress, parallel observations at 255 MHz were made with the 7.5m Würzburg antenna at Meudon. The radio astronomers noted that "At the maximum of the partial eclipse at radio wavelengths, 13 minutes after the optical event, the remaining radio emission was 83% that recorded when the Sun was not in eclipse." (Laffineur et al., 1952: 1529; our translation).

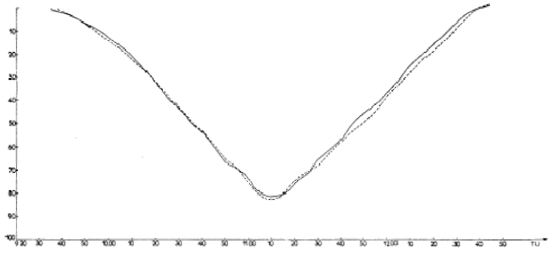


Figure 16: The 25 February 1952 555 MHz eclipse curve (solid line) and the theoretical curve (dotted line) corrected for localised variations in coronal intensity (after Laffineur, Michard, Pecker and Vauquois, 1954: 369).

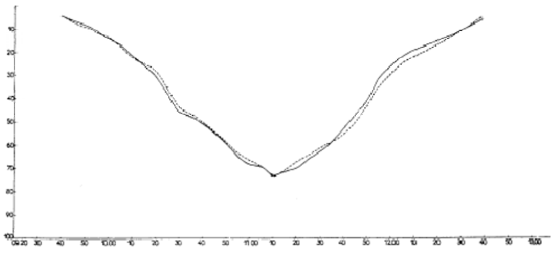


Figure 17: The 25 February 1952 255 MHz eclipse curve (solid line) and the theoretical curve (dotted line) corrected for localised variations in coronal intensity (after Laffineur, Michard, Pecker and Vauquois, 1954: 373).

2.4 The June 1954 Eclipse

On 30 June 1954 Laffineur's group from the IA in Paris observed a solar eclipse from Meudon (Paris) and Högby in Sweden (Laffineur, 1957; Laffineur et al., 1954). This eclipse was seen as total in Sweden and was partial in Paris. The Bureau of Longitudes once again provided funding for the overseas expedition.

The observing site at Högby (see Figure 1) was situated 8 km north of the line of totality, and the 6 m diameter radio telescope that had been used at Khartoum in 1952 was set up there. It again operated at 545 MHz, but a new equatorial mounting was required given the very different latitude of the observing site. The observers at Högby were Laffineur, Coupiac and Vauquois. Meanwhile parallel observations by Begot and Christiansen were carried out at Meudon with the 7.5 m Würzburg antenna, which operated at both 255 MHz and 545 MHz (Coupiac et al., 1955).

The corrected 545 MHz eclipse curve obtained at Högby is reproduced here in Figure 18, and Laffineur et al. note that it

> … presents fewer deviations than that observed two years previously; this is easily explained by the fact that the 30 June 1954 eclipse was associated with fewer active chromospheric and coronal regions than in 1952. It is remarkable to note that at the moment of totality the residual radiation was at 11% compared to 19.5% in 1952. (Laffineur et al., 1954: 1590; our translation).

Similar eclipse curves for 255 MHz and 545 MHz were obtained at Meudon (see Coupiac et al., 1955: 277), but no attempt was made to interpret any of these curves in terms of localised radio-emitting regions or the shape and size of the radio corona at 545 MHz.

3. DISCUSSION

By the time the 1949 eclipse occurred, four earlier solar eclipses had been observed at radio wavelengths (see Hey, 1955: 526-527), so the French radio astronomers were not the first to use partial or total solar eclipses as a means of investigating coronal physics. Yet despite the preliminary nature of their 1949 results, Steinberg (1953: 281; our translation) was proud to record: "To our knowledge, this is the first time that a partial eclipse of the Sun has been observed so intensively by means of radio astronomical techniques." Because of the notorious 'tyranny of distance' he was clearly unaware—at this time—of the 1 November 1948 eclipse, which was observed by five different teams of Australian radio astronomers from three quite separate geographically-spaced sites, and at three different frequences (see Christiansen et al., 1949a, 1949b; Minnett and Labrum, 1950; Piddington and Hindman, 1949).

Having said this, the French radio astronomers were the first to derive the form and areal extent of the radio corona at 169 MHz, when they analysed the 1951 and 1952 eclipses, while the way in which Laffineur accommodated variations in coronal intensity at 5,303 Å when deriving the expected eclipse curve at 555 MHz in 1952 was a particularly elegant piece of research.

One of the remarkable features of the 1949 eclipse was that it brought together radio astronomers from the Ecole Normale Supérieure and the Institute of Astrophysics, and even resulted in two joint publications. In general, there was a distinct element of rivalry between members of these two groups, so the eclipse collaboration was a notable anomaly. The fact that the event was visible from Paris and that neither institution decided to mount an expedition to attempt observations from the line of totality was an obvious factor, and it is telling that Laffineur and Steinberg mounted quite separate African expeditions in order to observe the 1952 eclipse (although the ENS initiative devolved quite naturally out of the 1951 eclipse program). The Bureau of Longitudes was instrumental in funding the Steinberg and Laffineur expeditions in 1951 and 1952 respectively, and it is interesting that no pressure was applied by this body, or the Academy of Science, to encourage collaborative expeditions on these occasions. Such scientific 'arm-twisting' was not unknown in other countries when the research potential of a particular major scientific investigation was obvious.

4. CONCLUDING REMARKS

Between 1949 and 1954 French radio astronomers from the Ecole Normale Supérieure and the Institute of Astrophysics in Paris observed four different solar eclipses. The 28 April 1949 partial eclipse was observed in Paris by a combined team from both institutes, and chromospheric plages were invoked to interpret the observed eclipse curve at 555 and 1,200 MHz. On 1 September 1951 a partial solar eclipse was observed from a site on the Niger River in Africa by an ENS team, and they were able to demonstrate that the radio corona at 169 MHz took the form of a flattened ellipsoid (that mirrored the shape of the optical corona at this time). Separate teams from the ENS and the IA observed the 25 February 1952 eclipse from Sweden and Paris and three different sites on the African continent, and on this occasion the ENS team was able to confirm the previously-reported elliptical nature of the radio corona, while Laffineur's group found that coronal irregularities went a considerable way towards explaining the nature of the eclipse curves obtained at 255 and 550 MHz. The final eclipse in this series observed by French radio astronomers was visible on 30 June 1954 and was monitored from Högby (Sweden) and Paris by Laffineur's group from the IA. While eclipse curves were obtained at both 255 and 545 MHz, no attempts were made to analyse these, and this marked the end of French interest in the radio properties of solar eclipses.

Radio astronomers at the ENS then went on to develop a range of different instruments that allowed them to investigate solar emission at various wavelength outside of eclipse, while those in the much smaller IA team threw their energies into constructing the Saint Michel Interferometer which was designed for Galactic and extragalactic research. These initiatives marked the launch of a campaign by French radio astronomers to develop sophisticated instrumentation dedicated to specific research programs and outputs (see Denisse, 1984). Gone were the days when surplus WWII equipment (as was used at Markala and Dakar) or small simple antennas (as employed at Khartoum and Högby) would suffice. French radio astronomy had entered a new era.

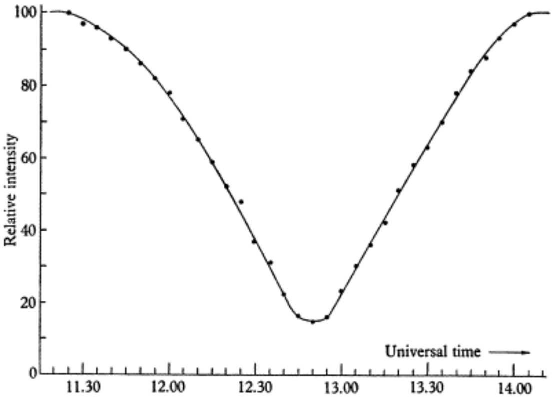


Figure 18: The 30 June 1954 545 MHz eclipse curve from Högby (after Laffineur, 1957: 304).

5. NOTES

1. This is the second in a series of papers documenting developments in early French radio astronomy. The first paper dealt with Nordmann's unsuccessful attempt to detect solar radio emission in 1901 (see Débarbat. Lequeux, and Orchiston, 2007).
2. Recently, the Australian observations were reviewed by Orchiston (2004) and Orchiston, Slee and Burman (2006), and a poster paper about the French *and* Australian eclipse programs was presented at the January 2007 meeting of the American Astronomical Society in Seattle (see Orchiston, Lequeux, Pick, Slee and Steinberg, 2007).
3. The Bureau of Longitudes provided a grant of 300,000 francs (Arsac et al., 1953).
4. Two other ENS staff members (Arsac and Lestel) and two navy personnel (Bosson and Seligman) were involved in building and testing the scientific equipment destined for the expedition.
5. The searchlight mirrors were particularly plentiful after the War, and were readily available.
6. Solar burst activity was most pronounced at frequencies below 200 MHz, but was rarely an issue at 9,350 MHz where the daily incidence of solar emission closely mirrored variations in sunspot area (e.g. see Minnett and Labrum, 1950: 65).
7. In 1946, Martyn and Pawsey published adjoining theoretical and observational papers in *Nature* establishing a coronal temperature of ~10^6 degrees at 200 MHz.

6. ACKNOWLEDGEMENTS

We are grateful to Laurence Bobis, Josette Alexandre, Danièle Destombes, Sandrine Marchal, Dominique Monseigny and Robert Zeganad (Paris Observatory Library), Emile-Jacques Blum and Andre Boischot (both formerly at Paris Observatory) and Sandra Ricketts (Anglo-Australian Observatory) for their assistance, and to Gérard Servajean at Meudon Observatory for supplying Figures 2, 3, 6 and 7 and A. Dollfus for kindly providing Figure 13.

In particular, we would like to acknowledge the Australia-France Co-operation Fund in Astronomy (Australia), Program of International Collaboration in Science (France), Paris Observatory and James Cook University for funding this project and allowing one of us (WO) to attend the January 2007 meeting of the American Astronomical Society where a poster paper on aspects of this research was presented.

Finally, we wish to thank James Lequeux and Monique Pick (Paris Observatory), and Bruce Slee (Australia Telescope National Facility), for reading and commenting on the manuscript. It was their encouragement and support that made this project possible.

7. REFERENCES

Arsac, J., Blum, E-J., Lestel, H., and Steinberg, J-L., 1953. Sur deux radiomètres U.H.F. (160 MHz et 9.350 MHz) et quelques applications radioastronomiques. *L'Onde Electrique,* 33, 527-532.

Blum, E.J., Denisse, J.F., and Steinberg, J.L., 1952a. Résultats des observations d'une éclipse annulaire de Soleil effectuées sur 169 MHz et 9350 MHz. *Annales d'Astrophysique*, 15, 184-198.

Blum, E-J., Denisse, J-F., and Steinberg, J-L., 1952b. Sur la forme ellipsoïdale du Soleil observé en ondes métriques. *Comptes Rendus de l'Académie des Sciences*, 234, 1597-1599.

Bosson, F., Blum, E-J., Denisse, J-F., Le Roux, E., and Steinberg, J-L., 1951. Observation de l'éclipse annulaire de Soleil du 1^er^ Septembre 1951. *Comptes Rendus de l'Académie des Sciences*, 233, 917-919.

Christiansen, W.N., Yabsley, D.E., and Mills, B.Y., 1949a. Measurements of solar radiation at a wavelength of 50 centimetres during the eclipse of November 1, 1948. *Australian Journal of Scientific Research*, A2, 506-523.

Christiansen, W.N., Yabsley, D.E., and Mills, B.Y., 1949b. Eclipse observations of solar radiation at a wave-length of 50 cm. *Natur*e, 164, 569-570.

Coupiac, P., Laffineur, M., and Vauquois, B., 1955. Observations radioélectriques de l'éclipse totale de Soleil du 30 Juin 1954. *Annales d'Astrophysique*, 18, 271-278.

Débarbat, S., Lequeux, J., and Orchiston, W., 2007. Highlighting the history of French radio astronomy. 1: Nordmann's attempt to observe solar radio emission in 1901. *Journal of Astronomical History and Heritage*, 10, 3-9.

Denisse, J-F., 1984. The early years of radio astronomy in France. In Sullivan, 303-315.

Denisse, J-F., Blum, E.J., and Steinberg, J-L., 1952. Radio observations of the solar eclipses of September 1, 1951, and February 25, 1952. *Nature*, 170, 191-192.

Dicke, R.H., and Beringer, R., 1946. Microwave radiation from the Sun and Moon. *Astrophysical Journal*, 103, 375-376.

Edge, D.O., and Mulkay, M.J., 1976. *Astronomy Transformed: The Emergence of Radio Astronomy in Britain*. New York, John Wiley.

Hey, J.S., 1955. Solar radio eclipse observations. *Vistas in Astronomy*, 1, 521-531.

Laffineur, M., 1957. Radio observations of the eclipse of 30 June 1954. In Van de Hulst, H.C. (ed.). *Proceedings from the 4th IAU Symposium*. Cambridge, Cambridge University Press. Pp. 304-306.

Laffineur, M., Michard, R., Pecker, J.C., and Vauquois, B., 1954. Observations optiques et radioélectriques de l'éclipse totale de Soleil du 25 Février 1952. III. Observations radioélectriques de la couronne. *Annales d'Astrophysique*, 17, 358-376.

Laffineur, M., Michard, M., Servajean, R., and Steinberg, J-L., 1950 Observations radioélectriques de l'éclipse de Soleil du 28 Avril 1949. *Annales d'Astrophysique*, 13, 337-341.

Laffineur, M., Michard, R., Steinberg, J-L., and Zisler, S., 1949. Observations radioélectriques de l'éclipse de Soleil du 28 Avril 1949. *Comptes Rendus de l'Académie des Sciences*, 228, 1636-1637.

Laffineur, M., Vauquois, B., Coupiac, P., and Christiansen, W.N., 1954. Observation de l'éclipse totale de Soleil du 30 Juin 1954 à Hagaby (Suède) et de l'éclipse partielle au radiotelescope de l'Observatoire de Meudon. *Comptes Rendus de l'Académie des Sciences*, 239, 1589-1590.

Laffineur, M., Michard, R., Pecker, J.C., Dollfus, A., Vauquois, B., and d'Azambuja, 1954. Observations optiques et radioélectriques de l'éclipse totale de Soleil du 25 Février 1952. *Annales d'Astrophysique*, 17, 317-319.

Laffineur, M., Michard, R., Pecker, J-C., d'Azambuja, Dollfus, A., and Atanasijević, I., 1952. Observations combinées de l'éclipse totale de Soleil du 25 Février à Khartoum (Soudan) et de l'éclipse partielle au radio-télescope de l'Observatoire de Meudon. *Comptes Rendus de l'Académie des Sciences*, 234, 1528-1530.

Martyn, D.F., 1946. Temperature radiation from the quiet Sun in the radio spectrum. *Nature*, 158, 632-633.

Martyn, D.F., 1948. Solar radiation in the radio spectrum. I. Radiation from the quiet Sun. *Proceedings of the Royal Society*, A193, 44-59.

Minnett, H.C., and Labrum, N.R., 1950. Solar radiation at a wavelength of 3.18 centimetres. *Australian Journal of Scientific Research*, A3, 60-71.

Orchiston, W., 2004. The 1948 solar eclipse and the genesis of radio astronomy in Victoria. *Journal of Astronomical History and Heritage*, 7, 118-121.

Orchiston, W., Slee, B., and Burman, R., 2006. The genesis of solar radio astronomy in Australia. *Journal of Astronomical History and Heritage*, 9, 35-56.

Orchiston, W., Lequeux, J., Pick, M., Slee, B., and Steinberg, J-L., 2007. The role of eclipse expeditions in early French and Australian radio astronomy. *Bulletin of the American Astronomical Society*, 38(4), 931.

Pawsey, J.L., 1946. Observation of million degree thermal radiation from the Sun at a wave-length of 1.5 metres. *Nature*, 158, 633-634.

Piddington, J.H., and Hindman, J.V., 1949. Solar radiation at a wavelength of 10 centimetres including eclipse observations. *Australian Journal of Scientific Research*, A2, 524-538.

Steinberg, J-L., 1953. Les récepteurs de bruits radio-électriques: III. Un radiomètre 1200 MHz et quelques applications. *L'Onde Electrique*, 33, 274-284.

Strom, R., 2005. Radio astronomy in Holland before 1960: just a bit more than HI. In Orchiston, W. (ed.). *The New Astronomy: Opening the Electromagnetic Window and Expanding our View of Planet Earth*. Dordrecht, Springer. Pp. 93-106.

Sullivan, W.T., 1984. *The Early Years of Radio Astronomy. Reflections Fifty Years after Jansky's Discovery*. Cambridge, Cambridge University Press.

Dr Wayne Orchiston is a Senior Lecturer in Astronomy at James Cook University, Townsville, Australia. His main research interests relate to Cook voyage, Australian and New Zealand astronomical history, with emphasis on the history of

radio astronomy, comets, historically-significant telescopes, early astronomical groups and societies, and transits of Venus. He has published extensively, and has edited the book *The New Astronomy. Opening the Electromagnetic Window and Expanding our View of Planet Earth* (2005). He also has a book on early Australian radio astronomy, co-authored by Woody Sullivan and Jessica Chapman, which will be published by Springer in 2007. He is the Chair of the IAU Working Group on Historic Radio Astronomy.

Dr Jean-Louis Steinberg joined the underground French Communist Party in June 1941 (at the age of 19), in order to resist the German occupiers and their French collaborators. In June 1944 he was arrested with three other members of his family under the German and French 'racist' laws. They were all sent to Auschwitz where they were so beaten, overworked and starved that three of them died; only Jean-Louis survived, thanks largely to help from the underground anti-fascist network in the camp.

Back in France after the War, Jean-Louis began working in radio astronomy with J-F. Denisse and E-J. Blum at the Ecole Normale Supérieure. He concentrated on technology and observations in the microwave range, while Blum focussed on metre waves and Denisse on plasma physics. In 1951 they observed a solar eclipse from Africa at cm and meter wavelengths. On his return from the 1952 URSI Congress in Sydney, Steinberg began developing the Nançay radio astronomy field station, building teams and, with them, designing and using microwave equipment and a variable baseline interferometer. In 1960 he and J. Lequeux wrote a book on radio astronomy, which was subsequently translated into English and Russian. From 1960 through to 1965, he and M. Parise led the design and construction in Nançay of the large radio telescope for dm waves, with a collecting area of 7,000 square meters. In 1962 he was appointed Editor-in-Chief of the French journal, *Annales d'Astrophysique*, which he and his wife ran until 1969. He then showed that this journal had very little audience, and convinced the authorities and his own colleagues to start the European journal, *Astronomy and Astrophysics*. For the first five years he was one of the two Editors-in-Chief. From three volumes a year, this journal has grown into a weekly. In 1965, following Denisse's suggestion, Steinberg began developing space research at Meudon Observatory. The founding group comprised just two individuals, but the laboratory now has 250 technicians, engineers and researchers and has successfully flown experiments on many projects (either French, or in collaboration with NASA or the Soviet Union). Jean-Louis has authored or co-authored about 80 scientific publications, and has received several scientific prizes and awards. Together with Daniel Périer, he has also written a book about his life and that of his family titled *Des Quatre, Un Seul est Rentré* (or, *Out of Four, only One Returned*).

Journal of Astronomical History and Heritage, 10(1), 21-30 (2007).

PLANET, ASTEROID, MINOR PLANET: A CASE STUDY IN ASTRONOMICAL NOMENCLATURE

David W. Hughes
Department of Physics and Astronomy, The University of Sheffield,
Sheffield S3 7RH, UK.
E-mail: d.hughes@sheffield.ac.uk

and

Brian G. Marsden
Smithsonian Astrophysical Observatory, 60 Garden Street, Cambridge,
MA 02138, USA.
E-mail bmarsden@cfa.harvard.edu

Abstract: Since 1801 the multitude of bodies that orbit between Mars and Jupiter have been called planets, small planets, minor planets, petites planètes, kleine Planeten, planetoids and asteroids. We investigate the popularities of these nomenclatures and chart the way in which, over the last 20 to 30 years, the use of Sir William Herschel's word 'asteroid' has become more widespread.

Key words: asteroids, minor planets

1 INTRODUCTION

Much has been made recently of the definition of the term 'planet' and specifically the question as to whether Pluto is 'in' or 'out' of the premier league. Many have questioned whether the Solar System has a host of spherical planets or a mere eight (four rocky mid-sized bodies, Mercury, Venus, Earth, Mars; and four gas giants, Jupiter, Saturn, Uranus and Neptune). In this short paper we principally investigate the terminology applied to the multitude of bodies that orbit the Sun between the orbits of Mars and Jupiter. In 1801 they started off as 'planets', but they were generally rather steadily demoted over the course of the following four decades. The demotion of Pluto took almost twice as long.

Let us start by questioning the status of Ceres. When Ceres was discovered serendipitously, at the beginning of January 1801, was it classed as a new planet? The short answer is not really. On 1 September 1801 its Palermo (Sicily) discoverer, Giuseppe Piazzi (Figure 1), was writing to William Herschel discussing the new "… étoile, qui par son movement ressemble beaucoup à une Planète." (see Lubbock, 1933: 269). It was only by the end of 1801 that the orbit of Ceres was known with any certainty and its position in the 'Bodian Gap' between Mars and Jupiter was established. Did Ceres immediately become ranked with the likes of Mercury, Venus, Earth, Mars Jupiter, Saturn and Uranus? Again the answer is not really. It was too faint. In February 1802 Joseph Banks was commenting on its "… little disc of the size of the 1st or 2nd satellite of Jupiter …" (ibid.). So there is no question of there actually being eight 'real' planets in 1801, and at the end of March 1802, with the discovery of Pallas, this number increasing to nine. Astronomers always seemed to be suspicious of the status of Ceres. It was regarded as being too faint, too small, of too little mass and having an orbit that was too eccentric and of too high an inclination to be worthy of joining the Sun's planetary team.

Were the 'Celestial Police', the illustrious group of astronomers led by Baron Franz von Zach in a hunt for the missing body between Mars and Jupiter (see Cunningham, 1988: 7), disappointed at the insignificance of Ceres? Or was this new celestial body just what was expected, considering the fact that the ancients had not discovered it, and the orbits of Mars and Jupiter were not affected by any unexplained perturbations?

Maybe the demotion from planetary status occurred when it was realised that Ceres was not alone, and was merely one of a host of objects that inhabit the region between the orbits of Mars and Jupiter. The discovery of this multiplicity happened relatively quickly. Johann Elert Bode (1749–1826) had been very excited by the discovery of Ceres, but the second 'moving star' worried him. Writing to Herschel in May 1802 (Lubbock, 1933: 271) he noted that Pallas "… is a planet travelling with Ceres, in the same orbit, at the same distance round the sun. Such a thing is unheard of!"

Let us investigate the introduction of the two most popular terms used to describe these bodies, 'asteroid' and 'minor planet'.

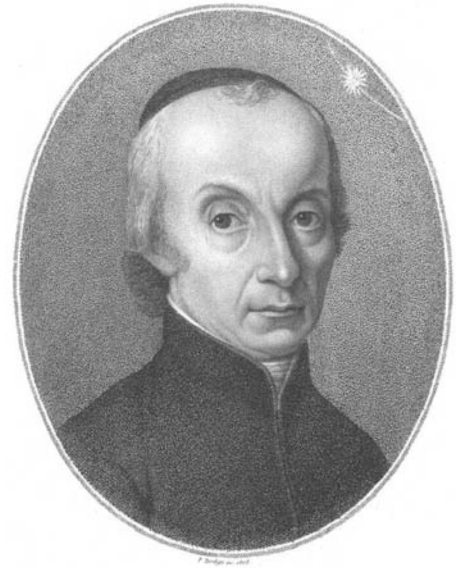


Figure 1: Sicilian astronomer, Giuseppe Piazzi, 1746–1826 (after en.Wikipedia.org/wiki/Giuseppe_Piazzi).

2 ASTEROID

The demotion of Ceres and Pallas started on 6 May 1802, the day on which William Herschel (Figure 2) read his paper "Observations on the two lately discovered bodies" to the Royal Society in London (see Herschel, 1802). Herschel had spent much of April observing Ceres and Pallas, using his 7-foot and 10-foot reflectors and his lucid disc micrometer. He estimated the size and the relative brightness of the two bodies and questioned whether they had detectable atmospheres or satellites. Herschel concluded that Ceres had a diameter of 162 miles, and Pallas a diameter of no more than 70 miles, values that are now known to be underestimated by factors of three or more. These diameter measurements were contained in a letter that Herschel wrote to Piazzi on 22 May 1802, a letter that ends:

> Moreover, if we were to call [Ceres] a planet, it would not fill the intermediate space between Mars and Jupiter with the proper dignity required for that station. Whereas, in the rank of Asteroids it stands first, and on account of the novelty of the discovery reflects double honour on the present age as well as on Mr. Piazzi who discovered it. I hope you will see the above classification in its proper light, as so far from undervaluing your eminent discovery it places it, in my opinion, in a more exalted station. To be the first who made us acquainted with a new species of primary heavenly bodies is certainly more meritorious than merely to add what, if it were called planet, must stand in a very inferior situation of smallness. (see Cunningham, 2002: 252).

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

Herschel (1802: 220) then posed the question "What are these new stars, are they planets, or are they comets?" He enjoyed classifying objects (planetary nebula was another Herschelian first), and to help him answer the question he went on to define the term 'planet', noting that

> This cannot be difficult, since we have seven patterns to adjust our definition by. I should, for instance, say of planets,
>
> 1. They are celestial bodies, of a very considerable size.
> 2. They move in not very excentric [sic] ellipses round the sun.
> 3. The planes of their orbits do not deviate many degrees from the plane of the earth's orbit.
> 4. Their motion is direct.
> 5. They may have satellites, or rings.
> 6. They have an atmosphere of considerable extent, which however bears hardly any sensible proportion to their diameters.
> 7. Their orbits are at certain considerable distance from each other.

Herschel then concludes that Ceres and Pallas are not planets because they are too small, too far from the ecliptic, free of satellites, rather comet-like in appearance (as seen through his instruments) and have orbits that are too close together. He then goes on to define 'comet' (and remember this was in the days before the existence of the Jupiter family of comets had been established):

> 1. They are celestial bodies, generally of a very small size, though how far this may be limited, is yet unknown.
> 2. They move in very excentric ellipses, or apparently parabolic arcs, round the sun.
> 3. The planes of their motion admit to the greatest variety in their situation.
> 4. The direction of their motion also is totally undetermined.
> 5. They have atmospheres of very great extent, which shew themselves in various forms of tails, coma, haziness, &c.

Since Ceres and Pallas had insignificant observable comae, Herschel realised that they were not comets either. In the world's first scientific paper on these bodies he wrote:

> Since, therefore, neither the appellation of planets, nor that of comets, can with any propriety of language be given to these two stars, we ought to distinguish them by a new name, denoting a species of celestial bodies hitherto unknown to us … they resemble small stars so much as hardly to be distinguished from them, even by very good telescopes. It is owing to this very circumstance, that they have been so long concealed from our view. From this, their asteroidical appearance, if I may use that expression, therefore, I shall take my name, and call them *Asteroids*; reserving to myself, however, the liberty of changing that name, if another, more expressive to their nature, should occur. These bodies will hold a middle rank, between the two species that were known before; so that planets, asteroids, and comets, will in future comprehend all the primary celestial bodies that either remain with, or only occasionally visit, our solar system. (Herschel, 1802: 228).

So William Herschel, the discoverer of Uranus, the Royal Astronomer (not to be confused with the Astronomer Royal, who at the time was Nevil Maskelyne), the most prominent astronomer working in England, coined the term *asteroid* and defined it:

> Asteroids are celestial bodies, which move in orbits either of little or of considerable excentricity [sic] round the sun, the plane of which may be inclined to the ecliptic in any angle whatsoever. Their motion may be direct, or retrograde; and they may or may not have

> considerable atmospheres, very small comas, disks, or nuclei. (Herschel, 1802: 229).

This premature definition is far from accurate. To lean on Greek to imply that something between the orbits of Mars and Jupiter is 'like a little star' is misleading to say the least. The invention of the new word did not pass without critical comment. Some liked it. For example, on 17 June 1802 Heinrich Wilhelm Olbers (Figure 3), the discoverer of Pallas, wrote to Herschel:

> I agree with you, honoured Sir, in your sagacious suggestion that Ceres and Pallas differ from the true planets in several respects, and the name *asteroid* seems to me to fit these bodies very well.

Olbers' friend and countryman, Karl Friedrich Gauss (1777–1855), disagreed, however. On 25 June 1802 he wrote to Olbers:

> Mr Herschel also gave me information on his "Asteroids". What surprises me is (1) that he doesn't announce it as being a modest proposal, but rather says simply "I call them," and (2) that his reason in Ceres' case consists in that it now "is out of the zodiac". That shows a very biased and, it seems to me, unphilosophical outlook. (Cunningham, 2006: 227).

Pierre Laplace (1749–1827) was also not so sure:

> Quant au nom que vous donnés [sic] à ces astres, je ne vois pas encore de motif suffisant pour ne pas leur conserver le nom de planètes.

And on 4 July 1802 Piazzi wrote Herschel:

> Et pour la dénomination, ne pourroit-on pas appeler les petites planètes, planetoides? Car je vous avoue, le nom d'Asteroides me paraît plus propre aux petites étoiles. (See Lubbock, 1933: 274).

The original Piazzi letter has a capital P for planetoides and this word is underlined (Michael Hoskin, private correspondence, 2006). Two days before, on 2 July 1802, Piazzi had written to his Milanese astronomical friend and collaborator Barnaba Oriani (1752–1832):

> I hope you won't be sorry if I transcribe a letter recently received from Herschel. What do you think? It looks to me (1) Whatever the name given to this new star doesn't really matter. Are they moving stars? You can call them planetoids or cometoids, but not asteroids. (2) For me the only difference between comets and planets is their eccentricity and inclination. Consequently Ceres is a planet and Pallas a comet. (3) Ceres' diameter ... has to be much larger than 162 miles. (4) If we call Ceres an asteroid so we must call Uranus an asteroid. (This English translation is given in Cunningham, 2002: 192).

In 1803, an unsigned review of Herschel's 1802 *Philosophical Transactions of the Royal Society* article was published in a new Scottish quarterly journal titled *The Edinburgh Review*, which somewhat controversially concentrated on literary and political criticism. In *The Edinburgh Review* (1803: 428) we read:

> … and first we must positively object to the unnecessary introduction of new terms into Philosophy. The science of Astronomy is, beyond any other branch of mixed mathematics, loaded with an obscure and difficult technology … Knowing, as we do, the great power of words in misleading and perplexing our ideas, we cannot allow the unnecessary introduction of a new term to escape unnoticed. Where a new object has been discovered, we cheerfully admit the right of the discoverer to give it a new name; but we will not allow needless multiplication of terms or an unnecessary alteration in the old classification of things, to be either justifiable or harmless, a substitute for real discovery, or a means of facilitating the progress of invention. It remains, therefore, to enquire, whether the circumstances of Ceres or of Pallas, distinguish them from the bodies formerly known?

The reviewer thought that Ceres and Pallas, as described by Herschel, were too similar to known planets and comets to deserve a separate definition:

> … we must enter our protest to the formation of a separate class, distinguished by a new and uncouth name. (ibid.).

To justify this statement the author notes that Herschel had suggested that comets cool as time progresses and slowly lose their atmospheres, thus reducing themselves to the state of planets in everything but their magnitudes and eccentricities.

Figure 3: Heinrich Wilhelm Olbers, 1758–1840 (after en.wikipedia.org/wiki/Heirich_Wilhelm_Matth%C3%A4us_Olbers).

The article goes on to criticise Herschel's general writing style and scientific approach. Herschel is accused of "… great prolixity and tediousness of narration." The author suggests that Herschel is prone to expressing "… loose, and often unphilosophical reflections … [and] above all that idle fondness for inventing names, without any manner or occasion." Furthermore,

> Dr Hershell's [sic] passion for coining words and idioms, has often struck us as a weakness wholly unworthy of him. The invention of a name, is but a poor achievement in him who has discovered whole worlds.

The Edinburgh Review author then completely ignores his (or her) own advice and suggests some new words to describe Ceres and Pallas:

> Such being our opinion, it is of much less consequence to inquire, whether the new name of Asteroid is the most appropriate that could be imagined. To us, that

> name presents the idea of some body resembling fixed stars; whereas the two new planets have no one circumstance in common with those distant bodies. If a new name must be found, why not call them by some appellation which shall, in some degree, be descriptive of, or at least consistent with, their properties? Why not, for instance, call them *Concentric Comets*, or *Planetary Comets*, or *Cometary Planets*? Or if a single term must be found, why may we not coin such a phrase as *Planetoid* or *Cometoid*?

We wonder if the writer of *The Edinburgh Review* article had independently generated the words 'planetoid' and 'cometoid' or had somehow been privy to the earlier Piazzi correspondence. The general rudeness of the article seems to us to rule out the possibility that it was a translation of a piece by Piazzi. The style of the article is typical of one of the co-founders of *The Edinburgh Review*, Henry Brougham (Figure 4),[1] who was never one to mince his words.

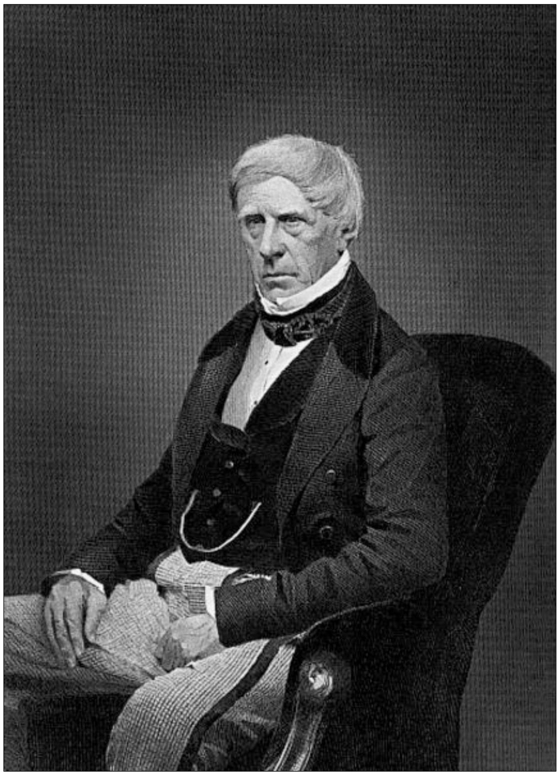


Figure 4: Henry Peter Brougham, 1778–1868 (after en.wikipedia.org/wiki/Henry_Peter _Brougham).

William Herschel seemed to have been rather fond of his new word 'asteroid', and he used it frequently. He, however, was not a man to insist that others followed his lead. In a paper he read to the Royal Society on 9 June 1803 he writes:

> It is not in the least material whether we call them asteroids, as I have proposed; or planetoids, as an eminent astronomer in a letter to me, suggested; or whether we admit them at once into the class of our old seven large planets. (Herschel, 1803: 339).

So in early 1803 the word 'planetoid' was used by the writer of the article in *The Edinburgh Review* and also by Herschel's correspondent, the 'eminent astronomer'. Were these one and the same person? It seems more likely that the 'eminent astronomer' was Piazzi.

The *Oxford English Dictionary* attributes the first recorded use of the word 'planetoid' to H. Brougham in *The Edinburgh Review.* At the time, Brougham was studying to be a lawyer. On page 428 in *The Edinburgh Review* the author of the article suggests that, when it came to discussing the astronomy of Ceres and Pallas, he is

> … as well qualified to judge the truth of these, as if we had ourselves made or verified the observations upon which they are founded.

William Herschel's grand-daughter, Lady Constance A. Lubbock, had no doubt that the "… very ill-natured criticism …" came from Brougham (see Lubbock 1933: 282), and "Had Herschel adopted the better word *planetoids*, suggested by Piazzi, he might have saved himself from the aspersions cast upon him by some critics." (Lubbock 1933: 276).

The Edinburgh Review article was subsequently mentioned by the Edinburgh amateur astronomer Hector Copland Macpherson (1888–1956):

> In 'the Edinburgh Review' Brougham declared that Herschel had devised the word 'asteroid' so that the discoveries of Piazzi and Olbers might be kept on a lower level than his own discovery of Uranus. Many scientists would have been much offended at this contemptible insult, but Herschel merely remarked that he had incurred "the illiberal criticism of 'The Edinburgh Review,'" and that the discovery of the Asteroids "added more to the ornament of our system than the discovery of another planet could have done." (Macpherson, 1906: 20).

This rather surprising and contentious suggestion is completely unsupported by a detailed reading of the original article. The expression 'kept on a lower level' was not mentioned in 1803. The ending of the Macpherson quotation comes from the last line of Herschel (1805: 64). Here Herschel emphasised that "… the specific difference between planets and asteroids …" becomes even more apparent due to the discovery of Juno, and

> It will appear then, that when I used the name asteroid to denote the condition of Ceres and Pallas, the definition I then gave of this term will equally express the nature of Juno … The propriety of therefore using the same appellation for the lately discovered celestial body (i.e. Juno) cannot be doubted. (ibid.).

Brougham was certainly a forceful critic: in recent private correspondence, Mary Brück used adjectives such as 'arrogant', 'witty', 'clever' and 'highly opinionated'. His attack on Lord Byron prompted the poet to reply with the poem *English Bards and Scotch Reviewers*. Brougham also wrote a damaging and contemptuous review of Thomas Young's suggestion, and demonstration, of the wave nature of light.

When the fourth asteroid was announced Herschel (1807: 260) rushed to the telescope to observe it and rejoiced in the "… valuable addition to our increasing catalogue of asteroids …" He also hoped that

> … the great success that has already attended the pursuit of the celebrated discoverers of Ceres, Pallas, Juno and Vesta, will induce us to hope that some further light may soon be thrown upon this new and most interesting branch of astronomy. (Herschel, 1807: 265).

How quickly was the word 'asteroid' taken up during the first decade of the nineteenth century? Well, some professional astronomers started to use it very quickly. Olbers wrote to Bode on 3 April 1807:

> … with great delight, dearest friend, I hasten to tell you that I was lucky enough to find yet another planet (Vesta) belonging to the family of the asteroids, on 29th March. This time, however, the discovery was no mere chance … According to my hypothesis concerning the asteroids … I have, as you know, concluded that all asteroids, of which there are probably a large number, must pass through the north-western portion of the constellation Virgo and the western portion of the Whale. Regularly each month, therefore I check a particular section of these two constellations, having first thoroughly acquainted myself with the star content … (see Roth, 1962: 28).

Note that Olbers uses the term 'asteroids' three times in this short quotation.

Moving to the more popular astronomical literature, we note that Squire (1820: 18) refers to Ceres, Pallas, Vesta and Juno as 'small planets' or 'segments of planets', but lists them under the heading *'Asteroïds'*. Much is made of their glyphs. Jehoshaphat Aspin, the divisor of the popular constellation card collection, *Urania's Mirror* (see Hingley, 1994), certainly uses the word 'asteroid' in the associated book, *A Familiar Treatise on Astronomy* (see Aspin 1825: 18) and defines it as follows:

> ASTEROIDES. This appellation has been give to four planets recently discovered between the orbits of Mars and Jupiter … They differ from all the other planets in their diminutive sizes, and in the form and positions of their orbits, which cross each other, and extend their planes beyond the limit of the zodiac. Hence Sir W. Herschel,[2] not feeling himself warranted to refer them either to the class of planets or comets, denominated them *Asteroides*, or star-like

Aspin goes on to quote the *Edinburgh Encyclopaedia*, which concludes that the four bodies were once "… combined in a larger body." This idea, that there was once a large planet between Mars and Jupiter and this had been broken up, was common place at the time.

Why Aspin thought it necessary to introduce the letter 'e' between the final 'd' and 's' of the word asteroids is a mystery. Maybe he was influenced by happenings on the French side of the Channel. In Brussels, Quételet (1826: 204) discusses *Astéroïdes* and divides planetary bodies into three groups: Mercure, Vénus, Terre and Mars are "… planètes tellustriques …"; Vesta, Junon, Cérès and Pallas "… désignées sous le nom d'Astéroïdes ou de Planètes télescopiques …"; with Jupiter Saturne and Uranus being "… les Grandes Planètes" or "Planètes à cortèges." The expression 'planètes télescopiques' was also used by Laplace (1836: 89).

Returning to the 1820s, two general texts, *Wonders of the Heavens* (Richard Phillips, London, 1822) and *First Steps to Astronomy* (Hatchard and Son, London, 1828), simply refer to Ceres, Pallas, Juno and Vesta as 'planets', as does Carey (1831: 34). The latter does, however, note that "… they are so very unlike the other primary planets …", and states that "Dr Herschel has given the name of Asteroids." Dick (1840: 542) also refers to the four as 'planets'. Tomlinson (1840: 186) talks of "… four little planets called *Asteroids*, because they have the *appearance of stars* …", while Nichol (1844: 22) has 'small planets', and Lardner (1856: 166) follows suit. In the same year, Reid (1856: 144), under the heading of *The Asteroids*, talks of "… thirty-eight small recently discovered planets, situated between the orbits of Mars and Jupiter. They are sometimes called *telescopic*, as they are not visible to the naked eye …". Arago (1857, 4, 141) refers to them as 'petites planètes'.

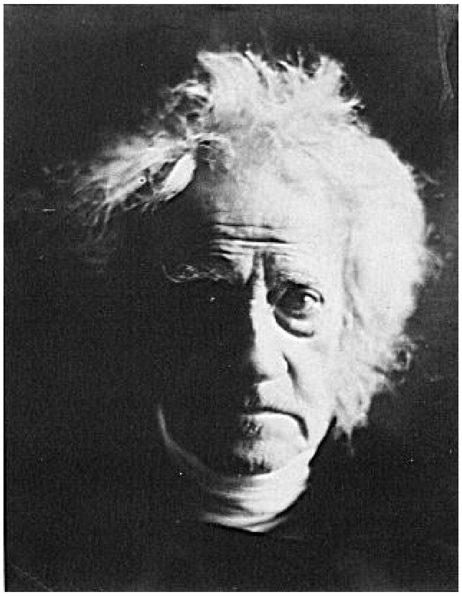


Figure 5: Sir John Herschel, 1792–1871 (after en.wikipedia.org/wiki/John_Herschel).

William Herschel's son, John Frederick William Herschel (Figure 5), pointedly ignores his father's invented word 'asteroids'. In his contribution to the Reverend Dionysius Lardner's *The Cabinet Cyclopædia*, Herschel (1833: 243) writes only of planets:

> Among the stars there are several, – and those among the brightest and most conspicuous, – which, when attentively watched from night to night, are found to change their relative situations among the rest; some rapidly, others much more slowly. These are called *planets*. Four of them, – Venus, Mars, Jupiter, and Saturn, – are remarkably large and brilliant; another, Mercury, is also visible to the naked eye as a large star, but for a reason which will presently appear, is seldom conspicuous; a fifth Uranus, is barely discernable without a telescope; and four others, – Ceres, Pallas, Vesta and Juno, – are never visible to the naked eye. Beside these ten, others yet undiscovered may exist; and it is extremely probable that such is the case, – the multitude of telescopic stars being so great that only a small fraction of their number has been sufficiently noticed to ascertain whether they retain the same places or not, and the five last-mentioned planets having only been discovered within half a century from the present time.

Ten planets! John Herschel completely ignores the one he is observing from—planet Earth. Our home planet only gets a mention when (on page 416) Herschel produces a 'Synoptic Table of the Elements of the Solar System' and under 'Planet's name', lists, in order of mean distance from the Sun, Mercury, Venus, Earth, Mars, Vesta, Juno, Ceres, Pallas, Jupiter, Saturn and Uranus.

To appreciate fully the above discussion, it is important to remember that after Vesta was found in 1807 all the initial excitement quickly waned, as there were no similar discoveries for some considerable time. The 38-year 'fallow period' was finally broken with the discovery (on 8 December 1845) of Astraea by Karl Hencke (see Hughes, 1997). More significantly still, some nine months later, in September 1846, the discovery of the distant Neptune, from the recognition of its gravitational effect on Uranus, made it very clear that *here* was a new body truly worthy of being called a planet. And with the discovery in 1847 of Hebe, Iris and Flora, interest in the rapidly-growing number of small bodies in the Mars-Jupiter region was clearly being reignited. Indeed in the first edition of his famous text *Outlines of Astronomy*, John Herschel (1849) deigned to use the word 'asteroids' (if a little reluctantly), as he added Neptune to his tabulation of planets and removed the small Mars-Jupiter bodies that were present in his 1833 tabulation.

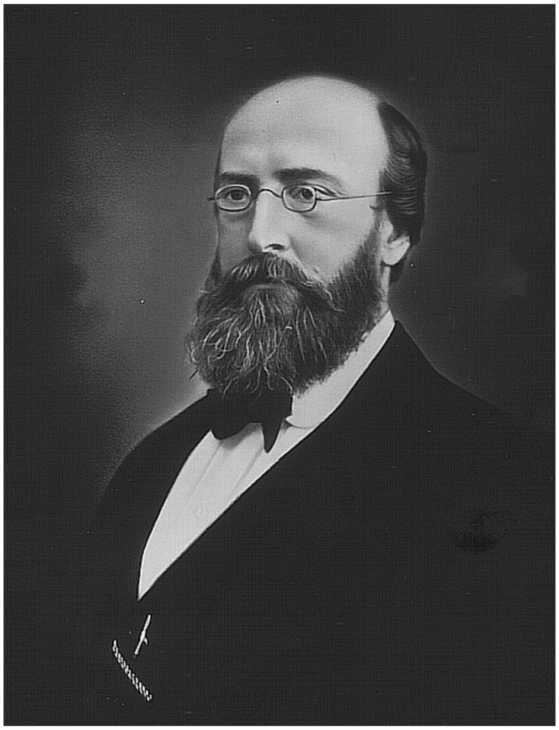


Figure 6: Benjamin Apthorp Gould, 1824–1896 (courtesy of the Argentine National Observatory).

The year 1849 also saw the publication of the first volume of *The Astronomical Journal*. Edited in Boston by Benjamin Apthorp Gould (Figure 7), the *A.J.* quickly commandeered the word 'asteroid' (see, for example, Alexander, 1851), and although some papers were published under headings such as 'Observations of Hygea' (Ferguson, 1851), by the second volume the *A.J.* was routinely indexing items both under 'Asteroid' and under headings like 'Hygea (10th Asteroid)'. In that second volume, Gould (1852) conveniently listed the glyphs for 13 of the first 15 asteroids, noting that these glyphs were being replaced by new symbols that consisted of the numbers (1 to 15) enclosed in a circle (this nomenclature being suggested by Johann Rudolf Wolf (1816–1893) of Zürich Observatory). Eventually, when the numbers got too large, the circles were dispensed with. The old glyphs were in fact designed for the first 17 asteroids, as well as sporadically up to No. 37 (see Schmadel, 1992).

By the time John Herschel published the second edition of his *Outlines of Astronomy* in 1853, the word 'asteroid' only appears in the index. Instead, Herschel (1853: 243) writes of "… eight telescopic planets, — Ceres, Pallas, Juno, Vesta, Astraea, Hebe, Iris and Flora (which may therefore be termed *ultra-zodiacal*) …", while in the tabulation on page 543 he lists no fewer than 22 planets in order of semi-major axis (i.e. the eight 'conventional' planets, Mercury-Neptune, and the first fourteen minor planets).

During the latter half of the nineteenth century the number of known asteroids was increasing nearly exponentially. Arago (1857) listed the orbital parameters of 42, when Chambers wrote his *Descriptive Astronomy* (1867: 92) the number had grown to 89, and by 1890 the total stood at 287 (see Ball, 1893: 197).

3 MINOR PLANET

The term 'minor planet' seems first to have been introduced in *The Nautical Almanac and Astronomical Ephemeris* in 1835. In 1830 the Lords Commissioners of the Admiralty (who were responsible for the publication of *The Nautical Almanac*) asked the Astronomical Society (which became the Royal Astronomical Society in March 1831) to suggest possible ways in which the *Almanac* could be improved (see Dreyer & Turner, 1923: 56). The outcome was the conversion of the *Almanac* from a work that was only really useful for nautical astronomy to one that was useful for both nautical and practical astronomy. The size of the *Almanac* increased from just over 200 pages (1833 and before) to just over 500 pages (1834 and after). Tables concerning Ceres, Pallas, Juno and Vesta first appeared in 1834. These listed, at four day intervals, such coordinates as heliocentric longitude and latitude, geocentric right ascension and declination, length of radius vector, logarithm of distance from Earth and mean time of transit. (The interval was reduced to one day around opposition.)

In the context of the present paper, Lieutenant W.S. Stratford, R.N. (the Superintendent responsible for producing the *Almanac*) referred to Ceres, Pallas, Juno and Vesta as planets in the 1834 edition. When, in May 1835, he was writing the preface for the 1837 edition, they were referred to as 'minor Planets' (see page vii), and in December 1835, when writing the preface for the 1838 edition of the *Nautical Almanac* they were elevated to 'Minor Planets' (see page vii).

Despite the involvement of its parent organization in the improvements in *The Nautical Almanac*, the *Monthly Notices of the Royal Astronomical Society* did not immediately use the term 'minor planets', and it certainly did not use the nomenclature 'asteroids'. As new discoveries were made in the late 1840s, the *Monthly Notices* continued to refer to them as 'planets', or sometimes as 'small planets'. It finally took the plunge with 'Minor Planets' in February 1853, in *Monthly Notices* Volume 13 (for November 1852 to June 1853). This volume starts on page 1 with the announcement of the discovery, by J.R. Hind (on the evening of 15 December 1852 at Mr Bishop's Observatory, Regent's Park, London) of 'another small planet', Thalia. The journal follows this announcement by listing of the orbital parameters of Lutetia and Massilia. (The spelling of the latter, named after the French city of Marseilles, originally oscillated between the Latin 'Massilia' and the Greek 'Massalia'.) Minor planets were much in the news in 1853, eight having being discovered in the previous year. On page 93 in Volume 13 of *Monthly Notices* the announcement was made that, at the 11 February 1853 Annual General Meeting of the Society, Mr J.R. Hind had been awarded the Gold Medal "… for his astronomical discoveries, and in particular for the discovery of eight small planets." The report of the AGM ended with a table titled 'Catalogue of the Minor Planets at present known, in order of discovery', which listed the 'reference numbers', names, discovery dates and discoverers of the 23 such bodies then known. In all of this discussion there was the implication that the eight larger bodies, Mercury to Neptune, were the 'Major Planets' or, in popular parlance, simply the 'Planets'. The editors of the *Monthly Notices* were, however, not strict, for Lardner (1853) used the terms 'planetoids' and 'small planets'.

Although the *Astronomische Nachrichten* had briefly flirted with 'Asteroiden' in 1852 (see the index to Volume 34), the heading 'Planeten, Kleine' appeared in the index in 1855, principally due to the influence of Friedrich Wilhelm August Argelander (1799–1875; see, for example, Argelander, 1855). Interestingly, in Volume 39 under the heading 'Planeten, neue', there were entries such as 'Euphrosyne (31)', showing that the complete circles surrounding the numbers were already by then deteriorating into parentheses. Shortly afterwards the parenthetical numbers preceded the names in a custom that still continues, although some writers nowadays omit the parentheses.

Did the expression 'minor planet' catch on quickly? The literature indicates that its reception was mixed. In further editions of *Outlines of Astronomy*, John Herschel (1871, 11th edition, pages 333, 352, 727, 731) got bolder with his use of the term 'asteroids', and it also was used by Plummer (1873: 118). Meanwhile, Yale University's Professor Elias Loomis (1868: 224) 'sat on the fence', writing:

> On account of the close resemblance in appearance between these small planets and the fixed stars, Herschel proposed to designate them by the name *Asteroid* – a term which has been very extensively adopted. Some astronomers employ the term *Planetoid*; but the term *minor planet* is more descriptive, and is now in common use among astronomers.

The mixed reception is underlined by the fact that the word *asteroid* is "… very extensively adopted …", while *minor planet* is "… in common use." Chambers (1867: 91) embraced 'minor planets', and added a footnote:

> The old name of *asteroids*, proposed by Sir William Herschel, has nearly fallen into disuse. Nothing could be more inappropriate than such a designation; *planetoids* would have been better. However, *minor planets* is preferable to either.

In Chambers' later much extended Fourth Edition of his *A Handbook of Descriptive and Practical Astronomy*, the first part of the second sentence in the footnote has been softened slightly, and now reads: "Such a designation was not very appropriate." (Chambers, 1889, 1: 164). Newcomb (1878: 333), from the U.S. Naval Observatory, ignores the word 'asteroid' and refers to 'the small planets'. Flammarion (1881: 499) uses the expression 'les petites planètes'. In his *The Story of the Heavens*, Ball (1893: 193), sticks with 'minor planets'. The great astronomical populariser Richard Proctor (1892: 552) disagreed with Chambers and the previous three authors. His chapter on the subject is titled 'The Zone of the Asteroids', and he writes, as a footnote:

> This name, asteroids, is far better than 'minor planets' for these small bodies … It would have been convenient, but for this misuse of the term, to call the four outer planets the major, and the four inner the minor planets'. [Note that Loomis (1868) refers to the planets as 'superior' and 'inferior'.]

In publishing the Gresham Lectures that he gave between 1881 and 1882, Ledger (1882) agreed with Chambers. His eleventh lecture is entitled 'The Minor Planets', and he writes:

> But we ought perhaps to explain, before we make any further remarks with regard to these little bodies, why it is that we adopt for them the appellation *Minor Planets*, in preference to any other. We do so, because the orbits in which they travel round the Sun are not only governed by the same laws, but in many respects are similar to those of the larger planets. At any rate, we may confidently say, that in no one respect, except in the minuteness of their discs, can they be justly described as star-like. The name of Asteroid, which has this meaning, and which was originally assigned to them, is therefore about as unjustifiable a title as could well be selected. (Ledger, 1882: 266).

The doyenne of astronomical history, Agnes Clerke (1885: 100), nods in the direction of Herschel's 'asteroids', but seemingly prefers to discuss the 'little family of the minor planets'. Princeton's Professor Charles Young (1895: 339), refers to 'asteroids or minor planets', and continues to use both terms in his textbook. While still writing of minor planets, Chambers (1912: 111) notes that

> One remarkable fact about these planets is that their orbits are in many cases much more inclined to the Ecliptic than any of the orbits of the older planets. Hence the term 'ultra-zodiacal planets' was once suggested."

This term, 'ultra-zodiacal planets', was used by John Herschel between 1833 and 1870. It is particularly relevant that William Herschel's son resisted the use of the word 'asteroid' for so long.

In the updated version of their famous text-book, Russell, Dugan and Stewart (1926: 347), the term 'asteroid' predominates. Maybe there is much to be said for using one word instead of two! Spencer Jones (1924: 243), in section 142 of his text-book, writes:

> The Minor Planets. – The minor planets or asteroids, as they were named by Sir William Herschel, are a numerous group of very small planets circulating in the space between Mars and Jupiter …

The term 'asteroids' is then used in the following pages of description. Maybe Spencer Jones favours it because of the illustrious nature of the originator? In *The Splendour of the Heavens*, Crommelin (1923) titles his chapter 'The Asteroids or Minor Planets', but then uses the word 'asteroid' throughout what follows.

The word 'asteroid' often found its way into literature. Our favourite quote is from Sherlock Holmes:

> Is he not the celebrated author of *The Dynamics of an Asteroid*, a book which ascends to such rarefied heights of pure mathematics that it is said that there was no man in the scientific press capable of criticising it? (Doyle, 1966: 409).

4 RECENT USAGE

Cecilia Payne-Gaposchkin (1954: 232) discusses the asteroids in the same section as other 'lesser bodies of the solar system', and notes that "… they are sometimes called minor planets or planetoids, but we shall adhere to the general practice of calling them asteroids." Around the same time, Abetti (1954: 171) rather oversteps the mark by writing "… the misnomer 'asteroids', although sometimes still used, is being replaced by the designation 'minor planets' or 'planetoids'."

An early monograph on the subject is by the German astronomer Günter D. Roth (1962). The title is *The System of Minor Planets*, but this appellation probably owes much to the fact that the original German version of the book used to term 'kleine Planeten.'

Most of the modern major American and European astronomical textbooks, including Motz and Duveen (1977), Karttunen et al. (1987), Unsöld and Baschek (1991), Zeilik et al. (1992), Carroll and Ostlie (1996) and de Pater and Lissauer (2001), embrace the word 'asteroid'.

The early 1970s saw a 'sea-change' in asteroidal studies (see, for example Gehrels, 1984). The progress of the Space Age exploration of the Solar System was such that missions were being planned to the major planets. Obviously these spacecraft had to fly through the 'asteroid belt' so opportunities were presented for imaging some of the inhabitants. Also cosmogonists realised that asteroids provide an important key to the planetary building process and to the composition of the original solar nebula. At last we were past the time when Gehrels (1979: 7) could write

> By the 1950's the malaise in asteroid studies had come to the point where it was improper at the major observatories to work on these "minor" bodies that were called "the vermin of the sky." Even the old-timers wondered how many more useless asteroids should be discovered.

Apparently the expression 'the vermin of the sky' was a conversational epithet much loved of Austrian astronomer Professor Edmund Weiss (1837–1917), Director of the Vienna Observatory from 1878, who used often to object to the way in which asteroidal trails spoilt the photographic plates that he had exposed in order to reveal the details of nearby nebulae (e.g. see Seares, 1930: 10). As an example of how asteroidal experts became disillusioned, Metcalf (1912: 201) wrote:

> Formerly the discovery of a new member of the solar system was applauded as a contribution to knowledge. Lately it has been considered almost a crime.
>
> It is like the birth of a child in an already too large family; to keep track of it and bring it up properly is too much of a strain on the family exchequer.

The Twelfth Colloquium of the International Astronomical Union was held in Tucson (Arizona) in March 1971 under the title *Physical Studies of Minor Planets* (see Gehrels, 1971). One hundred and forty people (including the second author of the present paper) attended this meeting, the first on asteroids (the term used overwhelmingly in the papers presented) in the history of the subject. Eight years later a second conference was held, which attracted 144 people. This time the title was simply *Asteroids* (see Gehrels, 1979). Subsequent conferences have had their proceedings published under the titles of *Asteroids II* and *Asteroids III*.

5 THE INTERNATIONAL ASTRONOMICAL UNION

Ever since its founding in 1919, the International Astronomical Union has routinely shunned 'asteroids' and 'asteroides', and in its two official languages used 'minor planets' and 'petites planètes', most notably in the title of Commission 20, which deals with their positional observations, orbits and ephemerides. In 1947 the IAU's 'Minor Planet Center' (MPC) was established, this choice of name perhaps seeming a little surprising since the Center was located in the U.S.A. where the early use in the *A.J.* had tended to make the term 'asteroid' more popular than in Europe. But the MPC had evolved after World War II from the German Astronomisches Rechen-Institut (ARI) that previously attended to the 'Kleine Planeten'.

Since 1991 the ARI has published five editions of the *Dictionary of Minor Planet Names*, together with a recent appendix (see Schmadel, 2006).

Soon after its discovery in 1930, the object initially labelled 'Object Lowell Observatory' or 'The Trans-Neptunian Planet' came to be known widely as Pluto, 'the ninth (major) planet'. Right from the start, several astronomers around the world were opposed to this appellation, and their numbers increased as later research showed not only that Pluto was considerably less massive and much smaller than had been initially assumed, but that—rather like Ceres—it was not alone, but was a member of a belt of even smaller, but in many respects similar, bodies. As in the early nineteenth century, the early twenty-first century saw further arguments about what constitutes a planet, this time by committees established by the IAU.

The recognition in 2005 of a more distant object that was somewhat larger than Pluto brought matters to a head, and much of the 26th General Assembly of the IAU (which was held in Prague, in August 2006) was devoted to a consideration of the 'Pluto problem'. At the General Assembly's final session it was decided, by a substantial majority of the more than 400 members attending, that there are just eight planets in our Solar System—those known a century and a half ago —bodies both moving in orbits that dominate their semi-major axis regions and being (more or less) spherical, because they are in hydrostatic equilibrium.

A new category of 'dwarf planets' was defined, this category also consisting of objects large enough to be in hydrostatic equilibrium but *not* moving in orbits dominating their regions (or "… clearing out their neighbourhood …", as the actual resolution put it). This new category would initially consist of Ceres, Pluto and the larger more distant object, previously known as 2003 UB$_{313}$, which received the number and name 136199 Eris a couple of weeks later. Actually, Pluto—now 134340 Pluto—was defined to be the prototype of the trans-Neptunian variety of 'dwarf planet', in the expectation that more members would be added when (and, indeed, if) it became possible to establish which objects were in hydrostatic equilibrium. It was not clear whether more of the traditional main-belt asteroids would also be deemed 'dwarf planets', but if so, Ceres would presumably become the prototype for these bodies.

As for the remainder—that is, asteroids (as the resolution actually stated), comets, meteoroids (yet another 'kettle of fish'!), trans-Neptunian objects, etc.—they were to be known collectively as 'small solar-system bodies'. Earlier versions of the resolution recommended that the term 'minor planet' be discontinued, although the term was not even mentioned in the final version, so the MPC is presumably still permitted to exist. After all, since the vast majority of the hundreds of thousands known are not in hydrostatic equilibrium, they cannot really be considered any type of 'planet'. But, then, it was also firmly decided by democratic vote that a 'dwarf planet' is not a 'planet' either!

So we really need a different term for 'dwarf planet', preferably a single word. One possible term, brought up informally at the IAU meetings, is 'planetino'. Popular though the single word 'asteroid' may have become in recent decades, it seems to us that this is now the ideal time to resurrect Piazzi's original 1802 proposal of 'planetoid'.

The three elements of the 2006 IAU resolution would therefore refer to eight 'planets', three (with more to come) 'planetinos' and a quite overwhelming number of 'planetoids' (not to mention comets, etc.). Although the Prague resolution to designate the trans-Neptunian 'planetinos' (if we may be so bold ...) as 'plutonians' was rejected by a very small majority, it does make sense to divide the 'planetinos' into 'plutonians' and (why not?) 'cereans'. (After all, a possible alternative, sometimes mentioned in the backrooms of the Prague Congress Centre, would be 'plums' and 'cereals'!)

6 NOTES

1. Henry Brougham was born in Edinburgh on 19 September 1778. He turned out to be a gifted scholar, and at the age of 14 became a student at Edinburgh University where he studied science and mathematics (in fact all students at Edinburgh did mathematics and moral philosophy in their first year). He even presented a paper on "Experiments and Observations of the Inflection, Reflection and Colours of Light" to the *Royal Society* whilst still a student. In 1800 Brougham changed courses, transferring to the Faculty of Law. Apart from founding *The Edinburgh Review* in October 1802 with Francis Jeffrey and Sydney Smith, he also wrote 35 articles for this publication in the first two years. In 1803 or 1804 Brougham moved to London to further his law career. He then went on to become a Member of Parliament (in 1816), and was elevated to the House of Lords in 1830, becoming Lord Chancellor in Earl Grey's Whig Government.
2. Herschel was knighted in 1816.

7 ACKNOWLEDGEMENTS

We are very thankful to Mary Brück, Allan Chapman and Michael Hoskin for their insightful comments.

8 REFERENCES

Abetti, G., 1954. *The History of Astronomy*. London, Sidgwick and Jackson.

Alexander, S., 1851. On the similarity of arrangement of the asteroids and the comets of short period, and the possibility of their common origin. *Astronomical Journal*, 1, 181-184.

Arago, F., 1857. *Astronomie Populaire*. Paris, Gide.

Argelander, F.W.A., 1855. Über die Helligkeiten der kleinen Planeten. *Astronomische Nachrichten*, 41, 337-342.

Aspin, J., 1825. *A Familiar Treatise on Astronomy Explaining the General Phenomena of The Celestial Bodies*. London, Samuel Leigh.

Ball, R.S., 1893. *The Story of the Heavens*. *New and Revised Edition*. London, Cassell.

Carey, G.G., 1831. *Astronomy as it is Known at the Present Day*. London, Chidley.

Carroll, B.W., and Ostlie, D.A., 1996. *An Introduction to Modern Astrophysics*. New York, Addison-Wesley.

Chambers, G.F., 1867. *Descriptive Astronomy*. Oxford, Clarendon Press.

Chambers, G.F., 1889. *A Handbook of Descriptive Astronomy and Practical Astronomy. Fourth Edition*. Oxford, Clarendon Press.

Chambers, G.F., 1912. *Astronomy*. London, Hutchinson.

Clerke, A.M., 1885. *A Popular History of Astronomy During the Nineteenth Century*. Edinburgh, Adam & Charles Black.

Crommelin, A.C.D., 1923. The asteroids or minor planets. In Phillips, T.E.R., and Steavenson, W.H. *Hutchinson's Splendour of the Heavens: A Popular Authoritative Astronomy*. London, Hutchinson. Pp. 323-333.

Cunningham, C.J., 1988. *Introduction to Asteroids: The Next Frontier*. Richmond, Willmann-Bell.

Cunningham, C.J., 2002. *The First Asteroid: Ceres 1801-2001.* Surfside (Florida), Star Lab Press (Historical Studies in Asteroid Research, Volume 1).

Cunningham, C.J., 2006. *Jousting for Celestial Glory: the Discovery and Study of Ceres and Pallas*. Surfside (Florida), Star Lab Press (Historical Studies in Asteroid Research, Volume 2).

de Pater, I., and Lissauer, J.J., 2001. *Planetary Science*. Cambridge, Cambridge University Press.

Dick, T., 1840. *The Sidereal Heavens and other Subjects Connected with Astronomy*. London, Thomas Ward.

Doyle, A.C., 1966. The Valley of Fear. In *The Complete Sherlock Holmes Long Stories*. London, John Murray. Pp. 408-571. [This 'long story' was first published in the *Strand Magazine* between September 1914 and May 1915.]

Dreyer J.L.E., and Turner, H.H., (eds), 1923. *History of the Royal Astronomical Society*, *1820-1920*. London, Wheldon and Wesley.

Ferguson, J., 1851. Observations of Hygea, made with the filar-micrometer of the Washington Observatory. *Astronomical Journal*, 1, 164-166.

Flammarion, C., 1881. *Astronomie Populaire*. Paris, Marpon et Flammarion.

Gehrels, T. (ed.), 1971. *Physical Studies of Minor Planets*. Washington, NASA SP-267.

Gehrels, T. (ed.), 1979. *Asteroids*. Tucson, University of Arizona Press.

Gehrels, T., 1984. Fundamental studies of asteroids. *Bulletin of the Astronomical Society of India*, 12, 16-39.

Gould, B.A., 1852. On the symbolic notation of the asteroids. *Astronomical Journal*, 2, 80.

Herschel, F.W., 1802. Observations on the two lately discovered celestial bodies. *Philosophical Transactions of the Royal Society*, 48, 213-232.

Herschel, F.W., 1803. Account of the changes that have happened, during the last twenty-five years, in the relative situation of double-stars; with an investigation of the cause to which they are owing. *Philosophical Transactions of the Royal Society*, 51, 339-382.

Herschel, F.W., 1805. Experiments for ascertaining how far telescopes will enable us to determine very small angles, and to distinguish the real from the spurious diameters of celestial and terrestrial objects: with an application of the results of these experiments to a series of observations on the nature and magnitude of Mr Harding's lately discovered star. *Philosophical Transactions of the Royal Society*, 53, 31-64.

Herschel, F.W., 1807. Observations on the nature of the new celestial body discovered by Dr Olbers … *Philosophical Transactions of the Royal Society*, 59, 260-266.

Herschel, J.F.W., 1833. *A Treatise on Astronomy*. Longman, Rees, Brown, Green & Longman.

Herschel, J.F.W., 1849. *Outlines of Astronomy*. London, Longman Green & Co.

Hingley, P.D., 1994. Urania's mirror – a 170-year old mystery solved. *Journal of the British Astronomical Association*, 104, 238-239.

Hughes, D.W., 1997. Only the first four asteroids. *Journal of the British Astronomical Association*, 107, 211-213.

Karttunen, H., Kröger, P., Oja, H., Poutanen, M., and Donner, K. J., 1987. *Fundamental Astronomy*. Berlin, Springer-Verlag.

Laplace, M. le Marquis de, 1836. *Exposition du Système du Monde*. Paris, Bachelier.

Lardner, D., 1853. On certain results of Laplace's formulae, expressing the relationship between the inclinations, eccentricities, and other elements of the planetary orbits. *Monthly Notices of the Royal Astronomical Society*, 13, 252-256.

Lardner, D., 1856. *Popular Astronomy*. London, Walton and Maberly.

Ledger, E., 1882. *The Sun: Its Planets and Their Satellites*. London, Edward Stanford.

Lubbock, C.A., 1933. *The Herschel Chronicle. The Life-story of William Herschel and his Sister Caroline. Edited by his Granddaughter*. Cambridge, Cambridge University Press.

Loomis, E., 1868. *A Treatise on Astronomy*. New York, Harper & Brothers.

Macpherson, H., 1906. *A Century's Progress in Astronomy*. Edinburgh, William Blackwood and Sons.

Metcalf, J.H., 1912. The asteroid problem. *Popular Astronomy*, 20, 201-206.

Motz, L., and Duveen, A., 1977 *Essentials of Astronomy*. New York, Columbia University Press.

Newcomb, S., 1878. *Popular Astronomy*. New York, Harper and Brothers.

Nichol, J.P., 1844. *Contemplations of the Solar System*. Edinburgh, William Tait.

Payne-Gaposchkin, C., 1954. *Introduction to Astronomy*. New York, Prentice-Hall.

Plummer, I., 1873. *Introduction to Astronomy*. London, William Collins.

Proctor, R.A., 1892. *Old and New Astronomy*. London Longmans, Green and Co.

Quételet, A., 1826. *Astronomie Élémentaire*. Paris, de Malher et Cie.

Reid, H., 1856 *Elements of Astronomy*. Third Edition. London, Oliver & Boyd.

Roth, G.D., 1962. *The System of the Minor Planets*. London, Faber and Faber.

Russell, H.N., Dugan, R.S., and Stewart, J.Q., 1926. *Astronomy: A Revision of Young's Manual of Astronomy. Volume 1. The Solar System*. Boston, Ginn and Company.

Schmadel, L.D. 1992. *Dictionary of Minor Planet Names*. Berlin, Springer-Verlag

Schmadel, L.D. 2006. *Addendum to the Fifth Edition of the Dictionary of Minor Planet Names: 2003-2005*. Berlin, Springer-Verlag.

Seares, F.H., 1930. Address of the retiring president of the society in awarding the Bruce Medal to Professor Max Wolf. *Publications of the Astronomical Society of the Pacific*, 42, 5-22.

Spencer Jones, H., 1924. *General Astronomy*. London, Edward Arnold & Co.

Squire, T., 1820. *A Popular Grammar of the Elements of Astronomy*. London, R. Phillips & Co.

The Edinburgh Review, 1803 (January). ART. XV Observations on the two lately discovered celestial bodies by William Herschel L.L.D., F.R.S. From *Phil Trans* 1802.

Tomlinson, L., 1840. *Recreations in Astronomy*. London, John W. Parker.

Unsöld, A., and Baschek, B., 1991. The New Cosmos. Berlin, Springer-Verlag.

Young, C.A., 1895. *A Text-book of General Astronomy*. Boston, Ginn & Co.

Zeilik, M., Gregory, S.A., and Smith, E.v.P., 1992. *Introductory Astronomy and Astrophysics*. Fort Worth, Saunders College Publishing.

David Hughes is Professor of Astronomy at the University of Sheffield (U.K.) and enjoys researching the minor bodies of the solar system, with particular emphasis on cometary decay, asteroidal evolution and the relationship between comets and meteoroid streams. He is also interested in the history of solar system astronomy, and is the author of many research papers and the book *The Star of Bethlehem: An Astronomer's Confirmation* (1979). David is a Committee member of the IAU Working Group on Transits of Venus.

Dr Brian G. Marsden is a senior astronomer at the Smithsonian Astrophysical Observatory in Cambridge, Massachusetts, where he specializes in celestial mechanics and astrometry, with particular application to the study of comets and minor planets. He has studied in particular the nongravitational forces that affect the motions of comets, successfully predicted the return of several lost comets and is the discoverer of the Marsden Group of sungrazing comets. An Associate at the Harvard College Observatory, he was Associate Director for Planetary Sciences at the Harvard-Smithsonian Center for Astrophysics (1987-2002). As Director of the IAU Central Bureau for Astronomical Telegrams (1968-2000) and Minor Planet Center (1978-2006), he was responsible for the timely dissemination of information about transient astronomical objects and events and for cataloguing positional and orbital information on minor planets and comets. Brian has also served as President of IAU Commission 20 (Positions and Motions of Minor Planets, Comets and Satellites) and IAU Commission 6 (Astronomical Telegrams). He continues to be a member of both the IAU Working Group on Planetary System Nomenclature and the Committee for Small-Body Nomenclature, serving as Secretary of the latter.

Journal of Astronomical History and Heritage, 10(1), 31-48 (2007).

E.E. BARNARD AND THE ECLIPSE OF IAPETUS IN 1889

James Bryan
McDonald Observatory, 1 University Station, C1402, Austin,
Texas 78712-0259, USA
E-mail: bryan@astro.as.utexas.edu

Abstract: On 1-2 November 1889 E.E. Barnard observed Iapetus as it passed through the shadows of Saturn and its ring system. Over 2.6 hours he produced 75 differential visual magnitude estimates of Iapetus relative to Tethys and Enceladus. The resulting light curve demonstrated the C ring's already known translucence, but it also showed something unexpected. As Iapetus crossed the sunlit gap between Saturn's upper atmosphere and the C ring's inner boundary, instead of remaining constant in brightness, the satellite steadily faded. Apparently, it passed through a shadow, but in 1889 nothing was known to exist in this space. Barnard dismissed the effect as unreal. Although he could not have known, his light curve also implied greater density in the C ring than exists today near the B-C ring boundary. What is the significance of his observation? Were Barnard's visual magnitude estimates wrong? Was the inner ring system significantly different in 1889? Did Barnard observe an event that temporarily affected the ring's density in the line of sight? There are no conclusive answers because he observed the eclipse alone and visually. Yet his method of observation and light curve are thought-provoking. What he recorded conforms in certain ways to the presence of spokes on Saturn's rings. Spacecraft have observed spokes only on the B ring, but visual observers as early as 1873 have seen spokes and spoke-like objects in the A, B and C rings. I speculate on the possibility that Barnard observed spoke shadows intermingled with ring shadows on Iapetus in eclipse.

Keywords: Barnard, Saturn, Iapetus, spokes, Lick Observatory

1 INTRODUCTION

Occultations of stars and of spacecrafts' radio signals by Saturn's rings are used to identify the existence and density of thousands of ringlets and plateaus in this huge planetary ring system. However, the technique of observing planetary rings in transmission was not developed in modern times. Saturn's satellite Iapetus has an orbital inclination of almost 15° and an orbital period of about 79.33 days that make possible rare eclipses. On 1-2 November 1889 Iapetus passed through the complicated shadows of the rings (Figure 1) in what was the first predicted opportunity to observe the rings in transmission. In 1889 four men made direct and indirect contributions to the scientific outcome of the eclipse. Edward E. Barnard is remembered for his role as the only person to observe and report upon what he saw. Albert Marth, a Prussian astronomer and mathematician, provided the prediction that enabled Barnard to act. Edward S. Holden, first Director of Lick Observatory, assigned Barnard to the task, made it difficult for him to do the job, and required a less than truthful account of what happened. Finally, James Clerk Maxwell, who had been dead for ten years, may also have been an influence, not by what he had explained of a particulate ring system but by what he had not explained.

Barnard became famous as a pioneer astrophotographer, but he was fundamentally a visual observer who lived during the transitional years of the late nineteenth and early twentieth centuries when photography replaced eyesight as the chief method in observational astronomy (see Sheehan, 1995). At a time when no instrument was superior to the human eye and good judgment to measure magnitudes for stars and objects that looked like stars, Barnard produced respectable visual photometry for Iapetus in eclipse. In evaluating his work, he confined himself to an unremarkable conclusion that ignored strange evidence. Apparently, he saw on Iapetus a shadow of something unknown that originated between the planet and C ring. Further, although he was unaware of it, his data imply density greater than modern optical depths in the outer C ring. I examine influences that affected his observation, modern concepts of Saturn's ring system, modern and historical visual observations of the rings, and the opinions of Barnard's contemporaries of his skill as a planetary observer. I consider the possibility that his estimated magnitudes for Iapetus were affected not only by normal shadows of the inner ring system but also by shadows from transitory ring spokes.

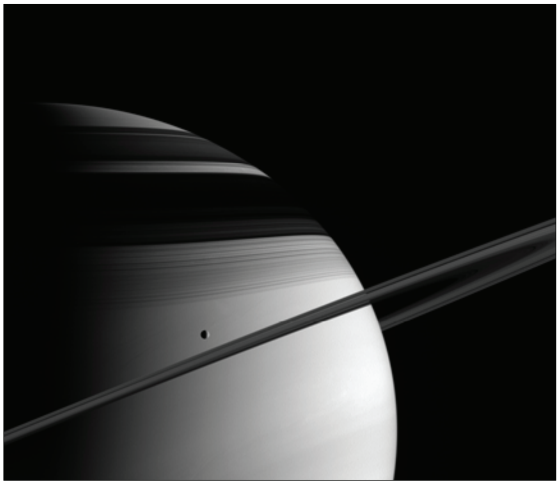


Figure 1: Shadows of Saturn's rings projected upon the planet's northern hemisphere as observed by the Cassini spacecraft from orbit on 10 June 2005. The shadow of the A ring with its narrow, sunlit Encke gap is northernmost, followed southward by the wide, sunlit Cassini division, the very dark B ring shadow, and the tenuous C ring shadow, respectively. Barnard's result for the eclipse of Iapetus implies that normal optical depths in Saturn's inner ring system were different than they are today. What does his observation mean? Tethys, one of the satellites that Barnard used to visually estimate Iapetus' changing brightness in eclipse, appears in the foreground (after NASA/JPL/SSI, 2005b, PIA 07545 with permission from the Cassini Imaging Team, NASA/JPL/SSI).

2 BACKGROUND

The bright supernova, S Andromedae (S And), that appeared in the Andromeda Nebula in August 1885, and Saturn's moon, Iapetus, are very different objects, but for Barnard they were related. In 1885, at age 28 (Figure 2), he had been for two years a student at Vanderbilt University. The 'new star in the Andromeda Nebula' created serious theoretical problems for astronomers. However, Barnard's notebook and published papers make clear that for him the star was strictly an observational matter. Conspicuously absent from his account was a series of visual magnitudes for the supernova. Instead of magnitudes, like many of his contemporaries, Barnard provided picturesque descriptions of its changing brightness. This was a sign of the times, but it also indicated his skill level. In 1885 he did not know how to produce estimated stellar magnitudes according to the best methods available.

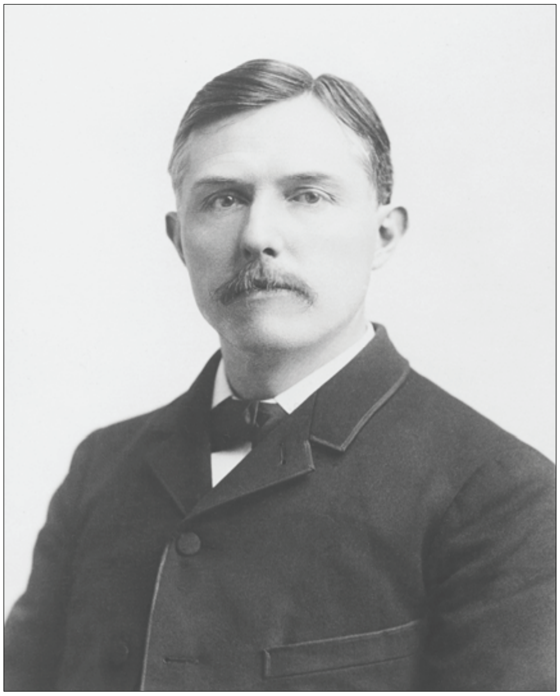


Figure 2: Edward E. Barnard (1857–1923) as photographed in 1885 at age 28. The eclipse of Iapetus was one of several unique observations that Barnard made at Lick Observatory from 1888 to 1895. These secured his reputation as a pre-eminent visual observer (courtesy: Mary Lea Shane Archives of the Lick Observatory, University of California, Santa Cruz).

The most important aspect of Barnard's record of S And was not its contents but who it was that read it. Holden, who awaited his new post as Director at Lick Observatory, read Barnard's earliest report and wrote to him. Knowing what was at stake, Barnard cultivated the contact. In July 1887 he received and accepted Holden's offer of employment as the Junior Astronomer at Lick. Not long after they began working together their relationship deteriorated. Barnard eventually despised Holden. His new job became a source of great scientific opportunity and relentless personal antagonism. He discovered more comets, detected surface markings on Io, and found the expanding shell of Nova Aurigae. His discovery of Jupiter's fifth satellite, Amalthea, was an international sensation. If his approach to S And was not as rigorous as it could have been, at Lick he had an opportunity to improve when he observed the eclipse of Iapetus. His response to the eclipse showed how much he had learned since the supernova about how to deal with stars and objects that look like stars.

Figure 3: James Clerk Maxwell (1831–1879) as photographed in 1855 at age 24 while a Fellow of Trinity College Cambridge. In his Adams Prize Essay for 1856 he demonstrated mathematically that Saturn's rings would remain stable as a system of particles. However, when it came to the collisional environment of a vast number of particles with unknown sizes and shapes, Clerk Maxwell had no mathematics to explain particle motions. He privately speculated that the equatorial region of Saturn might be constantly bombarded by particles that were displaced from their orbits by collisions. By 1889 many, including Barnard, had accepted Clerk Maxwell's hypothesis of particulate rings, but they apparently did not know his equatorial bombardment hypothesis. Barnard disbelieved and ignored his own evidence for activity between the C ring and the planet that he obtained during the eclipse of Iapetus. If he had known Clerk Maxwell's concept of equatorial bombardment, would this have influenced his interpretation? (Courtesy: Master and Fellows of Trinity College, Cambridge).

In the last half of the nineteenth century interest in Saturn was high. Observers saw two obvious bright rings that seemed to change constantly. Evidence was claimed for their rotation, lateral spreading, eccentricity, color differences, and transitory markings (Chambers, 1889). In 1850 a third ring was discovered. It became alternatively known as the gauge, gauze veil, obscure, dark, dusky, crape veil, crape, crepe or C ring between the planet and the B ring. Following his examination of historical observations, Otto W. von Struve (1853) claimed that the ring system's overall width was increasing. He estimated that the inner edge of the C ring was approaching the planet at a rate of about 60 miles annually, and that in about 300 years ring particles would reach the planet (Obituary, 1905). His controversial hypothesis that the ring system was unstable was an influence on selection of a topic for the 1856 Adams Prize Essay at the University of Cambridge. Clerk Maxwell (Brush et al., 1983: 154) (Figure 3) provided the only entry to the competition in that year. In his famous solution for how Saturn's ring system might remain stable, he concluded that

> … the rings must consist of disconnected particles … [being] either solid or liquid, but they must be

> independent [and organized as either] … a series of many concentric rings, each moving with its own velocity, and having its own systems of waves, or else a confused multitude of revolving particles, not arranged in rings, and continually coming into collision with each other.

When Barnard became interested in astronomy in 1876, Clerk Maxwell's work was still not known to or preferred by all astronomers. However, by 1889 it was common knowledge (Brush et al. 1983) so that Barnard (1890) could allude to changes in ring particle density as the obvious explanation of what he had seen during the eclipse.

Barnard could have done nothing with Iapetus without Marth (Figure 4), whose special interest was the orbital motion of planetary satellites. As a manual computer in 1889, he may have consulted *Journal für die reine und angewandte Mathematik* or Crelle's Journal, but references he certainly depended upon were older. Marth (1889d) mentioned use of Urbain-Jean-Joseph Le Verrier's tables for the Sun and planets from 1858, 1861, and 1876. Surprisingly, he still used Alexis Bouvard's tables for Jupiter and Saturn dated 1808 and Bernhard A. von Lindenau's tables for Venus dated 1810 and for Mars dated 1811 (Marth, 1889a). It is no wonder that he routinely published his predictions to encourage observers to supply new information so that he could improve orbital elements. Marth (1889c) offered some sense of his task when he wrote:

> … since my return [to Ireland] I have been hard at work and the cloudiness of the sky has been a favour; but the days are not long enough to allow more than a portion of the work to be got through, which I have laid out for myself.

In the late 1880s Iapetus was one of Marth's projects, and since Holden, who was his long-time correspondent, was in control of the world's largest telescope, Marth (1888) kept him up to date:

> If the ephemeris of Japetus is not too much in error, you may have the very rare opportunity of observing on [1888] Nov. 8 … a close passage of the satellite in the direction of the minor axis of the ring at a distance of only 14" from the centre of Saturn. As the present and the next apparition of the planet are the most favourable for procuring the best observations for determining the orbit of Japetus, I hope it may suit your plans to devote your splendid instrument also to these observations.

He also predicted an eclipse for 1-2 November 1889, and five months in advance encouraged observers with advice that

> … the rare eclipses of Iapetus by the ring-system offer the only chance of deciding several questions … No such observation has ever yet been made … Will Iapetus be visible when Cassini's division … is between the satellite and the Sun? What will be the effect of the shadow of the crape ring upon the appearance of the satellite? Favourably-placed observers will have to answer such questions … and their time will be well spent in doing so. (Marth, 1889b: 427, 429).

The eclipse lasted about 19.1 hours. Iapetus had already passed through the shadow of the rings on the evening or preceding side and was in the planet's shadow when Barnard began to observe not long after Saturn rose at Mt. Hamilton. At Lick Observatory the effect of the Cassini division on Iapetus was not observable, but eclipses by the C ring and part of the B ring on the morning or following side were visible. In his revision of Reverend Thomas W. Webb's *Celestial Objects for Common Telescopes* (1896: 198), Reverend Thomas H.E.C. Espin reported that Barnard observed the event with the 36-inch refractor. That was wrong, but it was an understandable mistake for why should he not have used the great telescope for such an event? While Holden was responsive to Marth's appeal, he did not utilize his 'splendid instrument' for the occasion. Instead, he assigned his Junior Astronomer to do the eclipse with the 12-inch refractor (Figure 5). It was not the greater priority of some competing need, but strife with Holden that kept Barnard from the great telescope that night. Holden had arranged that Barnard should have neither a regularly-assigned night on the 36-inch nor a share of its idle time. Such was their animosity that Barnard refused to make special requests for time. Holden had the 36-inch on the night of the eclipse. Regardless of Marth and Iapetus, he closed early and left for his residence without inviting Barnard to switch telescopes. Later, when the importance of Barnard's result became obvious, Holden instructed his seething subordinate to publish a false explanation as to why the 36-inch was not used (Sheehan, 1995).

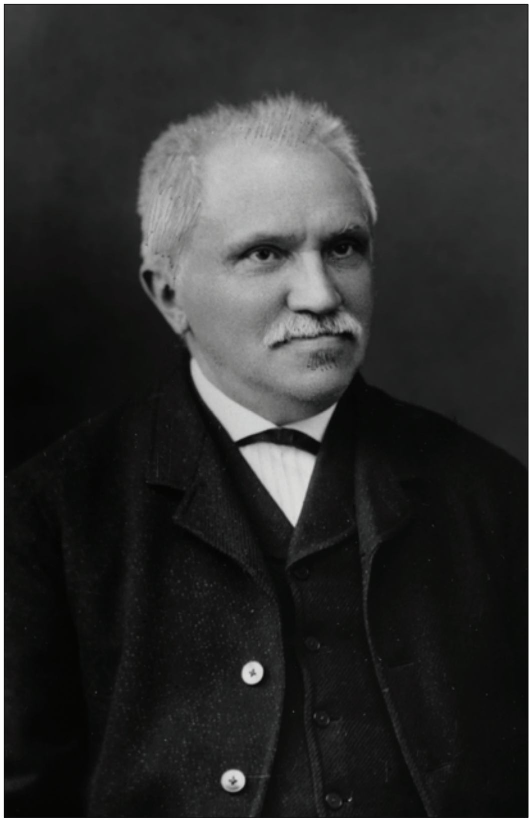


Figure 4: Albert Marth (1828–1897) as photographed in 1885 at age 57. Marth computed the ephemeris for the 1889 eclipse of Iapetus and encouraged others to observe it. Only Barnard observed the eclipse and published results (courtesy: Royal Astronomical Society).

After Marth's death in 1897, Edward B. Knobel (Obituary, 1898: 141) urged that " ... he well earned the gratitude of astronomers for the tables and ephemerides he regularly prepared for so many years for observations of the satellites ..." However, it was Marth (1890) who sent thanks to Holden regarding the eclipse of Iapetus:

> I am much obliged to you for having kindly sent me No 5 of the Publications of your new Astron. Soc. [of the Pacific], so that I have learnt how successful Prof. Barnard has been in observing the reappearance of Japetus and that the prediction has not been made in vain.

Holden would have been courteous and possibly grateful as well, but he would not have revealed to Marth the extent to which his indifference or bungling affected the event.

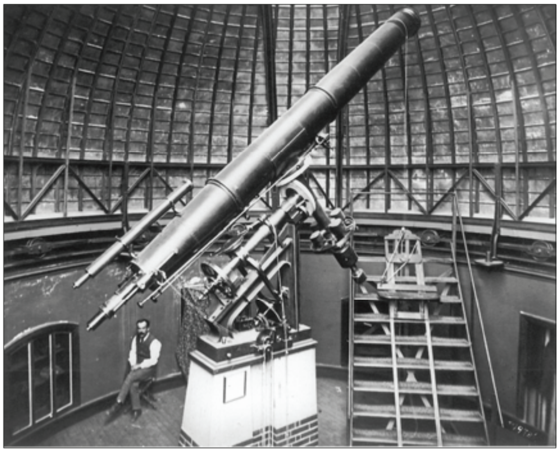


Figure 5: The Alvan Clark 12-inch refractor that Barnard used to observe the eclipse of Iapetus in 1889. It was installed in 1881 and remained in service until 1979 when it was removed in favor of the Anna Nickel 40-inch reflector. The 12-inch is stored on Mt. Hamilton (courtesy: Mary Lea Shane Archives of the Lick Observatory, University of California, Santa Cruz).

3 BARNARD'S METHOD

Not long after Saturn rose, Barnard found that Iapetus was still eclipsed by the planet. He described its reappearance:

> At 5h 25m sidereal time the satellite was faintly caught [as it emerged from the planet's shadow], and for at least one half minute before this it was seen, but so faint and uncertain that it was not recorded. At the above time it was about as bright as Enceladus. Its light increased pretty rapidly. The point of appearance formed a right-angled triangle with Tethys and Enceladus …
>
> The idea at once occurred that it would be an excellent plan to test the effect of the shadow of the crape ring on the visibility of the satellite, by frequent comparisons of the light of Iapetus with that of Tethys and Enceladus. A series of comparisons was therefore begun. The standard of comparison was the difference of brightness between Tethys and Enceladus—this quantity being mentally divided into ten equal parts. (Barnard, 1890: 107).

To divide into ten parts the difference in light intensity between Tethys and Enceladus resembles Edward C. Pickering's (1882) step-estimation process. This was a variant of Friedrich Argelander's (1844) method that was intended for use with variable stars. According to Seth C. Chandler, Jr. (1885: 247), "… Argelander's method of observation has proved, in precision, convenience, and fruitfulness, superior to any photometric apparatus yet devised." Barnard's record for S And makes clear that he did not know either form of step estimation in 1885. If he did not learn it independently, someone on Mt. Hamilton probably taught it to him. The most likely teacher was his friend Sherburne W. Burnham who had been a participant on the international Committee on Standards of Stellar Magnitudes. The Committee intended to produce a system of standard stars to reduce confusion created by multiple, competing magnitude scales (Pickering et al., 1881). Since Burnham (1889) used Argelander's stellar magnitude scale and was involved with Pickering's Committee, it is likely that he knew at least one form of step estimation. Holden was also on the Committee, but it seems unlikely that Barnard would have learned the technique from him. Regardless of how he learned it, 69 years after the eclipse of Iapetus, Allan Cook and Frederick Franklin (1958: 378) wrote that Barnard's magnitude estimates for Iapetus in eclipse were still "… probably the best transmission data concerning the optical thickness of both rings B and C."

Barnard's plan was simple. He intended to compare the changing illumination of Iapetus to unchanging Tethys and Enceladus. The surfaces of these icy worlds are active. Saturn's satellites emit dust into and collect it from the space in which they orbit. In particular, Enceladus is a spectacular emitter of water ice particles that probably compose the E ring (Spahn et al., 2006, NASA/JPL/SSI, 2006a). The photometric characteristics of these bodies are complex. Except for one fact, their environments were unknown in 1889. Iapetus has bright and dark hemispheres that cause its apparent magnitude to change significantly with orbital phase. By comparison, changes in the magnitudes of Tethys and Enceladus are subtle. The short time in which Barnard observed made his method practical.

Pickering (1879) found that Tethys and Enceladus differed by 0.94 magnitude, but Barnard (1890), with no standard stars and only instinct to guide him, correctly believed that the difference was greater. The three satellites each display one hemisphere to Saturn. Further, all three have non-uniform albedos, particularly Iapetus. Consequently, solar phase angle (α), orbital phase angle (θ) and sub-observer latitude control orbital cyclic changes in their apparent magnitudes. Solar phase is the angle subtended at Saturn by the Sun and Earth. The maximum value is about 6.3º, which occurs at quadrature. Barnard observed near 5.9º. Orbital phase is the angular distance of a satellite from geocentric superior conjunction (GSC) or the point at which the satellite is on the far side of Saturn, 180º from Earth. An orbital phase angle of 270° is western elongation. Iapetus reached GSC on 2.80 November 1889 (U.S. Naval Observatory, 1886: 479). Barnard observed a little more than one day before GSC so that Iapetus' orbital phase angle should have been about 355º. Tethys was approaching western elongation with perhaps $250^\circ \leq \theta \leq 260^\circ$. Enceladus was trailing at about $220^\circ \leq \theta \leq 240^\circ$. The sub-observer latitude was in the southern hemispheres of both Tethys and Enceladus. Anne Verbiscer (pers. comm., 2005) used the Hubble Space Telescope to observe the satellites at southerly sub-observer latitudes. With $\alpha = 6.014^\circ$, she found for Enceladus $V = 11.807 \pm 0.006$ and 11.844 ± 0.006 at respective orbital longitudes of 289.70º and 303.38º. With $\alpha = 6.258^\circ$, she found for Tethys $V = 10.418 \pm 0.003$ at orbital longitude 177.00º, and estimated that the satellite would be 0.05 magnitude fainter at greatest western elongation. I adopt a difference between the two satellites of 1.4 ± 0.1 magnitudes.

According to William Gray (pers. comm., 2005), Saturn was about magnitude 1, and the major axis of the rings was about 37.5 arc seconds. Barnard (1889a) observed the entire event at a magnification of 150× in an actual field of view of about 16 arc minutes. He found Iapetus, Tethys, and Enceladus to be closely arranged in a triangle with sides of 13, 17, and 19 arc seconds. Enceladus was about 6 arc seconds from the outer edge of the rings. A modern ephemeris shows the triangle of satellites with sides of 11, 22, and 23 arc seconds with Enceladus at about 13 arc seconds from the rings (Gray, pers. comm., 2005). Motion by Tethys and Enceladus, with orbital periods of 1.888 and 1.370 days, respectively, could have noticeably rearranged the triangle. However, both satellites were approaching western elongation so that both had diminished angular movement on the sky. Iapetus' orbital period of about 79.33 days made its apparent motion slow.

Barnard (1890, 1889a) presented his observations in a table with an accompanying light curve (Figure 6), but selected entries from his notebook give a better sense of his experience at the telescope:

5h 25m	K = Japetus = F [,] faint at sid[ereal] 5h 25m [,] saw it probably 1m earlier v. faint [a second note] at sid 5h 25m Japetus = F = Enceladus
5 29	a little brighter than F say 2/10 the dist[ance]
5 30	nearer br[ightness] of C than F[;] = 7/10 ±
5 35	K is 8/10 from F to C in brightness …
7 50	= 0/10 or perhaps a little less than F …
7 56	-3/10 or -4/10 vvf
7 58	-5/10 eef
7 59	-6/10 eeef
8 01	no trace[;] F easy at this time

Letters denote the three satellites: Iapetus (K), Tethys (C), and Enceladus (F). Time is local sidereal time. Fractions give the changing light intensity, e.g. 8/10 means 8 arbitrary steps brighter than Enceladus and 2 arbitrary steps fainter than Tethys while -4/10 means 4 steps fainter than Enceladus and 14 steps fainter than Tethys. The notes also use language that was intended for comets and nebulae. The terms 'vvf', 'eef', and 'eeef' are descriptions of faintness in which 'v' means very and 'e' means extremely.

4 BARNARD'S RESULTS

Barnard (1892: 121) summarized the eclipse as having "… given us more information about the crape ring of Saturn, perhaps, than could possibly have been obtained by a hundred years of ordinary observing." His advantages of natural ability, experience, location, equipment, and the rare opportunity to observe the rings in transmission promised a leap forward. Barnard's (1890: 109) conclusion for the density of the B ring was new information, but his result for the C ring was a model of anticlimax:

> … the crape ring is truly transparent—the sunlight sifting through it. The particles composing it cut off an appreciable quantity of sunlight. They cluster more thickly … as it approaches the bright rings … so far as the penetration of the solar rays is concerned, the bright ring is fully as opaque as the globe of Saturn itself.

In 1852 August William S. Jacob became probably the first person to see the translucent condition of the C ring (Alexander, 1962). However, Etienne L. Trouvelot (1877: 191) thoroughly pre-empted Barnard's conclusion by reporting that

> … the inner portion of the dusky ring disappears in the light of the planet at that part which is projected upon its disk … the dusky ring is not transparent throughout, contrary to all the observations made hitherto; … it grows more dense as it recedes from the planet … at about the middle of its width, the limb of the planet ceases entirely to be seen through it.

Barnard's largely confirmatory result ensured a quiet public reception, but the matter of what he saw and what it meant is not repetitious of others' work. It is also not easily explained.

Ten years before Pioneer 11 arrived at Saturn, Pierre Guérin (1970) announced his photographic discovery of what he called the D ring. It was a tenuous object located between the C ring and the upper atmosphere of the planet. According to Mark Showalter (1996: 677), "… for the remainder of the 1970's, numerous astronomers attempted to confirm the D Ring's existence, with mixed results …" In considering Cook's and Franklin's reliance on Bar-nard, Ignacio Ferrín (1974: 168) ventured "… that these [Barnard's] observations contain evidence of ring D discovered by Guérin …" Ferrín concluded that his own measurements of Guérin's images were "… in excellent agreement with the observations of Barnard … Without suspecting its existence, this ring had been observed by him in 1890."

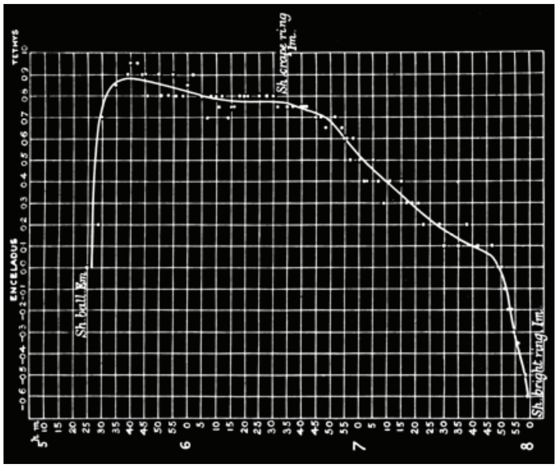


Figure 6: Barnard's light curve for the eclipse of Iapetus. Originally oriented with its text horizontal, his figure is presented here with a vertical magnitude axis according to modern use. Iapetus emerged from the planet's shadow at "Sh[adow] ball Em[ersion]," reached greatest brightness soon thereafter, faded slightly and entered the shadow of the C ring at "Sh.[adow] crape ring Im[mersion]," and disappeared into the shadow of the B ring at "Sh[adow] bright ring Im[mersion]." (Reprinted from "Observations of the eclipse of Iapetus in the shadows of the globe, crape ring, and bright ring of Saturn, 1889 November 1" by E.E. Barnard, *MNRAS*, 50, 107-110, 1890, figure entitled "Light curve of the eclipse of Iapetus …" with permission from Blackwell Publishing.)

However, Barnard did suspect something. He did not suggest that what he saw was real and certainly not that it was a discovery, but he was aware of a situation in his light curve. He could hardly have missed it since 40% of his data defined the effect. The longer he considered the matter, the more certain he became that it meant nothing. On 6 November 1889 he wrote for *Publications of the Astronomical Society of the Pacific* a preliminary account of the eclipse in which he identified an anomaly in the light curve for Iapetus, a decrease of 0.1 magnitude or one-tenth of the difference in brightness between Tethys and Enceladus. Since the two satellites differ by about 1.4 magnitudes, the change he noted was about 0.14 magnitude:

> Japetus required a little over ten minutes to become wholly free from the shadow of the ball. After remaining at its full brightness for fifteen minutes, it began very slowly to decrease in light; however changing less than 0.1 magnitude in forty minutes' time. (Barnard, 1889b: 127).

A month later he sent his first detailed account to *Monthly Notices of the Royal Astronomical Society*. Now he explained away the anomaly:

> I do not understand the slight decrease of light so soon after the maximum had been reached, as it is evident from the curve that the satellite did not experience the effects of the crape ring until 6h 35m. If, however, we consider that the variation of light between 5h 40m and 6h 15m represents only 0.1 of a magnitude, it has less signification … I would rather refer this peculiarity to the fact that the seeing became better, and a fairer estimation could therefore be made of the relative light; if so the curve should be flatter near 5h 35m … (Barnard, 1890: 108-109).

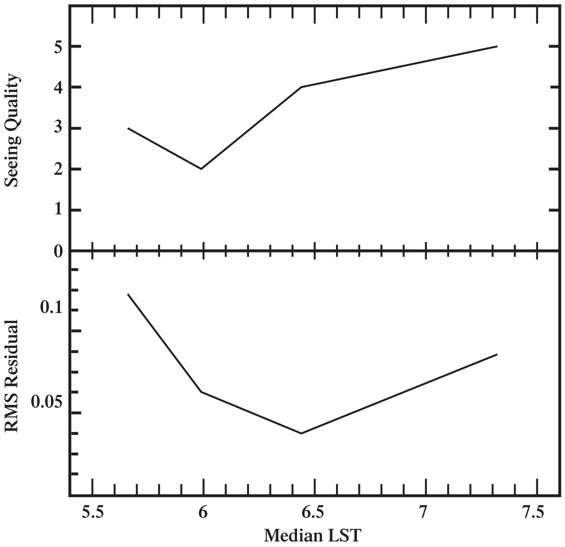


Figure 7: Seeing quality as well as root mean square residual for seeing-related subsets of estimated magnitudes are shown as functions of local sidereal time (LST) during the eclipse of Iapetus. Barnard explained Iapetus' apparent decline in brightness before it entered the C ring shadow as an effect of changes in seeing quality that adversely affected his magnitude estimates. He rated seeing quality on an arbitrary scale in which 1 was worst and 5 was best. During the eclipse Barnard (1889a) noted four episodes in which seeing changed. For the second episode, at the median time of 6 hours LST, I inferred a seeing quality of 2 since he did not assign a rating but wrote only that seeing worsened. Linear or low order polynomial fits to the data in each subset yielded root mean square residuals. Presumably, these residuals describe the accuracy or consistency of Barnard's visual estimation process. If his hypothesis is correct, decline in seeing quality should be correlated with increased residuals. There is no such correlation. Consequently, his accuracy or consistency was not affected by changes in seeing quality. Iapetus' fading trend before the C ring eclipse had another explanation.

In *Astronomy and Astro-Physics*, he treated the situation as meaningless and omitted it entirely:

> Near the predicted time the satellite re-appeared from the shadow of the ball into the sunlight shining between the ball and rings. It quickly assumed its normal light, and after remaining thus for an hour and twenty minutes, it began to fade and so continued for an hour, having during that time entered and passed through the shade of the crape ring. (Barnard, 1892: 121).

In describing seeing, Barnard (1890: 109) explained that it "… ranged from 2 at the first observations of Iapetus up to 5 as dawn appeared …", with worst seeing represented as 1 and best as 5. In his notebook, Barnard (1889a) wrote that at 5h 5m "seeing = 3"; at 5h 51m "seeing getting bad"; at 6h 15m "seeing = 4 for some time"; at 6h 48m "seeing has got v[ery] good"; and at 6h 55.5m and for the duration "seeing = 5." He added that

> … in the last part of the records the seeing = 5—does not mean that it was 5 all along, I waited for steadiness to make an estimate, it fluctuated very much from 1 to 5.

To test his suggestion that accuracy of the magnitude estimates was a function of seeing, I binned Barnard's observations according to time intervals defined by seeing values that he assigned or implied. There were four recorded episodes in which seeing changed during the 2.6-hour event. To the observations in each of these subsets, I fitted linear or polynomial functions that approximated related segments of the light curve. I obtained a root mean square residual for each subset and inferred that a small residual meant more accurate or at least more consistent magnitude estimates. The results appear in Figure 7. Subset 1, in mediocre seeing, had a residual of 0.107. Against Barnard's expectation, the residual decreased to 0.055 when seeing worsened to poor in subset 2. When seeing improved to good in subset 3 the residual fell to 0.033. When seeing became excellent in subset 4, the residual contrarily grew to 0.075. Because seeing quality is not correlated with the residuals, seeing was not a controlling influence on his magnitude estimates. Something else caused Iapetus to fade prior to the C ring eclipse. In 1889 there was an hypothesis to explain what Barnard saw, but few people knew it.

Barnard and others believed that nothing existed in the space between Saturn and the C ring. However, the nineteenth century's master of planetary ring mathematics thought differently. In a letter to William Thomson (later Lord Kelvin), Clerk Maxwell (1857) described a fantastic scene that may reflect Struve's claim for spreading of the ring system:

> What shall we say to a great stratum of rubbish jostling and jumbling round Saturn without hope of rest or agreement in itself till it falls piecemeal and grinds a fiery ring round Saturns equator, leaving a wide tract of lava with dust and blocks … on each side and the western side of every hill battered with hot rocks? … As for the men of Saturn I should recommend them to go by tunnel when they cross the 'line'.

This private expression has no clear counterpart in the published Adams Prize Essay, but it may be related to his statement that

> … when we come to deal with collisions among bodies of unknown number, size, and shape, we can no longer trace the mathematical laws of their motion with any distinctness … whatever catastrophes may be indicated by the various theories we have attempted. (Brush et al., 1983: 136).

By 1889 there had been no new observational evidence that the rings were measurably spreading. Struve was no longer a factor. Clerk Maxwell, however, was very credible. His ring theory was well known, but his bombardment hypothesis was effectively unknown. Given that Barnard believed the particulate ring theory, it is likely that he would have believed, or at least considered, equatorial bombardment, if he had known about it. In that case, he might have interpreted what he saw before the C ring eclipse as Iapetus in the shadow of an unknown—in Clerk Maxwell's words—hailstorm ring (Brush et al., 1983: 48), a source of projectiles for an equatorial catastrophe. Without the bombardment hypothesis, Barnard was alone with an observation that he refused to believe and eventually excluded from the account.

5 THE D RING

When Pioneer 11 reached Saturn in 1979, the wonders it observed did not include Guérin's ring: "The D ring was not seen in any viewing geometry and its existence is doubtful." (Gehrels et al., 1980). In 1980 and 1981, Voyagers 1 and 2 swept through the Saturn system. Their imaging capability was superior to Pioneer's which enabled them to detect "… a faint inner D ring, extending to within 7,000 km of the planet's atmosphere." (Smith et al., 1982: 530). According to Showalter (1996: 677), the ring was "… vastly fainter than previous Earth-based claims…", meaning that it "… could never have been detected from the ground." He identified three narrow ringlets (D68, D72, and D73) and broad, faint, wave-like regions. However, 25 years after Voyager 1, the Cassini spacecraft observed

> … very significant changes in the appearance of the D ring … D72, which was the brightest feature in the D-ring … has decreased in brightness by more than an order of magnitude relative to the other ringlets … [and has] moved inward about 200 km … (NASA/JPL/SSI, 2005a: 1).

Amanda Bosh and Catherine Olkin (1996) used the Hubble Space Telescope to observe the first occultation of a star (GSC5249-01240) by the tenuous D ring. At wavelengths in the range 350-700 nm they found a line-of-sight optical depth of $\tau \cong 0.019$ for the densest part of the ring. From Voyager results Showalter (1996) concluded that D73 had an optical depth normal to the ring plane of $\tau_0 \cong 0.00002$. If the condition of the D ring in the 1980s and 1990s was substantially the same as its state in 1889, what do these observations mean for Barnard?

Bosh and Olkin observed when Saturn's rings had an opening angle of 2.7º. Where zero indicates unobstructed translucence and 1.0 effectively means no transmission of light, their value for the ring's optical depth normal to the ring plane is

$$\tau_0 \cong 0.019 \times \sin(2.7^\circ) \cong 0.0009. \qquad (1)$$

Barnard saw an opening angle on the rings of -8.49º (Gray, pers. comm., 2005). If in 1889 the D ring had features with normal optical depths of $\tau_0 = 0.0009$ and 0.00002, Barnard would have encountered optical depths in the line of sight of

$$\tau = 0.0009 / \sin(8.49^\circ) = 0.006 \qquad (2)$$

and

$$\tau = 0.00002 / \sin(8.49^\circ) = 0.00014. \qquad (3)$$

Line-of-sight optical depth is related to change in magnitude (Δm) by the approximation,

$$\tau = \Delta m / 1.09. \qquad (4)$$

In the shadows of such features, Iapetus should have faded by approximately 0.007 and 0.00015 magnitude. This would have been a non-event for Barnard. Yet he recorded a gradual decrease in Iapetus of about 0.14 magnitude in the vicinity of the D ring. An opening angle of -8.49º implies a line-of-sight optical depth of $\tau = 0.13$ and a normal optical depth of $\tau_0 \cong 0.02$, about 20 times what Bosh and Olkin observed and about 1,000 times Showalter's result. The Cassini spacecraft has demonstrated that the D ring changes relatively quickly, but for Barnard to have seen its shadow, the ring needed radically greater density. If it was not dense enough to create a visual effect 117 years ago, another cause must explain what he saw. Observational error is the most obvious possibility.

6 FACTORS THAT AFFECTED BARNARD'S OBSERVATION

Barnard's 'excellent plan' was not easy to accomplish. Interference by light from Saturn, the large difference of 1.4 magnitudes between the standard objects, the smallness of the unanticipated change in Iapetus' brightness prior to the C ring eclipse, a risk of bias due to the short time interval between his observations, and a risk of position angle error in his magnitude estimates were all factors, influences, and possibilities.

Saturn introduced scattered light into the telescopic field, but Barnard neither commented nor complained. His discovery of the fifth satellite of Jupiter gives the best indication of his sense of a faint satellite near a bright planet. He discovered Amalthea ($V \geq 14.1$) when he moved the brilliant planet just outside the field. Otherwise, with any part of Jupiter in view, the satellite became invisible. In congratulating Barnard, E. Walter Maunder (1894), at the Royal Observatory (Greenwich), described his own experience:

> I have tried hard again & again to catch a glimpse of your fifth satellite with our new 28 inch telescope, but only succeeded on two occasions in just fancying I saw it for a moment … my ill-success has given me a very high idea of the skill, patience, & keenness of sight which you must possess to have made the original discovery.

Presumably, three years earlier the same skill, patience, and keenness of sight had no trouble with three satellites near Saturn.

The difference of 1.4 magnitudes between Enceladus and Tethys is large for visual magnitude estimation, but it seems not to have been a problem. Barnard divided the intensity difference between Enceladus and Tethys into tenths or 0.14 magnitude units. However, his step—the smallest difference in light intensity he could actually see—was yet smaller. When Barnard detected the inner edge of the C ring, he did so by showing change in Iapetus of 0.07 magnitude. Similar fine changes appear elsewhere in the light curve. Many visual variable star observers have steps of 0.1 magnitude, but the best observers are more sensitive. Two of Barnard's steps describe how Iapetus faded between its maximum brightness and the C-D ring boundary, meaning that the detection was for him somewhat better than marginal. In a related matter, color is an issue for visual magnitude estimation, but

Barnard's response to color may not have been a factor since the three satellites have about the same color. Cox (2000) reported mean color indices in the range $0.70 \leq B\text{-}V \leq 0.73$. For Iapetus in orbital phases $92^\circ \leq \theta \leq 270^\circ$, $0.82 \leq B\text{-}V \leq 0.69$ (Millis, 1977). For Tethys in orbital phases $247^\circ \leq \theta \leq 327^\circ$, $0.63 \leq B\text{-}V \leq 0.79$ (Blair and Owen, 1974). Similar ranges for Enceladus were not available.

He expected to obtain a featureless light curve for Iapetus in the gap between planet and rings as he implied when he wrote, "… the curve should be flatter near 5h 35m, to correspond with that near 6h 25m." (Barnard, 1890: 109). This was an assumption not a fact. His anticipation of events could have been a problem since visual observers are known to see what they believe. Yet, given that he recorded Iapetus' declining light, not its constancy, in 30 estimates before the C ring eclipse, Barnard's objectivity went unharmed. He also observed at a rate of about one estimated magnitude every two minutes. That pace invited other bias because his estimates were not independent. He had no time to forget previous estimates and trends. The C-D ring boundary is a subtle transition that a biased observer might miss. Barnard correctly identified the boundary at 1.235 planetary radii. Regardless of what he anticipated and of what he knew about his own observations, he remained objective.

The arrangement of stars in a field of view can affect a visual observer's sense of their brightness. When two stars of equal brightness and similar color are arranged one above the other, "… the lower will appear the brighter, perhaps by as much as half a magnitude." (Isles and Lewis, 1990: 40-41). The effect is known as position angle error. Barnard probably knew nothing about it. An observer can avoid the problem by arranging pairs of stars horizontally. It is not possible to be certain how Barnard oriented the three satellites. His notes do not discuss this, but his sketch of the scene implies that he kept Iapetus low relative to Tethys and Enceladus throughout the eclipse. When Iapetus arrived at 1.06 planetary radii it reached maximum brightness. "It was then about 0.1 magnitude less than Tethys." (Barnard, 1889b: 127) or about magnitude 10.6. He observed when the solar phase angle was about 5.9° and Iapetus' orbital phase was about 355°. Under similar circumstances, Robert Millis (1973) measured Iapetus to be $V \cong 10.9$, corrected to Saturn's mean opposition distance. The difference of 0.3 magnitude may have been position angle error that affected all of his estimates. If so, peak brightness for Barnard's light curve can be set according to Millis.

With the exception of position angle error, Barnard's estimated magnitudes appear to be reasonably correct.

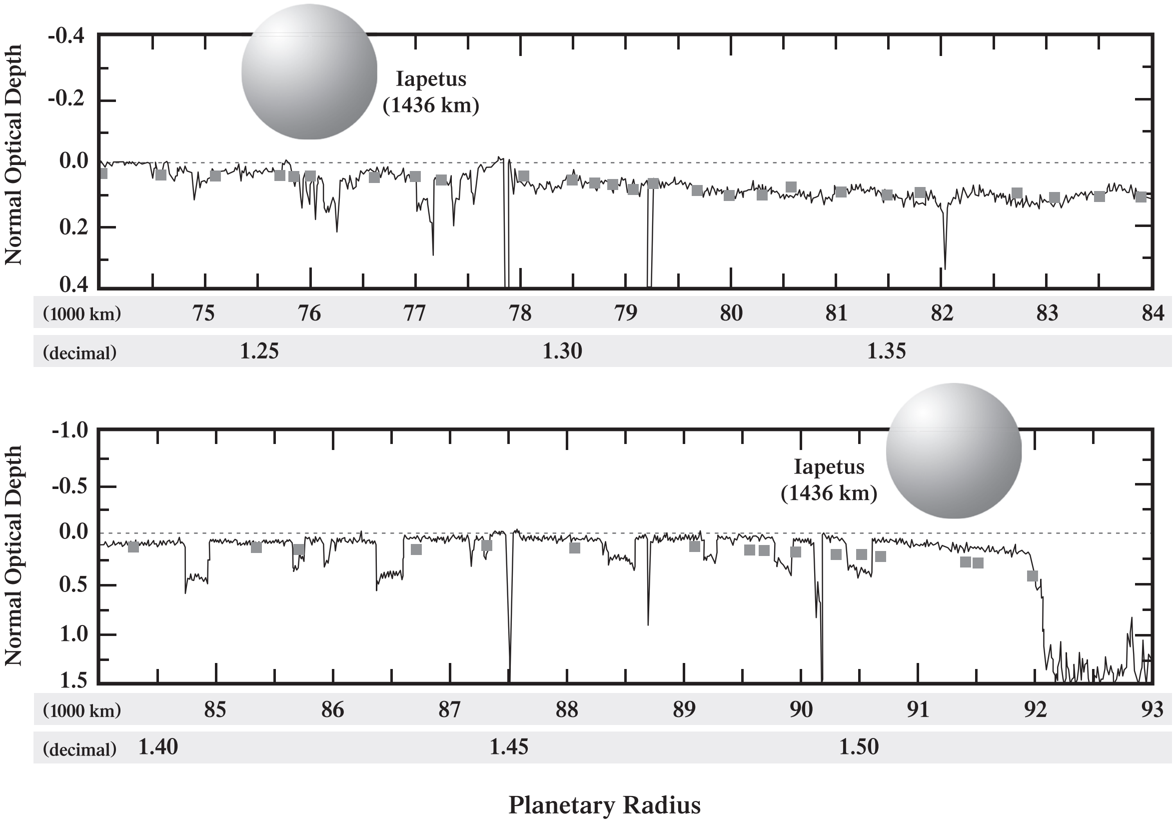


Figure 8: High resolution transmission curve of the C ring. Superimposed upon the modern transmission curve are normal optical depths (solid squares) derived from each of Barnard's visual magnitude estimates of Iapetus in eclipse. Squares indicate the center of Iapetus at the time of each of his observations. In the first half of the C ring eclipse Barnard's results agree with modern values. In the second half, his results are consistently greater than modern values. Uncertainty is about ± 0.02 normal optical depth based on an uncertainty of ± 0.1 magnitude in his estimates. Iapetus was large relative to the C ring's fine ringlets and plateaus which explains why Barnard did not resolve these features. (Transmission curve reprinted with permission of R.G. French and Elsevier from "Geometry of the Saturn system from the 3 July 1989 occultation of 28 Sgr and Voyager observations" by R.G. French et al., *Icarus*, 103, 163-214, 1993, figure 4, "Atlas of ring feature designations," copyright 1993 Elsevier.)

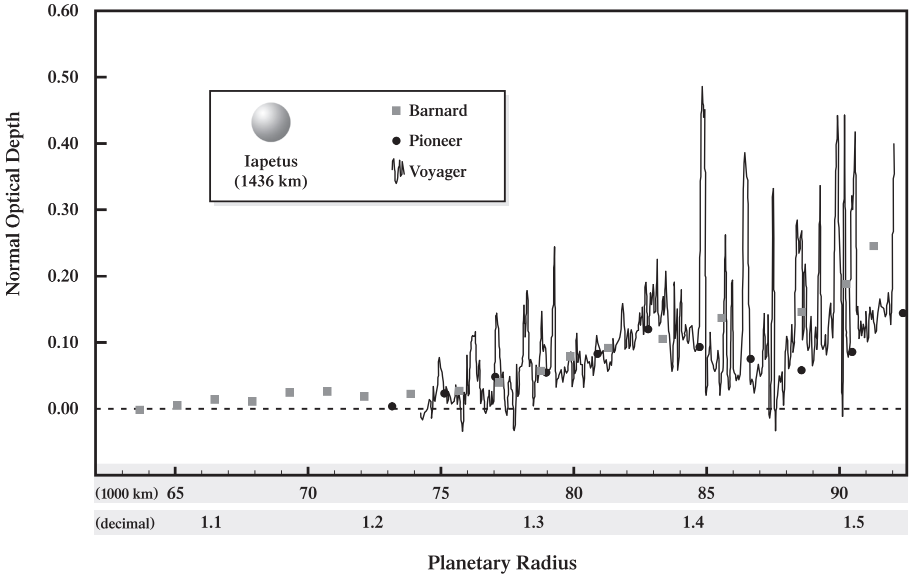


Figure 9: Low resolution transmission curve for the C ring. The plotted curve represents observations by Voyager 2. Filled circles are Pioneer 11 results. Filled squares are means of four normal optical depths as derived from Barnard's visual magnitude estimates of Iapetus in eclipse. Barnard correctly identified the faint C-D ring boundary at 1.235 planetary radii. He also correctly concluded that there is no gap at the B-C ring boundary at 1.525 planetary radii. He agreed with the general trend of modern transmission values in the inner C ring but disagreed in the outer C ring and D ring. Is this disagreement due to observational error or is it evidence for a condition that was not present for Pioneer 11 and Voyager 2? (Transmission curves for Voyager 2 and Pioneer 11 reprinted with permission of B.R. Sandel and Science from "Extreme ultraviolet observations from the Voyager 2 encounter with Saturn" by B.R. Sandel et al., *Science*, 215, 548-553, 1982, figure 4, "Normal optical depths in the C ring determined by the UVS during the delta Sco exit of the rings," copyright 1982 Science.)

7 BARNARD AND THE C RING

It would help to compare Barnard's results with those of other observers, but there are none. There are, however, modern observations of optical depth in the C ring. To compare Barnard's observations to what is known about the C ring today requires the assumption that the visual appearance of the ring has not changed significantly in more than a century. I converted Barnard's visual magnitude differences to differences in V magnitude and further converted these to optical depth in the line of sight and finally to optical depth normal to the plane of Saturn's rings. I plotted his transmission curve with transmission curves from modern sources including Pioneer 11 and Voyagers 1 and 2 as shown in Figures 8 and 9. The uncertainty of Barnard's estimated magnitudes is about ±0.01 while uncertainty of derived normal optical depths is about ±0.02.

Figure 8 plots all of Barnard's optical depths on a transmission curve of the C ring by Richard French et al. (1993) obtained from an occultation of 28 Sgr and the Voyagers. The C ring has wave-like structure that is interrupted by optically deep ringlets and plateaus that are narrower than the diameter of Iapetus. Shadows from all of this fell upon the satellite. Did Barnard resolve structure in the C ring? From 1.488 to 1.502 planetary radii, Iapetus encountered the shadows of two plateaus and a ringlet with widths from 60 to 200 km and normal optical depths from 0.25 to more than 0.50. The -8.49º opening angle on Saturn's rings created line of sight optical depths for these features that were great enough to substantially dim an occulted star, but Barnard saw no effect on Iapetus from this combination of narrow, deep shadows. Presumably, the satellite was too large to be affected.

However, he was able to recognize the C-D ring boundary and the B-C ring boundary. As to the latter, since the 1850s some observers argued that a division existed between the B and C rings (Alexander, 1962). Barnard (1895: 369) gave his opinion:

> No division was seen between the Crape ring and the inner bright ring, as has sometimes been shown on drawings. This supposed division, however, was proved to have no real existence by my observations of the eclipse of Japetus in the shadows of the rings 1889 November 1.

Yet conflicting reports of the division's existence persisted into the twentieth century. Based on Pioneer 11's observation of the unilluminated side of Saturn's rings, Gehrels et al. (1980) continued to identify and discuss such a division. As observed in forward scatter, the claimed location, between 1.50 to 1.52 planetary radii, was second in brightness only to the dusty Cassini division. After the Voyagers, Esposito (1984: 470) described this as "… a region of increased transparency containing a number of opaque ringlets …" that was unlike other divisions. Based on Cassini observations, Joshua Colwell (pers. comm., 2005) commented that

> … the transition to the B ring inner edge is morphologically very similar to that at the inner edge of the A ring, and the outer C ring does look very much like the Cassini Division interior to the A ring.

He concluded, as did Barnard, that no division exists between the B and C rings.

Figure 9 compares Barnard's results to normal optical depths from Voyager 2 and Pioneer 11, as presented by Bill R. Sandel et al. (1982). In this figure no modern transmission curve is shown inside the C-D ring boundary because Pioneer 11 did not observe the D ring and because the D ring's intensity, according to the Voyagers, was very much weaker than the C ring.

Consequently, there is nothing to compare to Barnard within this boundary. The C ring is about 17,500 km wide. Barnard agrees with Voyager 2 and Pioneer 11 over the first 9,000 km, from 1.235 to about 1.384 planetary radii. Agreement means only that he anticipated the general trend of normal optical depth as measured by the spacecrafts. However, elsewhere there is disagreement. From 1.074 to 1.235 planetary radii his optical depths are greater than can be explained by the modern D ring. That he was accurate at this point must be inferred from his corroborated results in the nearby C ring where, from 1.235 to about 1.28 planetary radii, he obtained similar intensities. Another disagreement occurs from about 1.384 planetary radii to the B-C ring boundary where Barnard's values dramatically diverge into greater densities. He did not recognize this because he could not distinguish normal from abnormal densities in the C ring. His last nine positive magnitude estimates were made in the range 1.485 to 1.517 planetary radii when Iapetus approached and then entered the B-C ring boundary. These observations are significant because he saw Iapetus become fainter than Enceladus. That should not have happened as early as he saw it.

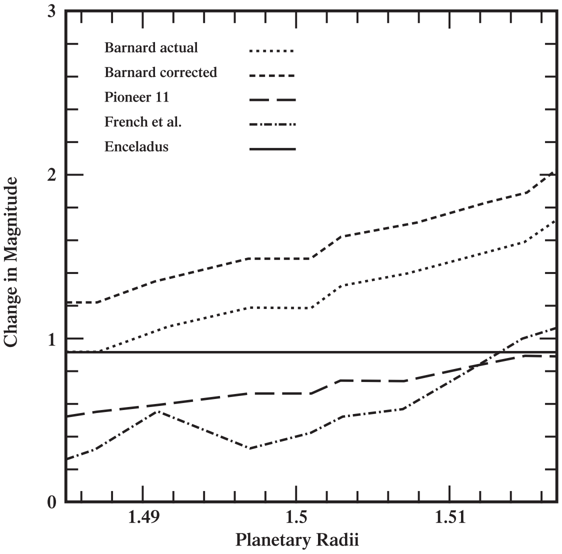


Figure 10: The plot compares Barnard's last nine differential magnitudes for Iapetus in eclipse to predicted differential magnitudes for the satellite based on modern optical depths for the C ring from 1.485 to 1.517 planetary radii. The unchanging magnitude of Enceladus (V = 11.8) is indicated for further comparison. Zero on the differential magnitude axis corresponds to Iapetus' magnitude outside eclipse (V = 10.9). Barnard saw Iapetus equal Enceladus at 1.485 and 1.487 planetary radii. Thereafter, Iapetus became fainter than Enceladus until he saw it disappear into the B ring's shadow. If the C ring's transmission characteristics in 1889 were as they are today, Iapetus should have equalled Enceladus' magnitude only near Barnard's last two observations at 1.515 and 1.517 planetary radii. As a complication, his magnitude estimates of Iapetus appear to be systematically too bright by about 0.3 magnitude due to position angle error, a fault peculiar to visual magnitude estimates in specific circumstances. Even with an apparent tendency to over-estimate its brightness, Barnard saw Iapetus become fainter than Enceladus too early. The C ring in 1889 might have been visually different than it is today, but another possibility is that spokes were present in the C ring at the time of the eclipse. At the B-C ring boundary the difference between Barnard's results and modern normal optical depths is about 0.1 which is similar to the observed density of spokes.

Figure 10 presents his last nine positive estimates of Iapetus as differences in magnitude. It relates Iapetus' observed magnitude during eclipse to its magnitude outside of eclipse and to predicted magnitudes based on modern optical depths for the C ring from 1.485 to 1.517 planetary radii. It also compares changing Iapetus to unchanging Enceladus. At 1.485 planetary radii Barnard estimated that Iapetus and Enceladus were equally bright. That is relevant for two reasons: the effect of position angle error on his estimates, and, most importantly, the implication of modern optical depths. Barnard identified Iapetus' peak visual magnitude at about 10.6. I correct that to V = 10.9 and apply the difference of 0.3 magnitude to his other estimates as a uniform correction for likely position angle error. Iapetus at V = 10.9 was about 0.9 magnitude brighter than Enceladus at V = 11.8. For the range 1.485 to 1.517 planetary radii, based on Pioneer 11 in Figure 6, the general trend of normal optical depth increased from about 0.07 to 0.12. Alternatively, Figure 5 shows approximately 0.04 to 0.14. Assuming that modern optical depths are relevant to the C ring in 1889, at 1.485 planetary radii, Barnard should not have seen equality. Iapetus was probably already fainter than Enceladus, with an approximate visual magnitude of 12.1. Only position angle error made it seem as bright as Enceladus. According to modern optical depths and without the effect of position angle error, he should have seen Iapetus to be brighter than Enceladus by 0.4 to 0.7 magnitude. The only moments during the C ring eclipse at which the two satellites should have been nearly equal were his last two observations at 1.515 and 1.517 planetary radii, very near the B-C ring boundary. However, by 1.517 planetary radii, despite his apparent over-estimates of its brightness, he still recorded Iapetus as about 0.8 magnitude fainter than Enceladus. At an opening angle of -8.49°, an increase in normal optical depth of about 0.11 could have affected Iapetus in this way. Modern optical depths imply that Barnard observed the C ring to be unusually dense over a large radial distance. If the ring has not significantly changed since 1889, another possibility is that something transitory affected it simultaneously with the eclipse.

Showalter (1998) observed transient clumps in Saturn's F ring. These appeared unexpectedly and faded in brightness over about two weeks. He interpreted them to be "… burst events …" and suggested that they are caused by high-speed impacts of approximately 10 cm-diameter meteoroids on ring bodies. Alternatively, burst events may be produced by relatively slow collisions among rubble pile moons (Barbara and Esposito, 2002).

With the equation

$$A' / A = (\pi r^2 N^{1/3}) / \pi R^2, \qquad (5)$$

it can be shown that a cloud of particles from the destruction of a small moon could produce a shadow that would reduce Iapetus' magnitude by 14% as Barnard observed just before the satellite entered the shadow of the C ring. Where A'/A is the ratio of the area obscured by the particle cloud to the area of Iapetus, r is the radius of the disrupted moon, R is the radius of Iapetus, and N is the number of particles created in the burst event, the equation shows that for moons with radii of 5 and 10 km, 2.4×10^{10} and 3.8×10^8 particles, respectively, spread across the area of Iapetus (1.6×10^6 km^2) would cause 14% obscuration.

However, even if objects with radii of 5 to 10 km exist in the outer D ring, this scenario does not explain Barnard's situation. What he observed involved a very much larger area. Ring spokes can cover large areas.

8 RING SPOKES

Spokes are tenuous, dark, ephemeral objects that appear to be "… confined to the central B ring with an inner boundary at 1.72 ± 0.01 … [planetary radii] and an outer boundary at approximately the outer edge of the B ring." (Smith et al., 1982: 535). Maximum radial and azimuthal dimensions of 8,000 and 20,000 km, respectively, have been observed, but narrow and filamentary shapes also occur (Grün et al., 1983). They are dark in back-scattered light and bright in forward scatter which indicates that they consist of fine dust. Spokes appear at any azimuth on the rings but most often at the eastern or morning ansa. They last for about one-fourth to one-third of the orbital period of the magnetic field (de Pater and Lissauer, 2001), 10 hours 39.4 minutes. Their typical normal optical depth is about 0.1 (Grün et al., 1983).

There is no consensus for the cause of spokes (de Pater and Lissauer, 2001), however, they are thought to be charged dust particles with a size of a micrometer or less that are levitated over the rings through electrostatic repulsion. Their radial orientation seems to last as long as dust is being added, but they spread and become patchy through loss of dust and Keplerian motion. Spokes are active at and near the corotation distance, 1.86 planetary radii, where Keplerian circular velocity equals the planet's angular velocity as defined by the rotational period of the planet's magnetic field. Carolyn Porco and Edward Danielson (1982) and Porco (1988) found that changes in the appearance of spokes are correlated with the orbital periods of the magnetic field and of broadband radio emissions called Saturn Electrostatic Discharges. Even so, Christoph Goertz and Gregor Morfill (1983) urged that gravity, not electromagnetic force, dominates the motion of ring dust particles. They proposed that dense plasma columns are created as meteoroids impact ring bodies and that these columns eventually corotate with the planet's magnetic field. Charged dust particles in the plasma cloud are electrostatically expelled from their resting places in the ring when the electric force becomes stronger than gravity. Colleen McGhee et al. (2005: 517, 508) examined the photometric properties of spokes as recorded in Hubble Space Telescope (HST) images from 1994 to 2004. Spokes were visible on either side of ring plane crossing but became fewer and fainter until no spokes were observed beyond an opening angle of -15.43º. After modeling alternative arrangements of dust relative to the rings, they concluded that "… the strong tilt effect on spoke contrast can be accounted for as a result of varying viewing and illumination geometry of an extended layer of dust that lies above the ring itself." Although they predicted that "… spokes should be easily detectable during the Cassini mission when the rings are viewed at relatively small ($|B| \leq 10°$) ring opening angles …" it took from July 2004, when Cassini achieved orbit, until September 2005 before the spacecraft observed spokes. These appeared on the dark side of the rings when the angle to the spacecraft was 13.5° (Mitchell et al., 2006: 1587). Colin Mitchell and Mihaly Horányi (2005: 1) proposed

> … that the absence of spokes [earlier in the mission] is due to a seasonal modulation of the plasma environment in the rings. The photoelectron density above the rings is determined by solar irradiance, hence the elevation angle of the Sun.

Porco et al. (2005: 1229) further described the plasma environment.

> High Sun creates a layer of photoelectrons above the rings that can negatively charge small dust particles above the rings, pulling them back to the (positively) charged rings. A low … [Sun] angle reduces the … photoelectron layer, causing dust particles to have a net zero (or slightly positive) charge and therefore to be repelled by the … ring. The relatively high Sun elevation at present may create an environment hostile to the appearance of spokes.

Mitchell et al. (2006: 1589) anticipated that spokes will be seen in mid to late-2006 "… if the plasma conditions are favorable for their formation and either the observer or the Sun is near the ring plane."

Barnard's observation is consistent with three properties of spokes. First, spokes are best seen at low opening or solar illumination angles. McGhee et al. (2005: 517) found that "… a relatively low optical depth … is sufficient to produce the observed contrast [between spokes and their surroundings] …" when viewing or solar illumination angles are small. For Barnard, a relatively low angle deepened ring shadows on Iapetus presumably making it easier for him to see changes in illumination. Secondly, spokes can occupy long radial distances and broad areas. His transmission curve identifies what could be interpreted as two radially extended features that were superimposed upon the B, C, and D rings. The D ring feature had a radial extent of about 9,000 km. The orbital period is about 5.3 hours in the middle D ring. Since Iapetus took about 35 minutes to transit the feature, spokes would have had to extend about 40° or 49,000 km along the arc of the ring. The C ring feature covered at least 7,500 km and continued into the B ring. At a radius of 90,000 km the orbital period is about 7.7 hours. Iapetus took about 40 minutes to transit the feature. Spokes would have had to extend about 30° or 47,000 km along the arc of the C ring. Thirdly, spokes have normal optical depths of about 0.1. What Barnard saw had a normal optical depth of about 0.1 at its densest point on the B-C ring boundary. Nevertheless, to explain his observation with spokes is unconventional. Planetary scientists believe that they are limited to the central B ring.

Evidence for spokes in the B ring and for spoke-like features in the A and C rings has been collected by visual observers for a long time. Stephen J. O'Meara is the most successful visual observer of spokes. Beginning in 1976 he used 9 and 7.25-inch refractors to visually estimate 0.1 magnitude azimuthal variations in the brightness of the A ring. Observing in twilight to diminish the apparent brightness of the A ring, he unexpectedly found dark radial features in the B ring. These had the rotational period of the planet and did not exhibit Keplerian motion. They tended to prefer the morning ansa and their visibility was related to ring opening angle. His reports were disbelieved, and his attempts to publish were refused. After the Saturn Conference in Tucson, Arizona in May 1982 O'Meara was recognized for his results, but these made no lasting impression outside amateur astronomy even when events were fresh:

> Visual observers have also occasionally claimed to see transient, dark radial features and bright spots in the rings (Alexander 1962). These reports are especially intriguing in the light of Voyager discovery of spokes … however like many other visual reports, they are difficult to assess objectively. (Cuzzi et al., 1984: 75).

What O'Meara did entitled him to discovery credit, but historically others came close to that distinction.

9 HISTORICAL OBSERVERS AND O'MEARA

Barnard did not believe that dark features on Saturn's rings were real. Even so, his result for the eclipse of Iapetus appears to be consistent with claimed activity in the C ring in the years around 1889. It was a routine matter for nineteenth-century observers to describe apparent changes in Saturn's rings. By the middle-twentieth century, however, Alexander's (1962) comprehensive analysis of the historical record explained many unusual claims as unreal effects created by illusion and error. While Alexander made valid points, he did not have the advantage of knowing that spokes in the B ring are real. Further, the Cassini-Huygens mission makes clear that spokes are difficult to explain. Interesting and perhaps significant historical examples of reported change in the rings include the following episodes.

Trouvelot (1877: 191) identified spoke-like features in what he called the B ring but that is now known as the A ring:

> … the inner margin of the [A] ring … limiting the outer border of the principal [Cassini] division, has shown on the ansae some singular dark angular forms … the surface of the [A and outer B] rings … has shown a mottled or cloudy appearance on the ansae during the last four years …

François J.C. Terby (1887: 163) announced the presence of "… masses sombres dans l'anneau obscur …" (big dark blotches in the dusky ring). Not everyone was convinced, but Thomas G.E. Elger (1887: 512) was emphatic about their reality:

> 26th February [1887] … the p[receding] ansa [of the C ring] exhibits on its inner border three or four large re-entering angles like the teeth of a saw, the intervening spaces being apparently as dark as that between the ball and the ring, and extending nearly to the outer edge of the ring. 12th March [1887] … p ansa is very evidently broken up into several areas of different degrees of darkness, so that, except a short section of it, np, it is impossible to recognise it as a ring surrounding the planet. The f[ollowing] ansa … is easily visible.

In April 1890, about six months after the eclipse of Iapetus when the rings were slightly more open, Paul Stroobant (1890) observed dark notches with puzzling shapes on the inner edge of the evening ansa of the C ring. In April 1896 Eugène M. Antoniadi (1896: 339), reported, "… instead of the Encke division, ring A shows (just now) some enormous white spots separated by dusky intervals. This ring appears broken (as it were) into fragments." In June he added that the "… [A] ring showed itself lately composed of successive groups of white spots, separated by dusky intervals, which seemed to shoot forth in the direction of radii emanating from Saturn's centre." (Green, 1897: 240). Others saw similar effects. Charles Roberts (Green, 1897: 244) reported that

> … the serrated appearance of this [A] ring where it borders Cassini's division was seen with great certainty on several nights … [On] June 28, the inner edge of ring A looked … sharp. On July 3 some very conspicuous serrations were seen on the f. ansa … On May 8 the inner edge of the p. ansa [of ring C] appeared serrated somewhat like that of ring A …

Rev. T. H. Foulkes (Green, 1897: 237) left this record:

> Noticed a remarkable appearance of the [C] ring where it crossed the ball, it did not possess its usual uniform appearance, but was decidedly 'lumpy.' I counted six or seven of these darker shadings, which seemed to have a tendency to circular formation … Though … [I have observed Saturn] for the last 25 years, I have never before seen this curious formation.

However, Foulkes' observation may be unrelated to the C ring. In 1993 Richard McKim (pers. comm., 2005) and others saw similar clumping in the C ring. They did not suspect changes in the ring. Instead, they preferred the possibility that dark spots in the North Equatorial Belt, which lay beneath the ring, were visible through it. An observation by the Cassini spacecraft on 28 April 2006 appears to justify that interpretation. Bright clouds in the planet's atmosphere were only partially obscured by the shadow of the C ring (NASA/JPL/SSI, 2006).

Barnard (1895: 369-371), who saw none of this, was not persuaded. He treated the matter with sarcasm:

> The Crape ring has appeared uniformly even in shade at the two ansae. It was of a steelly blue colour, and was not strongly contrasted with the sky. No markings whatever were seen upon it. The inner edge was a uniform curve; the serrated or saw-toothed appearance of its inner edge, which had previously been seen with some small telescopes, was … beyond the reach of the 36-inch.

Considering the vivid drawings of Saturn that others produced, Barnard was unapologetic for his own art that "… appears abnormally devoid of details … I am satisfied, however, to let it remain so." His assessment of results by George Davidson, a San Francisco amateur astronomer, could have applied to his own: "One great thing must commend itself to every observer familiar with Saturn in a telescope is that he has not shown a single abnormal feature." Although he apparently did not know it, his situation was not so simple.

On 7 January 1888, the night of the first successful test of the 36-inch refractor, James E. Keeler drew an image of Saturn that became known as "… the best existing picture of the planet for many years, and [that] was widely admired by professional astronomers of the time …" (Osterbrock and Cruikshank, 1983: 168). As Keeler drew it, the B ring had three faint, dark, diffuse, radial shapes upon it that suggest spokes. The shapes are not obvious and are only noticeable as departures from circularity within the ring. It was Barnard's opinion that Keeler had artistic ability that few other observers possessed (Sheehan, 1995: 149). There is every reason to suppose that he drew the B ring just as he saw it, including the likeness to faint spokes. Sheehan (1988: 133) also thought that his drawing "… gives hints of the 'spokes' on the surface of Ring B …" Keeler eventually presented a copy of his famous drawing to Barnard as a gift (Osterbrock and Cruikshank, 1983). In receiving it, Barnard had evidence that odd features on Saturn's rings were well within reach of the 36-inch refractor. Surviving letters written by Keeler to Barnard, now collected at Vanderbilt University, do not discuss the Saturn drawing. Since Keeler's observing record for January 1888 is lost, it is not possible to know what he thought about the appearance of the B ring.

Historical observers tend to differ with O'Meara over the intensity of transitory dark ring features. Elger is a good example. He insisted upon the obviousness of these objects and drew them vividly (Figure 11). O'Meara (pers. comm., 2005) emphasized their subtle appearance, but drew them just as vividly (Figure 12):

> I've never seen black markings or gauges. And I'm certain no one in history has (not even Elger). You have to consider the artist's style when he or she is trying to portray a dim feature. For instance, even in my drawings, the spokes look very intense, but they are not. They are definite but delicate to the eye, very hard to render in a way that will reproduce, unless you intensify them.

Elger was an experienced lunar and planetary observer, and it is certainly possible that he meant exactly what he wrote. The degree to which something is obvious depends, after all, on the observer. However, Elger had critics who saw no dark features. They suggested that either his equipment or his eyesight was faulty. Elger (1888) replied that only dabblers in Saturn could fail to see what he reported. Disagreement that declined into personal jabs makes it possible, even likely, that some of his emphasis was intended to defeat critics. Then differences over intensity may be an effect of non-observational influences such as the need to create a reproducible illustration or the desire to make a point. Not all the historical cases are so. Although Keeler apparently made no claim for dark features, his drawing is consistent with tenuous spokes in the B ring as O'Meara described. However, visual observations, both historical and modern, disagree with two presently-accepted conditions for spokes—that they occur only in the B ring, and that their visibility is limited to small ring opening or solar illumination angles.

10 A DIFFERENT PERSPECTIVE

Are spokes confined to the central B ring? O'Meara (pers. comm., 2005) described the A ring as being prone to ephemeral shaded patches. He has seen "… Ring C appear patchy at times in larger scopes." At Pic du Midi on 1-2 August 1992, he saw "… two dark radial features on the southeast quadrant of Ring C—not at the ansa … The preceding [evening] ansa looked uniform." These features were azimuthally associated with but were not connected to five B ring spokes that had a saw-toothed and curved appearance:

> They were radial but certainly different than those in Ring B … [being] broader and more linear … I … [recall] their dimness. The spokes in Ring B were much more obvious … because … I was looking at a 'dark' shading against a bright ring … in Ring C, I was observing 'slightly darker' features against a relatively dark ring, which is much harder to do … I had to use averted vision to see them … I believe this might be why Bill [Sheehan, who saw the five B ring spokes] did not see the features in Ring C.

Sheehan (pers. comm., 2005) did not look for spokes in the C ring.

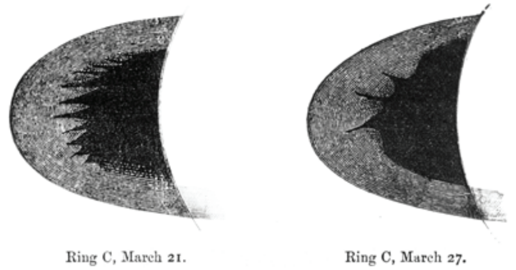

Ring C, March 21. Ring C, March 27.

Figure 11: Engraved drawings of the C ring from March 1888. In 1887 and 1888 Thomas Elger was one of several observers who reported unusual dark markings on the C ring. Their existence was controversial. Elger (1888) described what he saw as follows: "March 21 … Inner edge ragged and clearly indented on p. side, but indentations not very deep … March 27 … Inner edge on p. ansa scalloped, exhibiting three or four convex projections, and dark patches visible on its surface." South is up in these images. If the engravings are faithful to Elger's original drawings, the dark features had an angular extent along the ring that approached 45°. Taking into account rotation rates in the C and D rings, what Barnard observed 19 months later during the eclipse of Iapetus must have had a similar extent to have affected his observation as it did. (Reprinted from "Physical observations of Saturn in 1888" by T.G. Elger, *MNRAS*, 48, 362-370, 1888, figure entitled "Ring C, March 21 [and] Ring C, March 27" with permission from Blackwell Publishing.)

Is the visibility of spokes limited to small ring opening angles? McGhee et al. (2005) detected no spokes beyond an opening angle of -15.43°. In August 1988, when the angle was about 26°, O'Meara (pers. comm., 2005) observed spokes with the Mount Wilson 1.5-m telescope. He described their color as ice blue. In August 1992 he used the 1-m telescope at Pic du Midi. On that occasion, when the angle was about 16°, he described spokes as gray in color and stronger in appearance than they had been in the 1.5-m. Presumably, changed opening angle affected his sense of the color and contrast of spokes. In both cases opening angles were in excess of 15°. One of his earliest observations of spokes was made at an opening angle of about 18° (Robinson, 1980). As to smaller angles, he saw spokes in greatest numbers from October 1976 to March 1978 when the angle closed from about -17° to -12°. He saw far fewer spokes from November 1978 to January 1979 as the angle changed from about -5° to -4°. His count increased again from February to June 1979 when the angle opened from about -5° to -7°. Unlike McGhee et al. (2005), who continued to detect spokes between angles of +4° and +5°, very narrow viewing geometry was a disadvantage for O'Meara.

Why were O'Meara's results different? While the spoke process apparently has a time-scale of years that is related to solar illumination of the rings, individual spokes clearly exist and change on a time-scale of minutes and hours. To observe spoke dynamical changes with HST, Bradford Smith (1984: 709-710) anticipated that orbital limitations would impose non-continuous data sets so that

> … we cannot escape … the same problems encountered with the Voyager images. Statistically, one can partially overcome the problems of a 0.4 observing duty cycle by extending the total observing time. Typical spoke lifetimes are ~5 hr … and thus the accumulation of several tens of hours of observing time by recording the rings for 30-45 minutes per orbit would likely yield many … [spoke events] … the Voyager data have

> taught us that sporadic observations are of relatively little value for dynamical studies.

McGhee et al. (2005: 508) observed the planet with several hundred high resolution images obtained on 34 dates from 1994 to 2004. They identified "… 36 spokes or spoke complexes, predominantly on the morning (east) ansa." From 1976 to 1983 O'Meara observed several times each week during apparitions of the planet. Representative of his pace, O'Meara (pers. comm., 2005) observed 29 spokes in a period of 43 days from 24 January to 8 March 1977. If gaps in coverage on a time-scale of hours prevent correct understanding of spoke dynamics, gaps that range to months must interfere with understanding other properties. That O'Meara reported spokes outside the B ring and at large ring opening angles while planetary scientists through January 2004 observed neither does not necessarily mean that he was wrong. It may be that his rate of observation made a meaningful difference.

Figure 12: Saturn as drawn by Stephen J. O'Meara on 1-2 August 1992 at the 1-m telescope of Pic du Midi Observatory. O'Meara saw spokes in the B ring. He also saw even fainter spoke-like features in the C ring, but in this image he exaggerated their density for clarity. O'Meara is not first to record such activity in the C ring. In the 1880s and 1890s astronomers argued the reality of claimed transitory dark features in that ring. Illustrative of the challenge that spokes present to visual observers, O'Meara put the planetside edge of B ring spokes in contact with the B-C ring boundary. Other observers have done the same. However, spacecrafts show spokes in the central B ring that are not in contact with the B-C ring boundary. Visual observers may not be able to distinguish the darkness of spokes from the darkness of the inner B ring leading to a perception that spokes reach the B-C ring boundary. As shown here, north is up (courtesy: Stephen J. O'Meara).

11 WHY NOT BARNARD?

The ring system appears to have been visibly active in years before and after the eclipse of Iapetus. If that is true, did Barnard see anything unusual on the rings during the eclipse? He described what he saw:

> The superb definition of the planet in the last part of the observations showed no abnormal appearance of the rings where the shadow of the ball crosses them, nor have I at any time seen a white spot on the rings at this or any other point. (Barnard, 1890).

The white spot Barnard referred to was a contrast effect seen seven months earlier by Terby. He said nothing about dark features on the rings. If spokes were there, why did he not see them?

Barnard left no indication that he anticipated anything other than an exhibition of the C ring's normal transmission characteristics, but it would not have been excessive for him to have wondered about possibilities. The advantage of being first to observe the inner ring system in transmission was more than enough reason to anticipate something new. Yet his first report to the Astronomical Society of the Pacific was so narrowly directed to Marth's question about the C ring as to imply that he had only one purpose on his mind, so much so that he may have overlooked subtle spokes. How likely is that? Using a telescope of comparable size, O'Meara (pers. comm., 2005) found that when his attention was directed to ring divisions, he could not see spokes that were present on the rings. The converse was also true.

Barnard observed with a magnification of 150×. O'Meara (pers. comm., 2005) believes that this is too low a power to distinguish spokes on the bright B ring, especially at night as opposed to during twilight. In O'Meara's case, 250× was probably the minimum useful magnification with 275× to 350× being better. Because spokes are delicate and their surroundings are bright, he often "… would observe only one side of the rings … at a time …" by using the edge of the field as an occulting bar. Historically, Elger (1887) worked in the range 284× to 420×, Antoniadi used 220× to 600× with a preference for 300× (Green, 1897), and Stroobant (1890) used 360×.

Another factor concerns what Barnard could see on planets. People respected and were sometimes amazed by his visual observations, but they were also vexed by what they thought he could not see. Barnard's successses and access to the world's largest telescopes encouraged him to assume superior authority in answering observational questions. He was not reluctant to disappoint others over their claimed discoveries. Because those whom he contradicted included experienced planetary observers, there was an inevitable consequence. According to Antoniadi (1909a), "… Barnard n'est pas un observateur de détails planétaires délicats …" [Barnard is not an observer of delicate planetary details]. Others agreed. Given his record, did he really not see what others saw?

Antoniadi criticized on the occasion of Barnard's inability to see a fourth ring reported by Georges Fournier to be just outside Saturn's A ring. Antoniadi (1909a: 450) sardonically observed that since the new ring

> … a été absolument invisible à l'illustre découvreur … on conviendra qu'il ne saurait plus être question de

> l'existence d'un anneau extérieur crépusculaire de Saturne. [… is absolutely invisible to the great discoverer … we will agree there can be no question of whether or not Saturn's exterior crepe ring exists].

With similar bite, he alluded to another disagreement in the 1890s when Barnard could not see spots in Saturn's atmosphere that were reported by Arthur Stanley Williams, "… one of the most outstanding non-professional astronomers of modern times …" (Obituary, 1939: 313-314). That disagreement was as much sociological as it was observational. Professionals were replacing amateurs as leaders in astronomy. Barnard, who used a great telescope, and Stanley Williams, who used a small one, produced "… scientific knowledge … [that] rested on strikingly different perceptions of the natural world." (Lankford, 1981: 27). A conclusion for the reality or unreality of the spots depends on which facts are emphasized. However, one result was certain. Some European astronomers were sure that they had found the limit of Barnard's ability.

Barnard did not see obvious geometrical patterns of canals on Mars as did Percival Lowell, but he was not alone. In contradiction of the consensus that there are no Martian canals, Dobbins and Sheehan (2004: 117) found that

> … many of the canals appear to be artifacts of edge enhancement of the boundaries of adjoining regions of different albedo that correspond physically to adjoining surfaces strewn with bright or dusky surface materials.

That canals exist as indistinct features is an old idea. Giovanni V. Schiaparelli depicted them with sharp lines, but Nathaniel Green (1880: 332), observed them to be "… boundaries of faint tones of shade, so delicate that they escape the notice of any but a well-trained eye …" Green (1890) complained that those who drew canals as distinct lines did not represent them as they actually saw them. The canal debate was in full swing in 1909 when Antoniadi mocked Barnard over a fourth Saturnian ring. Similar to Green and Dobbins and Sheehan, Antoniadi (1909b) saw canals as "… the optical products of very complex and irregular natural duskiness sporadically scattered all over the Martian surface." He opposed Lowell's unnatural geometrical canal network. Now Antoniadi (1909b) wrote to Barnard, "… with the highest admiration for your genius …" that he was honored "… to find that we are in perfect agreement regarding the appearance of the so-called 'canals' of Mars." Further, "… you called my attention to the fact that the streaks of Mars appeared to you larger in great telescopes than they were drawn with small instruments." (Antoniadi, 1910). As for Lowell, who rebutted all who did not see canals as he saw them, Barnard's observational talent was better suited to faint stars and star-like objects than it was to planetary surface markings (Sheehan, 1988). It seems clear that others' opinions of Barnard's visual skill at the telescope were influenced as much by self-interest as by what there was to see.

Assuming that spokes were there to be seen during the eclipse of Iapetus, insufficient magnification and Saturn's brightness are likely reasons why Barnard did not see them. He may also have been too preoccupied to notice them. It is true that he never saw transient dark markings on Saturn's rings. However, rather than conclude that these were beyond him, it seems more likely that he suffered from bad timing with ephemeral objects. While it would be helpful or even final if Barnard had seen wispy, dark features on the morning side of Saturn's rings during the eclipse, that he saw none is unrelated to his ability to estimate Iapetus' magnitude in eclipse. His record demonstrates that faint stars and star-like objects were less of a problem for him than they were for others (Burnham, 1889; Sheehan, 1988).

12 EVALUATION AND CONCLUSION

I have relied on visual observations to speculate on the meaning of Barnard's observation of the eclipse of Iapetus, but visual observations are problematic evidence. They are subjective. Without independent confirmation, their scientific significance is arguable. Most importantly, the visual method has lost credibility among professional astronomers so that it is difficult to make any point that is visually supported:

> Although groundbased observers had reported seeing streaks in the A ring as early as 1873 (Alexander, 1962) and had even computed a rotational period for features seen in the B ring in the 1970s (Robinson, 1980), the B ring's vast panorama of spokes seen in the Voyager images was unexpected. (McGhee et al., 2005: 509).

It was unexpected because historical and modern visual observations, that constitute knowledge of spokes prior to Voyager 1, were too different from the experience of planetary scientists to be taken seriously.

I have considered the possibility that Barnard saw ring shadows mixed with spoke shadows on Iapetus because he observed at a time when the C ring was apparently affected by spoke-like activity. Observers have seen transient objects in Saturn's rings for 133 years. Nevertheless, planetary scientists largely ignore the visual record. Historically, when scientists could not collect information for themselves, they were obliged to receive it from other people and to evaluate its credibility. If the visual history of spokes recalls the old problem of knowing what to do with observational evidence contributed by others, perhaps an old answer still applies. Steven Shapin (1994: 212) identified criteria that were once used to test the credibility of contributed information. The criteria are common sense that remains familiar. A contribution may be credible if it:

1. Is plausible.
2. Comes from multiple sources.
3. Is without internal or external contradiction.
4. Is first-hand to the contributor.
5. Comes from knowledgeable, skilled, disinterested, and honest persons.

Consider O'Meara's situation before Voyager 1. A young person with an old telescope saw in the A and B rings transitory, faint, diffuse, dark, radially-oriented objects with non-Keplerian orbital motion. These had a period similar to the planet's rotation rate. The objects preferred the morning ansa of the rings, but also appeared on the evening ansa. He saw them in greatest numbers at intermediate and small ring opening angles and watched their numbers apparently decline as the angle became very small. Although his result was firsthand, it was not plausible, came from him alone, and contradicted fundamental knowledge of the ring system. Further, astronomers whom O'Meara consulted either did not know him or did not fully trust him. Although it was substantially correct, his result was too strange and too unsatisfactory to be believed before Voyager 1. That outcome was either an understandable mistake or appropriate conservatism. Either way, a fundamentally correct description of a previously-unknown phenomenon of the ring system was disbelieved.

The prospects for Barnard after the eclipse of Iapetus were entirely different. Unlike O'Meara, he was well known as a planetary scientist. In 1889 his form of photometry was generally accepted, his instrumentation was suitable, and his application was novel. Nobody published doubt over the correctness of his result and conclusion even though he alone saw the eclipse. Barnard's three published accounts show that he was in a dilemma over interpretation of what he saw. Either the anomalous decline in Iapetus' magnitude before the C ring eclipse was real or it was not. If he called it real, what explained it?

An interpretation that remained attractive until Pioneer 11 visited Saturn in 1979 was that he had observed the shadow of an unseen ring interior to the C ring. If he had made that claim, it would have been plausible because the relatively recent discovery of the C ring made the existence of another tenuous interior ring believable. It would have contradicted no established fact about the ring system. His evidence was first-hand. He had a first-rate international reputation as a skillful and conservative observer and discoverer. The only obvious fault was lack of independent confirmation. Presumably, with four out of five favorable indications, a majority of his colleagues would have been justified to believe him. However, Barnard, who preferred to avoid critics, offered no opportunity for others to evaluate what he had seen. He never suggested the existence of an unseen ring. He certainly did not associate his observation with controversial dark spots on the C ring for he publicly rejected their reality. He refused to believe that the first 30 of his 75 magnitude estimates of Iapetus revealed anything unknown about the ring system. Did he sacrifice a meaningful observation because it was unexpected, extraordinary, and not confirmable? Common sense and his own considerable experience must have guided his decision, but as was true at other times, his fear of ridicule may also have been at work. The conservative answer was safe, but did it downplay a real phenomenon as happened in O'Meara's case.

The conjecture that Barnard saw spoke shadows on Iapetus contradicts modern scientific understanding of where spokes occur. It is otherwise consistent with the normal optical depth of spokes, their increased contrast at small ring opening angles, and their ability to cover long radial distances and broad areas. It appears to be consistent with observations of transitory dark markings on the C ring in the late 1880s as well as with a similar case in 1992. See Figure 13 for an observation made by Stroobant a few months after the eclipse of Iapetus. However, even if this interpretation is wrong, I urge that two aspects of Barnard's observation are significant. He saw Iapetus begin to fade before it reached the C-D ring boundary. He also saw Iapetus become fainter than Enceladus too soon. Apparently, a condition existed in the inner ring system on 1-2 November 1889 that does not, for whatever reason, exist now.

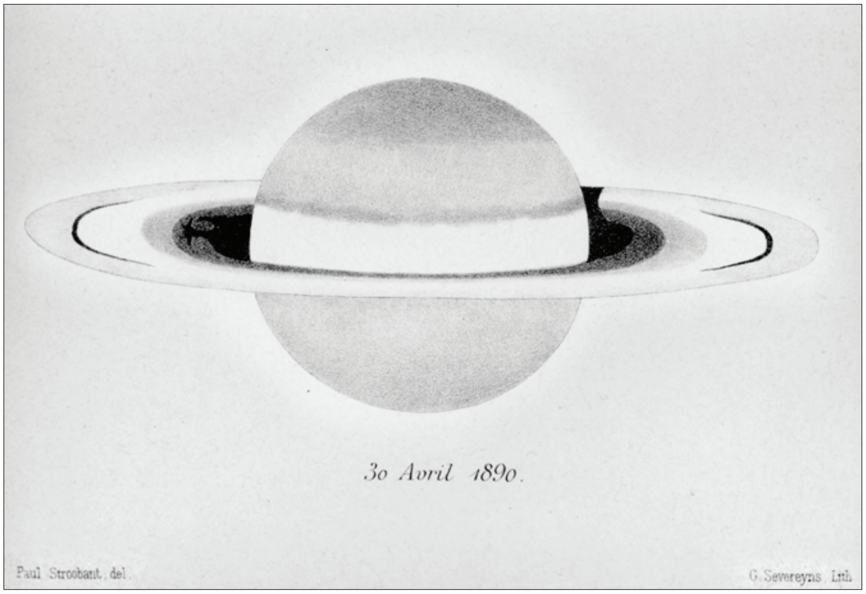


Figure 13: On 30 April 1890 Paul H. Stroobant (1890) drew Saturn and its ring system. The opening angle on the rings was about -11°, only slightly more open than what Barnard saw about six months earlier. Stroobant described the western ansa of the C ring as displaying two dark notches. The notch at the middle of the ansa had " … une forme dont il était difficile de saisir les contours exacts" [… a shape that made it difficult to grasp the exact contours]. What is the significance of dark markings in the C ring that were seen by Terby, Elger, Stroobant and others? If they were real, were they spokes? If spokes were present in the inner ring system during the eclipse of Iapetus in 1889, would they have affected Iapetus' brightness in eclipse as Barnard recorded it in his light curve? The irregular shape that Stroobant drew for the planet's shadow on the rings is incidentally relevant. The shadow is naturally curved, but observers sometimes report non-curved shapes. In the nineteenth century some thought these anom-alous shapes were produced by topography on the rings, but for most of the twentieth century non-curvature was dismissed as an illusion. Modern critics have suggested that awareness of non-curvature may indicate an observer's susceptibility to illusion. However, Mark Bailey, David Stewart and Mark Stronge (2005) now explain non-curvature of Saturn's shadow as an optical phenomenon like the black drop in transits of Venus (after Stroobant, 1890: insert between Pp. 774 and 775, figure entitled "30 Avril 1890" with permission from the Council of l'Academie Royale des Sciences, Lettres et Beaux-Arts de Belgique).

13 ACKNOWLEDGEMENTS

I thank the following people for their help. Stephen O'Meara described in detail his visual observations of spokes. Amanda Bosh, Joshua Colwell, Jeffrey Cuzzi, Richard French, Mihaly Horanyi, Mark Showalter, Laurence Trafton and Anne Verbiscer answered scientific questions. John Isles advised on visual estimation of stellar magnitudes. William Gray computed various circumstances for the eclipse of Iapetus. Francis Everitt answered questions about James Clerk Maxwell. For copies of documents and other information by and about Barnard, Clerk Maxwell, Keeler, Marth and Maunder, I thank Dorothy Schaumberg and Cheryl Dandridge at the Mary Lea Shane Archives for Lick Observatory; Teresa Gray at Special Collections and University Archives for Vanderbilt University; Peter Hingley and Mary Chibnall at the library of the Royal Astronomical Society; [Dr.] Stephen W. Taylor, B.A., Ph.D., a historical researcher and genealogist in the United Kingdom; Jane Wess at the National Museum of Science and Industry; and Joanna Ball at Trinity College Library. Richard Dreiser at Yerkes Observatory provided a copy of Keeler's January 1888 drawing of Saturn. Howard Adkins did artwork for the figures. Finally, I thank Harold Corwin, Jeffrey Cuzzi, Jay Holberg, Richard McKim, Stephen O'Meara, William Sheehan, Anne Verbiscer and Tom Williams for comments on a draft of this paper.

14 REFERENCES

Alexander, A.F.O'D., 1962. *The Planet Saturn: A History of Observation, Theory, and Discovery*. New York, Faber & Faber.

Antoniadi, E.M., 1896. Appearance of Saturn's rings. *Journal of the British Astronomical Association*, 6, 339.

Antoniadi, E.M., 1909a. Corpuscules en dehors du plan de l'anneau de Saturne. *Bulletin de la Société Astronomique de France*, 23, 448-450.

Antoniadi, E.M., 1909b. Letter to E.E. Barnard, dated 11 December. Vanderbilt University Special Collections, E.E. Barnard papers.

Antoniadi, E.M., 1910. Letter to E.E. Barnard dated 14 December. Vanderbilt University Special Collections, E.E. Barnard Papers.

Argelander, F., 1844. Aufforderung an Freunde der Astronomie. In Schumacher, H.C. (ed.). Jahrbuch für 1844. Stuttgart und Tübingen, J.G. Cotta'sche Buchhandlung. Pp. 183-254.

Bailey, M.E., Stewart, D., and Stronge, M., 2005. Extending the black drop to Saturn. *Astronomy & Geophysics*, 46, 1.7.

Barbara, J.M., and Esposito, L.W., 2002. Moonlet collisions and the effects of tidally modified accretion in Saturn's F ring. *Icarus*, 160, 161-171.

Barnard, E.E., 1889a. Logbook 24, Lick Observatory 12-inch equatorial, Oct. 2-Nov. 1, E.E.B. Observer. Lick Observatory, Mt. Hamilton.

Barnard, E.E., 1889b. Eclipse of Japetus, the VIII satellite of Saturn, on November 1, 1889. *Publications of the Astronomical Society of the Pacific*, 1, 126-127.

Barnard, E.E., 1890. Observations of the eclipse of Iapetus in the shadows of the globe, crape ring, and bright ring of Saturn, 1889 November 1. *Monthly Notices of the Royal Astronomical Society*, 50, 107-110.

Barnard, E.E., 1892. Transparency of the crape ring of Saturn, and other peculiarities as shown by the observations of the eclipse of Japetus on November 1st, 1889. *Astronomy and Astro-Physics*, 11, 119-123.

Barnard, E.E., 1895. Micrometrical measures of the ball and ring system of the planet Saturn, and measures of the diameter of his satellite Titan. Made with the 36-inch equatorial of the Lick Observatory. *Monthly Notices of the Royal Astronomical Society*, 55, 367-382.

Blair, G.N., and Owen, F.N., 1974. The UBV orbital phase curves of Rhea, Dione, and Tethys. *Icarus*, 22, 224-229.

Bosh, A.S., and Olkin, C.B., 1996. Low optical depth features in Saturn's rings: the occultation of GSC5249-01240 by Saturn and its rings. *Bulletin of the American Astronomical Society*, 28, 1124.

Brush, S.G., Everitt, C.W.F., and Garber, E., 1983. *Maxwell on Saturn's Rings*. Cambridge, MIT Press.

Burnham, S.W., 1889. The trapezium of Orion. *Monthly Notices of the Royal Astronomical Society*, 49, 352-358.

Chambers, G.F., 1889. *A Handbook of Descriptive and Practical Astronomy. Volume 1. Fourth Edition*. Oxford, Clarendon.

Chandler, S.C., Jr., 1885. The change in the great nebula in Andromeda. *Science*, 6, 247-248.

Clerk Maxwell, J., 1857. Letter to William Thomson, dated 1 August. Glasgow University Library, Kelvin Papers M7.

Cook, A.F. II, and Franklin, F.A., 1958. Optical properties of Saturn's rings: I. Transmission. *Smithsonian Contributions to Astrophysics*, 2, 377-383.

Cox, A.N. (ed.), 2000. *Allen's Astrophysical Quantities. Fourth Edition*. New York, Springer-Verlag.

Cuzzi, J.N. et al. 1984. Saturn's rings: properties and processes. In Greenberg, R., and Brahic, A. (eds.). *Planetary Rings*. Tucson, University of Arizona Press. Pp. 73-199.

de Pater, I., and Lissauer, J.J., 2001. *Planetary Sciences*. Cambridge, Cambridge University Press.

Dobbins, T.A., and Sheehan, W., 2004. The canals of Mars revisited. *Sky & Telescope*, 107, 3, 114-117.

Elger, T.G.E., 1887. Physical observations of Saturn in 1887. *Monthly Notices of the Royal Astronomical Society*, 47, 511-515.

Elger, T.G.E., 1888. Physical observations of Saturn in 1888. *Monthly Notices of the Royal Astronomical Society*, 48, 362-370.

Esposito, L.W., et al., 1984. Saturn's rings: structure, dynamics, and particle properties. In Gehrels, T., and Matthews, M.S. (eds.). *Saturn*. Tuscon, University of Arizona Press. Pp. 463-545.

Ferrín, I.R., 1974. Saturn's rings. I. Optical thickness of rings A, B, D and structure of ring B. *Icarus*, 22, 159-174.

French, R.G., et al., 1993. Geometry of the Saturn system from the 3 July 1989 occultation of 28 Sgr and Voyager observations. *Icarus*, 103, 163-214.

Gehrels, T., et al., 1980. Imaging photopolarimeter on Pioneer Saturn. *Science*, 207, 434-439.

Goertz, C.K., and Morfill, G., 1983. A model for the formation of spokes in Saturn's ring. *Icarus*, 53, 219-229.

Green, N.E., 1880. On some changes in the markings of Mars, since the opposition of 1877. *Monthly Notices of the Royal Astronomical Society*, 40, 331-332.

Green, N.E., 1890. Report of the meeting of the British Astronomical Association, Dec. 1890. *Journal of the British Astronomical Association*, 1, 111-114.

Green, N.E., 1897. Reports of the directors of the observing sections, Saturn Section, 1896. *Journal of the British Astronomical Association*, 7, 236-244.

Grün, E., Morfill, G.E., Terrile, R.J., Johnson, T.V., and Schwehm, G., 1983. The evolution of spokes in Saturn's B ring. *Icarus*, 54, 227-252.

Guérin, P., 1970. The new ring of Saturn. *Sky & Telescope*, 40, 88.

Isles, J.E., and Lewis, J. 1990. *Variable Stars*. Enslow, Berkeley Heights (Volume 8 in the Webb Society Deep-Sky Observer's Handbook series).

Lankford, J., 1981. Amateurs versus professionals: The controversy over telescope size in late Victorian science. *Isis*, 72, 11-28.

Marth, A., 1888. Letter to E.S. Holden dated 19 October. Mary Lea Shane Archives of the Lick Observatory, University of California, Santa Cruz.

Marth, A., 1889a. Letter to W.H. Wesley, dated 14 February. Royal Astronomical Society, RAS Letters 1889.

Marth, A., 1889b. On the eclipse of Iapetus by Saturn and its ring-system, on November 1-2, 1889. *Monthly Notices of the Royal Astronomical Society*, 49, 427-429.

Marth, A., 1889c. Letter to W. H. Wesley, dated 12 September. Royal Astronomical Society, RAS Letters 1889.

Marth, A., 1889d. Letter to RAS Library Committee, dated 6 November. Royal Astronomical Society, RAS Letters 1889.

Marth, A., 1890. Letter to E.S. Holden, dated 6 February. Mary Lea Shane Archives of the Lick Observatory, University of California, Santa Cruz.

Maunder, E.W., 1894. Letter to E.E. Barnard dated 2 January. Vanderbilt University Special Collections, E.E. Barnard papers.

McGhee, C.A., French, R.G., Dones, L., Cuzzi, J.N., Salo, H.J., and Danos, R., 2005. HST observations of spokes in Saturn's B ring. *Icarus*, 173, 508-521.

Millis, R.L., 1973. UBV photometry of Iapetus. *Icarus*, 18, 247-252.

Millis, R.L., 1977. UBV photometry of Iapetus: results from five apparitions. *Icarus*, 31, 81-88.

Mitchell, C.J., and Horanyi, M., 2005. Where did the spokes go? American Geophysical Union Spring Meeting 2005, abstract P21C-07.

Mitchell, C.J., Horanyi, M., Havnes, O., and Porco, C.C., 2006. Saturn's spokes: lost and found. *Science*, 311, 1587-1589.

NASA/JPL/Space Science Institute, 2006a. Double-Banded E Ring (PIA 07803). http://ciclops.org/view.php?id=2000 (accessed on 5 July 2006).

NASA/JPL/Space Science Institute, 2006b. Shadow Strands (PIA 08191). http://ciclops.org/view.php?id=1995 (accessed on 2 June 2006).

NASA/JPL/Space Science Institute, 2005a. D Ring Revelations (PIA 07714, 07715). http://ciclops.lpl.arizona.edu/view.php?id=955 (accessed on 18 May 2005).

NASA/JPL/Space Science Institute, 2005b. Fantasy Made Real (PIA 07545). http://ciclops.org/view.php?id=1172 (accessed on 26 August 2005).

Obituary: Albert Marth. *Monthly Notices of the Royal Astronomical Society*, 58, 139-142 (1898).

Obituary: Otto Wilhelm von Struve. *Observatory*, 28, 251 (1905).

Obituary: Arthur Stanley Williams. *Monthly Notices of the Royal Astronomical Society*, 99, 313 (1939).

Osterbrock, D.E., and Cruikshank, D.P., 1983. J.E. Keeler's discovery of a gap in the outer part of the A ring. *Icarus*, 53, 165-173.

Pickering, E.C., 1879. *Annals of Harvard College Observatory, 11, part 2, Photometric Observations*. Cambridge (Mass.), Harvard University Press.

Pickering, E.C., 1882. *A Plan for Securing Observations of the Variable Stars*. Cambridge (Mass.), Harvard Univ. Press.

Pickering, E.C., et al., 1881. Report of the committee on standards of stellar magnitude. *Observatory*, 4, 327-329.

Porco, C.C., 1988. Dual periodicity in the appearance of spokes. *Bulletin of the American Astronomical Society*, 20, 852.

Porco, C.C., and Danielson, G.E., 1982. The periodic variation of spokes in Saturn's rings. *Astronomical Journal*, 87, 826-833.

Porco, C.C., et al., 2005. Cassini imaging science: initial results on Saturn's rings and small satellites. *Science*, 307, 1226-1236.

Robinson, L.J., 1980. Closing in on Saturn. *Sky & Telescope*, 60, 481.

Sandel, B. R., et al., 1982. Extreme ultraviolet observations from the Voyager 2 encounter with Saturn. *Science*, 215, 548-553.

Shapin, S., 1994. *A Social History of Truth*. Chicago, University of Chicago Press.

Sheehan, W., 1988. *Planets & Perception: Telescopic Views and Interpretations, 1609-1909*. Tucson, University of Arizona Press.

Sheehan, W., 1995. *The Immortal Fire Within: The Life and Work of Edward Emerson Barnard*. Cambridge, Cambridge University Press.

Showalter, M.R., 1996. Saturn's D ring in the Voyager images. *Icarus*, 124, 677-689.

Showalter, M.R., 1998. Detection of centimeter-sized meteoroid impact events in Saturn's F ring. *Science*, 282, 1099-1102.

Smith, B.A. 1984. Future observations of planetary rings from groundbased observatories and earth-orbiting satellites. In Greenberg, R., and Brahic, A. (eds.). *Planetary Rings*. Tucson, University of Arizona Press. Pp. 709-710.

Smith, B.A., et al., 1982. A new look at the Saturn system: the Voyager 2 images. *Science*, 215, 504-537.

Spahn, F., et al., 2006. Cassini dust measurements at Enceladus and implications for the origin of the E ring. *Science*, 311, 1416-1418.

Stroobant, P., 1890. Observations de Saturne faites en 1890, à l'observatoire royal de Bruxelles. *Bulletin de L'Académie Royale des Sciences, des Lettres et des Beaux-Arts de Belgique*, 3rd series, 19, 6, 772-775.

Struve, O.W., 1853. Les dimensions des anneaux de Saturne. *Memoires de l'academie Imperiale des Sciences de St. Petersbourg*. 6 ser.: Sciences mathematiques, physiques et naturelles. 1re ptie: Sciences mathematiques et physiques, T.5. (Memoires de l'academie imperiale des sciences de St. Petersbourg. 6 ser.: Sciences mathematiques, physiques et naturelles; T.7), 439-475.

Terby, F.J.C., 1887. Saturne—compagnon de Sirius. *Observatory*, 10, 163-164.

Trouvelot, E.L., 1877. Physical observations of Saturn. *Monthly Notices of the Royal Astronomical Society*, 37, 191.

U.S. Naval Observatory, Nautical Almanac Office, 1886. *American Ephemeris and Nautical Almanac for the year 1889, Second Edition*. Washington, Bureau of Navigation.

Webb, T.W., 1896. *Celestial Objects for Common Telescopes, Volume 1. Sixth Edition (by T.H.E.C. Espin)*. London, Longmans Green.

James Bryan is a Research Fellow at McDonald Observatory, a branch of the University of Texas at Austin. His research interests include history of astronomy in early Texas, historical observations of the supernova S Andromedae in 1885, and historical visual observations of Saturn's ring system. He is a member of the American Astronomical Society and is a Fellow of the Royal Astronomical Society.

Journal of Astronomical History and Heritage, 10(1), 49-64 (2007).

WILLIAM DOBERCK – DOUBLE STAR ASTRONOMER

P. Kevin MacKeown
Department of Physics, The University of Hong Kong,
Hong Kong.
E-mail: hrsppkm@hkucc.hku.hk

Abstract: We outline the role of astronomy in the career of William Doberck (1852–1941). After taking a PhD in astronomy at the University of Jena in 1873, he accepted a position as superintendent of Markree Observatory in the west of Ireland. There he refurbished the great 13-inch refractor and spent nine years observing mostly double star systems, paying only such attention to meteorological monitoring as was required of his position. In 1883 he became the founding Director of a new observatory in Hong Kong, a post which he held for 24 years. His frustrations in attempting to continue his purely astronomical work, not assuaged by his combative and prickly personality, and in the face of the strictly practical demands of that mercantile society for comprehensive storm forecasting, are described. Finally, his observations in retirement in England, and his overall contribution to astronomy, are summarised.

Key words: William Doberck, Markree Observatory, Hong Kong Observatory, colonial astronomy, double stars.

1 INTRODUCTION

William Doberck (1852–1941) had a long career in astronomy. He was already a sufficiently accomplished astronomer to have earned a doctoral degree in the subject by the age of 21, and was still making observations well into his 70s, resulting in more than 200 published reports on his astronomical researches. But, as we will see, astronomy was sometimes to be but a private diversion for a quarter of a century in an eventful, if stormy, career in meteorology as Director of the Hong Kong Observatory. A well-regarded astronomer when he died, whose work is still sometimes cited, very little has been written about him, and most of his personal life seems totally lost to posterity. His years at the Hong Kong Observatory receive some coverage in the three published histories of that institution (see Dyson, 1983; Ho, 2003; Starbuck, 1951), while his career and achievements outside astronomy have recently been described by MacKeown (2004).

We describe the four epochs in Doberck's career, his youth and education, his spell as Director of the Markree Observatory in the West of Ireland, his stay in Hong Kong, and finally, his own 'gentleman's observatory' in England, and give a brief overview of his opus. Of these epochs, his period in Hong Kong is by far the best documented. Incidental to an account of Doberck's career, this paper will also try to throw some light on the early history of astronomy in Hong Kong; a comprehensive account of astronomy there from the 1940s onwards has been compiled by Alan Chu (2003).

2 EARLY YEARS

We know that August William Doberck was born in Copenhagen on 12 September 1852, one of four children of Frederik Wilhelm Doberck and Marthe Stine Johansen; another son, Carl Alfred, was to have a distinguished career in the Danish Naval Service, while one of his sisters, Anna, will feature briefly in this story. The children were introduced to intellectual life from an early age for their father was an internationally-recognised arts smith master and art collector, and an accomplished artist, as is witnessed by the portrait of the young William shown in Figure 1 that he painted around 1870. The household of the young man was one of culture and learning, and the writer Hans Christian Andersen, the philosopher Søren Kierkegaard and many art professors were regular visitors. In family folklore the young William is reputed to have shown an early and keen interest in learning and matters scientific.[1]

Figure 1: A portrait of the young William Doberck painted by his father (by kind permission of Michael Doberck).

When he was a student at the University of Copenhagen the Professor of Astronomy there was Heinrich L d'Arrest, a distinguished scientist who had played an important part in the discovery of Neptune. Two other fellow-citizens and near contemporaries of Doberck who attained some astronomical fame thanks to the influence of d'Arrest were Carl Frederik Pechüle and, more especially, John Louis Emil Dreyer. We have little information on Doberck's early life, but can assume that it had a lot in common with his fellow-student, Dreyer, who recounts his enthusiastic visits to d'Arrest's observatory (Obituary, 1927), even before he entered the University. He speaks especially of the encouragement he received from the junior astronomer, Professor Schjellerup. D'Arrest's main interest was in comets, of which he had discovered three, and it would seem that he passed on this interest to the young Doberck, who proceeded to work on comets, both in Copenhagen and at the Pulkovo Observatory in St Petersburg. In 1873, at the age of 21, Doberck was awarded a doctorate by the University of Jena. At the time, this University was a major centre of optical and astronomical research. Although the University of Copenhagen also offered doctorate degrees in astronomy,[2] Doberck may have been directed by d'Arrest (who was a German) to register at the University of Jena where a doctorate could be obtained *in absentia* and at moderate cost, with perhaps better prospects of finding a position at an observatory elsewhere in Europe. His main activity at this time seems to have been theoretical rather than observational, and his thesis, *Bahnbestimmung der Cometen I 1801, III 1840 und II 1869*, was published in Copenhagen in the same year (Doberck, 1873d). In addition to these three comets, by the end of 1874 he had also published orbital elements for three other comets.

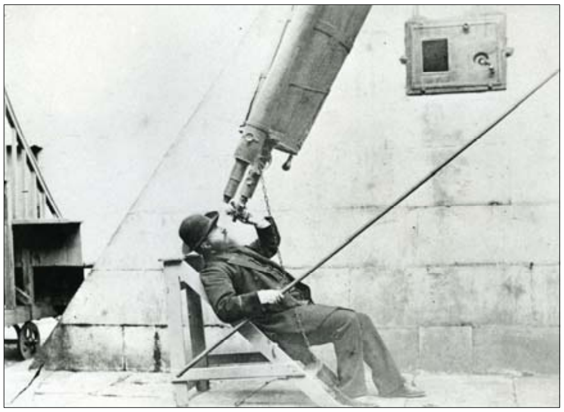


Figure 2: Dr Doberck with the 13-inch refractor at the Markree Observatory in the late 1870s (by kind permission of the Librarian, Royal Astronomical Society).

3 MARKREE OBSERVATORY

3.1 Doberck's Appointment

Opportunities for professional astronomers were even less common then than they are today, so upon graduating Doberck had a formidable search to make if he wished to pursue such a career. Even so, his next step may seem a little surprising. He went to take charge of Markree Observatory in the west of Ireland (Ireland at that time was still part of the United Kingdom). Twenty years earlier, however, in connection with staffing the new Melbourne Observatory, Thomas Romney Robinson (Director of the Armagh Observatory, and a very influential figure in astronomical circles) wrote: "If Britain cannot furnish a qualified person, let us carry out free trade and seek him at Berlin or Poulkova." (Robinson, 1852). This was the approach which presumably led to Doberck taking up the position at Markree—and note that both of his successors in the position were Germans. It is interesting to contrast Doberck's career with that of his near contemporary, J.L.E. Dreyer: both of them born in Copenhagen in the same year (1852), both moved to Ireland in 1874, and both transferred to 'tenured' positions in 1882-1883, Dreyer to the prestigious position as Director at Armagh and Doberck to the 'astronomical backwater' of Hong Kong. It is hard to imagine that there was not some rivalry in these parallel careers, but the only evidence we have of their relationship is a photograph of Doberck posing with the great refractor at Markree (Figure 2). This photograph, which was probably taken around 1880, was found in one of Dreyer's photographic albums.

3.2 Doberck's Stay at Markree

Doberck's move to Sligo was certainly a step down from the professionalism he experienced at Pulkovo Observatory, when he spent time there during his doctoral research. Markree Observatory was a 'gentleman's observatory' of a type not uncommon in those days—Ireland, in particular, had a number of them (McKenna, 1967). They were nurtured by enthusiastic, and sufficiently wealthy, amateurs—in this case by a certain Edward J. Cooper, who had studied at Eton and Christ Church (Oxford) but had never taken a degree. Remote it may have been, but it was known to astronomers, professionals and amateurs alike, mainly for its 13.3-inch refracting telescope.[3] To quote Doberck (1884a): "… in a remote corner of Ireland the largest telescope ever made had been erected by a gentleman [Edward Cooper] then unknown to astronomical fame." (cf. Doberck, 1884b; Hoskin, 1982). In fact, the most important work done with this telescope was completed before Doberck's arrival, and involved the discovery of the asteroid 9 Metis (in 1848) and the publication of the 'Markree Catalogue' of over 60,000 stars (in 1851-1856). Following Cooper's death in 1863, "… all the astronomical instruments [including, presumably, the large refracting telescope], transit circle, clocks etc. and library …" (*The Astronomical Register*, 1863) were advertised for sale as one job lot, but there was presumably no response to the asking price of £2,500 for dispersal fortunately did not occur. The Observatory then remained inactive for nine years and no annual reports were issued, then a nephew of the founder, Colonel Edward H. Cooper, succeeded to Markree Castle in 1872 and reactivated the Observatory. However, his interests were mainly meteorological, and in his obituary Markree Observatory is described as "… one of the best meteorological stations in Ireland." (Obituary, 1903).

Doberck took up his position at Markree Observatory on 1 May 1874, and as part of his integration into a new academic *milieu* he was elected a Member of the Royal Irish Academy in January 1876. Although he did read a few papers at the Royal Astronomical Society in London in 1874 and 1875, he does not seem to have been a Fellow of that mainstream organisation of which almost all major astronomers in the Anglo-centred world became members. He did, however, forward the Society brief annual reports of the Markree Observatory for 1875 through 1882, and these were published in their *Monthly Notices of the Royal Astronomical Society* (see Doberck, 1876k; 1877k; 1878n; 1879g; 1880e; 1881d; 1882 and 1883a). These reports, together with an essay on the Observatory that he wrote after he moved to Hong Kong (Doberck, 1884a, 1884b), form the basis of almost all we know about his activities at Markree. This essay seems to be the only outcome of a project he mentioned in his report for 1881 where he states that "… a sketch of the History of Astronomy in Ireland is in progress." (Doberck, 1882).

Notwithstanding its former fame, Markree Observatory had been neglected during the nine years since its founder's death, and in his first report to the Royal Astronomical Society Doberck (1876k) writes that he "… found the instruments decaying from neglect ... and the building in such a dilapidated condition that the rain penetrated through the roof. The Refractor, exposed during forty years in the open air to the winds and rains of Connaught, was of course in the worst condition." It was not until December 1875 that he could use the large telescope to make observations, but even then he feared that the harmful effects on it of having been exposed to the open air for so many years could never be remedied. In observing double stars, as distinct from comets on which his earlier work was based, he was, to some extent, following fashion (Wright, 1993). The study of such systems had been a glamour subject in the first quarter of the century, but had fallen by the wayside only to be revived by the publication of a new catalogue by the Chicago amateur S.W. Burnham in 1873 (see Aitken, 1918). Apart from double stars, a topic he was to make very much his own throughout his later career, Doberck would have liked to work on variable stars but, as he lamented in his 1877 Annual Report, "… the sky in this part of Ireland is too seldom and too irregularly clear to allow of the observation of variable stars with any chance of success." (Doberck, 1878n). Nevertheless, by the time he left Markree he had published almost one hundred reports on his researches, mainly in *Astronomische Nachrichten* and in *Monthly Notices of the Royal Astronomical Society*. These included some observations of comets, but most reported observations of double stars and the calculation of their orbital elements. In the obituary notice for Doberck that he prepared, Aitken (1941) wrote that "… he was favourably known for the great number of double-star orbits he computed."

In his report for the year 1875 Doberck (1876k) relates how he was assisted by his sister Anna, although this seems only to have lasted for a year or so. A contemporary local historian at the time wrote of his 'accomplished' sister, and described them as "… a brother and sister that remind one forcibly, by their common love of science and their mutual affection, of William and Caroline Herschel …" and that they "… are not only maintaining, but extending daily the fame and usefulness of Mr Cooper's great foundation." (O'Rorke, 1878). Miss Doberck, he claimed, was at the time "… elaborating, on the continent, an 'Essay on the Climate of Ireland.'" (ibid.). It seems unlikely that she published any such account, for her brother would surely have mentioned it when he wrote, fifteen years later, to support her application for the position of meteorological assistant at the Hong Kong Observatory.

3.3 Departure from Markree

When a vacancy arose in 1882 for a founding Director of a new observatory in Hong Kong Doberck somehow brought himself into consideration for the job. It is not clear how this arose, but it was probably through informal communications between the Astronomer Royal, William H.M. Christie, and the Director of the Dunsink Observatory, Robert S. Ball (who is likely to have been in regular contact with Markree). Doberck ended up second on a short list of five, and after the other favoured candidate[4] withdrew he was appointed to head the new institution.

Why did he leave Markree? Obviously there was no financial inducement because less than two years into his new position—if we are to believe a letter he wrote to the Colonial Secretary—he was complaining: "I should have been materially better off had I not resigned my position in Ireland." (Doberck, 1885b). He gave no indication of discontent in his last report from Markree to the Royal Astronomical Society in 1882, where he wrote at length about the many projects he had on the drawing board. He mentioned plans for a more comprehensive monitoring of the meteorological data, "… as soon as the desired sum of money is placed at [his] disposal." (Doberck, 1883). New 'first-class' magnetic instruments had been added to the Markree Observatory during the year, as had a 'rain-band spectroscope' and a 'Browning's solar eyepiece' for the telescope. Furthermore, he had started to experiment with photography, and a photographic eyepiece for the large refractor was on order (ibid.). We have no surviving copies of his correspondence, so we can only ask why? In the nine years he was there he restored the Observatory to something like its former glory, but it was to be a brief revival.[5] Having journeyed from St Petersburg to Sligo, perhaps it was a renewed wanderlust that led William Doberck to depart for the Orient.

3.4 Doberck's Career in Astronomy to Date

Doberck's departure from Markree marks an opportune time to take stock of his achievements in astronomy, because for several years in his new position he would be too preoccupied with meteorology and managerial issues to pursue his interests in the subject. As noted, his earliest work was on comets, and his first four research papers,[6] written while he was in Copenhagen and Pulkovo, were concerned with determining the orbital elements of Comets II 1869, III 1840, II 1867 and I 1801 (see Doberck, 1872; 1873a; 1873b and 1873c respectively), on the basis of published data provided by other astronomers. Some of this material also went into his doctoral thesis. Doberck continued with these analyses when he moved to Markree, extending them to determine orbital elements for comets I 1845 (see Doberck, 1874c; 1875a; 1875h) and I 1824, (Doberck, 1874b). Almost twenty years later, he wrote one further, final, paper on cometary orbital elements (Doberck, 1895c). Although comets were only a small part of his endeavours, he gets a respectable fourteen citations in Kronk's *Cometography* (1999).

Apart from the odd paper on planetary astronomy, a few on the history of astronomy, and reports on the 1882 transit of Venus (Doberck, 1883b, 1884c), Doberck's publications from Markree Observatory relate to the study of double stars. Altogether there are seventy-three publications, and most of them are in *Astronomische Nachrichten*. These papers contain technical details of his observing methods (Doberck, 1878g; 1878h); his raw data from these observations (Doberck, 1878j; 1878k; 1878l; 1879b); his methods of data analysis to determine double star orbits, using his own observations and data provided by others (Doberck, 1878a; 1878b); and, finally, the computed orbital elements. The latter reports are sometimes very short, and merely list the final values that he obtained for the orbital elements.

It would be some time before Doberck could return to these double star studies, but return he eventually did, both in Hong Kong and later, during his retirement, in England.

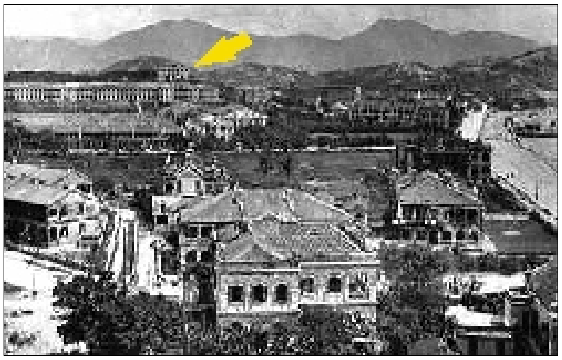


Figure 3: Panoramic view across the Tsim Sha Tsui area showing the Hong Kong Observatory (yellow arrow), high on the hillside above the harbour (courtesy Hong Kong Observatory).

4 A CAREER IN HONG KONG

4.1 The Hong Kong Observatory

Let us briefly take a look at the background to the new institution that Doberck was about to lead. Within the first thirty years of the setting up of the Colony of Hong Kong its commercial importance grew significantly, and this attracted increasing numbers of vessels to the port. Consequently, there was soon a need for a reliable time service, and the demand for a time-ball justified the setting up of an observatory. The depredations wrought on the Colony by unannounced typhoons, and the extent to which the effects of these could be ameliorated—as evidenced by the warnings heeded in the Bay of Bengal and, after 1880, by warnings issued from the observatory in Manila—were yet another justification for such a proposal. Finally, the Royal Society in London was keen to monitor geophysical phenomena, and particularly geomagnetic variations, on a global scale and by establishing an observatory in Hong Kong a major gap in the coverage would be plugged.

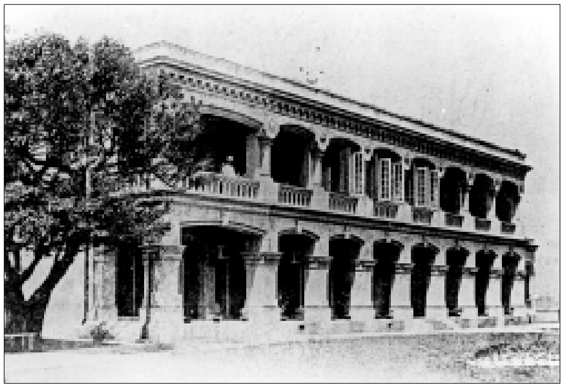


Figure 4: Close-up of the 1883 Hong Kong Observatory building, taken in 1913 (courtesy Hong Kong Observatory).

In October 1877 the Surveyor General, J.M. Price, submitted a proposal for a 'small Observatory', which emphasized the operation of a time ball and allowed for some automated meteorological monitoring. The report noted that "… it may not be too much to aspire perhaps in future years to a sufficiently powerful Equatorial [telescope] to join usefully in the general work of British Colonial Observatories." (*Hong Kong Government Gazette*, 1877). Four years later this proposal was amplified, it must be said without any obvious acknowledgement, by a Colonel Palmer from the Corps of Engineers, who was an aide-de-camp to the Governor, J. Pope Hennessy. Apart from a time ser-vice and meteorological observations, Palmer's plan also encompassed geomagnetic monitoring, but no further astronomical component was included. In 1882 Palmer's proposal was rejected in London as being too expensive—but the acrimonious politicking involving Pope Hennessy at the time, which found Price on the 'other side of the fence' from where most of the Colonial Office's sympathies lay, must not be discounted in this decision. Price was asked to produce and cost a new proposal, so he simply revised his 1877 version, added basic geomagnetic monitoring, and came up with a total cost which was a little more than half that submitted by Palmer. He submitted his 'new' proposal, and it was accepted in 1882.

In a letter from the Secretary of State for the Colonies written in January 1883, Lord Derby formally offered the position of Director of the new Observatory to Doberck, and he accepted it without quibble (for him, a not very common procedure). The appointment, formally from 2 March, was endorsed by the Astronomer Royal, Christie (1882), who gave as his opinion that "… Dr Doberck is … best fitted for the post. From what I know of his scientific attainments I should not hesitate … in recommending him for the appointment." Doberck's new position was welcomed by a commentary in *Nature* (1883) where he was referred to as the "… astronomer to the new institution." At this time there were 'Government Astronomers' at Mauritius, Madras and in the Australian Colonies, and it was noted that the opportunities afforded for independent and original work in Hong Kong were very great. As we will see later, this early ambiguity in some quarters as to Doberck's exact title—'Director' or 'Government Astronomer'—was to become a major bone of contention in his relations with the authorities in Hong Kong. In addition to providing a time service, the new Observatory (Figures 3 and 4) was charged with making meteorological and magnetic observations, but strangely there was no mention of weather forecasting!

It became clear very early on that Doberck's view of his new position (which was essentially the Head of a Department in the Civil Service) was radically different from what was expected in such an appointee. Even before he had taken up his position there was the first indication of what Dyson (1983b) calls "… Doberck's generally irreverent attitude to the accepted formalities of the colonial service." Within a month of his accepting the appointment we have Colonial Office minutes (1883a) that state "… this gentleman is likely to give trouble …", followed by one a day later: "… the sooner this apparently unpleasant man goes out the better … he must be prepared to be in the same subordinate position to the Governor as any other officer." (Colonial Office: minutes, 1883b). Such comments were precursors to very many more in a similar vein over the next quarter of a century.

Doberck arrived in the Colony in June of 1883, and his appointment as 'Director' of the Hong Kong Observatory was gazetted in November (*Hong Kong Government Gazette*, 1883). He was to hold the position until retirement at age 55, twenty-four years later. In the early days he was also often referred to as the 'Government Astronomer', especially by himself, and until 1886 signed his annual reports to Government as such. He was accompanied by his assistant, Frederick George Figg, who, although without credentials in astronomy, turned out to be an invaluable colleague, and eventual succeeded Doberck as Director of the Observatory.

4.2 Astronomy in Hong Kong

Astronomy was a science well known to the Chinese, and 'modern' astronomy had been introduced at Peking by the Jesuits many years before (e.g. see Pigatto, 2004; Shi and Xing, 2006; Zhang, 1998). There had, however, been a long hiatus of about one hundred and thirty years in the publication in Chinese of translated European texts on modern developments in astronomy, when such developments were flourishing elsewhere. After about 1850, with the re-entry of Christian missionaries into the mainland, a new spurt of publishing works on modern science occurred; these were mostly produced by Protestant missionaries. In Shanghai, in 1849, two popular works on astronomy were published in Chinese: *A Digest of Astronomy* by a medical missionary, Benjamin Hobson, and a translation of Andrew P. Happer's *Q&A in Astronomy*. More important was the translation of the Fifth Edition of Herschel's *Outlines of Astronomy* (1858), which appeared in Shanghai in 1859, and thereafter was widely read (Hu, 2005). Whether by the time of Doberck's arrival there was any intellectual base among readers of Chinese in Hong Kong to take advantage of these publications is something that requires further study. However, there was some popular interest in astronomy among English-speakers, as evidenced by the newspaper columns. For example, a long article on the comet of May 1881,[7] extracting mainly from overseas publications, was published in the *China Mail* on 20 August of that year.

The earliest figure on the 'astronomical scene' in Hong Kong appears to have been Henry Spencer Palmer, who has already been mentioned in connection with the establishment of the Observatory.[8] A surveyor in the Royal Corps of Engineers, he came to Hong Kong in March 1878 as an engineer for the Admiralty, at the same time serving as aide-de-camp to the Governor, John Pope Hennessy, with whom he had served in the same position earlier in Barbados. Colonel Palmer had more competence and interest in astronomy than his fellow surveyor colleagues. In 1873 he had spent about ten months training at the Royal Observatory, Greenwich, prior to leading an expedition to New Zealand to observe the 1874 transit of Venus (see Orchiston, 2004). He was elected a Fellow of the Royal Astronomical Society in the same year. Although he believed that his transit observations made on 9 December were of little value (because of heavy cloud cover), when all the global measurements were analysed his results were seen to be highly reliable and were commended by the Astronomer Royal, Sir George Airy. Palmer was very keen to play a similar role during the 1882 transit, and he canvassed Airy in the matter and on several occasions volunteering his services. However, by the time this event occurred Airy had retired from his position, and a suitable opportunity to engage Palmer on an observing team did not present itself. Palmer was always keen to write for popular publications, and in his later years, while based in Japan, he would contribute a regular column to the *Times* of London. He also published an article on "The Great Comet of 1882" in a local newspaper, *The Daily Press*, on 20 November 1882, and this was reprinted in the *Japan Mail* on 2 December, and between 9 and 14 March 1882 he had engaged in a correspondence on the determination of longitude in the other local newspaper, *The China Mail*. Palmer's chief contribution was his determination of the latitude of the site proposed for the new observatory. This he made with a 2.5-inch aperture transit instrument, borrowed from the commander of a U.S. survey ship which was making a local survey. Using this as a zenith telescope, and employing Talcott's method, which he explained in his report (see *Hong Kong Government Gazette*, 1882), Palmer obtained a final result of 22° 18′ 11.89±0.19″, which can be compared with the currently-accepted value of 22° 18′ 12.82″. Palmer had left Hong Kong by the time Doberck arrived, but the Director always referred wistfully to the full Palmer proposal (as much for the higher Director's salary suggested there as for its other substance), which, as we have seen, was sidetracked in favour of a more economical version. Palmer enters our story again, very briefly but crucially, in 1890.

The only other astronomy-related report dating to those early days seems to be one written by James Painter McEwen (1882) who in a December 1882 issue of *Nature* described his naked eye observations of a comet, and gave its position in the sky and an upper limit to its brightness on 27 November. From 1875, McEwen was variously Assistant Harbour Master and Superintendent of Victoria Gaol. He remained in Hong Kong until about 1887, but seems not to have contributed to the subject of astronomy again.

That Doberck's plans for the new Observatory included astronomy (other than the transit observations required for time-keeping)—even though this was not included in his brief—is clear from the beginning. Apart from using the title of 'Government Astronomer' at every opportunity, as early as February 1884 Doberck sent a report to the Government on cloud cover throughout the year[9] and commented (Doberck, 1885c), with perhaps a bit of wishful thinking, that "… the part of the Northern sky which it is most difficult to observe in England can be particularly well explored from this Colony."

The astronomical equipment in the new Observatory was inferior to what Doberck had been used to in earlier years, but that did not hold him back. To establish the time-service he had an f/15 2.75-in Troughton and Simms transit telescope, which was also fitted with a micrometer for zenith observations (although it seldom could be spared for the latter work). From early 1885 onwards Doberck also had what he refers to as the '6-in Lee Equatorial'. This was housed in a separate building, and apparently was a gift or loan to the Observatory from the Astronomer Royal, William

Christie. I believe that this instrument can be identified with the 5.9-in refractor—with a highly-regarded lens by Tully—that was installed by Admiral W.H. Smyth in his Bedford Observatory in the 1830s and was used in compiling his 'Bedford Catalogue' (see Figure 5). In 1839, Smyth sold the telescope to Dr John Lee, who used it in his Hartwell House Observatory until some time after 1865. In 1886 Doberck wrote: "The Lee Equatorial is described by Admiral Smyth in the "*Speculum Hartwellianum*" and the "*Celestial Cycle*" and particulars concerning the magnifying powers of the eye pieces and the scale values of the micrometers are to be found in *Copernicus* (Vol. II p. 93)." (Doberck, 1886b). In 1902 Doberck (1902c) described its limitations, and noted that it was "… upward of eighty years of age, and nearly past use." The last mention of its use seems to be in the Director's report for 1910, where J.I. Plummer writes of an attempt to photograph Comet 1P Halley transiting the Sun in May of that year—Plummer (about whom more anon, and who retired in January 1911) seems to have rarely used the telescope after Doberck's departure. It was finally returned to the Royal Observatory, Greenwich, in April 1914 (Hong Kong Observatory, *Annual Report* …, 1914), and its dome at the Hong Kong Observatory was eventually demolished in July 1933. The telescope is now in the Science Museum, London.

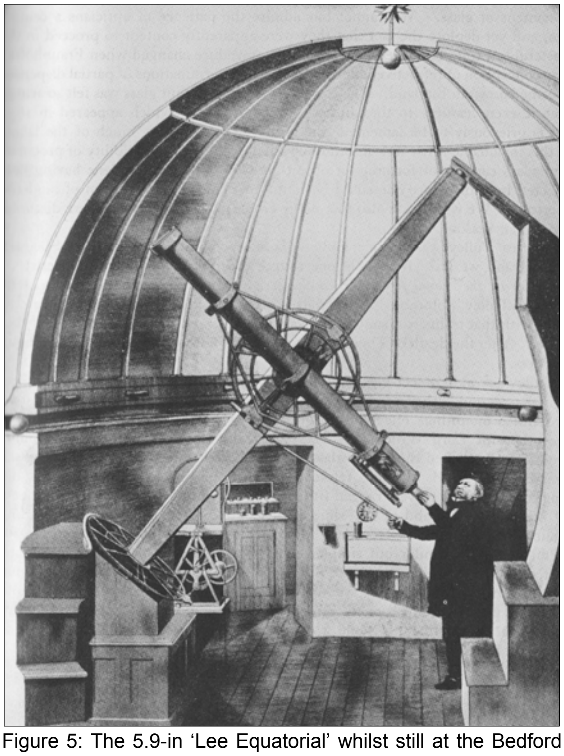


Figure 5: The 5.9-in 'Lee Equatorial' whilst still at the Bedford Observatory (after King, 1979: 195).

Seeing conditions for most of the year in Hong Kong are notoriously bad, and in connection with observing transits for the time-service Doberck (1895b) remarked that "… in early spring … sometimes not a single observation can be obtained for five weeks consecutively." We can also assume that he was minimally frustrated by the seeing conditions, as he had previously pursued the same goals at Sligo where conditions were, if anything, even worse. In the above quote Doberck is being somewhat more honest than in his optimistic report to the Government a year earlier: in its first year of operation he reports viewing Jupiter, Saturn and a few double stars. However, seeing conditions were to prove but a small handicap compared to his on-going struggle with officialdom.

4.3 Adjusting to Colonial Life

Doberck was a dynamic Director, and he readily sacrificed the time he would have liked to devote to astronomy to the many other concerns relevant to the evolution of the Observatory. But his adaptation to colonial life was not without its frustrations. Within four months of Doberck's arrival in Hong Kong, the Governor was writing to the Colonial Office in London, chastising them for their choice of Director, and noting that his "… appearance and manner resemble those of a Professor from one of the smaller German Universities, who has been domesticated in Connaught." (Bowen, 1883). We should also note an unfortunate antipathy on Doberck's part towards the Jesuit-run observatories at Zicawei (Shanghai) and Manila, which coloured his career and can be seen as having had a regressive effect on the development of meteorology in the region. We will only keep track Doberck's meteorological work—which, perforce, occupied most of his time in Hong Kong—in so far as it impinges on his astronomical activities at the time.

Doberck had only been Director for three years, and already had several run-ins with officialdom, when the Colonial Secretary (1886) wrote (with unusual familiarity):

> My dear Dr Doberck, the point you refer to in connection with the publication of your Annual Report was duly considered. While Government Astronomer may be a convenient local designation Director of the Observatory is your official title. In the Dispatch announcing your appointment you were so designated and by that designation you were gazetted on your arrival in the Colony. Yours very truly …

Like all departmental heads, Doberck was obliged to write an annual report on his department's performance for the Governor. The proofs of his draft report for 1886 were acknowledged as follows by the Colonial Secretary (1887):

> … His Excellency is unable to authorize the publication of your Annual Report for 1886 in its present shape ... your remarks on the alleged shortcomings of the Observatory are unbecoming a public report and might be considered as a disrespectful criticism of the decision of the Secretary of State. Statements made in paragraphs 4 and 13 are inaccurate …

Apart from the fact that the draft was still signed 'Government Astronomer', what probably most riled the Governor was the gratuitous comment in it that the "… Royal Alfred Observatory, Mauritius, where such improvements have been lately effected under the genial rule of a Governor well qualified to grasp the importance of scientific research." The draft report also contained the following unremarkable sentence: "Micrometric measurements of Jupiter and Saturn have been reduced and published in the Astronomical Report and progress has been made in the reduction of Double Star Observations." A series of exchanges—both locally and with the Colonial Office in London—followed, which resulted in Doberck finally forwarding a two-page report, which was still signed 'Government Astronomer'. It also retained the mention of double star observations, which some Government officials realised had nothing to do with Dobeck's assigned task of keeping-time. The upshot of the affair was that Dobeck was effectively barred from conducting pure astronomical research in the future. As previously mentioned, he was in the habit of submitting brief annual reports on the Observatory to the Royal Astronomical Society (see Doberck, 1885a; 1886a; 1887; 1889; 1890a; 1891a; 1892; 1893; 1894a; 1895a; 1896a; 1897a; 1898h; 1899a; 1900a; 1902g; and 1905d). However, none appeared for 1887, which in his report for the following year (Doberck, 1889), he attributed to "… circumstances connected with a change of government in Hong Kong. His Excellency the present Governor has decided that purely astronomical observations are not to be subsidised here in future, but the magnetic observations are to be continued."

In November 1887 we again have a concerned minute from the Governor:

> … I observe that Dr Doberck signs himself as Government Astronomer and I request that he will cease so to sign himself, so giving a wrong idea of his position. He is Director of the Observatory and was appointed as such for specific purposes, though, after these are provided for, there is, of course, no objection to his giving his spare time to the general interests of science. (Governor's minute, 1887; his underlining).

By now, Doberck's insistence on using the title 'Government Astronomer' is seen by the Governor to be not as petty as it first appeared, and Doberck's motivation is further clarified by the afore-mentioned minute and a later one: "I cannot see sufficient justification for the publication, at Government expense, of the tables on Double Stars. The printing for the Observatory already costs very disproportionately to the advantage obtained by the Colony …" (ibid.), and "… Dr Doberck in his paper on Double Stars shows that he has abundance of spare time for other objects than those specific ones which occasioned his appointment." (Governor's minute, 1888). The double star material referred to was unfinished work from Doberck's Markree days.

It was not delusions of grandeur, or presumption to a local equivalent of the Astronomer Royal,[10] that drove Doberck's enthusiasm for his title. Rather, it was the expectation that 'Government Astronomer' would entitle him to material support for the pursuit of astronomy in the Observatory, for he saw himself, above all, as a professional astronomer; and with some justifycation. When he died in 1941 he merited an obituary notice in *Nature* that lauded his astronomical achievements, while working "… in various parts of the world, including Kowloon …" (Obituary, 1941a) was the only mention that his twenty-four years in Hong Kong received. So after many years of bureaucratic conflict he may not have been unhappy to leave Kowloon when he reached the earliest retirement age of 55 in 1907; now he could finally return to what he had always wanted to do, observing double stars.

Neither did the publication setback on double stars abort Doberck's astronomical ambitions in Hong Kong. He just had to be more circumspect, and omit such topics from future Annual Reports. Thus he became an 'undercover astronomer'. In his published Report for 1887, in which paragraph 6 had been censored—a more effective technique on the Governor's part than engaging his confrontational Director in extended exchanges—Doberck had yielded on the title of Government Astronomer and signs as 'Director'. But he was clearly enamoured with the former title, even though it was never legally his, because in a report on a lecture that he delivered to the Liverpool Astronomical Society he is referred to as "… Her Majesty's Astronomer at Hong Kong …" (Doberck, 1888), and in the less official 'Meteorological Register', carried in the daily press, he was still signing himself 'Government Astronomer' as late as 16 October 1889. However, he was presumably stung by a letter that appeared in the *China Mail* on that same day pointing out that he had no right to the title ('Veritas', 1889), because from 17 October he signed as 'Director of the Observatory'. Even so, as late as February 1925, in a letter resigning from an IAU Commission on double stars, he signs himself as "… late Government Astronomer, Hong Kong." (Doberck, 1925a). But (with one exception), no mention occurs in later annual reports, either formally to the Government or in his annual *Observations and Researches* of astronomical work, although in his brief annual submissions to the Royal Astronomical Society he does mention such work (which, if anything, appears to increase in output).

4.4 'Undercover Astronomer'

An opportunity to advance astronomy in the Observatory arose from an unexpected quarter in 1890. There was widespread dissatisfaction among the commercial and maritime circles in the Colony with the Observatory's performance and, in particular, the failure to raise warning signals for a typhoon that struck the Hong Kong in October 1889. The Governor hoped to rein in his recalcitrant Director, so he appointed a Commission to enquire into the workings of the Hong Kong Observatory (*Hong Kong Government Gazette*, 1890). The first of five Commissions or Committees of Enquiry that Doberck was to face during his tenure, this one was formed with six members, and was chaired by the Captain Superintendent of Police. It had a broad remit, including the rather pointed question of "… whether the Commission would recommend the continuance of the Observatory in its present form."

The report of the Commission was never published, but its findings seem to have been very sympathetic to the Director, at one point noting that "… an Observatory is essentially one of those Institutions on which, if thoroughly good results are to be obtained, a considerable sum of money must be spent." (Hong Hong Observatory, *Annual Report for 1890*). Much of the credit for such a favourable outcome to the enquiry, as far as Doberck was concerned, must be put down to an unforeseen event: the intervention of (by now) Major General H.S. Palmer, the person who earlier drafted plans for an observatory in Hong Kong. In late January 1890 when the Commission was in session Palmer just happened to be passing through Hong Kong on his way from Japan to England, and since he was seen as a well-qualified and independent authority he was asked for an opinion on the status of the Observatory. He wrote—to quote the Committee's report—a "… very valuable memorandum … in whose conclusions we in the main concur." We do not have the full text of this memorandum, although we know that it did—for the last time in official documents, at least—refer to the Director as the 'Government Astronomer'. But, from a commentary on it in the *China Mail* (1980), an organ that was unrelentingly hostile to the Director, we learn that Palmer's report was "… almost entirely a special pleading for Dr Doberck." The Government accepted the Commission's findings (perhaps reluctantly), and provision was made for the addition of a Chief Assistant and an Assistant Meteorologist. The latter turned out to be William Doberck's sister, Anna (who had assisted him at Markree), and she was to hold this position for twenty-five years.

Figure 6: The only known photograph of J.I. Plummer, 1845–1925 (by kind permission of Kenneth J. Goward, F.R.A.S.).

Knowing of Doberck's enthusiasm for astronomy and his desire to boost the astronomical output of the Observatory, one might suspect that it was no accident that the new Chief Assistant, John Isaac Plummer (1845–1925), was an astronomer of some competence and that Doberck engineered his appointment. A Fellow of the Royal Astronomical Society from 1876, Plummer (Figure 6) had worked at Glasgow Observatory, Durham Observatory and Orwell Park Observatory in England before his arrival in Hong Kong and, among other things, he had published the book, *Introduction to Astronomy*, in 1872. He held an honorary M.A. from the University of Durham. On the face of it, Doberck's hand would not seem to have swayed this choice as the prescribed form detailing the position, signed by him and sent to the Colonial Office, simply specified qualifications that were "… the same as for an Assistant Astronomer in the British Isles …" and stated that the selection should be made by the Astronomer Royal. However, in a letter Doberck (1894b) subsequently revealed that he had been in contact with Christie, the Astronomer Royal: "Mr Plummer was selected according to my own suggestion for his fitness as an assistant astronomer …" By the time the relevant paperwork reached Christie, he had already received several informal approaches about the position, including one on behalf of Plummer, who "… is not of an uneven or irritable temper …" (no doubt in contrast to the candidate he recommended for a Hong Kong appointment eight years earlier!), and since "Mr Plummer seems such a good man for the post …", Christie did not think it was necessary to advertise the post. So Christie recommended Plummer, whom he knew to be in search of a new position (as his previous post at Orwell Park had come to an end with the death of Colonel Tomline and the abandonment of astronomy at that institution). Nor were Plummer's chances harmed by the submissions he received from members of the aristocracy: both Lord Colville of Culross and the Marquess of Bristol wrote to Lord Knutsford, the Secretary of State for the Colonies. The latter reported how he had met Plummer while "… staying with the late Col. Tomline who kept a 'tame' astronomer about the place … he seemed a very respectable, pleasant man … anxious to do anything for a living being a candidate for a mastership of a Union House in ??? [illegible]! I should be glad if I heard in the future that he could keep to his congenial pursuits at Hong Kong." (Marquess of Bristol, 1891). And so Plummer was appointed to the post, but he was hardly a solution to the perennial problem of typhoon-prediction—which continued to preoccupy the minds of those in the Colony—for he had no meteorological publications to his name.

Professionally, Plummer had much in common with Doberck: he was yet another import from a private observatory in the British Isles, and his work had mostly been on comets (e.g. he is cited sixty-one times in Kronk (1999)). On the other hand, for Plummer this was a major step down in the world, for someone who was in the habit of submitting annual reports to the Royal Astronomical Society, and would now be asked by Doberck to perform a range of different tasks, including the cleaning of the time-ball! One would imagine that Doberck welcomed the news of Plummer's appointment; although probably not known to him personally, there had been some interaction in the past as Plummer referred to Doberck's work in some of his early papers. And in the published literature Doberck lauds Plummer on several occasions for his observational skill. For example, in Doberck (1905c) we find: "… Mr Plummer's skill is well known and the smallness of the probable errors prove that the work was accurately done …", yet their relationship was far from smooth. Plummer was already 46 (eight years older than Doberck) when he took up the position on 1 May 1891, and a later Colonial Office minute (1894) reveals that "… it was evident that Dr Doberck did not hit it off with Mr Plummer." Within six months we have the Colonial Secretary (1891) writing Doberck: "… I am to inform you that His Excellency trusts that you, as Head of the Department in which you are both working, will find the means of placing your relations with Mr Plummer on a sounder footing …" A frosty, if not unprofessional, relationship existed between the two men for the next sixteen years, and Plummer was never permitted to forget his junior status. He was entrusted with many observations and calculations that had no obvious connection with time-keeping, and although his contributions were acknowledged he never shared any authorial credit (and apart from a pamphlet on the origin of typhoons seems not to have published anything during his time in Hong Kong). Although bypassed for the Directorship in favour of his nominally junior colleague, Figg, upon Doberck's retirement, Plummer remained in the Observatory until he reached the compulsory retirement age of 65 in 1911. His Fellowship in the Royal Astronomical Society presumably lapsed along the way, as no obituary notice for him is to be found in *Monthly Notices*.

In this context it should be noted that Doberck in no way encouraged astronomy among local people; his earliest expressed opinions on the scientific ability of the locally-employed staff was very negative, although he did mollify his views in later years and commend the ability of some junior staff—but only in the context of meteorological work.

4.5 Doberck's Astronomical Work Whilst in Hong Kong

Of more than eighty reports that Doberck published during his years at the Hong Kong Observatory, less than 15% relate to subjects other than astronomy. But, after his abortive attempts to have his work on double stars published in 1886, he makes no further mention of purely astronomical work in his annual reports and, in fact, seems to have set aside this work for some years. Then in three longish papers that were published in 1890-1891, he reduced some of the observations he made earlier at Markree (see Doberck, 1890b, 1890c, 1891b), and he returned to them again briefly later (Doberck, 1902e). Then from the mid-1890s, he begins presenting new astronomical material in the literature, and at least sixty-one publications appeared between 1895 and his departure from Hong Kong in 1907. This not only represents surreptitious astronomical work conducted at the Observatory, but research carried out while he was away from Hong Kong and on long leave. The early dedication evidenced by an eleven-year tour of duty gave way to his taking regular leave every three years. In 1897, for example, he spent time at the McMillin Observatory at Ohio State University in Columbus. His wife, Harriet Elizabeth Harris, was possibly American, and apparently had some connection with Dayton, Ohio, which could explain Doberck's decision to work at this new Observatory. We have a photograph of him taken about this time in a salon in Dayton (see Figure 7). Then in 1900, and again in 1903, Doberck spent time observing at the Copenhagen Observatory.

As at Markree, Doberck spent most of his time observing double stars, although from 1898 he tended to focus on Southern Hemisphere stars (Doberck, 1898f, 1898g, 1899c, 1900b). And in publishing the orbital elements, he frequently acknowledged Plummer's assistance. Doberck's double star observations made at Hong Kong, Columbus and Copenhagen are reported in a series of papers (see Doberck, 1896e, 1898a, 1901a, 1902c, 1902d, 1903e, 1907a). In Table 1, below, we list references for double star orbital parameters that he calculated whilst in Hong Kong.

4.6 Departure After a Stormy Career

Much of Doberck's later career in Hong Kong is overshadowed by his hostile attitude to the Jesuit observatories in Shanghai and Manila, whose staff he accused of incompetence and plagiarism. This attracted much attention from the Hong Kong press at the time, which echoed more complaints about the Hong Kong Observatory's performance. Doberck (1898i) asserted that the Manila Observatory was

> … in the hands of the Spanish priests, who possess very little scientific education, and who derive much of the matter which they print from the publications, weather telegrams etc issued from this Observatory without however in any way acknowledging their indebtedness to this Observatory …

Furthermore, "… one of the objects of the Jesuits is to undermine non-Roman Catholic scientific institutions and for this and similar reasons they have been expelled from most countries." (ibid.). However, he retained the confidence of the Governor, and seemed indifferent to what he saw as ignorant criticism, devoting more of his time to (semi-furtive) astronomical research. Such an attitude is reflected in his response to an enquiry from the Governor, motivated by a letter from a reader of a local newspaper who asked why no advance notice of the lunar eclipse of 27 June 1899 had been issued by the Observatory. Doberck (1899b) noted that notice had been given four years earlier in the *Nautical Almanac*, and that it was not in the *Gazette* "… because that is not the business of newspapers."

Figure 7: A studio photo of Dr Doberck in Dayton, Ohio, around 1897 (by kind permission of the Librarian, Lick Observatory Archives).

Doberck retired at age 55. It may have been that he felt twenty-four years in the tropics was enough, or he may have hankered for the life of a 'gentleman astronomer', which was to be his fate. But equally likely, he may have been encouraged to go, for his original contract did provide for retirement on full pension at 55. He had long antagonised his superiors, both in Hong Kong and in London, and one more episode of sparring with the Government may have been the deciding factor. Even if, by now, he did not draw attention to his astronomical activities, some awareness of them seems to have existed outside the Observatory. In its editorial of 20 September 1906, the *China Mail* asked "… [do] we have a perfectly equipped and officered astronomical station and meteorology is rather contemptuously relegated to the second place?" This was in the context of calling for an enquiry into why the Observatory had failed to raise storm-warning signals in time prior to a disastrous typhoon that had struck Hong Kong two days earlier. The first signal only went up at 8:00 a.m. on 18 September, and by 11 a.m. the same day it was all over. Probably the worst typhoon to strike Hong Kong in recorded times, it resulted in enormous loss in ships and at least 10,000 lives (i.e. ~3% of the Colony's population). In the subsequent enquiry—which exonerated the Observatory and its Director from incompetence—surprisingly nobody raised questions about the time Doberck and his staff devoted to astronomy (*Hong Kong Government Gazette*, 1907).[11] Several, for the most part unfriendly, communications followed, including one from the Governor: "I must express my surprise at the tone adopted in the minute of the Director of the Observatory … The language used by Dr Doberck is calculated to shake one's confidence in his fitness to occupy the position he fills …" (Governor's minute, 1907). One suspects that the Governor was not entirely pleased by the conclusions of the enquiry, but he sent the report, along with ones supplied by the Zicawei and Manila Observatories, in his dispatches to London, and requested that the opinion of the Royal Observatory at Greenwich on the matter be obtained. Ignoring any possible role of rivalry with the Jesuits, the Astronomer Royal reported promptly and stated that he could find no reason to disagree with the Committee's conclusions: "….a review of the evidence placed before the Committee of investigation points to the conclusion that the finding of the Committee was practically inevitable … [and] there remains no question of dereliction of duty at the Observatory." (Hong Kong Legislative Council, 1907).

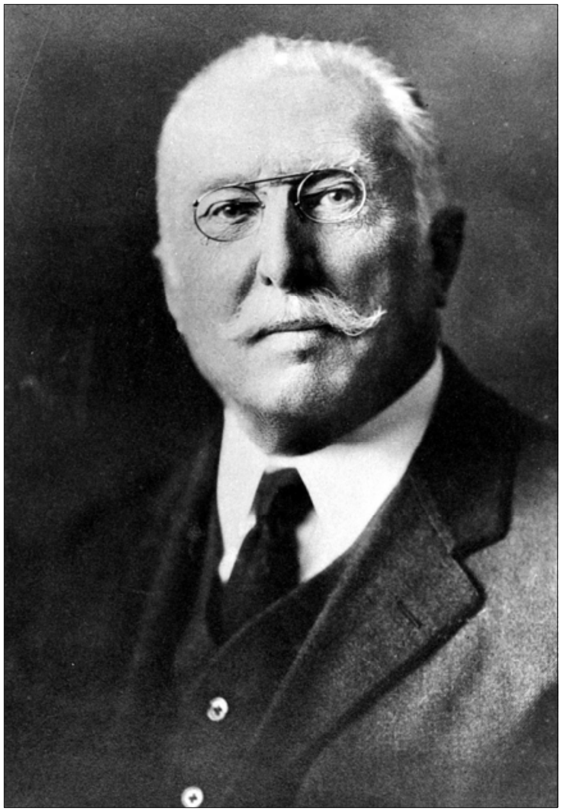


Figure 8: Dr Doberck in later life (by kind permission of the Director, Hong Kong Observatory).

5 'KOWLOON OBSERVATORY', SURREY

In June 1907 Doberck went to England on leave, and he retired on a pension of £360 per year in September. Whatever the reason for his departure in June, he stayed on in England, where he was destined to live for another thirty-four years. He immediately proceeded to set up his own gentleman's observatory, 'Kowloon', at Sutton in Surrey, which was equipped with a 6-in refractor (Doberck, 1909a). Although it lacked the grandeur of Markree, Kowloon Observatory was grand enough to keep Doberck occupied for another quarter of a century, and he continued his stellar observations, initially with a little financial support from Harvard College Observatory. It was only in 1908—after his retirement—that he was elected a Fellow of the Royal Astronomical Society, but later he appears to have let his membership lapse as no obituary for him appeared in *Monthly Not-ices*. A photograph of Doberck in later life is shown in Figure 8.

At Sutton Doberck continued with his analysis of double star orbits, made regular observations of different doubles, and from 1919 reported observations of variable stars. His wife helped him to some extent, for in the account of his new observatory in *Monthly Notices* he speaks of her cooperation in measuring double stars (ibid.), and many years later, in a letter to R.G. Aitken he wrote that "… my wife has promised to assist me for one year, and with her help I am able to do twice the amount of work in the same time as when I work alone." (Doberck, 1922). But any suggestion that it was more than a domestic duty is contradicted by his elaboration: "… if you have to appoint a double star observer, you ought to make it a condition that his wife assists him." His attitude in these matters was also illustrated in a letter he wrote in 1891 recommending his sister for appointment as his meteorological assistant at the Hong Kong Observatory (Doberck, 1891c). After noting the frequent employment of females in observatories, including Greenwich, Kew, Durham (where only the Director was a man), Madras etc., because "… they are very steady at such work…", he also gave as an excuse for employing her that "… it is important that I should have somebody living with me who would be at my beck and call at any hour day or night."

At some point, Doberck was appointed to the IAU Commission on Double Stars, but in early 1925, he found himself in a minority on some aspects of a report prepared by the Commission and resigned (Doberck, 1925a).

6 DOBERCK'S LEGACY IN ASTRONOMY

It is with the field of astronomy—rather than meteorology, to which he was obliged to devote so much of his life—that Doberck's reputation lies. Doberck started off working on cometary orbits (Doberck, 1872; 1873a; 1873b; 1873c; 1873d; 1874a; 1874b; 1874c; 1874d; 1875a), and after his retirement published a couple of further papers on comets (Doberck, 1912b; 1915). Then between 1918 and 1925 he reported some observations of variable stars (Doberck, 1917; 1918a; 1918b; 1918c; 1919a; 1919b; 1919c; 1919d; 1919e; 1919f; 1919g; 1920a; 1920b; 1920c; 1924a; 1924b; 1924c; 1924d; 1924e; 1925c).[13] But he should be remembered mainly for his work on double stars.

Table 1 summarises his double star work by listing all of the stars he investigated during his intervals at Markree Observatory, Hong Kong Observatory and his Kowloon Observatory, along with the associated publications. Raw data from his Sutton Observatory observations are presented in a long series of reports in *Astronomische Nachrichten* (Doberck, 1909h, 1910b, 1911a, 1911b, 1911d, 1912d, 1912e, 1913a, 1913c, 1914a, 1914b, 1914c, 1923, 1925b, 1926, 1930, 1931, 1933, 1935). Note that his final report appeared in 1935, when he was 83 years old.

Doberck did not have equipment suited to carrying out exhaustive searches for new (close) double stars, and consequently all of the major Northern Hemisphere systems that lay within the resolving power of his telescopes had been discovered long before he began investigating them.[12] Nor did he have photographic facilities that would enable him to study spectroscopic binaries, and his tentative excursions in that direction—as we have seen—were aborted by his move from Markree to Hong Kong. And so it was that known visual binaries were to occupy him for most of his years. During his quarter century in Hong Kong his observing schedule had to be somewhat furtive, and necessarily conducted at a slow pace, yet this was a time when others were forging ahead with the study of binaries, especially using new spectrographic techniques. But to these studies he could bring his natural affinity for precise quantitative measurement, where biases and measuring uncertainties could be systematically evaluated, and this was an aspect that he emphasized. In his publications he always laid great stress on taking statistical and systematic errors into account when evaluating the published data (e.g. see Doberck, 1908c; 1909k). From chronologically-accumulated data he could then produce reliable orbits for several stellar pairs, and Aitken (1918: 240) referred to Doberck as a "… the veteran computer … who has investigated more double star orbits than any other astronomer …" (and he specifically endorsed fourteen of Doberck's orbits). For example, in calculating the orbit for τ Ophiuchi, Doberck (1906a) used data that extended over thirty years, and for his investigation of ξ Bootis the measurements extended from 1877 to 1921 (Doberck, 1921b). We should also note that the orbital parameters he derived for the difficult long period double star, Castor AB (α Geminorum), are similar to the currently-accepted value (see Heintz, 1988). Doberck was also the first to draw attention to a tendency for the eccentricity in double star orbits to correlate with their periods (see Doberck, 1878e, 1898b; cf. Aitken, 1918: 195), a phenomenon whose interpretation is still a current topic of interest (Dommanget, 2003).

Doberck always held his own in any argument or confrontation, and deferred to none, but one cannot fail to see some hints of frustration in his long career, in part due to his limited access to suitable observing facilities and in part because of his irascible nature. His systematic study, over the best part of fifty years, of the motions in some double star systems was of value to the astronomical community, and to quote from his obituary (1941a), "… it is as an exceptionally diligent and successful student of visual double stars that he will always be remembered." Meanwhile, Aitken (1941) says this in the obituary that he wrote for Doberck: "… his entire career exemplifies what an enthusiastic amateur can accomplish even when he must content himself with a small telescope located where atmospheric conditions are only moderately favourable …" This very much echoed Doberck's own opinion, as expressed in an essay written almost sixty years earlier where he quotes a remark by Bessel: "… a practical astronomer ought to be able to do something, even if he has only a cart-wheel and a gun-barrel at his disposal." (Doberck, 1884b).

Table 1: Double stars investigated by William Doberck, 1875-1935.

Double Star	Doberck's Publications
α Centauri	1879c, 1896b, 1907d, 1910c
α Geminorum(Σ 1110)	1878d, 1878f, 1898c, 1902a, 1904a, 1910c
β 101	1913b
β 416	1903b, 1910c
β 733 (85 Pegasi)	1906c
γ Centauri	1906b, 1910c
γ Coronae Australis	1912c
γ Coronae Borealis(Σ 1967)	1877b, 1877d, 1878c, 1905a, 1909e, 1910c
γ Leonis(Σ 1424)	1875c, 1875e, 1875m, 1876g, 1879d, 1897d
γ Virginis	1896c, 1908a, 1910c
ζ Aquarii(Σ 2909)	1875c, 1875f, 1875n
ζ Cancri(Σ 1196)	1880d, 1907b, 1909b, 1910c
ζ Herculis	1881a, 1897c, 1910c
ζ Librae	1877c
ζ Sagittarii	1904b, 1910c
η Cassiopeiæ(Σ 1424)	1876a, 1876d, 1876g, 1878d, 1901b, 1909d, 1910c
η Coronae Borealis(Σ 1937)	1881b, 1886d, 1910c
θ Orionis	1908b
ι Leonis	1875c, 1875f, 1876e
λ Ophiuchi(Σ 2055)	1876i, 1877e, 1878d
μ^2 Boötis(Σ 1938)	1875b, 1875d, 1875g, 1876g, 1878i, 1897b, 1910c
μ Draconis(Σ 2130)	1876a, 1876e
μ^2 Herculis	1880a, 1907c, 1910c
ξ Boötis(Σ 1888)	1877a, 1877f, 1878c, 1903a, 1909g, 1921b, 1910c, 1921b
ξ Librae(Σ 1998)	1876j, 1878c
ξ Scorpii	1907c, 1910c
O(Σ 235)	1880b, 1880c
O (Σ 298)	1879f
O(Σ 387)	1898d, 1910c
O(Σ 400)	1898e
σ Coronae(Σ 2032)	1875c, 1875d, 1875i, 1876h, 1878d, 1905b, 1910c
Σ 228	1898e, 1910c
Σ 1757	1876e
Σ 1768	1877i, 1878c
Σ 1819	1876e
Σ 2173	1907c, 1910c
Σ 2525	1911c
Σ 3062	1877j, 1878c, 1879e
Σ 3121	1877i, 1878c, 1907c, 1910c
τ Ophiuchi(Σ 2262)	1875c, 1875e, 1875j, 1875k, 1875l, 1876g, 1877e, 1878d, 1906a, 1910c
φ Ursae majoris	1903c, 1910c
ω Leonis(Σ 1356)	1876b, 1876c, 1876f, 1878c, 1907b, 1910c
4 Aquarii	1880a, 1912a
25 Canum Venaticorum(Σ 1768)	1881c, 1910c

36 Andromedæ(Σ 73)	1875f, 1875p, 1879a
40 O2 Eridani	1910d
42 Comae Berenices(Σ 1728)	1909c, 1910c
44 ι Boötis(Σ 1909)	1875q, 1876a, 1876g, 1878d, 1909i, 1910c
70 p Ophiuchi	1906d, 1910c
85 Pegasi	1910c
99 Herculis	1903d, 1910c
H I 39	1907b, 1910c
p Eridani	1877h, 1878c
Sirius	1904c, 1910c

7 NOTES

1. What little we do know about Doberck's early life is almost entirely due to an entry in the *Dansk Biografisk Lexikon* (see Bricka, 1890)—which only takes us up to about 1900—and two short obituary notices (Obituary, 1941a; 1941b).
2. Working under d'Arrest, Dreyer was awarded a University of Copenhagen doctorate in 1874.
3. By a strange coincidence, in the 1930s this telescope ended up at a Jesuit seminary in Aberdeen, Hong Kong. However, Doberck does not seem to have been involved in this transfer.
4. This was Colonel A.R. Clarke, F.R.S.
5. Doberck's successor at Markree Observatory was the German, Albert Marth. A well-respected astronomer (Dreyer, 1897), Marth was 55 when he took up the post. However, he seems to have made no use of the astronomical instruments during his fourteen years there, and spent most of his efforts calculating ephemerides. After his death, in 1897, all astronomical work ceased and the Observatory functioned purely as a meteorological facility.
6. These four papers, and his doctoral thesis, were the only astronomical contributions that Doberck wrote in German.
7. This was the Great Comet of 1881, which was discovered by the well-known Australian astronomer, John Tebbutt. For information on this comet see Orchiston (1999).
8. Henry Spencer Palmer (1838-1893) spent much of his later life in Japan, where he died in 1893. A highly-appreciative obituary occurs in *Monthly Notices* (Obituary, 1894), and an outline biography has been written by Higuchi (2002).
9. This was based on data accumulated during the previous four years, and indicated that 70% cloud cover could be expected from February through to May.
10. Doberck was in fact accorded this title by at least one writer: "… Dr. Doberck, the present distinguished Astronomer-Royal of Hong Kong, has rendered services to science which are spoken of with respect in all the observatories of the world." (O'Rorke, 1889: 529).
11. The full report was a Supplement to the *Gazette*.
12. The Markree telescope had a resolving power of ~0.43″, and the instruments he had at Hong Kong and in retirement had only half this resolving power.
13. Around 1917 Doberck took a break from double stars in favour of observing variable stars. They had attracted him as long ago as his time in Sligo, but he realised that the atrocious weather conditions there made such observations almost impossible. While he does not mention an improvement in seeing in Surrey, he does confidently report on the variability of a large number of stars.

8 ACKNOWLEDGEMENTS

Many thanks are due to the Director of the Hong Kong Observatory, Mr C. Y. Lam, and his staff, staff of the Hong Kong Public Records Office, and the staff of the Library Special Collections at the University of Hong Kong for courteous assistance in accessing their holdings. For useful information on William Doberck I am indebted to his great grand nephew, Michael Doberck of Copenhagen, as I am for permission to publish Figure 1. For other assistance, I would like especially to thank Margaret N. Burri of the Milton S. Eisenhower Library; The Johns Hopkins University; Kenneth J. Goward F.R.A.S. (Orwell Park); Peter D Hingley, Librarian of the Royal Astronomical Society; Professor Kerrie L. MacPherson (Hong Kong); Dr Patrick O'Neill (Dublin); Dorothy Schaumberg and Cheryl Dandridge of the Lick Observatory Archives; and Dr Wolfgang Steinicke (Umkirch).

9 REFERENCES

Minutes from the Public Records Office (U.K.) are from files CO129/297 (1882) through CO129/340 (1907). Material from the Hong Kong Public Records Office is in files HKRS149-2-1558, HKRS356 and HKRS842. Different spellings in early documents, Hongkong, Hong-Kong, Kaulung, etc., are all standardised throughout to 'Hong Kong'. I have not distinguished between 'Governor', 'Acting Governor' and 'Officer Administering the Government', nor between 'Colonial Secretary' and 'Acting Colonial Secretary', unless such a distinction is essential.

Aitken, R.G., 1918. *The Binary Stars*. New York, Douglas C. McMurtrie.

Aitken, R.G., 1941. Obituary. *Publications of the Astronomical Society of the Pacific*, 53, 263-264.

Bowen, G.F., 1883. Letter to the Secretary of State for the Colonies, dated 22 November. In CO129/212.

Bricka, C.F. (ed.), 1890. Doberck. August William. In *Dansk Biografisk Lexikon*. Copenhagen, Gyldendalske Boghandels Forlag (F. Hegel & Son).

China Mail, 20 August, 1881.

China Mail, 1 May, 1890.

Christie, W.H.M., 1882. Letter to the Under-secretary of State for the Colonies, dated 12 October. In CO129/206.

Chu, A., 2003. Amateur Astronomy in Hong Kong – A Brief History. www.alanchuhk@HK_Astro.doc.

Colonial Office: Minutes of 20 April 1883(a). In CO129/214.

Colonial Office: Minutes of 21 April 1883(b). In CO129/214.

Colonial Office: Minutes of 17 July 1894. In CO129/263.

Colonial Secretary, 1886. Letter to W. Doberck, dated 6 April. In HKRS356 1-1(2).

Colonial Secretary, 1887. Letter to W. Doberck, 8 January. In HKRS356 1-1(2).

Colonial Secretary, 1891. Letter to W. Doberck, dated 21 October. In HKRS842.

Derby, Lord, 1883. Letter to W. Doberck, dated 3 January. In HKRS356 1-1(1).

Doberck, W., 1872. Neue parabolische Elemente des Tempel' schen Cometen II. 1869. *Astronomische Nachrichten*, 79, 383.

Doberck, W., 1873a. Parabolische Elemente des Cometen III. 1840. *Astronomische Nachrichten*, 80, 377-378.

Doberck, W., 1873b. Ephemeride des Cometen II 1867 von Herrn Doberck in Pulkowa. *Astronomische Nachrichten*, 81, 189.

Doberck, W., 1873c. Parabolische Elemente des Cometen I 1801. *Astronomische Nachrichten*, 81, 321-324.

Doberck, W., 1873d. *Bahnbestimmung der Cometen I 1801, III 1840 und II 1869. Inaugural-Dissertation ... zu Jena*. Copenhagen, Cohens Buchdrukerei.

Doberck, W., 1874a. Definitive elements of the Comets I. 1801; I. 1824; III. 1840, II. 1869. *Monthly Notices of the Royal Astronomical Society*, 34, 426.

Doberck, W., 1874b. Definitive determination of the orbit of Comet I. 1824. *Astronomische Nachrichten*, 84, 75-80.

Doberck, W., 1874c. Hyperbolic elements of Comet I. 1845. *Monthly Notices Royal Astronomical Society*, 35, 104.

Doberck, W., 1875a. On the first comet of 1845. *Transactions of the Royal Irish Academy*, 25, 459-480.

Doberck, W., 1875b. On μ^2 Boötis (as a revolving double star). *Transactions of the Royal Irish Academy*, 25, 481-490.

Doberck, W., 1875c. On the binary stars σ Coronae, τ Ophiuchi, γ Leonis, ζ Aquarii, 36 Andromedae and ι Leonis. *Transactions of the Royal Irish Academy*, 25, 581-603.

Doberck, W., 1875d. Elements of binary stars. *Monthly Notices Royal Astronomical Society*, 35, 373.

Doberck, W., 1875e. Elements of τ Ophiuchi and γ Leonis, Algieba. *Monthly Notices Royal Astronomical Society*, 35, 397.

Doberck, W., 1875f. Elements of ζ Aquarii, 36 Andromedæ, and ι Leonis. *Monthly Notices Royal Astronomical Society*, 35, 408-409.

Doberck, W., 1875g. New elements of μ^2 Boötis. *Astronomische Nachrichten*, 85, 147-152.

Doberck, W., 1875h. Hyperbolical elements of Comet I 1845. *Astronomische Nachrichten*, 85, 205-8.

Doberck, W., 1875i. Elements of Σ Coronae Borealis. *Astronomische Nachrichten*, 85, 323-328.

Doberck, W., 1875j. Provisional elements of τ Ophiuchi. *Astronomische Nachrichten*, 85, 331.

Doberck, W., 1875k. Elements of τ Ophiuchi. *Astronomische Nachrichten*, 86, 13-16.

Doberck, W., 1875l. Comparison of elements of τ Ophiuchi with Pulkowa observations. *Astronomische Nachrichten*, 86, 79.

Doberck, W., 1875m. Elements of γ Leonis. *Astronomische Nachrichten*, 86, 129-132.

Doberck, W., 1875n. Elements of ζ Aquarii. *Astronomische Nachrichten*, 86, 155-158.

Doberck, W., 1875p. Elements of 36 Andromedae Σ 73. *Astronomische Nachrichten*, 86, 187-190.

Doberck, W., 1875q. Elements of 44 Bootis. *Astronomische Nachrichten*, 86, 377-380.

Doberck, W., 1876a. On the binary stars 44 Boötis, η Cassiopeiæ and μ Draconis, with addenda to previous memoirs. *Transactions of the Royal Irish Academy*, 26, part 1.

Doberck, W., 1876b. On ω Leonis considered as a revolving double star. *Transactions of the Royal Irish Academy*, 26, 165-186.

Doberck, W., 1876c. Provisional elements of ω Leonis. *Astronomische Nachrichten*, 87, 223.

Doberck, W., 1876d. Elements of η Cassiopeiae. *Astronomische Nachrichten*, 88, 45-48.

Doberck, W., 1876e. Formulae for some double stars. *Astronomische Nachrichten*, 88, 63.

Doberck, W., 1876f. Elements of ω Leonis. *Astronomische Nachrichten*, 88, 109.

Doberck, W., 1876g. Double star observations compared with elements. *Astronomische Nachrichten*, 88, 199-202.

Doberck, W., 1876h. New elements of Σ Coronae Borealis. *Astronomische Nachrichten*, 88, 233-236.

Doberck, W., 1876i. Provisional elements of λ Ophiuchi. *Astronomische Nachrichten*, 88, 287.

Doberck, W., 1876j. Circular Orbit of ξ Librae (=Scorpii). *Astronomische Nachrichten*, 88, 297.

Doberck, W., 1876k. Markree Observatory. *Monthly Notices of the Royal Astronomical Society*, 36, 171.

Doberck, W., 1877a. Elements of ξ Boötis. *Astronomische Nachrichten*, 89, 95.

Doberck, W., 1877b. First elements of γ Coronae Borealis after Struve's observations, *Astronomische Nachrichten*, 89, 134.

Doberck, W., 1877c. Elliptical elements of ζ Librae. *Astronomische Nachrichten*, 89, 135.

Doberck, W., 1877d. Second elements of γ Coronae Borealis. *Astronomische Nachrichten*, 89, 171.

Doberck, W., 1877e. Elements of λ Ophiuchi and τ Ophiuchi, *Astronomische Nachrichten*, 89, 215.

Doberck, W., 1877f. Elements of Xi Boötis. *Astronomische Nachrichten*, 89, 259-262.

Doberck, W., 1877g. On double-star-calculation. *Astronomische Nachrichten*, 90, 57-64.

Doberck, W., 1877h. Elements of p Eridani. *Astronomische Nachrichten*, 90, 191.

Doberck, W., 1877i. Provisional elements of Σ 1768 and Σ 3121. *Astronomische Nachrichten*, 90, 313.

Doberck, W., 1877j. Elements of Σ 3062. *Astronomische Nachrichten*, 90, 319.

Doberck, W., 1877k. Markree Observatory. *Monthly Notices of the Royal Astronomical Society*, 37, 173.

Doberck, W., 1878a. Binary stars. *The Observatory*, 2, 110-116.

Doberck, W., 1878b. Binary stars (part II). *The Observatory*, 2, 140-144.

Doberck, W., 1878c. Binary stars (part III). *The Observatory*, 2, 169-174.

Doberck, W., 1878d. Binary stars (part IV). *The Observatory*, 2, 209-217.

Doberck, W., 1878e. On double-star calculations. *Astronomische Nachrichten*, 91, 119-122 and 317.

Doberck, W., 1878f. Provisional-elements of α Geminorum. *Astronomische Nachrichten*, 91, 123-128.

Doberck, W., 1878g. On double-star observations, *Astronomische Nachrichten*, 92, 33-44.

Doberck, W., 1878h. The large meridian-circle in Markree. *Astronomische Nachrichten*, 92, 65-68.

Doberck, W., 1878i. Elements of μ^2 Boötis. *Astronomische Nachrichten*, 92, 157.

Doberck, W., 1878j. Markree double-star observations. *Astronomische Nachrichten*, 92, 181-190.

Doberck, W., 1878k. Markree double-star observations. *Astronomische Nachrichten*, 92, 201-208.

Doberck, W., 1878l. Markree double-star observations. *Astronomische Nachrichten*, 92, 209-220.

Doberck, W., 1878m. Markree double-star observations. *Astronomische Nachrichten*, 92, 227-236.

Doberck, W., 1878n. Markree Observatory. *Monthly Notices of the Royal Astronomical Society*, 38, 183.

Doberck, W., 1879a. New elements of 36 Andromedae Σ 73. *Astronomische Nachrichten*, 94, 121.

Doberck, W., 1879b. Markree double-star observations. *Astronomische Nachrichten*, 94, 147-152.

Doberck, W., 1879c. Provisional elements of α Centauri, *Astronomische Nachrichten*, 94, 207.

Doberck, W., 1879d. New elements of γ Leonis. *Astronomische Nachrichten*, 94, 255.

Doberck, W., 1879e. New elements of Σ 3062. *Astronomische Nachrichten*, 95, 331.

Doberck, W., 1879f. Elements of OΣ 298. *Astronomische Nachrichten*, 95, 383.

Doberck, W., 1879g. Markree Observatory. *Monthly Notices of the Royal Astronomical Society*, 39, 251-252.

Doberck, W., 1880a. Elements of 4 Aquarii and μ^2 Herculis. *Astronomische Nachrichten*, 96, 111.

Doberck, W., 1880b. Elements of OΣ 235. *Astronomische Nachrichten*, 96, 221.

Doberck, W., 1880c. Elements of OΣ 235. *Astronomische Nachrichten*, 96, 235.

Doberck, W., 1880d. Elements of ζ Cancri. *Astronomische Nachrichten*, 97, 283-286.

Doberck, W., 1880e. Markree Observatory. *Monthly Notices of the Royal Astronomical Society*, 40, 226.

Doberck, W., 1881a. Elements of ζ Herculis. *Astronomische Nachrichten*, 98, 59.

Doberck, W., 1881b. Elements of η Coronae Borealis. *Astronomische Nachrichten*, 98, 157.

Doberck, W., 1881c. Elements of 25 Canum Venaticorum = Σ 1768. *Astronomische Nachrichten*, 98, 271.

Doberck, W., 1881d. Markree Observatory. *Monthly Notices of the Royal Astronomical Society*, 41, 206.

Doberck, W., 1882. Markree Observatory. *Monthly Notices of the Royal Astronomical Society*, 42, 164-165.

Doberck, W., 1883a. Markree Observatory. *Monthly Notices of the Royal Astronomical Society*, 43, 197-199.

Doberck, W., 1883b. The transit of Venus. *Nature*, 27, 157-158.

Doberck, W., 1884a. Markree Observatory. *The Observatory*, 7, 283-288.

Doberck, W., 1884b. Markree Observatory (continued). *The Observatory*, 7, 329-332.

Doberck, W., 1884c. The transit of Venus observed at the Markree Observatory. *Copernicus*, 3, 19-20.

Doberck, W., 1885a. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 45, 238.

Doberck, W., 1885b. Letter to the Acting Colonial Secretary, dated 5 October. In CO129/222.

Doberck, W., 1885c. *Observations and Researches made at the Hong Kong Observatory in the year 1884*. Hong Kong, Hong Kong Observatory. Appendix B.

Doberck, W., 1886a. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 46, 227.

Doberck, W., 1886b. *Report on the Astronomical Instruments at the Observatory and on the Time Service of Hong Kong in 1885*, in *Observations and Researches made at the Hong Kong Observatory in the year 1885*. Hong Kong, Hong Kong Observatory.

Doberck, W., 1887. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 47, 171.

Doberck, W., 1888. *English Mechanic*, 20 April 1888, p. 165.

Doberck, W., 1889. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 49, 205-206.

Doberck, W., 1890a. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 50, 212-213.

Doberck, W., 1890b. Markree double-star observations. *Astronomische Nachrichten*, 125, 195-208.

Doberck, W., 1890c. Markree observations of double stars. *Transactions of the Royal Irish Academy*, 29, 379-426.

Doberck, W., 1891a. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 51, 230.

Doberck, W., 1891b. Markree double-star observations. *Astronomische Nachrichten*, 126, 365-380.

Doberck, W., 1891c. Letter to the Acting Colonial Secretary, dated 16 December. In CO129/252.

Doberck, W., 1892. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 52, 263.

Doberck, W., 1893. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 53, 256.

Doberck, W., 1894a. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 54, 233.

Doberck, W., 1894b. Letter to the Colonial Secretary, dated 6 May. In CO129/265 p.677.

Doberck, W., 1895a. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 55, 225-226.

Doberck, W., 1895b. The time-service of the Hongkong Observatory. *The Observatory*, 18, 297-301.

Doberck, W., 1895c. Definitive elements of the orbit of Comet 1824 I, *Astronomische Nachrichten*, 138, 321-332.

Doberck, W., 1896a. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 56, 242-243.

Doberck, W., 1896b. Elements of the orbit of α Centauri. *Astronomische Nachrichten*, 139, 273-280.

Doberck, W., 1896c. On the elements of the orbit of γ Virginis. *Astronomische Nachrichten*, 141, 57-70.

Doberck, W., 1896d. On the orbit of η Coronae Borealis, *Astronomische Nachrichten*, 141, 153-170.

Doberck, W., 1896e. Double star observations. *Astronomische Nachrichten*, 141, 289-298.

Doberck, W., 1897a. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 57, 264.

Doberck, W., 1897b. On the orbit of μ_2 Bootis. *Astronomische Nachrichten*, 144, 129-138.

Doberck, W., 1897c. On the orbit of ζ Herculis. *Astronomische Nachrichten*, 144, 241-251.

Doberck, W., 1897d. On the orbit of γ Leonis. *Astronomische Nachrichten*, 144, 251-254.

Doberck, W., 1898a. Double star observations. *Astronomische Nachrichten*, 145, 151-156.

Doberck, W., 1898b. On double star orbits: their periods and eccentricities. *Astronomische Nachrichten*, 147, 251.

Doberck, W., 1898c. On the elements of the orbit of Castor. *Astronomische Nachrichten*, 147, 337.

Doberck, W., 1898d. On the elements of the orbit of OΣ 387. *Astronomische Nachrichten*, 147, 339-342.

Doberck, W., 1898e. On the elements of the orbits of Σ 228 and OΣ 400, *Astronomische Nachrichten*, 147, 343-346.

Doberck, W., 1898f. On the proper motions of some southern stars. *Astronomische Nachrichten*, 148, 273-278.

Doberck, W., 1898g. Proper motions of some southern stars. *Astronomische Nachrichten*, 149, 395.

Doberck, W., 1898h. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 58, 190.

Doberck, W., 1898i. Letter to Dr Moore, Chief of the U S Weather Bureau, dated 5 November. In HKRS842.

Doberck, W., 1899a. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 59, 266-267.

Doberck, W., 1899b. Letter to the Colonial Secretary, dated 29 June. In HKRS842.

Doberck, W., 1899c. Observations on southern variable stars. *Astronomische Nachrichten*, 150, 169-176.

Doberck, W., 1900a. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 60, 360.

Doberck, W., 1900b. Proper motions of some southern stars. *Astronomische Nachrichten*, 152, 281-4.

Doberck, W., 1900c. On the magnitudes of 919 fixed stars determined from sequences observed by Sir John Herschel during the years 1835 to 1838, II. *Astrophysical Journal*, 11, 192-219 and 270-287.

Doberck, W., 1901a. Double star observations. *Astronomische Nachrichten*, 154, 165-186.

Doberck, W., 1901b. On the orbit of η Cassiopeiae. *Astronomische Nachrichten*, 156, 353-362.

Doberck, W., 1902a. Ephemeris of Castor. *Astronomische Nachrichten*, 158, 363.

Doberck, W., 1902b. On the distribution of binary stars. *Astronomische Nachrichten*, 158, 363.

Doberck, W., 1902c. Hongkong double star observations. *Astronomische Nachrichten*, **159**, 85-107.

Doberck, W., 1902d. Further Hongkong double star observations. *Astronomische Nachrichten*, 159, 107-114.

Doberck, W., 1902e. On the accuracy of Markree double star observations. *Astronomische Nachrichten*, 159, 307-309.

Doberck, W., 1902f. (with Pickering, E C) On the magnitudes of 923 fixed stars determined from sequences observed by Sir John Herschel during the years 1835 to 1838. *Annals Harvard College Observatory*, 41, 213-235.

Doberck, W., 1902g. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 62, 280-281.

Doberck, W., 1903a. On the orbit of ξ Bootis. *Astronomische Nachrichten*, 163, 177.

Doberck, W., 1903b. On the orbit of β 416. *Astronomische Nachrichten*, 163, 313-316.

Doberck, W., 1903c. On the orbit of φ Ursae majoris. *Astronomische Nachrichten*, 163, 371-373.

Doberck, W., 1903d. On the orbit of 99 Herculis = A. C. 15. *Astronomische Nachrichten*, 163, 373-376.

Doberck, W., 1903e. Double star observations. *Astronomische Nachrichten*, 164, 309-340.

Doberck, W., 1904a. On the orbit of Castor. *Astronomische Nachrichten*, 166, 145-148.

Doberck, W., 1904b. On the orbit of ζ Sagittarii. *Astronomische Nachrichten*, 166, 149.

Doberck, W., 1904c. On the orbit of Sirius. *Astronomische Nachrichten*, 166, 321-326.

Doberck, W., 1905a. On the orbit of γ Coronae borealis. *Astronomische Nachrichten*, 169, 133-138.

Doberck, W., 1905b. On the orbit of Σ Coronae borealis. *Astronomische Nachrichten*, 169, 289-298.

Doberck, W., 1905c. *Catalogue of Right-Ascensions of 2120 Southern Stars for the epoch 1900 from observations made at the Hong Kong Observatory during the years 1898 to 1903*. Hong Kong, Noronha & Co. Pp.75.

Doberck, W., 1905d. Hong Kong Observatory. *Monthly Notices of the Royal Astronomical Society*, 65, 374-375.

Doberck, W., 1906a. On the orbit of τ Ophiuchi. *Astronomische Nachrichten*, 170, 101.

Doberck, W., 1906b. On the orbit of γ Centauri. *Astronomische Nachrichten*, 170, 107.

Doberck, W., 1906c. On the elements of the orbit of β 733 = 85 Pegasi. *Astronomische Nachrichten*, 172, 91.

Doberck, W., 1906d. On the elements of the orbit of 70 p Ophiuchi. *Astronomische Nachrichten*, 172, 161.

Doberck, W., 1907a. Hongkong double star observations. *Astronomische Nachrichten*, 173, 17-40.

Doberck, W., 1907b. On the orbits of ζ Cancri, ω Leonis and H I 39, *Astronomische Nachrichten*, 173, 241-262.

Doberck, W., 1907c. On the orbits of ξ Scorpii, Σ 2173, Σ 3121, and μ 2 Herculis. *Astronomische Nachrichten*, 174, 257-278.

Doberck, W., 1907d. Elements of the orbit of α Centauri. *Astronomische Nachrichten*, 175, 209-214.

Doberck, W., 1908a. Elements of the orbit of Gamma Virginis. *Astronomische Nachrichten*, 177, 161-172.

Doberck, W., 1908b. On θ Orionis, *Astronomische Nachrichten*. 178, 363.

Doberck, W., 1908c. On the accuracy of measures made by the principal double star observers. *Astronomische Nachrichten*, 177, 65-70.

Doberck, W., 1909a. Dr. Doberck's Observatory, Sutton, Surrey. *Monthly Notices Roy. Astron. Soc*., 69, 281-282.

Doberck, W., 1909b. On the orbit of ζ Cancri C. *Astronomische Nachrichten*, 179, 1-8.

Doberck, W., 1909c. On the orbit of 42 Comae Berenices = Σ 1728. *Astronomische Nachrichten*, 179, 55-60.

Doberck, W., 1909d. On the orbit of Eta Cassiopeiae. *Astronomische Nachrichten*, 179, 383-385.

Doberck, W., 1909e. On the orbit of Gamma Coronae Borealis. *Astronomische Nachrichten,* 179, 385.

Doberck, W., 1909f. Sir William Herschel's observations of distances between the components of close double stars. *Astronomische Nachrichten*, 180, 155-160.

Doberck, W., 1909g. On the orbit of Xi Bootis. *Astronomische Nachrichten*, 181, 19-22.

Doberck, W., 1909h. Sutton double star observations. *Astronomische Nachrichten*, 181, 97-132.

Doberck, W., 1909i. Elements of the orbit of 44i Bootis. *Astronomische Nachrichten*, 182, 27-32.

Doberck, W., 1909j. Double-star observations compared with orbits. *Astronomische Nachrichten*, 182, 87-94.

Doberck, W., 1909k. On the accuracy of measures made by some double-star observers. *Astronomische Nachrichten*, 182, 107-110.

Doberck, W., 1910a. On the orbit of Σ 1768. *Astronomische Nachrichten*, 183, 233-238.

Doberck, W., 1910b. Sutton double star observations. *Astronomische Nachrichten*, 184, 17-56.

Doberck, W., 1910c. Synopsis of elements of orbits of some double stars. *Astronomische Nachrichten*, 184, 131.

Doberck, W., 1910d. On the orbit of 40 O_2 Eridani. *Astrononomische Nachrichten*, 184, 133.

Doberck, W., 1911a. Sutton double star observations. *Astronomische Nachrichten*, 187, 305-336.

Doberck, W., 1911b. Sutton double star observations. *Astronomische Nachrichten*, 188, 317-330.

Doberck, W., 1911c. On the orbit of Σ 2525. *Astronomische Nachrichten*, 189, 41-44.

Doberck, W., 1911d. Sutton double star observations. *Astronomische Nachrichten*, 189, 297-312.

Doberck, W., 1912a. On the orbit of 4 Aquarii. *Astronomische Nachrichten*, 190, 199-202.

Doberck, W., 1912b. Observations of comets. *Astronomische Nachrichten*, 190, 221-224.

Doberck, W., 1912c. On the orbit of γ Coronae Australis. *Astronomische Nachrichten*, 191, 125-128.

Doberck, W., 1912d. Sutton double star observations. *Astronomische Nachrichten*, 191, 201-214.

Doberck, W., 1912e. Sutton double star observations. *Astronomische Nachrichten*, 192, 277-288.

Doberck, W., 1913a. Sutton double star observations. *Astronomische Nachrichten*, 193, 421-432.

Doberck, W., 1913b. On the orbit of β 101. *Astronomische Nachrichten*, 194, 301-304.

Doberck, W., 1913c. Sutton double star observations. *Astronomische Nachrichten*, 195, 145-158.

Doberck, W., 1914a. Sutton double star observations. *Astronomische Nachrichten*, 196, 253-268.

Doberck, W., 1914b. Sutton double star observations. *Astronomische Nachrichten*, 197, 169-182.

Doberck, W., 1914c. Sutton double star observations. *Astronomische Nachrichten*, 198, 329-344.

Doberck, W., 1915. On ring-micrometer observations of comets. *Astronomical Journal*, 29, 28-29.

Doberck, W., 1917. Observations of RW Cygni. *Astronomical Journal*, 31, 23-23.

Doberck, W., 1918a. Observations of variable stars. *Journal des Observateurs*, 2, 43-44.

Doberck, W., 1918b. Observations of variable stars. *Journal des Observateurs*, 2, 125-126.

Doberck, W., 1918c. Observations of Z Aurigae. *Journal des Observateurs*, 2, 165.

Doberck, W., 1919a. Observations of variable stars, *Journal des Observateurs*, 2, 208-10.

Doberck, W., 1919b. Observations of variable stars (suite). *Journal des Observateurs*, 3, 1-5.

Doberck, W., 1919c. Observations of variable stars (suite). *Journal des Observateurs*, 3, 105-108.

Doberck, W., 1919d. Observations on variable stars. *Astronomical Journal*, 32, 31-32.

Doberck, W., 1919e. Observations on variable stars. *Astronomical Journal*, 32, 73-80.

Doberck, W., 1919f. Observations of variable stars, *Astronomical Journal*, 32, 121-26.

Doberck, W., 1919g. Observations of variable stars, *Astronomical Journal*, 32, 164-65.

Doberck, W., 1920a. Observations on variable stars. *Astronomical Journal*, 32, 181-84.

Doberck, W., 1920b. Observations on variable stars, summary of results. *Astronomical Journal*, 32,185-191.

Doberck, W., 1920c. Observations of variable stars, *Astronomical Journal*, 33, 63-69.

Doberck, W., 1920d. Sutton double star Observations. *Astronomische Nachrichten*, 211, 321-356.

Doberck, W., 1921a. Sutton double star Observations. *Astronomische Nachrichten*, 212, 499-504.

Doberck, W., 1921b. On the orbit of xi Bootis. *Astronomische Nachrichten*, 214, 89-98.

Doberck, W., 1922. Letter to R.G. Aitken, dated 28 December. In Lick Observatory Archives.

Doberck, W., 1923. Sutton double star observations. *Astronomische Nachrichten*, 217, 97-118.

Doberck, W., 1924a. Observations of variable stars, *Astronomische Nachrichten*, 221, 305-312.

Doberck, W., 1924b. Observations of variable stars, *Astronomische Nachrichten*, 222, 273-280.

Doberck, W., 1924c. Observations of variable stars, *Journal des Observateurs*, 7, 33-35.

Doberck, W., 1924d. Observations of variable stars, *Journal des Observateurs*, 7, 64-67.

Doberck, W., 1924e. Observations of variable stars, *Journal des Observateurs*, 7, 85-89.

Doberck, W., 1925a. Letter to R.G. Aitken, dated 7 February. In Lick Observatory Archives.

Doberck, W., 1925b. Sutton double star observations. *Astronomische Nachrichten*, 224, 81-94.

Doberck, W., 1925c. Observations of variable stars. *Journal des Observateurs*, 8, 10-14.

Doberck, W., 1925c. Sutton double star observations. *Astronomische Nachrichten*, 225, 337-352.

Doberck, W., 1926. Sutton double star observations. *Astronomische Nachrichten*, 228, 401-416.

Doberck, W., 1930. Sutton double star observations. *Astronomische Nachrichten*, 239, 49-60.

Doberck, W., 1931. Sutton double star observations. *Astronomische Nachrichten*, 243, 121-132.

Doberck, W., 1933. Double star observations. *Astronomische Nachrichten*, 248, 187-194.

Doberck, W., 1935. Sutton double star observations. *Astronomische Nachrichten*, 254, 41-48.

Dommanget, J., 2003. The mass/eccentricity limit in double star astronomy. *Journal of Astrophysics and Astronomy*, 24, 99-109.

Dreyer, J.L.E., 1897. Albert Marth, 1828-1897. *Astronomische Nachrichten*, 144, 223.

Dyson, A., 1983a. *From Time Ball to Atomic Clock*. Hong Kong, Hong Kong Government Printers.

Dyson, A., 1983b. *From Time Ball to Atomic Clock*. Hong Kong, Hong Kong Government Printers. p. 29.

Governor's minute, 28 November 1887. In HKRS356 1-1(2).

Governor's minute, 17 February 1888. In HKRS356 1-1(2).

Governor's minute, 15 January 1907. In HKRS356 1-2(1).

Heintz, W.D., 1988. The Castor system. *Publications of the Astronomical Society of the Pacific*, 100, 834-838.

Higuchi, Jirō, 2002. *Henry Spencer Palmer, 1838-93*. In Cortazzi, H. (ed.), 2002. *Britain & Japan: Biographical Portraits*. *Volume 4*. London, Japan Library. Pp. 198-212.

Ho P.Y., 2003. *Weathering the Storm: Hong Kong Observatory and Social Development* **—風雲可測: 香港天文台與社會的變遷**. Hong Kong, Hong Kong University Press.

Hong Kong Government Gazette, 17 November 1877.

Hong Kong Government Gazette, 4 March 1882; with correction in the *Gazette* for 16 December 1882.

Hong Kong Government Gazette, 10 November 1883.

Hong Kong Government Gazette, 18 January 1890.

Hong Kong Government Gazette (Supplement), 22 March 1907.

Hong Kong Legislative Council: Sessional Papers, 1907. In No. 33/1907.

Hong Kong Observatory. Annual Report for 1890. In *Observations Made at the Hong Kong Observatory in the Year 1890*. Hong Kong, Hong Kong Observatory (1891).

Hong Kong Royal Observatory. *Annual Report for 1914*. Hong Kong, Government Printer (1915).

Hoskin, M., 1982. Archives of Dunsink and Markree Observatories. *Journal for the History of Astronomy*, 13, 146-52.

Hu, D., 2005. *China and Albert Einstein*. Cambridge (Mass.), Harvard University Press.

King, H.C., 1979. *The History of the Telescope*. New York, Dover.

Kronk, G.W., 1999. *Cometography: A Catalog of Comets*. *Volume 2*. Cambridge, Cambridge University Press.

MacKeown, P.K., 2004. William Doberck – a stormy career. *Journal of the Royal Asiatic Society Hong Kong Branch*, 44, 5-39.

McEwen, J.P., 1882. On a comet. *Nature*, 27, 247-48.

McKenna, S.M.P., 1967. Astronomy in Ireland from 1780. *Vistas in Astronomy*, 9, 283-296.

Marquess of Bristol, 1891. Letter to Lord Knutsford, dated 18 January. In CO129/247.

Nature, xxvii, 565 (1883).

Obituary: Henry Spencer Palmer. *Monthly Notices of the Royal Astronomical Society*, 54, 196-198 (1894).

Obituary: Colonel Edward H Cooper. *Monthly Notices of the Royal Astronomical Society*, 63, 197-98 (1903).

Obituary: John Louis Emil Dreyer. *Monthly Notices of the Royal Astronomical Society*, 87, 251-57 (1927).

Obituary: Dr. William Doberck. *Nature*, 147, 409 (1941a).

Obituary. *Publications of the Astronomical Society of the Pacific*, 53, 263-64 (1941b).

O'Rorke, T., n.d. [1878]. *History, Antiquities and Present State of the Parishes of Ballysadare and Kilvarnet in the County of Sligo with notices of the O'Haras, the Coopers, the Percivals and other local families*. Dublin, James Duffy & Sons.

O'Rorke, T., n.d. [1889]. *History of Sligo: Town ad County, Volume II*. Dublin, James Duffy & Co., (reprinted, Sligo, 1986).

Orchiston, W., 1999. C/1881 K1: A forgotten "Great Comet" of the nineteenth century. *Irish Astronomical Journal*, 26, 33-44.

Orchiston, W., 2004. The nineteenth century transits of Venus: an Australian and New Zealand overview. *Journal of Astronomical Data*, 10, 219-308.

Pigatto, L., 2004. Astronomical observations made by Jesuits in Peking during the seventeenth and eighteenth centuries. In Orchiston, W., Stephenson, R., Débarbat, S., and Nha, I.-S. (eds.). *Astronomical Instruments and Archives From the Asia-Pacific Region*. Seoul, IAU Commission 41. Pp. 55-66.

Robinson, T.R., 1852. Cited in Glass, I.S., 1997. *Victorian Telescope Makers*. Bristol, Institute of Physics, p.42.

Shi, Y., and Xing, G., 2006. The first Chinese version of the Newtonian tables of the Sun and Moon. In Chen, K.-Y., Orchiston, W., Soonthornthum, B., and Strom, R. (eds.). *Proceedings of the Fifth International Conference on Oriental Astronomy*. Chiang Mai, Chiang Mai University. Pp. 91-96.

Starbuck, L., 1951. *A Brief General History of the Royal Observatory, Containing Extracts from Contemporary Records, Annual Reports and Memorandum*. Hong Kong, Hong Kong Government Printer.

The Astronomer Royal, 1891. Letter to H.W. Just, dated 21 January. In CO129/247 p.378.

The Astronomical Register, i, 157 (1863).

'Veritas', *China Mail*, 16 October 1889.

Wright, D.C., 1993. Double star astronomy in the UK 1832-1932. *Quarterly Journal of the Royal Astronomical Society*, 34, 423-440.

Zhang, B., 1998. The introduction of European astronomical instruments and the technology related into China during the 17th century. In the *8th International Conference on the History of Science in China*. Berlin, Technische Universitaet Berlin (on-line paper).

Kevin Mackeown, a graduate of University College Dublin and the University of Durham, spent the bulk of his career researching cosmic rays and teaching physics for much of the past 35 years at The University of Hong Kong, where he remains as an honorary Professor. Post-retirement interests include looking into local history, with particular emphasis on the role of the physical sciences.

Journal of Astronomical History and Heritage, 10(1), 65-71 (2007).

LOWELL OBSERVATORY ENTERS THE TWENTIETH CENTURY—IN THE 1950S

Joseph S. Tenn
Department of Physics & Astronomy, Sonoma State University,
Rohnert Park, CA 94928, USA.
E-mail: joe.tenn@sonoma.edu

Abstract: By the 1950s the Lowell Observatory was stagnant. The three senior astronomers had been there for decades, and they were no longer doing much research or publishing. Yet they jealously guarded the telescopes and prevented younger colleagues from using them effectively. V.M. Slipher, Director since 1916, had been a very productive astronomer in his youth, when he was guided by founder Percival Lowell, but now he devoted his remaining energies to his many business interests. The Observatory's sole Trustee, a nephew of the founder, was busy with his business and politics in Massachusetts and slow to exert authority in Flagstaff, Arizona. Finally, after C.O. Lampland died and V.M. and E.C. Slipher were in their seventies, the Trustee decided that he had to make a change. He brought in mathematician Albert Wilson, who had been leading the Palomar Sky Survey for Caltech. One of Wilson's qualifications seems to be that he was acceptable to the Slipher brothers. Wilson started the Observatory on the road to modernity but ran into personal problems as well as difficulty managing Observatory personnel, and he resigned after a little more than two years. John Hall became Director in 1958, just as the American reaction to Sputnik made abundant Federal resources available to science. In his nineteen years as Director Hall completely revived the historic institution and brought it into the late twentieth century.

Keywords: Lowell Observatory, V.M. Slipher, Albert G. Wilson, John S. Hall

1 BACKGROUND: V.M. SLIPHER

V.M. Slipher[1] (Figure 1) took over the Lowell Observatory as Acting Director upon the death of its founder, Percival Lowell (1855–1916), in 1916, and became permanent Director with the settling of the Lowell estate in 1926.

In 1951 he was still there, now age 76. He had worked at the Observatory since receiving his B.S. from Indiana University half a century earlier. He had been an extremely productive scientist in his youth, especially when Lowell provided direction as well as financial support, but the long squabble over Lowell's will and the Depression had driven him to consider financial security more important than astronomy. He invested in rental properties and build up a business empire, devoting less and less time to research and publishing no original research after 1939. In fact, he published very little after 1933 if we discount the papers of Arthur Adel (1908–1994) which Adel insisted were his own work and not even understood by Slipher, but on which Adel felt he had to list the Observatory Director as co-author (Adel, 1987).

The other two senior astronomers were V.M.'s younger brother, E.C. (Figure 2), who was 68 in 1951, and C.O. Lampland (Figure 3), who was 78. While V.M. devoted most of his time to business, it was politics for E.C. Very active in local affairs, he served as City Councilman and Mayor of Flagstaff and in both houses of the Arizona legislature, spending months in Phoenix when the legislature was in session. Although he had taken an enormous number of photos of Mars, he had to be prodded by the Observatory's sole Trustee[2], Roger Lowell Putnam (1893–1972), to finally publish them in the 1960s.

Lampland was a scholar and a perfectionist who would have made a great librarian. In fact he did supervise the Observatory library and built up a very large personal library which he ultimately left to the Observatory. A pioneer in infrared research, he controlled the Observatory's largest telescope, a 42-inch reflector built by Alvan Clark for Lowell in 1909, but hardly ever found his results sufficiently perfect to publish.

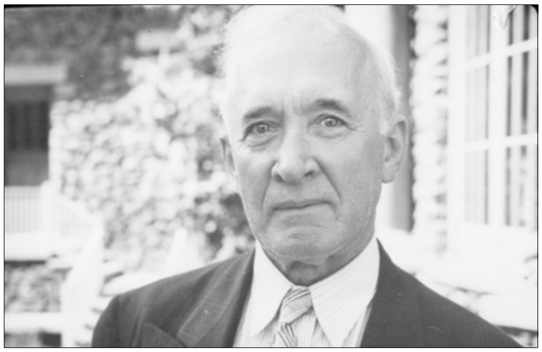


Figure 1: V.M. Slipher (1875–1969) was at the Lowell Observatory from 1901 to 1954 (Acting Director: 1916–1926; Director: 1926–1954) (Photograph courtesy Lowell Observatory Archives).

After World War II the Trustee persuaded the three old men to accept a few changes. The first Government grant—from the Weather Bureau, and later the Air Force, to monitor planetary atmospheres—was accepted, and one younger, more up-to-date astronomer, Harold L. Johnson (1921–1980), was hired in 1948. Henry Giclas (b. 1910), who had first worked at Lowell as a summer employee in 1931, was by now a full astronomer. He pursued research in photometry of the planets. Later, in 1957, he would begin an extensive proper motion survey, using the plates taken for Clyde Tombaugh's (1906–1997) search for planets for the first epoch. He also worked on the solar variation project, and took over much of the administrative burden from V.M., who could not be bothered with new-fangled things like social security.

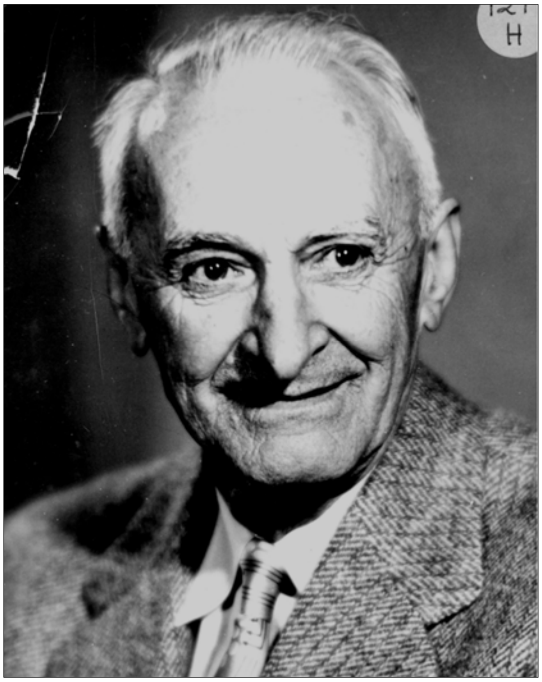


Figure 2: E.C. Slipher (1883–1964) was at the Lowell Observatory from 1906 to 1964 (Acting Director: 1957–1958) (Photograph courtesy Lowell Observatory Archives).

Figure 3: C.O. Lampland (1873–1951) was at the Lowell Observatory from 1902 to 1951 (Photograph courtesy Lowell Observatory Archives).

2 FIRST TRY: WHO WILL SUCCEED V.M.?

V.M. himself actually started considering a transition as early as March 1946 when he asked his good friend and confidant, John C. Duncan (1882–1967), for suggestions. Duncan, who had been the first Lawrence Fellow at Lowell in 1906 and had remained close to the Observatory astronomers, replied with a carefully constructed list of "Some of the Younger Astronomers of America, 1946 compiled with ages derived from 'American Men of Science'" (Duncan, 1946). Most were between 30 and 50.

> I have met nearly all the men listed and know some of them pretty well. Compared to the general run of humanity, they are an extremely fine lot, as might be expected of a list of astronomers. On the other hand, it is a bit difficult to see any one of them headed for the directorship of the Lowell Observatory, the position that you have held so long and so honorably … I believe that the two you mention, Whipple and Robley Williams[3] are both excellent.

Nothing seems to have come of this early correspondence, although there is a handwritten note, presumably from V.M., enclosed with this letter listing a smaller set of names with numbers next to them: 1 Whipple, 2 Williams, 3 Edmondson, 4 Dunham, 5 Babcock, 6 Hall, 7 Mohler, 8 Weaver, 9 Seyfert, 10 Herbig, and not numbered, Elvey.

In 1952 the Trustee received a letter from Bart Bok (1906–1983), who was very unhappy with the changes at Harvard and in open rebellion against plans to close or sell Harvard's South African station, asking to be considered as a successor to V.M. "… if the time for his replacement should arrive." (Bok, 1952). By some coincidence, Putnam had received a letter a little earlier from Bok's ally, Harlow Shapley, (1885–1972) hinting that the reorganization of Harvard "… may change things in such a way that a first-class local astronomer would be available for serious consideration for the top post at the Lowell Observatory." (Shapley, 1952).

Harold L. Johnson, who had abruptly resigned from Lowell in 1949 and now had a good position at the Yerkes Observatory, wrote Roger Putnam in 1950 asking to return (Johnson, 1950). He had tried the Director first, but V.M. was not encouraging. Johnson was rehired in May 1952 by the Trustee. He brought with him a contract with the Office of Naval Research on solar variations. He found the old 42-inch reflector to be in very poor shape and inadequate for his work.

Johnson soon became quite unhappy with the old men running the place, who were not appreciative of electronics and felt astronomers should make do with whatever equipment was at hand. Soon after returning, he wrote the Trustee:

> I have found the Lowell Observatory to be very different from the Yerkes Observatory in at least one respect. I have found the scientific atmosphere here to be extremely deadening. No one here now has much interest in the problems of modern Astronomy and Astrophysics, and I miss very much the stimulating atmosphere of the Yerkes Observatory. (Johnson, 1952a).

He continued by asking for the hiring of another photoelectric photometrist, Daniel L. Harris, III (1919–1962), as "It would be very much nicer here if there were someone else who talks my language." The following month he added:

> The point of all this is simply that it is not possible to have young and ambitious new men working here under the present administration. Intellectually and scientifically, the Lowell Observatory is defunct. Whatever these men have done in the long past (and we both know they have done good work), their total contribution now is to keep the Observatory 20 or 30 years behind modern developments in Astronomy. Before the Lowell Observatory can take its rightful place in the Astronomical world, it will be necessary to replace all of the deadwood with first rate men. The sooner this takes place, the better for the Observatory. (Johnson, 1952b).

It appears that arguments like this from the most productive member of the staff, along with the death of Lampland in December 1951, persuaded the Trustee that change had to come. John Duncan continued to keep a lookout for bright young men and was impressed by Albert G. Wilson (Figure 4) while visiting Palomar Observatory. Wilson, who had earned his Bachelor's degree in electrical engineering at Rice University and his Ph.D. in mathematics at Caltech, had returned to Caltech after serving in the Navy, and was then supervising observations for the Palomar Sky Survey.

After receiving an inquiring letter from Wilson, V.M. Slipher (1952) wrote the Trustee:

> He is a younger man than we have been thinking and talking about, 33, I believe. He has a family of a wife and three children, and apparently of a stable temperament. He is product of Cal.Tech. and except for war service has been there and at Palomar since. "Our Universe Unfolds New Wonders" by him is an account of observations he has made in the sky survey at Palomar with the giant Schmidt, published in 1952 February number of the *National Geographic Magazine*, which I hope you may have a chance to glance over. Dr. Duncan knows him quite well and speaks highly of him (It seems that we have had to give up the hope of finding a little older man who has shown interest and ability more in the planetary sphere. There is only or two of these and they would be much more expensive if we could get them interested coming to Lowell Observatory.) We are hoping he will come here for a discussion of matters before very long. Would be glad to have your thoughts on him individually and whether you agree that his age is no objection if has other qualifications. He seems to be very much the most encouraging prospect at present. He is not much younger than was Shapley when he went to Harvard.

After that a brief visit by Wilson to Flagstaff and a couple of letters between the candidate and the Trustee were all it took. It appears that Putnam, who had been Trustee for more than a quarter of a century but had never hired a Director, did not spend much time thinking about the matter. He was very busy with his business affairs in Massachusetts, including starting a television broadcasting business, and had just spent a year as the Director of the Economic Stabilization Administration in Washington.

On 8 January 1953 the Trustee formally appointed Wilson Assistant Director, effective 1 July, at a salary of $6,000 per year plus the house then occupied by Mrs. Lampland. Wilson was told (Putnam, 1953), "I hope and believe, as time goes on, we will see very real progress with the Observatory, and of course, as opportunities increase, remuneration should also." Wilson had been informed during his visit to Flagstaff that he could expect to move up to Director after a year if all went well.

There was much correspondence between the two even before Wilson moved to Flagstaff. For example, in February Wilson was involved in negotiations to obtain a contract from the Office of Scientific Research. He wrote the Trustee (Wilson, 1953): "…we must negotiate with OSR as though we had the research talent in our pocket, and we must negotiate with the talent as though we had the contract in our pocket …" and asked whether it would be possible to hire some talent immediately. He wanted to get Donald E. Osterbrock (1924–2007), whom he praised highly and about whom he assured the trustee: "We know he would be willing to come to Lowell on a one year trial basis, with opportunity for a permanent staff position at the end of that time, if all parties are satisfied … He would like $4500." Wilson also wanted to hire Robert H. Hardie (1923–1989) for a 6-month Fellowship to help Harold Johnson with photometry. Putnam agreed to Hardie, as a 6-month commitment could be afforded even if the contract were not won.

Figure 4: Albert G. Wilson (b. 1918) was at the Lowell Observatory from 1953 to 1957 (Assistant Director: 1953–1954; Director: 1954–1957 (Photograph courtesy Lowell Observatory Archives).

3 THE SHORT, UNHAPPY DIRECTORSHIP OF AL WILSON

As expected, Wilson became the Observatory's third permanent Director on 11 November 1954. When I asked him recently what he considered his greatest accomplishments as Director, he gave me essentially the same list he gave current Trustee William Lowell Putnam in a letter in 1990 (Putnam, 1994: 204-205). First on his list was the establishment of a retirement system for the astronomers. He did not want any future Directors to hang on until age 79 because of a lack of a pension.

Wilson hired a few young astronomers, among them Gerard de Vaucouleurs (1918–1995) and William Sinton (1925–2004), got the 42-inch telescope mirror realuminized, and organized the international Mars committee to coordinate observations during the 1956 opposition after getting the National Geographic Society to support E.C. Slipher's observations of the 1954 opposition from Pretoria. He held the first conference on exo-biology, and he hosted a meeting of the Astronomical Society of the Pacific. He is also proud of working with the Walt Disney Company in filming movies about Mars. He worked hard on attempts to get the forthcoming national observatory built near Flagstaff, hoping that Lowell could play some sort of host role, but eventually it went to Kitt Peak more than 400 km to the south. He also spent some effort getting the new image intensifiers, originated for medical use at Johns Hopkins, modified for astronomical use. When the Bendix Corporation took over the patents and development, Wilson worked to make Lowell a test facility for astronomical use of the devices.

Wilson has told me repeatedly that John Hall thanked him later for doing much of the necessary 'dirty work' which antagonized the staff but made it easier for his successor. Partly because of this his tenure as Director of the Lowell Observatory was short and unhappy.

Wilson fired Robert Hardie, who denounced him widely. Actually, the Trustee had told Wilson to reduce the photoelectric staff in order to increase the number of people doing planetary work, which had been declared the Observatory's primary mission by the founder, Putnam's uncle Percival Lowell.

By 1956 Wilson had severe problems in dealing with some of the staff. Harold Johnson, who had strenuously urged the Trustee to get rid of the deadwood during the last of the Slipher years, became extremely critical and wrote vituperative letters to Putnam (Johnson, 1956a) accusing Wilson of lacking ability to lead, knowledge of science, and even mental stability. Henry Giclas also became an enemy, and Wilson at one time discussed trying to fire him.

A year earlier Wilson (1955) had written the trustee:

> There will be a period of being tough. But we suffer from some deeply entrenched inefficiency. A completely new broom must be used for the sweeping. I, nor anyone else, could not get the Lowell Observatory on a productive basis with the existing set up. I tried for 7 months to sell my program, win them over, but all I got was some rather contemptible back stabbing. Now the program goes on whether they like it or not, and if they continue to drag their feet they will have to go.

In the same letter he pointed out that some staff members had been helpful and cooperative, among them E.C. Slipher and, amazingly considering later developments, Harold Johnson.

There is some evidence that Wilson had tried to get along with his staff. Shortly before becoming Director Wilson followed up a visit by the Trustee to the Observatory by writing Putnam:

> I know our group. They are all talented men. They are all competent scientists. Yet it takes a certain minimum of time for men to know and appreciate one another, and to learn to work together. We must now work toward creating an effective team, erasing prejudices and pettiness. The observatory is not only what we see on Mars Hill, it is also within us—especially the future. And what is really within is confidence, enthusiasm, and eagerness to be on our way after a tired period of uncertainty. Our first job, working together, is to release these human forces, assuring each man rightful use of his talents, and the opportunity to be and produce his best. Faith that this can be done is a sine qua non. (Wilson, 1954).

But by 1956 the situation was irreparable. Wilson found himself under constant attack and his marriage was breaking up, so on 9 November 1956 he asked the Trustee to accept his resignation effective not later than 1 July 1957. Apparently conditions continued to worsen, as he formally resigned in a letter of 31 December, effective 3 January 1957, and in June he returned to California and a career in industry. Although this was his last full-time position in astronomy, he served as the founding editor of *Icarus* in 1962, and he published on cosmology and general relativity in the 1960s.

4 SECOND TRY

When Roger Putnam received Wilson's letter of resignation he appointed E.C. Slipher Acting Director. The last of the old men served from January 1957 to September 1958.

That day the Trustee wrote to Harold Johnson:

> While in Flagstaff, I talked on the telephone with Dr. Bowen at Mt. Wilson, Dr. Shane at Lick, and Otto Struve at Berkeley, asking advice and suggestions from them which they are going to give me in the next few days, about suitable men to replace Dr. Wilson as Director. I felt I couldn't get better advice than theirs, and I already have the advice from Harvard. After I receive the advice from all these people, I shall make up my own mind, and plan then to pick a Director, myself. (Putnam, 1957).

Struve (1897–1963) replied immediately with a detailed letter including a paragraph about each of the 14 men he listed in rank order. Struve's (1956) list (with his ages, not necessarily correct) was as follows:

1. Olin J. Eggen, age 38.
2. Frank Edmondson "undoubtedly the best man on the list in so far as administrative ability is concerned."
3. John S. Hall, age 49
4. Dean B. McLaughlin, age 56
5. Daniel Harris, age about 37.
6. Harold F. Weaver, age 39.
7. Carl Seyfert, age 46
8. Arthur Adel, age 48
9. Allen [sic] Sandage, age about 32.
10. Arthur Code
11. John [sic] Leighton
12. Lawrence H. Aller, age 43
13. Bradshaw Wood
14. Merle Walker, age 30

Ira S. Bowen (1898–1973) and C. Donald Shane (1895–1983) probably replied by telephone.

Putnam's reference to Harvard is interesting. The new Harvard Director, Donald H. Menzel

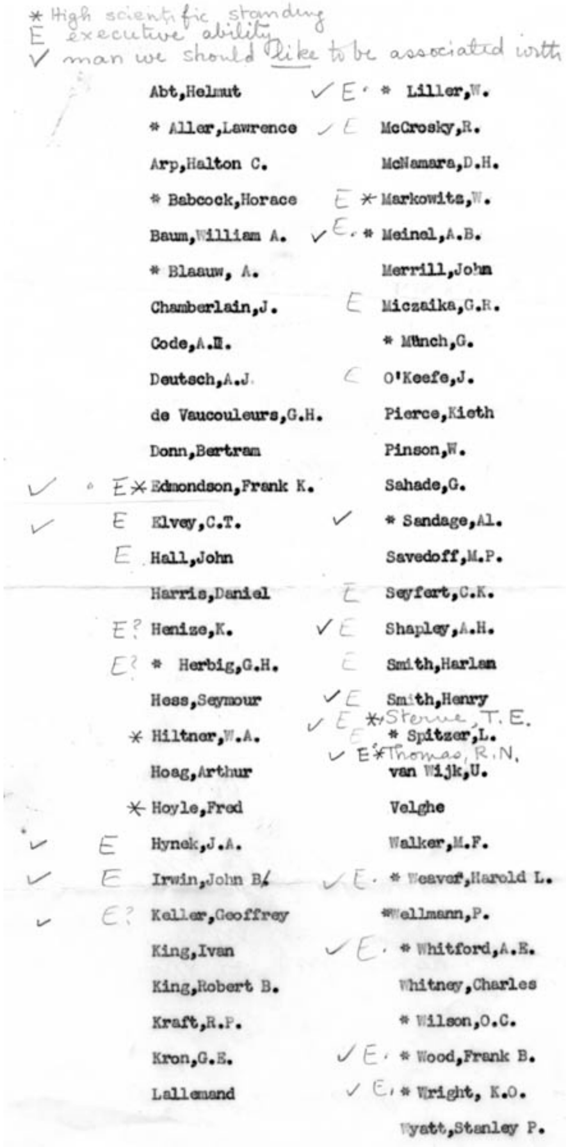
(1901–1976), having just established a relationship with the Smithsonian Astrophysical Observatory, which had moved from the nation's capital to Cambridge, Massachusetts, tried to include Lowell in a three-institution partnership. He offered to move Harvard's 61-inch telescope to Flagstaff, but in return he wanted a dominant say in who would be the next Director of Lowell. Menzel proposed an arrangement whereby Lowell astronomers would be Research Associates of the Harvard College Observatory and the new Lowell Director would hold the title of Professor at Harvard. The new Director would be selected by a committee of four, three of them chosen by Harvard and the Smithsonian Astrophysical Observatory, and would then have to be approved by both the Lowell Trustee and Harvard's Dean of Arts and Sciences.

Menzel sent Putnam a list of 60 astronomers considered by those at Harvard for the Directorship of Lowell. Some were marked with an asterisk for high scientific standing, some with an *E* for executive ability, and some with a check mark for "man we should like to be associated with." (Figure 5). Only nine had all three marks: Frank K. Edmondson (b. 1912), W. Liller (b. 1927), A.B. Meinel (b. 1922), T.E. Sterne (1907–1970), R.N. Thomas (b. 1921), Harold L. Weaver, A.E. Whitford (1905–2002), Frank B. Wood (1915–1997) and K.O. Wright (1911–2002). Of these Edmondson was already a Director at Indiana University, Liller eventually became Director of his own observatory in Chile, Meinel was the founding Director of Kitt Peak National Observatory, Whitford became Director of Lick Observatory, and Wright became Director of the Dominion Astrophysical Observatory. It is likely that the reference to Weaver was intended to refer to Harold F. Weaver (b. 1917), who was the founding Director of the Radio Astronomy Laboratory at the University of California at Berkeley.

After meeting with Menzel at Harvard, Putnam (1956) was at first amenable, asking V.M. Slipher to suggest an East Coast astronomer whom he could appoint as his representative to the four-man Nominating Committee, which would meet in Cambridge. However, after consulting with Harold Johnson, who replied, "… I am very much opposed to our 'buying' the 61-inch at the cost of accepting the Harvard Department's orders on policy and on the choice of the Director of the Lowell Observatory." (Johnson, 1956b), Putnam decided to choose his own Director first and then let the new Director carry on any negotiations with Harvard.

Putnam soon offered the Directorship to Frank Edmondson, who declined after some thought—and successful use of the offer to gain some concessions from his administration at Indiana University (Edmondson, 1957).

Putnam and the Lowell astronomers had become well acquainted over the past few years with John S. Hall (Figure 6), the Director of the United States Naval Observatory's Division of Equatorial Instruments (renamed the Astrometry and Astrophysics Division when he left). Hall was a leading photoelectric photometrist and spectroscopist with several major discoveries to his credit. He had initiated the drive to move the USNO's 40-inch Ritchey-Chretian (the first such telescope ever built) from its wretched site in Washington to a location with good seeing and dark skies. After some searching he had chosen a site near Flagstaff, and built an observatory there with Arthur Hoag (1921–1999) the on-site Director. He made many visits to the area to observe, and often visited with the other Flagstaff astronomers.



* High scientific standing
E executive ability
✓ man we should like to be associated with

Abt,Helmut
* Aller,Lawrence
Arp,Halton C.
* Babcock,Horace
Baum,William A.
* Blaauw, A.
Chamberlain,J.
Code,A.D.
Deutsch,A.J
de Vaucouleurs,G.H.
Donn,Bertram
✓ E* Edmondson,Frank K.
✓ E Elvey,C.T.
E Hall,John
Harris,Daniel
E? Henize,K.
E? * Herbig,G.H.
Hess,Seymour
* Hiltner,W.A.
Hoag,Arthur
* Hoyle,Fred
✓ E Hynek,J.A.
✓ E Irwin,John B.
✓ E? Keller,Geoffrey
King,Ivan
King,Robert B.
Kraft,R.P.
Kron,G.E.
Lallemand

✓ E. * Liller,W.
✓ E McCrosky,R.
McNamara,D.H.
E * Markowitz,W.
✓ E. * Meinel,A.B.
Merrill,John
E Miczaika,G.R.
* Münch,G.
E O'Keefe,J.
Pierce,Kieth
Pinson,W.
Sahade,G.
✓ * Sandage,Al.
Savedoff,M.P.
E Seyfert,C.K.
✓ E Shapley,A.H.
E Smith,Harlan
✓ E Smith,Henry
✓ E *Sterne, T.E.
E * Spitzer,L.
✓ E*Thomas, R.N.
van Wijk,U.
Velghe
Walker,M.F.
✓ E. * Weaver,Harold L.
*Wellmann,P.
✓ E. * Whitford,A.E.
Whitney,Charles
* Wilson,O.C.
✓ E. * Wood,Frank B.
✓ E. * Wright, K.O.
Wyatt,Stanley P.

Figure 5: Harvard's 1956 list of potential Lowell Observatory Directors (enclosed with Putnam, 1956).

Roger Putnam invited the Halls to an overnight visit at his home in Massachusetts, there was compatibility and mutual respect (Hall, like Putnam, was a New Englander), and John Hall was offered the position of Lowell Observatory Director (Putnam, 1958). His starting salary was $14,500 per year. According to his son, he almost accepted a position at the new Kitt Peak National Observatory instead, but was dissuaded by the length of the drive from Tucson to the telescopes. After some negotiations Hall (1958) accepted the Lowell Directorship, and the Observatory's deep problems were on their way to being overcome.

Since Hall wanted to finish some projects at USNO, the effective date of his appointment was put off to 1 September 1958.

Figure 6: John S. Hall (1908–1991) was at the Lowell Observatory from 1958 to 1977 (Director: 1958–1977) (Photograph courtesy Lowell Observatory Archives).

5 JOHN HALL RESCUES LOWELL OBSERVATORY

By all accounts Hall's Directorship was a total success. Not only did he stay 19 years, but he brought an open management style, leadership by example—he was a very productive scientist—and a warm personal relationship with the staff.

He came at the right time. While the Lowell Observatory had been starved for funds from the founder's death in 1916 until Mrs. Lowell's passing in 1954 (she had been receiving half the income from the estate), after Sputnik Federal funds began to flow into science in a big way. During Hall's Directorship, grants and contracts went from a tiny portion of the Observatory's budget to a very significant portion.

Hall rebuilt the infrastructure of the antiquated Observatory, adding or greatly improving the machine shop and electronics shop and buying computers as they became available. He formed a partnership not with Harvard but with Ohio State and Ohio Wesleyan Universities, whereby the 69-inch Perkins telescope was moved to Lowell, and he rebuilt it so that it became a modern 72-inch with a Zerodur mirror. He established a new, dark site at Anderson Mesa, 25 km from Flagstaff, and installed the Perkins and other new telescopes there.

Hall hired young astronomers to do photometry and interferometric spectrometry with new equipment. He brought visitors to the Observatory, including a number from Europe on short appointments. Perhaps the most significant work done at Lowell during his tenure was by Carnegie Institution of Washington astronomers W. Kent Ford, Jr. (b. 1931) and Vera Rubin (b. 1928), who measured rotation curves of galaxies with their new image tubes on a Lowell telescope.

A comparison between one of the last years of the Slipher Directorship with one twenty years later is made in Table 1. The number of astronomers was up by 40%, their median age had decreased by 42%, and one measure of their productivity—publications per astronomer per year—was up by 230%.

6 ACKNOWLEDGEMENTS

The author is grateful to Antoinette Beiser, Archivist of the Lowell Observatory, for much assistance and for permission to publish photographs and quotations from letters in the Lowell Observatory Archives (LOA). He is also grateful to Peter Boyce, Otto Franz, Henry Giclas, Richard Hall, Robert Millis, and Albert G. Wilson for sharing their recollections of Lowell Observatory in the 1950s and afterward. This paper is an elaboration of a talk given in a special session on "Case Studies in How 20th Century Observatory Directors Got Chosen" at a meeting of the Historical Astronomy Division of the American Astronomical Society on 7 January 2007. The session was organized by David H. DeVorkin of the National Air and Space Museum, Smithsonian Institution, Washington, DC.

7 NOTES

1. No one at Lowell Observatory (Figure 7) referred to the three senior astronomers by their names. Vesto Melvin Slipher, Earl Carl Slipher, and Carl Otto Lampland were always referred to and addressed by their first two initials (Henry Giclas, personal communication).
2. Percival Lowell established his Observatory with all authority vested in a sole Trustee. To date all Trustees have been relatives of the founder (Putnam, 1994).

Table 1: Lowell Observatory in 1950 and 1970.

Year	1950	1970
Director	V.M. Slipher, 75, 49 years at Lowell Observatory, 34 as Director	John S. Hall, 52, 12 years at Lowell Observatory, 12 as Director
Other Astronomers	C.O. Lampland, 77, 48 years	Henry L. Giclas, 60, 39 years
	E.C. Slipher, 67, 44 years	Peter Boyce, 34, 7 years
	Henry L. Giclas, 40, 19 years	William A. Baum, 46, 5 years
	Harold L. Johnson, 29, 1st year	Otto G. Franz, 39, 5 years
		Robert L. Millis, 29, 3 years
		Nathaniel M. White, 29, 1 year
Totals:	5 astronomers, 3 publications	7 astronomers, 14 publications

3. Robley Cook Williams (1908–1995) was an Associate Professor of Physics at the University of Michigan at the time. In 1950 he completed a gradual transition from astronomy to physics to biophysics and became a Professor of Virology at the University of California at Berkeley (Anonymous, 2006).

8 REFERENCES

The following abbreviation is used:

LOA = Lowell Observatory Archives, Lowell Observatory, Flagstaff, AZ, USA.

Abrahams, Peter, ~2001. "The Telescopes of Lowell Observatory." Available at http://home.europa.com/~telscope/ lowell.txt.

Adel, A., 1987. Oral history interview by Robert W. Smith, dated 13 August, American Institute of Physics Center for History of Physics.

Anonymous, 2006. Biophysicists in Profile: Robley Cook Williams, *Biophysical Society Newsletter,* January 2006, 2–3. See: http://www.biophysics.org/publications/jan06.pdf.

Bok, B.J., 1952. Letter to R.L. Putnam, dated 22 July. In LOA.

Duncan, J.C., 1946. Letter to V.M. Slipher, dated 23 March. In LOA.

Edmondson, F.K., 1957. Letter to R.L. Putnam, dated 24 January. In LOA.

Hall, J.S., 1958. Letter to R.L. Putnam, dated 17 February. In LOA.

Johnson, H.L., 1950. Letter to R.L. Putnam, dated "March." In LOA.

Johnson, H.L., 1952a. Letter to R.L. Putnam, dated 19 September. In LOA.

Johnson, H.L., 1952b. Letter to R.L. Putnam, dated 14 October. In LOA.

Johnson, H.L., 1956a. Letter to R.L. Putnam, dated 24 September. In LOA.

Johnson, H.L., 1956b. Letter to R.L. Putnam, dated 15 December. In LOA.

Millis, R.L., 1992. John Scoville Hall, 1908–91. *Bulletin of the American Astronomical Society*, 24, 1323-25.

Putnam, R.L., 1953. Letter to A.G. Wilson, dated 8 January. In LOA.

Putnam, R.L., 1956. Letter to V.M. Slipher, dated 26 November. In LOA.

Putnam, R.L., 1957. Letter to J.C. Duncan, dated 3 January. In LOA.

Putnam, R.L., 1958. Letter to J.S. Hall, dated 10 February. In LOA.

Putnam, W.L., III, 1994. *The Explorers of Mars Hill: A Centennial History of Lowell Observatory, 1894–1994*. West Kennebunk (Maine), Phoenix Publishing.

Shapley, H., 1952. Letter to R.L. Putnam, dated 5 May. In LOA.

Slipher, V.M., 1952. Letter to R.L. Putnam, dated 7 August. In LOA.

Struve, O., 1956. Letter to R.L. Putnam, dated 28 December. In LOA.

Wilson, A.G., 1953. Letter to R.L. Putnam, dated 16 February. In LOA.

Wilson, A.G., 1954. Letter to R.L. Putnam, dated 13 August. In LOA.

Wilson, A.G., 1955. Letter to R.L. Putnam, dated 8 July. In LOA.

Joseph S. Tenn has been a Professor of Physics and Astronomy at Sonoma State University in the California wine country since 1970. During this time his interests have evolved from physics to include astronomy and history of astronomy, although he teaches only the first two of these subjects. He maintains the Bruce Medalists website at http://phys-astro.sonoma.edu/brucemedalists/.

Figure 7: A recent view of the Slipher Building at the Lowell Observatory (photograph: Joseph S. Tenn).

Journal of Astronomical History and Heritage, 10(1), 72 (2007).

IAU ARCHIVES WORKING GROUP. TRIENNIAL REPORT (2003–2006)

1 Introduction

The Organizing Committee of the Working Group 2003–2006 consisted of the following members: Brenda Corbin (USA: Chair), Ileana Chinnici (Italy), Suzanne Débarbat (France), Wolfgang R. Dick (Germany), Daniel Green (USA), Wayne Orchiston (Australia) and Adam Perkins (UK).

2 The Years 2003–2005

Earlier reports on the WG's activities appeared in the *Journal of Astronomical History and Heritage*, Volume 7 No. 1 (June 2004), pages 61-63, and in the *ICHA Newsletter*, No. 8 (April 2006) as Section H on pp. 15-16.

An important event in 2005 for the Archives WG was the publication of the excellent collection of papers in *Astronomical Heritages*. The editors, Christiaan Sterken and Hilmar Duerbeck, state in the preface: "These Proceedings contain a selection of presentations and research papers emanating from the meetings of the *Astronomical Archives* and *Transits of Venus* Working Groups of Commission 41, and from presentations at the last three IAU General Assemblies. Some additional reports related to the topic of this book have also been added." There are 18 papers, 13 relating to archives and 5 on the transits of Venus. Originally published in *The Journal of Astronomical Data*, Vol. 10 Pt. 7 (2004), the papers were reprinted in book form in 2005 (ISBN 9080553867). The WG expresses deep appreciation to the editors for making these papers more widely available via the Journal issue and the book. It is hoped that *Astronomical Heritages* will become a part of every astronomical library's collection. For availability of this volume please contact C. Sterken (csterken@vub.ac.be). Following is a listing of the published papers.

Part I: IAU Archives Meetings:

Ansari, S.M. Razaullah, "Astronomical archives in India"

Corbin, B.G., "Archives at the U.S. Naval Observatory – recent projects"

Débarbat, S. and Bobis, L., "The French Astronomical Archives Alidade Project"

Dick, W.R., "Documents related to astronomy in German archives"

Herrmann, D.B., "The Sound Archive of Archenhold Observatory – an overview"

Moran, K. and Brück, M.T., "The Crawford Collection at the Royal Observatory Edinburgh"

Nakamura, T., "The Japanese Astronomical Archives Project"

Orchiston, W., "An introduction to the astronomical archives of Australia and New Zealand"

Orchiston, W., "Highlighting the history of nineteenth century Australian astronomy: The Tebbutt Collection in the Mitchell Library, Sydney"

Pigatto, L, Salmaso, M., and Zanini, V., "The Lorenzoni-Tacchini correspondence at Padova Observatory Archives – the "true" history of Italian astronomy of the second half of the nineteenth century"

Simonia, I., "Old Georgian astronomical manuscripts"

Stavinschi, M., and Mioc, V., "Storing astronomical information on the Romanian territory"

Wilkins, G.A., "The archives of the Norman Lockyer Observatory"

Part II: Historical Venus Transits:

Botez, E., "Maximilian Hell and the northernmost transit of Venus expedition of 1769"

Kopper, M., "Austria's contributions to the observation of the 1874 transit of Venus"

Misch, A., and Sheehan, W., "A remarkable series of plates of the 1882 transit of Venus"

Orchiston, W., "The nineteenth century transits of Venus: an Australian and New Zealand overview"

Sterken, C., Duerbeck, H.W., Cuypers, J., and Langenaken, H., "Jean-Charles Houzeau and the 1882 Belgian transit expeditions"

3 The Years 2005–2006:

Much of the WG's activities involved planning for the WG Archives sessions at the IAU General Assembly in Prague, August 2006. Two sessions of oral papers were presented, several poster papers were shown, and a business meeting was held. Both oral sessions were well-attended, and a lively question and comment period followed each paper as time allowed.

The full abstracts of all papers in each session can be seen in the program of the Commission's activities at the XXVIth IAU General Assembly in Prague (PDF format) at the following link: http://www.le.ac.uk/has/c41/

We are grateful to Professor Clive Ruggles for his careful preparation of the Program Booklet which was made available to all IAU GA participants.

The meeting featured two keynote papers:

"Historical archives in Italian astronomical observatories: the "Specola 2000" Project" by Ileana Chinnici, (INAF-Osservatorio Astronomico di Palermo G.S. Vaiana), Agnese Mandrino (INAF-Osservatorio Astronomico di Brera), and Fabrizio Bònoli (Università di Bologna, Dipartimento di Astronomia). This paper has been published in the *Journal of Astronomical History and Heritage* (Vol. 9, Pt. 2, pp. 200-202, 2006).

"A case of archival theft: the retrieval of the Greenwich Observatory Neptune Papers" by Adam J. Perkins (Curator of Scientific Manuscripts, Department of Manuscripts and University Archives, University Library, West Road, Cambridge CB3 9DR, United Kingdom). This paper will appear in the *Journal for the History of Astronomy*.

4 Prague GA Business Meeting

At the business meeting in Prague, Dr Ileana Chinnici was selected as Chair for the next triennium. Dr Wolfgang Dick indicated that he was not able to serve for another term and the WG thanked him for his many contributions to the WG and to C41 in general. The WG welcomed Dr Irakli Simonia to the WG Committee for the next triennium. The members for 2006-2009 are: Ileana Chinnici (Italy: Chair), Brenda Corbin (USA), Suzanne Débarbat (France), Daniel Green (USA), Wayne Orchiston (Australia), Adam Perkins (UK), and Irakli Simonia (Georgia). The Chair wishes to thank all WG members for their contributions and cooperation during 2003-2006.

Brenda G. Corbin, Chair, 2003–2006

Journal of Astronomical History and Heritage, 10(1), 73-75 (2007).

BOOK REVIEWS

***Kommandosache "Sonnengott". Geschichte der deutschen Sonnenforschung im Dritten Reich und unter alliierter Besatzung*, by Michael P. Seiler (Frankfurt/Main, Verlag Harri Deutsch, 2007; Acta Historica Astronomiae Volume 31), pp. 246, ISBN 978-3-8171-1797-0, €22.80.**

Michael Seiler's book contains 230 literature references and 661 footnotes. These form an impressive basis for a comprehensive presentation of the development of German solar physics during World War II, with special emphasis on solar-terrestrial relations. Two eminent scientists, Hans Plendl and Karl-Otto Kiepenheuer, worked closely together to build up a network of solar observation stations for the investigation of a possible connection between solar activity and ionospheric disturbances.

The author presents and summarizes an excellent collection of widely-spread documents, and thus provides good insight into how and why the German Luftwaffe was so interested in installing a network of solar observing facilities. These connections are systematically described in the thirteen chapters of this book, and they are also evaluated from today's viewpoint.

The individual chapters: 1. Prologue: Fundamentals of solar-terrestrial physics; 2. On the initial position of solar-terrestrial physics in Germany in 1939; 3. Origins of military use of solar-terrestrial physics in the Third Reich; 4. "Blitzkrieg": foundation of the first solar observatories of the Luftwaffe; 5. Sun, blood, and soil: occupation, seizure and cooperation in Europe; 6. Total war and mobilization of physics: Office of the Reich for high-frequency research; 7. The role of the Sun is over: "Zermürbungskrieg" and end of war; 8. The military orientation of research 1939–1945, 9. The military benefit of solar terrestrial physics; 10. Collaborations: scientists and Nationalsozialismus; 11. A comparison with the allied efforts 1939–1945; 12. New start and continuity 1945–1949; 13. Epilogue: Persons and institutions after 1949.

Upon reading these titles it is obvious that the book does more than describe the build-up of the individual facilities. Instead, all these activities are put into a larger context and they provide an overview of the relations and connections between the Luftwaffe and solar-terrestrial physics. This view of the historical context is most valuable, since there have already been presentations that concentrate on individual aspects and therefore were not well suited to recognizing the global picture.

The author provides a detailed introduction with information about the source material, especially about those documents that only recently became available and therefore were not used in earlier investigations. This leads to somewhat different conclusions, at least in some cases. The author also points out that documents most probably were destroyed close to the end of the war. A complete picture therefore cannot be established, although some witnesses are still alive and could be interviewed.

The author writes about the motivation for this research: "If the prediction of ionospheric conditions was the justification for the enormous investments in solar-terrestrial physics, what was then the contribution of this research for the radio consultations of the Wehrmacht and what was the real military benefit of the radio consultations for the conduction of the war activities in the Third Reich?"

The ionosphere is located several hundred kilometers above the Earth's surface, it has several layers, and is influenced by solar radiation, especially the ultraviolet wavelengths. The concentration of electrons in the ionosphere depends on the time of day, on the season and on the 11-year activity cycle of the Sun. There are also variations correlated with solar rotation, and solar energetic events may lead to significant changes in the ionosphere.

The state of the ionosphere is very important for radio communication. For long-range communication, the short-wave bands are especially well suited. Frequencies below 30 MHz are reflected in the upper layers of the ionosphere, while higher frequencies penetrate the entire atmosphere. These higher frequencies are nowadays used for radio astronomy and for satellite communication. The frequency limit between reflection and transmittance is variable and depends on the time and the season. Lower layers of the ionosphere also influence the use of radio communication and are subject to disturbances by the varying solar radiation. Solar flares may indeed lead to a complete loss of short-wave communications for hours at a time (Mögel-Dellinger-Effekt). The statistical relations between the influence of the ionosphere on radio communication on one hand and the variability of solar UV radiation on the other were known in the thirties, but not well understood. At this time, Germany did not pay much attention to solar-terrestrial research.

This changed drastically during World War II, especially after German troops occupied large parts of Europe and the air force operated even beyond that area. These circumstances increased the importance of long-range radio communication, and its unreliability due to the variable solar radiation was recognized as a problem. A permanent 'Funkberatung' (Radio consultation) was therefore established in 1939, based at the Air Force Research Center (Erprobungsstelle der Luftwaffe) in Rechlin. Hans Plendl was responsible for this activity. He had obtained his Ph.D. in 1925 with Jonathan Zennecke as supervisor, and he did pioneering work in short-wave communication. He also recognized that permanent monitoring of solar activity was needed if an efficient radio consultation was to be achieved.

In the fall of 1939, Karl-Otto Kiepenheuer joined the Plendl group. He was well prepared for the task of organizing the permanent monitoring of solar activity. He had studied physics in Berlin, received his Ph.D. in coronal physics under the supervision of Max von Laue, and was interested in new technologies. From 1936 on he had been Hans Kienle's scientific assistant in Göttingen.

Michael Seiler describes in great detail how all these solar facilities were established by Kiepenheuer, supported by Plendl and funded by the Luftwaffe. It was only after 1941 that the collaboration between Kiepenheuer and Plendl was put on a contractual basis; Kiepenheuer was not a member of the Luftwaffe, but was on leave from Göttingen Observatory. From 1942 on, the facilities at Wendelstein (Bavaria), Syracuse (Sicily), Zugspitze, Kanzelhöhe (Kärnten) and Schauinsland were put into operation. Besides these, observatories such as Arcetri (Florence), Paris-Meudon, Belgrade and Simeis (Crimea), located in areas occupied by German forces, were included in the observing network.

In 1943, the institute led by Kiepenheuer was moved from Göttingen to Freiburg, with the clear task to deliver data on solar activity that would allow for a forecast of possible disturbances in radio communication. The scientific staff included civilians as well as members of the Air Force. By the end of 1943, the institute that had adopted the name Fraunhofer-Institut, had about 50 staff members (this figure was only reached again in the year 2000). Even in January of 1945, the institute had 22 scientists, and several of those became well-known astronomers in the post-war era.

Seiler's book presents a detailed discussion of the extent to which the solar observations corresponded to the original goal. Were they of importance for the conduct of the war? Did the observation have the character of basic science? Was there any kind of science that could not be used for military purposes? The reader may or may not come to the same conclusions as the author.

At the end of the war, the military-funded network of solar facilities disappeared, but two observatories remained in operation. Kanzelhöhe Observatory in Kärnten/Austria was assigned to the University of Graz and even today is the only solar observatory in Austria. Meanwhile, the survival of the Fraunhofer-Institut in Freiburg was, in large part, due to the clever use of Kiepenheuer’s good personal relations with French and U.S. colleagues. Indeed, he had maintained a fruitful cooperation with his Meudon colleagues throughout the French occupation, and was never a member of the National Socialist party. Soon after the end of the war the Institut was put under the supervision of the French Navy, and it continued to collect and publish data on solar activity. After the establishment of the Federal Republic of Germany in 1949, the Fraunhofer-Institut became part of the federal state Baden (since 1952 unified as Baden-Württemberg). In 1978 the institute was renamed the *Kiepenheuer-Institut für Sonnen-physik*, and it is now a member of the Leibniz Society and is one of the world’s leading solar physics institutes.

This book reports on all these complex connections and relations, and many details are analyzed from today’s point of view, more than half a century after the end of the war.

We can thoroughly recommend Seiler’s book to any reader who is interested in solar physics or seeks information about the relationship between science and the people in power during a ‘Total War’.

Wolfgang Mattig and Wolfgang Schmidt
Freiburg i. Br., Germany

***The Stargazer of Hardwicke: the Life and Work of Thomas William Webb*, edited by Janet and Mark Robinson (Gracewing, Leominster, 2006), pp. xix+288, ISBN 0-85244-666-7(hardback), 235 x 156 mm, £14.99.**

The name of the nineteenth century clerical astronomer Thomas William Webb (1806–1885) is still widely known in astronomical circles. Webb was first and foremost a ‘man of the cloth’, spending the last thirty years of his life as vicar of a small English country parish: Hardwicke, near the Welsh border. However, it is for his assiduous stargazing and popularising of science that Webb is chiefly remembered today. His magnum opus, *Celestial Objects for Common Telescopes*, was first published in 1859 and ran to seven editions (most recently in 1962). *Celestial Objects* … is still a handy guide to the features of the night sky for amateur astronomers.

As related in the preface to *The Stargazer of Hardwicke*, this book has been a long time in coming. In the decades following Webb’s death, the need for a detailed record of his life and work was stressed from time to time, but one hundred and twenty years were to elapse before this hope was realised. Editors Janet and Mark Robinson are to be commended for bringing together a team of writers—both amateur and professional—to give a detailed record of Webb’s life and achievements.

This book divides into three main sections. The first four chapters provide an insight into Webb’s life: early years and education (M.A. at Oxford); curacies and marriage; his last thirty years spent as vicar of Hardwicke; and an overview of his ministry. Here we can trace Webb’s growing interest in science and particularly in astronomy. Chapters 5 to 7 provide a bridge between Webb’s life as a cleric and his astronomical work. Especially significant is the discussion by Allan Chapman on the important role played by clerical astronomers in nineteenth and early twentieth century England: men such as Pearson, Perry, Pritchard, Espin and Phillips—in addition to Webb himself. However, it is not until Chapter 8 that the reader encounters a detailed account of Webb’s many astronomical activities; this occupies most of the remainder of the book.

In Chapter 8 we find descriptions of Webb’s various telescopes: notably his largest instrument—the 9⅓–inch (23.6 cm) reflector which he used throughout the last two decades of his life. Sadly this historic instrument seems to have disappeared. Fortunately, as discussed in Chapter 9, Webb’s extensive observing notebooks are still preserved. These passed into the hands of his friend and executor, the Reverend Thomas Espin (who revised two editions of *Celestial Objects* …). Five precious notebooks, described by Espin as “… a model of neatness, patience and care …”, are now preserved in the library of the Royal Astronomical Society.

Details of Webb’s numerous observations of the Moon, the planets, comets, the Sun, and double stars—interspersed with examples of his careful drawings—form the basis of Chapters 10 to 14 of *The Stargazer of Hardwicke*. In the final Chapter (15) Bernard Lightman gives an illuminating discussion of Webb’s role as a populariser of science: both lecturer and writer.

The book has two valuable appendices: an account of the history and activities of the Webb Society, founded in 1967; and an extensive bibliography of Webb’'s published works. In addition to his three books (*Celestial Objects for Common Telescopes*, *Optics without Mathematics* and *The Sun: a Familiar Description of his Phaenomena*), approximately two hundred papers are cited—the earliest dating to 1835. Most of Webb’s papers were published in popular journals, but he frequently contributed to *Monthly Notices of the Royal Astronomical Society* and *Nature*. He was especially active between 1862 and 1882, sometimes publishing more than ten papers in a single year! Webb’s subjects were very varied, covering almost every aspect of contemporary observational astronomy. His particular interest was in double stars, of which he made an exhaustive investigation. Webb continued observing until only two months before his death.

My main criticism of this book is that there tends to be a rather limited continuity in both style and depth of content between chapters—largely because of the number of different authors. Some readers may find this aspect rather distracting. Nevertheless, this book should prove a valuable addition to the annals of leading personalities in science and a tribute to an outstanding observer and educator.

F. Richard Stephenson
Centre for Astronomy, James Cook University,
and Durham University

***The Man Who Found Time. James Hutton and the Discovery of the Earth’s Antiquity*, by Jack Repcheck (London, Simon and Schuster Pocket Books, 2004), pp. 247, ISBN 978-07434-5087-8 (paperback), £7.99.**
***James Hutton, the Founder of Modern Geology*, by Donald B. McIntyre and Alan McKirdy (Edinburgh, National Museums of Scotland Publishing Limited, 2001), pp. 51, ISBN 978-1901663-69-3, £8.99.**

James Hutton (1736–1797) is an important figure in the history of science. He was the first to demonstrate from geological observations that the Earth is a body of extreme antiquity, thus inferring a great age for the Sun and for the Solar System. Yet his name is little known outside the world of geologists and historians of the eighteenth century Scottish Enlightenment. Struck by this apparent neglect, Jack Repcheck, an American scientific book editor, was prompted to write an account of the life and work of Hutton whom he regards as one of the great pioneers of science, on a par with Copernicus, Galileo and Darwin.

Hutton lived for most of his life in Edinburgh, in a circle of scholars and independent thinkers that included such luminaries as the economist Adam Smith, the philosopher David Hume and the chemist Joseph Black. Having studied chemistry and medicine at university he went on to become an expert in scientific agriculture and in geology, where he made his mark. His *Theory of the Earth*, published only at the end of his life, postulated cyclical processes that required enormous spans of time to accomplish, and led him to the conclusion that the Earth’s duration past and future was impossible to determine: "… no vestige of a beginning, no prospect of an end." This view differed radically from the favoured one of an evolving Earth with a certain beginning, and it gave rise to prolonged controversy. Hutton's 'deep time' eventually triumphed, and was a key element in Darwin's theory of evolution.

Repcheck tells, with great verve, not only the fascinating story of Hutton's life and work but the entire history of attempts to determine the age of the Earth, from the Council of Nicea in AD 325 to the final result by radioactive dating of meteorites in 1956. He provides a glossary of geological terms, but unfortunately no illustrations—apart from a rather dull map of Great Britain. His occasional sweeping statements such as (in reference to his heroes of science) "Of the four, only Copernicus and perhaps Galileo were Christians …" or (on the question of calculating the age the Earth) "Kelvin had a physicist's arrogance …", may be put down to an excess of enthusiasm for his subject. This apart, he is surely to be thanked for making the achievement of this great pioneer available to the wider public.

James Hutton, the Founder of Modern Geology, is a beautifully-illustrated booklet written by two academic specialists in Scottish geology. In the compass of fewer than 50 pages they describe Hutton's life and personality, his place in the Scottish Enlightenment and his significance in the development of geological science. Their text is supported by an abundance of coloured illustrations that explain the geological evidence more clearly than any words can convey. These include stunning modern photographs of rock formations in Scotland and also in California, the Grand Canyon and other locations; and samples of meticulous drawings by Hutton's expedition companion John Clerk of Eldin (a member of the same talented family that produced the physicist James Clerk Maxwell in a later generation) to whose memory the book is dedicated. These illustrations alone make the book a delight to peruse. There are also portraits of the two geologists.

These books, preferably both together, supply the answer to anyone wishing to understand how the geologists' long time-scale entered the debate of the Sun's age, a problem which was not solved by astronomers for well over a century.

Mary Brück
Penicuik, Scotland

CONTENTS

Research Papers

IAU Reports

Book Reviews

Centre for
Astronomy
JCU
JAMES COOK UNIVERSITY