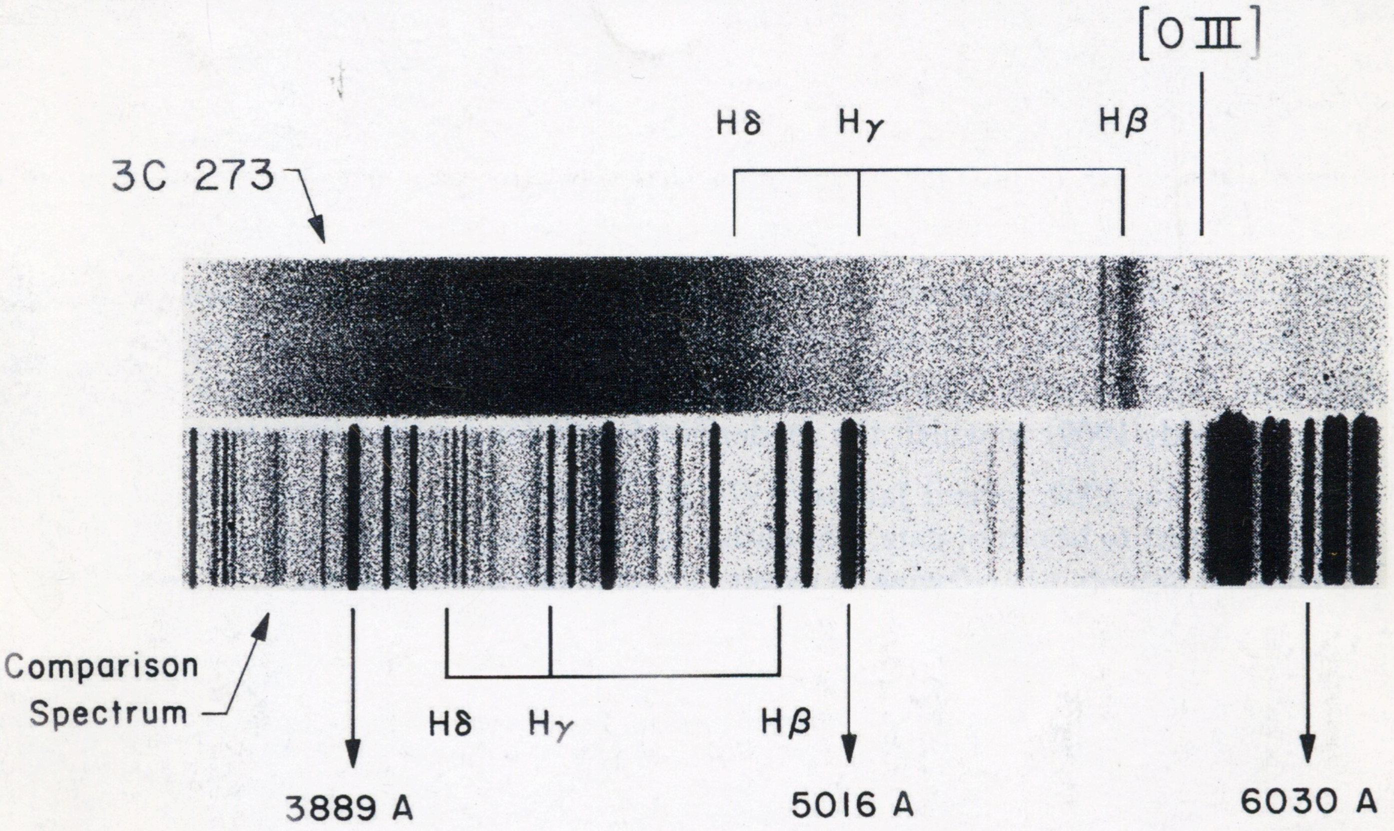
JOURNAL OF ASTRONOMICAL HISTORY AND HERITAGE



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COVER PHOTOGRAPH

The images on the cover all relate to the discovery and early investigation of quasars. Across the top is the spectrum of the 13th magnitude 'star' near 3C 273 obtained by Maarten Schmidt with the 200-inch Palomar Telescope. In 1963, he realised that this anomalous spectrum could be explained if the four emission bands were actually highly red-shifted hydrogen lines. The three astronomers featured are (from left to right) Jesse Greenstein (1909–2002), Maarten Schmidt (1929–) and Allan Sandage (1926–), and they worked closely with radio astronomers from the Owens Valley Radio Observatory in a bid to unravel the true nature of those mysterious objects that we now know as quasars. For details of the Owens Valley-Palomar collaboration see pages 79-91 in this issue of the journal.

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QUASARS AND THE CALTECH-CARNEGIE CONNECTION

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Abstract: A collaborative relationship existed between the California Institute of Technology (Caltech) and the Carnegie Institution of Washington (Carnegie) beginning in 1946, when a formal agreement was signed between the two groups of trustees. This agreement was designed to integrate Mount Wilson Observatory and the new unfinished Palomar Observatory into a single scientific entity. During the period from 1946 to 1979, much astronomical research was done at both institutions as a direct result of this collaboration. Part of this research included the first identification of a radio source with an apparently stellar object by Allan Sandage of Carnegie and Thomas Matthews of Caltech in 1960, and the first identification of spectral lines at large redshift from a radio source associated with such an object by Maarten Schmidt of Caltech in 1963. This paper examines how the discovery of these objects—which came to be known as quasars—and subsequent research on them, indirectly had an impact on the relationship between Caltech and Carnegie by leading to an environment of increased competitiveness that eventually resulted in the formal dissolution of the relationship in 1980. In this paper, the controversy surrounding the discovery and the interpretation of quasars is examined to provide further understanding about the working relationship when the two institutions were formally collaborating. Some of the data used in this paper were drawn from personal correspondence and interviews with the researchers themselves, and this research forms part of a dissertation for a Ph.D. degree in the Centre for Astronomy at James Cook University, Townsville, Australia.

Keywords: quasars, Carnegie Institution of Washington, California Institute of Technology, 200-inch Palomar Telescope, Owens Valley Radio Observatory, J. Greenstein, T. Matthews, A. Sandage, M. Schmidt

1 THE CARNEGIE INSTITUTION OF WASHINGTON AND ITS OBSERVATORIES

1.1 The Beginning

The Carnegie Institution of Washington was founded on 4 January 1902 when its Articles of Incorporation were signed. The institution was reincorporated by an act of the Congress of the United States, approved 28 April 1904, under the title of the *Carnegie Institution of Washington* (Carnegie Year Book No. 47, 1948: xi). Andrew Carnegie, a multi-millionaire steel baron and philanthropist, financed the institution with an endowment of registered bonds with a par value of ten million dollars, in order "… to encourage, in the broadest and most liberal manner, investigation, research, and discovery, and the application of knowledge to the improvement of mankind." (ibid.). Mr Carnegie made an additional contribution of two million dollars to this fund on 10 December 1907, and he contributed a further ten million dollars on 19 January 1911 (ibid.).

Carnegie gave the Board of Trustees "… full power to decide how the institution would meet its mandate, and even to amend his mandate …" (Sandage, 2004: 30). Accordingly, the Board selected a seven-man Executive Committee to formulate research methods in a variety of fields, and these were presented to the Board from time to time. The first move of the Executive Committee was "… to canvass the state of knowledge in seventeen different fields of human endeavor …" (ibid.), and to select leaders in each field to form Advisory Committees, which would write position papers outlining where major advances were likely to be made in their respective disciplines. Edward C. Pickering, Director of the Harvard College Observatory, was appointed Chairman of the Advisory Committee for Astronomy.

In 1904, George Ellery Hale (Figure 1), seeking clearer skies than existed near Chicago, obtained support from the Carnegie Institution to found the Mount Wilson Solar Observatory in the mountains near Pasadena, California. Hale, who had invented the spectroheliograph and discovered solar magnetism, wanted to understand the physics of the Sun and stars. In pursuit of this goal, the initial complement of solar telescopes at Mount Wilson was followed by the 60-inch Reflector and then the 100-inch Hooker Telescope, which was the largest in the world at the time of its construction (Carnegie Observatories, 2006). Hale's motivation came from an enduring goal "… to solve the problem of stellar evolution." (Sandage, 2006a).

The Observatories of the Carnegie Institution at Mount Wilson transformed astronomy and astrophysics with a succession of major breakthroughs, including Harlow Shapley's mapping of the globular clusters of our Galaxy, Edwin Hubble's extragalactic studies and his redshift-distance relation, and Walter Baade's recognition of stellar populations (ibid.).

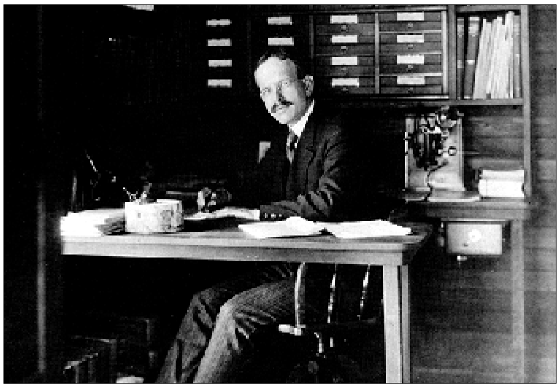

Figure 1: George Ellery Hale shown here in his office at Mt Wilson Observatory. This photograph dates to about 1905 (from http://www.mwoa.org/hale.html).

From the success of the Mt Wilson telescopes, Hale was determined to build a 200-inch or even larger telescope that would enable astronomers to see farther into space and to attack problems ranging from the structure of the Universe to the evolution of stars and the composition of stellar matter (Goodstein, 1991). In February 1928 Hale asked the editor of *Harper's* to send an advance copy of "The Possibilities of Large Telescopes", which he had written, to Wickliffe Rose, the General Education Board President at the Rockefeller Foundation. When Hale called on Rose on 14 March, Rose asked him, "Do you want a 200-inch or a 300-inch?" Hale replied "A 200-inch telescope." (ibid.). Rose wanted to put the proposed telescope into the hands of a school, not the Carnegie Institution or the National Academy of Sciences, as Hale had initially proposed. Rose's suggestion that Caltech would make better use of the new telescope if it belonged to them infuriated John Merriam, Carnegie Institution President. This hostility meant that no real progress on a joint Caltech-Carnegie astrophysics program was likely while Merriam remained in office. Nevertheless, Merriam changed his mind, and in the fall of 1928 the International Education Board of the Rockefeller Foundation gave the green light to Hale's $6 million proposal. This pledge, for which responsibility was later assumed by the General Education Board and which was supplemented by funds from the Rockefeller Foundation, was made to Caltech, of which Hale was a trustee.

Figure 2: Ira Sprague Bowen was Director of the Mount Wilson and Palomar Observatories from 1948 to 1964, and oversaw the completion of the 200-inch Hale Telescope and the 48-inch Schmidt Telescope (from http://www.ossc.org/bios/fellows-bowen.html).

Figure 3: Robert Bacher joined Caltech in 1949 and remained there for the rest of his career, serving as Chair of Physics, Mathematics, and Astronomy from 1949 to 1962, and as Caltech Provost from 1962 to 1969, and Vice-President and Provost from 1969 to 1970 (from http://en.wikipedia.org/Robert_Bacher).

1.2 Administration

In the fall of 1928, the Observatory Council, with Hale as Chairman, was formed to direct the planning, construction and operation of the 200-inch Telescope. Hale assembled the team of scientists and engineers to build the 200-inch Telescope, choosing John Anderson, a Mount Wilson astronomer, as the Executive Director (Goodstein, 1991: 221). The site was to be on Palomar Mountain, southeast of Los Angeles. This site was chosen to enable very long exposures at the limit of the telescope's reach, which Hale acknowledged might not be possible at Mount Wilson because of the illumination of the night sky from the sprawling development of Los Angeles (Florence, 1995). Title to the Palomar Telescope was given to Caltech, which joined with Carnegie to form the Mount Wilson and Palomar Observatories (Caltech, 1951).

The man picked to head the Mount Wilson and Palomar Observatories was Caltech Professor of Physics, Ira Sprague Bowen (Figure 2), who held the position from 1946 to 1964. Bacher (1981) has credited Bowen with making the 200-inch the best telescope in the world at the time. Equally important was the fact that the joint operation of the two staffs worked well under Bowen's tenure. This success was attributed to a mix of subtlety and power in his personality, coupled with good scientific judgment and wise decision-making in administration (Sandage, 2004). Because of Bowen's outstanding credentials, the Carnegie Institution was willing to allocate up to three million dollars of endowment for the Telescope, and this was to be given as either a single grant or as a series of endowments (see Florence, 1995).

The administration of the two Observatories was affected through an Observatory Committee which comprised Bowen (as Director), Robert Bacher (the Chairman of the Division of Physics, Mathematics and Astronomy at Caltech), plus two additional members from the Observatory and two from Caltech. When Bowen became Director of the Observatories, he also became an employee of the Carnegie Institution, and perhaps this was a contributing factor to "… the observatory problems that developed between Caltech and the Carnegie Institution." (Bacher, 1981).

Robert Bacher (Figure 3) was Caltech's first Provost, from 1962 to 1969, and when asked if there were any problems in administering Palomar he responded as follows:

> You know, the two Observatories have now separated. I have a certain sadness over this, because there were forces in this direction even during the period in which I was Provost, and I tried very hard to put the thing together in a way that would work better. But the forces toward separation became very large. When I came out here, one of the ways the operation was carried out was that there was an Observatory Committee and two *ex-officio* members—Bowen as Director of the Observatories, and myself as Chairman of the Division. At that time, I think, there were two additional members from the Observatory and two from Caltech. I used to talk to Bowen a great deal about the fact that we should talk about the research planning in the Observatory Committee, but Bowen never liked to do it that way. He was glad to talk to me about it, but he didn't really like to get into it in a meeting of that sort. And the Observatory Committee became a committee that sort of put the rubber stamp on things to be done and particularly

> supervised the allocation of Observatory time. (Bacher, 1981).

Bacher's comment that the Observatory Committee was never used to plan the research programs indicates that at this senior administrative level there was a basic lack of communication between Caltech and Carnegie. Consequently, the concerns of both institutions were never properly addressed, and "… the problems between Caltech and the Carnegie Institution … became worse as the years went by." (ibid.).

Another interesting comment from Bacher was that he and Bowen got along very well together, except when it came to staffing appointments:

> The only problem I ever had with Bowen was that he hated to act on any appointments at Caltech in astronomy. He was responsible, not I, for the research carried on at Caltech in astronomy. Things having to do with teaching reported through the Division, and things that had to do with research reported through the Observatory. But if somebody had to be appointed, connected with research and so on, he'd always say, "Well you do it, you do it." [But] Overall we got along just fine. (ibid.).

Bowen's reluctance to make appointments was somewhat disconcerting to Bacher, and the problem manifested itself later in conflicts which were to have serious repercussions (as reported below).

1.3 Conflict

Despite the agreement between Caltech and Carnegie regarding the equal right of access to all the equipment on either mountain, a letter written in 1969 by Jesse Greenstein (Figure 4) to Allan Sandage (Figure 5) indicated that there were problems:

> Your letter brought up anxieties about the relations between the two Institutions. I might feel them also, but I believe it is important to act as if there were no important problems which we could not solve by mutual agreement. Most certainly there are real problems, and they are not all one-sided. We are doing our best to keep our cool, and to work out a rational arrangement with mutual respect. I have completely disinvolved myself in any CARSO [Carnegie Southern Observatory] activities from the beginning; I have been involved in attempts to foster better planning for all of Caltech astronomy, and for the future of Palomar, the possibility of a search for a new location … (Greenstein, 1969).

However, these relational problems already existed in 1965 when Jesse Greenstein wrote John Bolton that the use of the Caltech and Carnegie telescopes was a delicate issue that impacted on the relations between the radio astronomers and their optical counterparts:

> I should point out to you that the question of the use of our telescopes for identification of radio sources and accurate optical positions has been one of the most delicate ones between relations of the radio observers, the optical observers and guest investigators. At the present time a precarious working arrangement exists in which John Wyndham is identifying the sources for which the Caltech Radio Observatory finds positions, quite on his own. Subsequent to the preparation of his manuscript these positions are made available to Sandage and Schmidt. Thus I should warn you that you will be coming into a fairly complicated situation. Sandage is taking direct photographs for accurate optical positions and doing the photoelectric photometry and Schmidt the redshifts. Consequently, where your new data might overlap any from Owens valley or Green Bank you are going to have direct competition with Sandage. (Greenstein, 1965).

Figure 4: Jesse Greenstein, who collaborated with Maarten Schmidt on the interpretation of quasars in 1963, and helped to instigate the founding of Caltech's OVRO. He became foundation Head of the graduate astronomy program at Caltech at the time of the inauguration of the 200-inch Telescope and the joint operation of the Mt Wilson and Palomar Observatories (from http://pr.caltech.edu/periodicals/336/articles/Volume%202/10-31-02/greenstein.html).

Figure 5: Allan Sandage has been a staff member of the Carnegie Observatories since 1952, and has been involved in researching the evolution of stars, galaxies and the Universe (from http://www.ociw.edu/research/sandage.html).

By 1969, there were indications of a possible rift in the relationship between the two institutions, as suggested in the following letter from Olin C. Wilson to Horace Babcock.

> … no one here, I feel sure, has the slightest desire to break up the arrangement for joint operation of the Observatories which began in 1948. If there is any interest in such a move it certainly does not come from the C.I.W. staff, but must have originated elsewhere.
>
> But I find another aspect of the matter even more unsettling, namely, what do we mean by the partnership of C.I.W. and C.I.T. in the astronomy business? My understanding is that it consists of an agreement for joint operation and joint use of certain expensive equipment, for the mutual benefit of both partners, but

> that in no way implies dominance by one nor the loss of identity and self-determination of either.
>
> If this view is basically correct, then I interpret your statement to mean that one of the partners does not subscribe to it. It seems to me that one partner is attempting to use threats and coercion against the other in order to force the latter to spend a large sum of its own money in a manner it deems deleterious to its own interests. Personally, I feel that such methods have no place in the partnership in question, and are entirely unworthy of either of the members. (Wilson, 1969).

What Wilson appears to suggest is that Caltech was coercing Carnegie into spending money in a manner that was not in its best interests. At the time, Wilson was the person who allocated observing time on the Mt. Wilson and Palomar Telescopes while Babcock (Figure 6), the recipient of his letter, was the Director.

Figure 6: Horace Babcock invented and built many astronomical instruments, including a ruling engine which produced excellent diffraction gratings, the solar magnetograph and microphotometers, automatic guiders, and exposure meters for the 100-inch and 200-inch Telescopes. By combining his polarizing analyzer with the spectrograph he discovered magnetic fields in other stars. He developed important models of sunspots and their magnetism, and in 1953 he was the first to propose adaptive optics. He was the Director of the Mt Wilson and Palomar Observatories from 1964 to 1978. During this time he founded the Las Campanas Observatory in Chile (from http://www.phys-astro.sonoma.edu/BruceMedalists/BabcockHW/).

In a recent interview, George Preston, the Director Emeritus at Carnegie, explained why there was conflict between the Caltech and Carnegie astronomers:

> There was a profound asymmetry in the relationship between Carnegie and Caltech by 1980, because we had been contributing, since the end of World War II, to the aging outmoded telescopes on Mt. Wilson—the 60-inch and 100-inch reflectors—in a light-polluted site. Because the telescopes were old and because the site was polluted, nobody was much interested in investing money in them, and they were growing more antiquated and inadequate with every passing year. Caltech was interested in supporting Palomar at that time, and I think that Caltech astronomers felt that we were not pulling our own weight in the joint operation. We were contributing aged telescopes in a light-polluted site that nobody wanted to use, and we were making demands for the much-coveted telescopes at Palomar (which were) bigger telescopes, more modern and in a darker sky. This led to a kind of estrangement and a feeling on the part of the Caltech astronomers that they were not getting their money's worth. They were giving telescope time and they were not getting anything back. (Preston, 2006).

It seems clear that even in the early 1960s Caltech and Carnegie had a somewhat precarious relationship, despite the contractual arrangement between the two institutions. This is similar to the way in which Maarten Schmidt saw the situation when he became Director more than a decade later.

When Schmidt (Figure 7) assumed the Directorship of the Hale Observatories in 1977, the 'Observatories' consisted of Palomar and the Big Bear Solar Observatory, and on the Carnegie side Mt. Wilson and Las Campanas in Chile. In an interview conducted in 1999, Schmidt commented that

> … the relationship between Caltech and Carnegie concerning the observatories had not been overly good. And curiously enough, that didn't apply so much to the astronomers but more to the administrative levels. Jesse Greenstein certainly had his conflicts with the Carnegie administration. (Schmidt, 1999).

Schmidt acknowledged that part of the conflict stemmed from the fact that while the two halves of the Hale Observatories were financially and organizationally independent and the facilities were utilized jointly.

An additional operational difficulty was that the Caltech astronomers had undue influence over the appointment of Carnegie staff, and *vice versa*:

> If the Caltech group proposed that a potential faculty appointee become a staff member of the Hale Observatories, that then had to be approved by the Observatory Committee, which consisted half of Carnegie and half of Caltech astronomers. So that meant that the Carnegie side was able to influence, or bias, or perhaps even veto, or make difficult, Caltech's academic appointments. (ibid.).

In October 1979 an appointment by the Carnegie side was rejected by the Caltech astronomers, and because of the bitterness that resulted Schmidt felt that the system was not working, so in his capacity as the Director he wrote a letter to the Carnegie and Caltech Presidents proposing that the operational agreement between the two institutions—which had existed since 1948 and been amended several times—should be terminated (ibid.). At the same time he tendered his resignation, effective from 1 July 1980 (i.e. in nine months time).

According to Schmidt (ibid.), telescope accessibility was not the issue. The problem seemed to be the organizational structure that created awkward relationships that could have devastating decision-making implications. Apparently, Carnegie President, James Ebert, and Caltech President, Marvin L. Goldberger, were very surprised by Schmidt's letter. As it turned out, the Carnegie side opposed the separation, while the Caltech side supported it. In Schmidt's opinion, Carnegie felt that part of their strength was in a solid union with Caltech in astronomy, while access to the 200-inch Palomar Telescope might be jeopardized by separation. However, physicists at Caltech involved in astronomy could have their appointments influenced by Carnegie, which did not seem right in Schmidt's view because "… if anybody influences our appointments, it ought to be the physicists and the mathematicians, with whom we are joined." (ibid).

Schmidt elaborated on the reasons for terminating the agreement between the two institutions in the 1979 *Year Book* of the Carnegie Institution:

> Problems manifested themselves in particular on the occasion of staff appointments. When one of the institutions would consider the appointment of an optical astronomer, the Observatory Committee would evaluate the person in parallel for appointment as a staff member of the Hale Observatories. This procedure was a potential source of conflict, since a President would get recommendations for a given appointment from both his institution's faculty and from the Observatory Committee. In practice, this resulted in administrative interference by one institution into the affairs of the other institution (Schmidt, 1980).

Schmidt elaborated on this during an interview conducted on 28 July 2006:

> The Hale Observatories had a staff. One was a member of the staff of the Hale Observatories, so appointments were made to it and they would be very naturally accepted by both sides so long as it was about people of the professorial faculty right here in astronomy, and on the Carnegie side appointments of the permanent staff over there. It was indeed true that when that letter was written [to the two Presidents] we had been through a period of disagreement about a particular proposal. This staff membership was a curious one and most of the difficulties would arise if somebody from elsewhere in Caltech or at positions that were not entirely full-blown observing astronomers came up. It was sort of in that nature. Now if it had been only that, I don't think that the situation would have developed the way that I proposed. The reason that we had this arrangement with Carnegie was that the Rockefeller Boards in the early thirties, in deciding to give money for the 200-inch Telescope, upon George Ellery Hale's proposal awarded it to Caltech rather than to Carnegie. So what happened was that the Telescope was essentially given to Caltech … and the understanding was that the two would go together, Carnegie and Caltech, in managing the place. The arrangement with Carnegie was accepted by the Caltech administration (and) was made without input from its astronomy staff, since there was none. (Schmidt, 2006).

Clearly, there were distinctive reasons for the way the Caltech-Carnegie collaboration was established, but one can imagine the reaction from the Carnegie staff when the 200-inch Telescope was effectively given to Caltech, a university that at the time had no astronomy staff members whatsoever. Later, this organizational structure was part of the reason for the rising dissonance mentioned in the foregoing quotes, and Schmidt's reference to "interference" (in 1980), says a great deal about why problems developed at an administrative level.

In spite of the organizational tension, important scientific work was carried by the Caltech and Carnegie astronomers during the 1960s. A particularly impressive collaboration involved the discovery of quasars, and this is discussed in the following Section of this paper. Yet even this research was not without its share of controversy and conflict, which may have contributed—at least in part—to the dissolution of the Caltech-Carnegie relationship.

2 QUASI-STELLAR OBJECTS (1959-1979)

Quasi-stellar objects (QSOs) are objects with a star-like appearance whose spectra show large redshifts. Another characteristic of QSOs is an excess of ultraviolet radiation. A more definitive feature of QSOs is the presence of broad redshifted emission lines (Burbidge and Burbidge, 1967). QSOs also exhibit variability in the emission of their radiation. All of these characteristics, when taken collectively, help to define a QSO.

Figure 7: Maarten Schmidt joined the staff of the Mt. Wilson and Palomar Observatories (now Hale Observatories) in 1959, and from 1978 to 1980 served as the Director. His discovery and interpretation of quasars challenged many previously-accepted theories on the origin and age of the Universe (from http://www.phys-astro.sonoma.edu /BruceMedalists/Schmidt/index.html).

In this paper, the terms 'quasi-stellar object' and 'quasar' will be used synonymously. 'Radio quasars' are those that have been detected as radio sources, and sometimes these are also referred to as 'quasi-stellar radio sources' (Schmidt, 1975). For the purposes of this paper, all of these objects will be collectively designated 'QSOs'. The term 'quasi-stellar' was first used by Maarten Schmidt, while the word 'quasar' was apparently coined by Hong-Yee Chiu from the Goddard Institute for Space Studies in the 1960s, when he was talking to a newspaper reporter (Kellermann, 2006).[1]

Back in 1953, very few discrete radio sources were identified with conspicuous optical astronomical objects. However, F. Graham Smith from the Cavendish Laboratory had reduced the uncertainties in the positions of Cassiopeia A and Cygnus A to $\pm 1^s$ in right ascension and $\pm 40''$ in declination (Baade and Minkowski, 1954a). The new positions were accurate enough for an unambiguous identification of both radio sources on plates taken in September 1951 by Baade and Minkowski with the 200-inch Palomar Telescope (ibid.). These two astronomers showed that one of the most intense radio sources, Cygnus A, was associated with an 18th magnitude galaxy with a redshift of $z = 0.056$, or 16,830 km $\sec^{-1}$ (cf. Greenstein, 1984).

As soon as the two 90-foot (27-m) radio telescopes at Caltech's Owens Valley Radio Observatory (henceforth OVRO) began working successfully as an interferometer, Thomas A. Matthews began a program to determine the precise positions of large numbers of discrete radio sources (see Matthews and Sandage, 1963). The first accurate positions were obtained in 1960 and these were published three years later (see Read, 1963).

When Matthews began his OVRO research, he suggested a collaboration with optical astronomers who had access to the 200-inch Palomar Telescope and therefore could search for optical identifications (Sandage, 1999). It was at this point that Allan Sandage became involved in the optical identification program, which was to last far beyond the discovery of quasars, until most of the radio sources in the Cambridge 3C Catalogue had been identified.

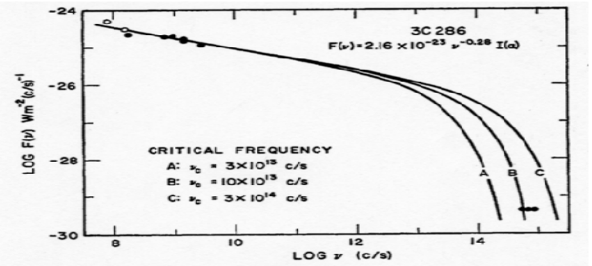

Figure 8: A comparison of the radio and optical power for 3C 286. Photometry over thirteen months shows variability by at least $\Delta V = 0.4^m$ (Matthews and Sandage, 1963: 43).

Sandage was one of the first Ph.D. students in astrophysics at Caltech, and he used the 200-inch Telescope to show that the most distant 'stars' that Edwin Hubble had observed were actually ionized hydrogen clouds. Later he would discover a new class of stellar object that came to be known as 'quasi stellar radio sources'.

The first identification of a radio source with a star-like object was made by Matthews and Sandage in 1960. Sandage began by taking plates of about 20 unresolved radio-loud quasars. From this work came optical identifications for 3C 48, 3C 196, and 3C 286 (Matthews and Sandage, 1963).

A plate of the 3C 48 field was exposed on 26 September 1960, and the only object lying within the error box of the radio position was a 16th magnitude stellar object with some faint associated nebulosity (ibid.; Matthews et al., 1961). Sandage found that the spectrum showed broad emission features that did not correspond to those seen in the spectra of any known Galactic stars. Meanwhile, optical photometry in 1960-1961 showed this object to possess an ultraviolet excess compared with Main Sequence stars; furthermore, it varied by 0.4 magnitude in the course of a thirteen month period (ibid). It was this seminal investigation during 1960-1961 that initiated the study of quasars (see Burbidge and Burbidge, 1967).

Matthews and Sandage (1963) subsequently identified the radio sources 3C 196 and 3C 286 with faint star-like objects, whose colors were similar to those of 3C 48, and from photometry carried out during 1961 they noticed there was a good fit between the optical and radio data for 3C 286 (see Figure 8).

What Matthews, Sandage or Schmidt could not explain at that time was the anomalous emission lines associated with these objects. It was the identification of the strong radio source 3C 273 that eventually led to the solution of this particular problem. Cyril Hazard (1961) pioneered the use of lunar occultations to determine radio source positions with high accuracy, and on 8 December 1960 he used the 250-foot Radio Telescope at Jodrell Bank to observe an occultation of 3C 212 (ibid). Hazard, Mackey and Shimmins (1963) subsequently applied this same method to 3C 273, using the 210-foot Parkes Radio Telescope in Australia. It was established that 3C 273 was a double source, where the ratio of the flux densities of the two components changed with frequency (ibid.). The positions of these two components, A and B, were determined with greater accuracy than any other sources known at that time, and were calculated from the observed times of disappearance and reappearance, which were estimated from the calculated flux density at the edge of the geometrical shadow and from the positions of the diffraction lobes (ibid.). At a frequency of 410 MHz, Component B had a diameter of ~3″ and a flat radio spectrum, and it coincided with a 13th magnitude star (ibid.). At 400 MHz, Component A had a diameter of 4″ and a spectral index of 0.9, and it was located at the end of a jet-like optical feature 20″ from the star (Greenstein and Schmidt, 1964).

Schmidt (1963) used the prime focus spectrograph on the 200-inch Telescope to photograph the spectrum of the 13th magnitude star seen near 3C 273 at dispersions of 400 and 190 Å per mm (see Figure 9). In February 1963 he realized that the spectrum could be explained if the four emission bands were actually hydrogen Balmer lines exhibiting the very considerable redshift of $z = 0.158$ (Schmidt, ibid.). The remaining lines could then be satisfactorily interpreted as [O III] at 5007 Å and Mg II at 2798 Å.

Two possible explanations of this stellar object were suggested (ibid.):

> (1) That it was a star with a large gravitational redshift, and a radius estimated to be ~10 km.
> (2) That it was the nuclear region of a galaxy with a cosmological redshift of 0.158, corresponding to an apparent velocity of 47,400 km/sec. The distance would be ~500 megaparsecs and the diameter of the nuclear region <1 kiloparsec.

Schmidt (ibid.) concluded that 3C 273 was an extragalactic object because the derived diameter would be unrealistic if it were located within our Galaxy. In a recent interview, he elaborated on this reasoning (Schmidt, 2006):

> Jesse and I [wrote up this work] soon thereafter and it was published in 1964 [i.e. Greenstein and Schmidt, 1964] … which was the one thing we ever did together, in which we tried to interpret 3C 273 and 3C 48. Our interpretation included an extensive discussion of gravitational redshift, and we found that it was essentially impossible because if you assume it was a gravitational redshift and you see an emission line spectrum like we did, including forbidden lines, you get into a spectroscopic squeeze through which you can show that the object has to be excessively faint intrinsically. So it had to be very nearby to be seen at thirteenth magnitude. In effect I could prove, and I had already done so before the article with Jesse, that if 3C 48 was a compact object of one solar mass, and it showed a gravitational redshift, which means it had to be very very small, I

> could show that the distance had to be ten kilometers. Now that's not very agreeable—to have a solar mass at ten kilometers. And then I increased the assumed mass and I found you kept getting in trouble every time again. So I think especially with the publication of the article with Jesse, that we made the most compelling argument that it was not a gravitational redshift …

Oke (1963) used the 100-inch telescope at Mount Wilson to determine the absolute distribution of energy in the optical region of the spectrum of 3C 273. Accepting Schmidt's redshift of 0.158, Oke confirmed that Hα should appear at 7599 Å, because "… this is in satisfactory agreement with observation, when it is recalled that the atmospheric A band absorbs strongly beyond 7594 Å." (ibid.). Oke's research also showed that the absolute energy distribution of the apparent spectral continuum for 3C 273 can be represented by $F_\nu = \nu^{0.28}$, where F_ν is the flux density per unit frequency interval and ν is the frequency (ibid.).

In 1962, Greenstein and Matthews (1963) used the Palomar Telescope to investigate the redshift of the optical correlate of 3C 48, and obtained a value of $z = 0.3675 \pm 0.0003$. They interpreted the radio source as the central core of an explosion in an abnormal galaxy, and from their research concluded that 3C 48 was at an estimated distance of 1.10×10^9 parsecs and had an absolute visual magnitude of –24.0, or –24.5 when corrected for interstellar absorption. By comparison, 3C 48 radiated about 50 times more powerfully in the optical region than intense radio galaxies, and Oke (ibid.) concluded that this unusually strong optical emission was associated with synchrotron radiation. Matthews and Sandage (1963) arrived at a similar conclusion concerning the optical and radio flux densities for 3C 48 and for 3C 196 by showing that the radiant flux in the optical region can be computed from the radio flux data if one invokes synchrotron radiation.

Schmidt and Matthews (1964) used the Owens Valley interferometer to confirm the identification of 3C 47 and 3C 147 as QSOs with large redshifts. It was found that the position of 3C 47 practically coincided with a stellar object of visual magnitude 18. With a Hubble constant of 100 km sec^{-1} Mpc^{-1}, the nominal distance of 3C 47 was 1,275 Mpc, and its absolute visual magnitude was about –23. Schmidt and Matthews (ibid.) concluded that 3C 47 clearly belonged to the class of QSOs like 3C 273 and 3C 48, which exhibited optical luminosities much larger than those of the brightest galaxies.

Sandage (1966) used the 200-inch Telescope in October 1965 and January 1966 to show that the colors of forty-three QSOs were correlated statistically with redshift, and he concluded that because the redshift of quasars varies across the U–B and B–V diagrams in a regular fashion, statistical predictions of the redshift are enabled using the U–B and B–V values alone. Observations during the outburst of 3C 446 revealed that the equivalent widths of the emission lines and the slope of the continuum both changed (Wallerstein and Oke, 2000). Similar results were found by Oke (1967) when he observed 3C 446 and 3C 279. Continuum changes of 20% were seen in 3C 279 on time scales of one day (ibid.). Yet despite these changes, Sandage (1966) still concluded that quasars were sufficiently similar in their continuum distributions and in the strengths of their emission lines relative to this continuum for the statistical correlations to be valid.

Wampler and Oke (1967) carried out spectrophotometric observations of QSOs from Palomar (between 5,100 and 6,000 Å, with a resolution of 25 Å) and from Lick Observatory (between 5,412 and 7,056 Å, with a resolution of 30 Å), and their investigations revealed the existence of several emission features previously unknown or only suspected. The evidence indicated that most of these were associated with Fe II, from which an electron density of the gas producing the line can be calculated. The results obtained by Wampler and Oke (ibid.) reinforced the conclusions of Greenstein and Schmidt (1964): that 3C 48 and 3C 273 were associated with distant superluminous objects in galaxies, or were intergalactic objects (if one accepted a cosmological interpretation of their redshifts). Assuming a Hubble constant of 100 km sec^{-1} Mpc^{-1} the distances for 3C 48 and 3C 273 were 1,100 and 474 Mpc respectively. The absolute visual magnitudes then became about –25 and –26 respectively, making these objects among the most luminous in the Universe (ibid.).

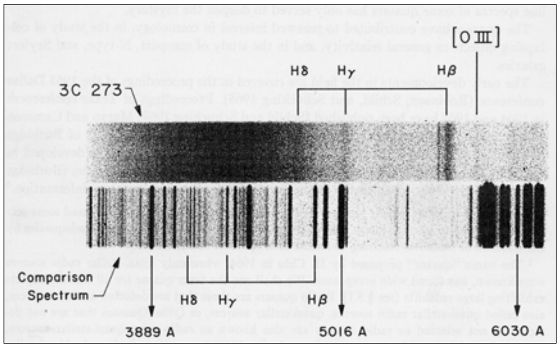

Figure 9: The spectrum of 3C 273 showing the hydrogen Balmer lines with a redshift of z = 0.158. Part of the emission indicated as [OIII] is due to [FeII] according to Wampler and Oke (1967). At the bottom is a comparison spectrum of hydrogen, helium, and neon with various wavelengths indicated (after Schmidt, 1975: 284).

Using the 100-inch Telescope and the 84-inch Kitt Peak telescope respectively, Sandage (1965) and Lynds et al. (1965) found that there were sometimes ultraviolet objects on their plates that did not lie close to the positions of any known discrete radio sources. When Sandage began taking photographs in ultraviolet and in blue light with the 48-inch Palomar Schmidt Telescope, he found such objects turning up with a frequency of ~3 per square degree down to a limiting magnitude of $B \approx 18.5^m$. Sandage (1965) detailed his results in a controversial paper, announcing that he had discovered in the quasi-stellar galaxies a "… major new constituent of the universe." but Burbidge and Burbidge (1967) demonstrated that there was considerable uncertainty associated with the claim that these objects were as common as Sandage indicated, and other researchers (e.g. Lynds et al., 1965) concluded that some of these objects at high Galactic latitudes might be Galactic. Burbidge and Burbidge (op.cit.) felt that it was ambiguous to describe such objects as galaxies unless indisputable evidence of the presence of stars could be produced.

3 CONTROVERSIES

3.1 The Non-Cosmological Interpretation of QSOs

A small minority of astronomers adopted an interpretation of quasars that was very different to that proposed by Schmidt and Greenstein, and these are presented here to illustrate the nature of the controversy. The commonly-accepted rebuttals supplied by present-day astronomers to these ideas is also included in order to provide the reader with a better understanding of historical developments associated with the discovery and interpretation of quasars.

The local-Doppler hypothesis was first proposed by James Terrell (1964) to avoid some of the problems that were believed to exist if quasars were indeed at very large distances. Terrell identified the nucleus of our Galaxy as the nearest possible explosion center. He then estimated the minimum distance of 3C 273 to be 200 kpc on the basis of the absence of a detectable proper motion and the minimum explosion age of 5×10^6 years (ibid.). He also showed that there were relativistic limits to the fluctuations in brightness which may be observed for a large spherical surface, and also for more general sources. He inferred that quasars were probably no more than light-days in diameter, and there was also a possibility that they may be close to our Galaxy. Terrell's conclusions were based on the relatively rapid fluctuations in the light intensity of known quasars.

Schmidt (1975), however, determined that 10^6 quasars will carry a total kinetic energy of about 10^{60} M ergs, where M is the average quasar mass in solar masses. This is about 10^{64} ergs, or the rest-mass energy of 10^{10} solar masses, which is approximately 10% of the mass of our Galaxy. This would make the local-Doppler hypothesis an unlikely one to account for the redshift of QSOs. It should be noted, in parentheses, that Schmidt's quoted numbers are 1975 values, and the typical quasar mass today is understood to be between 10^8 and 10^9 solar masses (e.g. see Vestergaard, 2002; Yu and Tremaine, 2002).

Other hypotheses included that by Halton Arp (1967), who claimed a correlation between peculiar galaxies and radio sources, including QSOs. Arp said that quasi-stellar radio sources (QSS) were associated with galaxies at 'intermediate' distances of 10-100 Mpc (ibid.). He added that one of the problems with the cosmological hypothesis included the difficulty of using conventional physics to understand the origin of the energy required for the high luminosities found in QSOs. Another argument against the cosmological hypothesis was that the diameters implied by the time-scale of radio variations were so small that they indicated the QSS to be much closer than cosmological redshifts would allow. A third difficulty was that the scatter in the redshift apparent-magnitude relation for QSS indicated "… that it is dubious whether there could exist a redshift relation." (ibid.). Finally, Arp claimed that in some QSS such as 3C 9 the expected absorption from intergalactic material was not present "… as it should be, if the light traverses such great distances." (ibid.). A graphical summary of Arp's observations is shown in Figure 10.

Arp (1987: 178) later contended that it

> … is ironic but appropriate that in this Hubble diagram we are able to see at the same time the refutation of the conventional viewpoint of quasars and redshifts, the reconciliation of intrinsic redshifts with expanding Universe concepts, and the clear continuity of how the intrinsic redshifts evolve from high redshift quasars into low redshift companion galaxies.

Arp's argument (1987: 38) against the cosmological hypothesis also relates to Figure 11, and is as follows:

> The analysis of this photograph seems very simple to me. There are only two possibilities. Either the quasar placed at the head of the filament is an accident, or the two objects are physically connected. Since the configuration has negligible probability of arising by chance, I conclude that this demonstrates the physical association of quasar and galaxy. There goes the whole cosmological quasar hypothesis!

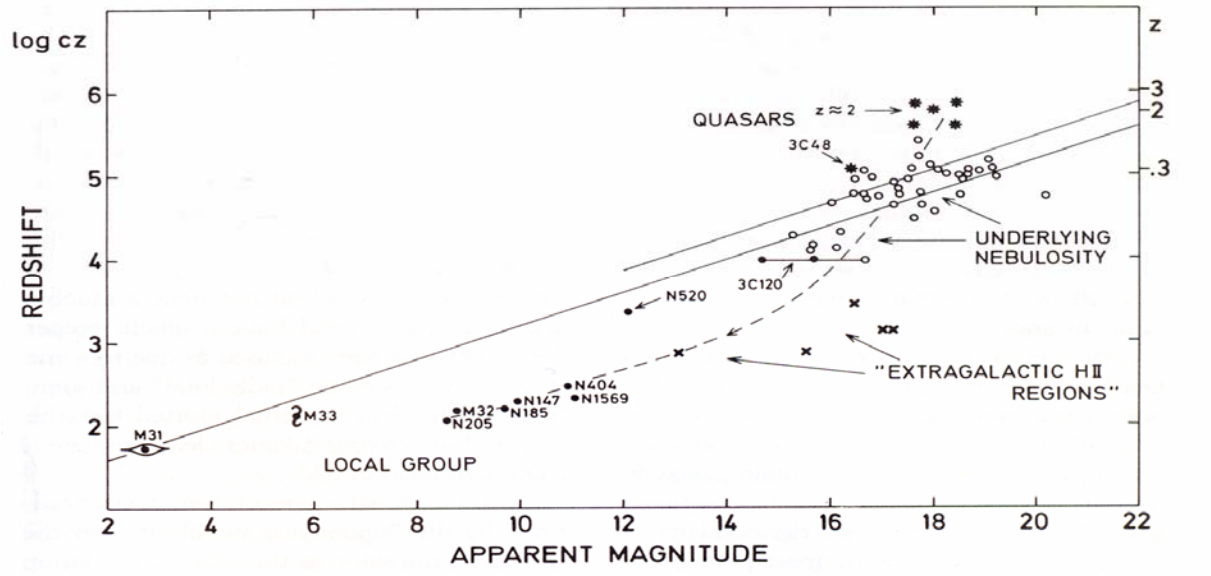

Figure 10: The Hubble Diagram for Local Group objects. The open circles represent measures of underlying nebulosity for a selection of quasar images (after Arp, 1987: 175).

> … Another interpretation, since the quasar is quite bright in apparent magnitude, is that, along the lines of Chapter 5, they could both be close to us in space and have been expelled by a nearby galaxy. They might also simply represent a rare accidental collision of a galaxy and quasar in the same locality of space. One thing that is inescapable, however, is that the high redshift quasar is at the same distance as the low redshift galaxy. (Arp, 1987: 38).

I do not find Arp's analysis sufficiently compelling to draw the conclusions that are cited above, because the analysis makes *a priori* assumptions. Most astronomers in the 1970s assumed quasar redshifts were cosmological, in spite of the superluminal motions. Marshall Cohen (pers. comm., 2006) relates that his research involved making measurements of objects such as quasars, blazars and BL Lac objects using Very Long Baseline Interferometry (VLBI):

> With VLBI you measure a proper motion, that is, the angular motion on the sky, and then if you have the distance you get the linear motion. We interpreted it in terms of velocity; and the numbers came out at speeds that were faster than light.

This is explained by the fact that there is a relativistic beam aimed nearly at the observer, from which it can be shown that the apparent velocity sideways on the sky looked faster than the speed of light (ibid.).

There were astronomers, however, who supported Arp's views and established hypotheses of a controversial nature. These astronomers used the idea of relativistic motion as an argument against the common cosmological interpretation of the redshifts. As Cohen (2006) indicated in an interview:

> There is nothing wrong with something being relativistic at the Galactic Center. There is an enormous amount of evidence showing that there are extraordinarily energetic things going on in the centers of galaxies. So I think that Arp is wrong. You cannot use (the) superluminal motion picture as evidence against the redshift interpretation …

Geoffrey and Margaret Burbidge are two astronomers who did not accept the cosmological interpretation of quasars (see Burbidge and Burbidge, 1967), and they suggest that the discovery of quasars had an impact on subsequent studies in cosmology, which continues up to the present day (G.F. Burbidge, 2006). This is reflected in a recent paper by Cohen et al. (2006) which contains strong evidence that relativistic beams emanate from quasars. Cohen et al. plotted the apparent transverse velocity or superluminal velocity against the apparent luminosity for 119 discrete radio sources, and found a correlation for the jets in quasars: high apparent velocities were only noted for radio jets with high luminosities. This implied a similar correlation between the Lorentz factor and peak intrinsic luminosity, namely that high Lorentz factors must preferentially exist in jets with high intrinsic luminosities.

3.2 The Role of J. Beverly Oke

In a letter to the author, Allan Sandage (2006b) claims that Schmidt was not alone the day that the 3C 273 spectrum was examined, and that J. Beverly Oke was present. Furthermore, Sandage (2006c) stated that Oke told him that Schmidt could not have made the identification of 3C 273 without a spectrum that Oke obtained on one of his Mt. Wilson observing runs. It should be noted, however, that in Schmidt's 1963 paper, Oke's contribution is specifically acknowledged:

> It thus appears that six emission bands with widths around 50 Å can be explained with a redshift of 0.158 … *The present explanation is supported by observations of the infra-red spectrum communicated by Oke in a following article* … (Schmidt, 1963; our italics).

The paper that Schmidt refers to is Oke (1963), which states, *inter alia*:

> During the course of the infra-red observations a strong emission feature was found near 7600 Å with a possible error of about 10 Å … Using this line and others in the visible spectrum Schmidt has shown that the most prominent emission features are Balmer lines and that the line at 7590 Å is $H\alpha$.

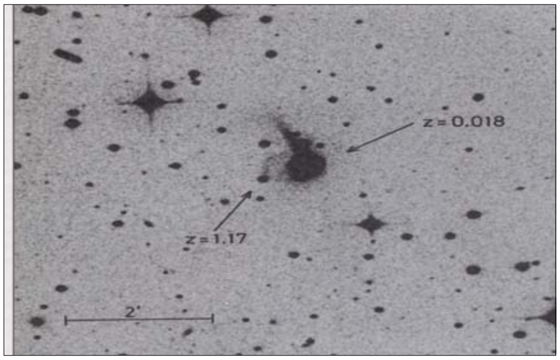

Figure 11: The quasar, Parkes 1327-2006 connected by a luminous filament to a galaxy with a jet (Arp, 1987, 38).

The aforementioned quotations indicate that Oke's contribution was not ignored by Schmidt, and in a presentation at the NRAO in 1983, Schmidt made the following pertinent remarks:

> J.B. Oke observed 3C 273 spectrophotometrically at the 100-inch telescope on Mt. Wilson and detected a strong emission line in the infrared, at 7600 Å. A total of seven emission lines were [*sic*] now known in 3C 273 and in hindsight it seems strange that with so much information no larger effort was undertaken to identify the lines …
>
> It was on February 5, 1963 that the puzzle was suddenly resolved. Cyril Hazard had written up the occultation results for publication in *Nature* and suggested that the identification results be published in an adjacent article. It was in the process of writing the article that … I noticed that four of the six lines exhibited increasing spacing and strength toward the red. I attempted … to construct an energy-level diagram based on these lines, then made an error which seemed to deny the regular pattern … to check on that, I started taking the ratio of the wavelength of each line to that of the nearest Balmer line. The first ratio was 1.16, the second 1.16, the third … 1.16!
>
> Realizing that this was a redshift, I divided the wavelengths of the other two lines by 1.16 and found that they landed near those of the [Mg II] doublet at 2800 Å and forbidden [O III] line at 5007 Å. Oke's line observed at 7600 Å came close to the wavelength of H-alpha. Clearly, a redshift of 0.16 explained all the observed emission lines! (Schmidt, 1983).

This long yet invaluable quotation illustrates the crucial insight that Schmidt had when he interpreted the spectrum of 3C 273, and Oke's contribution *is* fully acknowledged when understood in its proper context.

However, the comments by Sandage show the depth of feeling relating to this discovery that still exists more than forty years after the event.

3.3 The Breakdown of the Caltech-Carnegie Collaboration

Allan Sandage (2006c) offers his opinion as to why the Caltech-Carnegie collaboration ended:

> … at the higher levels of the Caltech administration the Caltech physicists had always been dissatisfied with the joint operation of Carnegie at Palomar, and after Bowen's time began to work for the separation of Caltech from Carnegie so that Caltech could pursue an independent path in fund-raising and development of new astronomical facilities. The Caltech physicists were also unhappy with Babcock's push for a Southern Observatory in Chile that would be part of the Hale Observatory organization. This became an increasingly severe problem with the Directorship of Horace Babcock, and when Schmidt succeeded him as Director of the joint Hale Observatories, Schmidt recommended a 'divorce' of the two institutions. He was pressured into this recommendation by the three Caltech physicists, Robert Christy (Provost of Caltech), Robbie Vogt (Head of the Division of Physics, Mathematics, and Astronomy), and Robert Leighton (a senior physicist).[2] The Trustees of each institution agreed to the divorce. It was not a pleasant event, and has led to the severe estrangement on both sides that yet exists. I suspect it will continue until each of the astronomers working at that time will be dead, perhaps in 20 years.

The rivalry that spawned the remarks made by Allan Sandage can perhaps be explained in light of the following comments by George Preston (2006), Director Emeritus at Carnegie:

> … several individuals and groups of individuals within the Hale Observatories staff in the 1960s and 1970s were pursuing the great issues of extragalactic astronomy in those times—the nature of quasars and the expansion of the Universe. These people were all in pursuit of goals that could only be conducted effectively at the 200-inch Telescope, and they brought their rivalries to the meetings of the Time Allocation Committee and the Observatory Committee. Such stuff just went on and on. For these people this research was extraordinarily important. Fame and sense of accomplishment hinged on being able to do it. We had the biggest telescope in the world, the one that was best able to pursue these issues, and a bunch of people on the staff all wanted to do more or less the same things. And with regard to quasars in particular there was Halton Arp, who had his own interpretation of the quasars at odds with well-established laws of physics, and who felt that he wasn't getting a sufficient share of the Telescope time to support his heretical conclusions. I sat in the Time Allocation Committee and Observatory Committee meetings and listened to the endless wrangling. It was unbelievable. I should add that some views were much more moderate and reasoned than others. To say the least, those were exciting times in Pasadena.

Clearly there is some difference of opinion about what actually happened when quasars were discovered. There are also some possible reasons for the relational problems that began to be manifest between Caltech and Carnegie. These differences and reasons are discussed in the following section of this paper on the basis of oral history interviews and what has been preserved in correspondence and in the published literature.

3.4 Quasars and the Caltech-Carnegie Nexus

The discovery of quasars impacted on the relationship between Caltech and Carnegie because of the competitive nature of the astronomers involved, and because quasars were a major area for research interest in the 1960s. The Caltech-Carnegie split, however, appears to be a direct result of administrative problems rather than scientific differences. It is certainly true that the creation of Caltech's radio observatory opened up many opportunities for collaboration between the astronomers at the OVRO, Mount Wilson, and Palomar, but equally important were the bonds in nuclear astrophysics that those in Caltech's Astronomy and Physics Departments forged with their Carnegie colleagues at the same time. It should also be noted that under the agreement between Carnegie and Caltech, all staff members—including graduate students—had equal rights of access to all of the instrumentation at Mount Wilson and at Palomar (Greenstein, 1982).

Yet the two institutions were very different, for research was emphasized at Carnegie while at Caltech the professors have many other duties, including teaching. But while there were obviously cultural differences between Caltech and Carnegie, these do not appear as a pragmatic reason for the breakup.

It is apparent that the discovery of quasars played a role in the conflict between Caltech and Carnegie, as suggested by Allan Sandage. In a recent letter to me (Sandage, 2006a), he says that although he and Tom Matthews were very much involved in the optical identification of quasars, Matthews was never given enough credit for the discovery. After all, it was his precise radio sources positions that allowed the optical identifications to be made. It would be interesting to obtain Matthews' perspective on these views, but to date all of my attempts to contact him have proved unsuccessful.[3]

The competitive nature of the Caltech-Carnegie astronomical environment in the 1960s has already been referred to by George Preston, and this is also mentioned by Sandage (1999: 477):

> Beginning in 1963 the quasar program became quite frenzied with the 3C 273 redshift discovery, not only at Palomar, but also at Kitt Peak, Lick, and Hawaii, with rivalry between all groups and within each group often leading to severe tension.

In a previously-cited interview, Maarten Schmidt (2006) commented that the use of telescope time was not an issue in the breakup of the Caltech and Carnegie nexus, but the previously-mentioned letter to Allan Sandage, Jesse Greenstein (1969) states that the use of the Caltech and Carnegie telescopes was a delicate issue between the radio astronomers and their optical counterparts. Perhaps the conflicts that arose in the 1960s were at the operational level and proved to be more surmountable. As time went on, however, the conflicts appear to have risen to an administrative level, where the decision-making affected the careers of several people. It was at this point, in 1979, that Maarten Schmidt took the action that he did which led to the dissolution of the Carnegie and Caltech relationship.

The controversies surrounding the interpretation of quasars by Arp, Burbidge and Terrell would seem to have had little if any effect on the Caltech-Carnegie nexus, but the controversy surrounding the *discovery* of quasars does deserve closer examination. The views expressed by Sandage (2006b) and Schmidt (1999) in the aforementioned correspondence probably filtered through to the administration of these two institutions and helped precipitate the breakdown of the Carnegie-Caltech relationship. This is a logical conclusion because even though the two institutions were financially independent, their facilities were utilized jointly.

When scientific organizations compete for facilities, it is difficult to imagine that scientific differences of opinion do not affect how these entities operate, and the interviews cited above with Sandage and Schmidt provide evidence that this was indeed the case with the Caltech-Carnegie nexus. Quasars were a major area of astronomical research in the 1960s and 1970s, and any scientific group that could claim a discovery as its own would want to be protective of its position. The discovery and subsequent interpretation of quasars was not without controversy, which led indirectly to a deterioration of the relationship between Caltech and Carnegie staff.

In interviews conducted with some of the current staff at Carnegie who were present at the time of the breakup, I found some memories of a rather bitter nature. When asked about the reaction to the decision to formally separate the two institutions, Eric Persson, a staff astronomer at Carnegie, responded:

> Well I can tell you that there was a very bad feeling on an October morning in '79 when—and anybody who was here then will tell you the same thing—we came in and it was there in our mail boxes—this short paragraph from the Director, Maarten Schmidt, saying, well I hereby dissolve the Observatories and there is no longer any Hale Observatories. It came as a real shock. My colleague, Steve Schectman, downstairs, and I, remember it like it was yesterday … it was just a bad feeling. (Persson, 2006).

Persson's comments are not atypical of how many of the Carnegie staff felt as a result of the breakup. In retrospect, however, this was not a bad thing because both institutions have subsequently acquired astronomical instruments that are unique and have established their own independent world-class research programs. Today, Caltech operates two 10-meter telescopes and Carnegie two 6.5-meter telescopes, yet this may never have happened if the two institutions had not separated.

The actual separation was executed by both Presidents on 1 July 1980, when Maarten Schmidt stepped down from the Directorship. The joint operation of the observatories was replaced by joint utilization, and it meant that the Time-assignment Committee still consisted of Carnegie and Caltech astronomers. According to Schmidt (1999), this arrangement worked "… very well, until the late eighties; it ran smoothly and was appreciated by both sides …" As soon as the separation took place, each of the institutions became aware that they were responsible for their own astronomical facilities and destinies.

This acknowledgement carried over into the 1990s when discussions began about the next generation of very large telescopes. These were attended by representatives from both institutions, but by this time the Carnegie already had a major investment in Chile—which they wished to develop—while the Caltech astronomers favored an Hawaiian-based project (Cohen, 2006).

4 CONCLUDING REMARKS[4]

The results of this research may be summarized as follows:

1. The administrative organization of Caltech-Carnegie never provided a unified sense of identity to each institution. There was always an 'us-them' syndrome which was competitive rather than cooperative, and this led ultimately to a breakdown of the collaboration and the dissolution of the Caltech-Carnegie nexus.

2. In a 5 July 2007 email to one of the Editors (W.O.) Maarten Schmidt disputes this: "I can only say that I was never pressured into recommending a separation by Christy, Vogt and/or Leighton."

3. The discovery of quasars in the 1960s augmented the competitiveness within the Caltech-Carnegie nexus, because these objects fundamentally altered our understanding of cosmology. In effect, astronomers realized that astronomical history was being made, and many astronomers wanted to be part of this process.

4. The controversies surrounding the interpretation of quasars by Arp, Burbidge and Terrell were short-term distractions that did not contribute significantly to the breakup of the Caltech-Carnegie nexus.

5. The availability of telescope time was not an issue in the Caltech-Carnegie breakup, even though some friction was felt by both sides at various times.

6. The inherent difference in the duties and responsibilities of staff members at the two institutions did not seem to be a factor in the breakup.

7. Any conflicts that developed regarding the actual discovery of quasars did not materially affect the Caltech-Carnegie nexus. The majority of astronomers acknowledged Schmidt's interpretation of quasars, and the contributions by Oke and Matthews were properly credited in the associated literature.

8. In the long term, the integrity and prestige of both Caltech and Carnegie has not been diminished by the breakup. In fact, the acquisition of new 6.5m and 10m telescopes by the two institutions was a result of the breakup of the nexus.

5 NOTES

1. Chiu's term 'quasar' was first used in his paper on "Gravitational Collapse" presented at the First Texas Symposium in Relativistic Astrophysics, held on 16-18 December 1963 in Dallas, Texas (Chiu, 1965), but it took some time for it to be generally accepted by astronomers.
2. Maarten Schmidt (pers. comm., July 2007) disputes this claim: "I can only say that I was never pressured into recommending a separation by Christy, Vogt and/or Leighton."
3. If any reader can supply me with the current address of Thomas A. Matthews, please email me at: Edward.Waluska@jcu.edu.au
4. This research is part of a continuing doctoral project, and as additional information comes to light hopefully it will lend further credence, or otherwise, to some of the controversial statements contained in this paper.

6 ACKNOWLEDGEMENTS

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7 REFERENCES

The following abbreviation is used:

CA = California Institute of Technology Archives

Arp, H., 1967. Peculiar galaxies and radio sources. *The Astrophysical Journal*, 148, 321-390.

Arp, Halton, 1987. *Quasars, Redshifts, and Controversies*. Berkeley, Interstellar Media.

Bacher, R.F., 1981/1983. Interviews with Mary Terrall at Pasadena (California) in June-August, 1981 and February 1983. Oral History Project, CA. Retrieved 23 November 2005 from the World Wide Web: http://resolver.caltech.edu/Caltech OH:OH_Bacher_R

Baade, W., and Minkowski, R. 1954a. Identification of the radio sources in Cassiopeia, Cygnus A, and Puppis A. *The Astrophysical Journal*, 119, 206-214.

Burbidge, G.F., 2006. Interview with Edward Waluska at La Jolla (California) on 10 November.

Burbidge, G., and Burbidge, M., 1967. *Quasi-Stellar Objects*. San Francisco, Freeman and Company.

Caltech, 1951. *Frontiers In Space*. Pasadena, Mount Wilson and Palomar Observatories.

Carnegie Observatories, 2006. http://www.ociw.edu/ociw/about/

Carnegie Year Book, No. 47, 1948. Maryland, The Lord Baltimore Press.

Chiu, Hong-Yee, 1965. Gravitational collapse. In Robinson, I., Schild, A., and Schucking, E.L. (eds.). *Quasi-Stellar Sources and Gravitational Collapse*. Chicago, University of Chicago Press. Pp. 3-14.

Cohen, M.H., Lister, M.L., Homan, D.C., Kadler, M., Kellermann, K.I., Kovalev, Y.Y., and Vermeullen, R.C., 2006. Relativistic beaming and the intrinsic properties of extragalactic radio jets. At Xiv: astro-ph/0611642 v1 20 November 2006.

Cohen, M.H. 2006. Interview with Edward Waluska at Pasadena (California) on 19 December.

Feldman, Paul A., 2007. Telephone interview with Edward Waluska on 31 January.

Florence, R., 1995. *The Perfect Machine*. New York, Harper Perennial.

Goodstein, J.R., 1991. *Millikan's School. A History of the California Institute of Technology*. New York, Norton & Company.

Greenstein, Jesse L., 1965. Letter to John Bolton, dated 28 October. Original in CA.

Greenstein, Jesse L., 1969. Letter to Allan Sandage, dated 3 June. Original in CA.

Greenstein, Jesse L., 1982. Interview with Rachel Prud'Homme at Pasadena (California) between 25 February and 23 March. Oral History Project, CA. Retrieved 1 November 2005 from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Greenstein_J

Greenstein, Jesse L., 1984. Optical and radio astronomers in the early years. In Sullivan, W.T. III (ed.). *The Early Years of Radio Astronomy*. Cambridge, Cambridge University Press. Pp. 67-81.

Greenstein, J.L., and Matthews, T.A., 1963. Red-shift of the unusual radio source: 3C 48. *Nature*, 197, 1041.

Greenstein, J.L., and Schmidt, M., 1964. The quasi-stellar radio sources 3C 48 and 3C 273. *The Astrophysical Journal*, 140, 1-34.

Hazard, C., 1961. Lunar occultation of a radio source. *Nature*, 191, 58.

Hazard, C., Mackey, M.B., and Shimmins, A.J., 1963. Investigation of the radio source 3C 273 by the method of lunar occultations. *Nature*, 197, 1037-1039.

Kellermann, K., 2006. Interview with Edward Waluska at the IAU General Assembly (Prague) on 21 August.

Lynds, C.R., Stockton, A.N., and Livingstone, W.L., 1965. New spectroscopic observations of quasi-stellar sources, *The Astrophysical Journal*, 142, 1667-1673.

Matthews, T.A., and Sandage, A.R., 1963. Optical identification of 3C 48, 3C 196, and 3C 286 with stellar objects. *The Astrophysical Journal*, 138, 30-56.

Matthews, T.A., Bolton, J.G., Greenstein, J.L., Münch, G., and Sandage, A., 1961. American Astronomical Society meeting, New York, 1960. *Sky and Telescope*, 21, 148.

Oke, J.B., 1963. Absolute energy distribution in the optical spectrum of 3C 273. *Nature*, 131, 1040.

Oke, J.B. 1967. Spectrophotometric observations of rapid variability in 3C 279 and 3C 446. *The Astrophysical Journal*, 147, 901-907.

Owens Valley Radio Observatory. 1971. The Owens Valley Interferometer. Original in CA.

Persson, E., 2006. Interview with Edward Waluska at Carnegie Institution of Washington (Pasadena) on 14 July.

Preston, G., 2006. Interview with Edward Waluska at Carnegie Institution of Washington (Pasadena) on 10 August.

Read, R.B., 1963. Accurate measurement of the declination of radio sources. *The Astrophysical Journal*, 138, 1-29.

Sandage, A., 1965. The existence of a major new constituent of the Universe: the quasi-stellar galaxies. *The Astrophysical Journal*, 141, 1560-1579.

Sandage, A., 1966. The correlation of colors with redshifts for QSS leading to a smoothed mean energy distribution and new values for the K-correction. *The Astrophysical Journal*, 146, 13-24.

Sandage, A., 1975. Quasars. In Sandage, A., Sandage, M., and Kristian, J. (eds). *Galaxies and the Universe*. Chicago, University of Chicago Press. Pp. 761-785.

Sandage, A., 1999. The first 50 years at Palomar: 1949-1999 The early years of stellar evolution, cosmology, and high-energy astrophysics. *Annual Reviews of Astronomy and Astrophysics*, 37, 445-486.

Sandage, A., 2004. *Centennial History of the Carnegie Institution of Washington. Volume I The Mount Wilson Observatory*. Cambridge, Cambridge University Press.

Sandage, A., 2006a. Letter to E. Waluska, dated 14 June (Waluska Collection).

Sandage, A., 2006b. Letter to E. Waluska, dated 20 November (Waluska Collection).

Sandage, A., 2006c. Interview by Edward Waluska at Carnegie Institution of Washington (Pasadena) on 28 July.

Schmidt, M., 1963. A star-like object with large redshift. *Nature*, 197, 1040.

Schmidt, M., 1975. Quasars. In Sandage, A., Sandage, M., and Kristian, J. (eds). *Galaxies and the Universe*. Chicago, University of Chicago Press. Pp. 283-308.

Schmidt, M., 1980. *Year Book 1979, Carnegie Institution of Washington*. Tallahassee, Rose Printing Co. Pp. 593-594.

Schmidt, M., 1983. Discovery of quasars. In Kellermann, K., and Sheets, B. (eds.). *Serendipitous Discoveries In Radio Astronomy*. Green Bank, National Radio Astronomy Observatory. Pp. 171-173.

Schmidt, M., 1999. Interview with Shirley Cohen at Pasadena (California). Oral History Project, CA. Retrieved 1 November 2005 from the original in the CA.

Schmidt, M., 2006. Interview with Edward Waluska at Pasadena (California) on 28 July.

Schmidt, M., and Matthews, T. A. 1964. Redshift of the quasi-stellar radio sources 3C 47 and 3C 147. *Astrophysical Journal*, 139, 781-785.

Terrell, J., 1964. Quasi-stellar diameters and intensity fluctuations. *Science*, 145, 918-919.

Vestergaard, M., 2002. Determining central black hole masses in distant active galaxies. *The Astrophysical Journal*, 571, 733-752.

Yu, Q., and Tremaine, S., 2002. Observational constraints on growth of massive black holes. *Monthly Notices of the Royal Astronomical Society*, 335, 965-976.

Wallerstein, G., and Oke, J.B., 2000. The first 50 years at Palomar, 1949-1999. Another view: instruments, spectroscopy and spectrophotometry and the infrared. *Annual Reviews of Astronomy and Astrophysics*, 38, 79-111.

Wampler, E.J., and Oke, J.B. 1967. The emission-line spectrum of 3C 273. *The Astrophysical Journal*, 148, 695-704.

Wilson, O.C., 1969. Letter to H.W. Babcock, dated 19 March. Original in the Huntington Library Archives, San Marino (California).

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JOHN MICHELL, THE PLEIADES, AND ODDS OF 496,000 TO 1

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Abstract: John Michell, M.A., B.D., F.R.S. (1724–1793), was the first scientist to apply statistics to the spatial distribution of the stars on the celestial sphere. He was the first to realise that certain groupings, like the Pleiades cluster in Taurus, were non-random, thus indicating that these stars were a physical group in space, held together by gravity.

This paper presents a step-by-step exposition of Michell's rather convoluted mathematical approach, and discusses the implication of his findings when it came to the acceptance of Newtonian gravitation, the search for stellar parallax and the investigation of binary stars.

Key words: John Michell, stars, statistics, clusters, binaries, gravity

1 INTRODUCTION

Unfortunately Geikie (1918: 3), Davison (1927), Kopal (1986: 398) and Sheynin (1995) were unable to establish the date, or place of the birth, of the somewhat obscure Reverend John Michell, M.A., B.D., F.R.S. Davison hinted that Michell was probably born in Nottingham in either 1724 or 1725. However, extensive recent research by Crossley (2003) has established that Mitchell was actually born in the tiny village of Eakring in north Nottinghamshire (see Figure 1). Eakring is three and a half miles to the south of Ollerton and two and a half miles south east of Rufford Abbey, which, at the time of his birth, was the home of the Savile family, of Oxford University Savilian Chair fame. Considering Michell's prowess in the field of geology it is interesting to note that Eakring is now the site of England's first productive oil well.

Michell was born on Christmas day, 1724. His father, Gilbert, was appointed rector of Eakring in 1722, the advowson belonging to Sir George Savile.

John Michell went up to Queen's College, Cambridge as a Pensioner on 17 June 1742, and took his Bachelor's degree in 1748, appearing as Fourth Wrangler. In 1749 he became a Fellow of Queen's and stayed at that College for the next fifteen years, taking an M.A. in 1752 and a B.D. in 1761. He was elected a Fellow of the Royal Society on 12 July 1760, and was appointed to the Woodwardian Chair of Geology at Queen's, in preference to his friend Neville Maskelyne (1732–1811), the fifth Astronomer Royal (see Howse, 1989: 43), at the end of 1762. Career highlights during that time were Michell's epoch-making dissertation on earthquakes and seismicity (Michell, 1760); his membership of the six-man committee that, in 1765, started to investigate John Harrison's chronometrical solution, using the H-4, for the determination of longitude at sea; his invention and manufacture of the mass balance that was eventually used by Henry Cavendish in 1798 to measure Newton's constant of gravity (see McCormack, 1968); and his pioneering investigations of black holes (Michell, 1784; see also Schaffer, 1979) and artificial magnets (Michell, 1750; see also Hardin, 1966); and his detection of radiation pressure.

In 1763 Michell left Cambridge and became Rector of Compton near Winchester. In the following year he resigned from his Cambridge chair, and forsook the celibacy required by that institution. On 23 August 1764 he married Sarah Williamson, who, according to the *Cambridge Chronicle* of 8 September 1764, was "… a young lady of considerable fortune". A year later their daughter Mary was born. Seven weeks after this happy event disaster struck and his wife died.[1]

In 1767 Michell moved up the church hierarchy to become the Rector of St Michael's Thornhill, near Dewsbury and Leeds, the patron of this benefice being the afore-mentioned Sir George Savile. Thornhill parish was not only well endowed, but was also in the heart of the geologically-interesting great Yorkshire Coalfield and close to the home of Michell's friend, Joseph Priestley.

In this paper we would like to stress the fact that Michell was the first *statistical* astronomer, and that he pioneered the application of probability theory to stellar distributions. We investigate his approach to stellar statistics, and specifically his estimations of the probability of stars being separated by specific distances on the celestial sphere.

2 THE STATISTICS OF THE DISTRIBUTION OF STARS ON THE CELESTIAL SPHERE

Throughout the history of pre-Michellian astronomy it was assumed that the bright stars were scattered at random on the celestial sphere. It is this very randomness that produces the differing shapes of the constellation groupings. One of the breakthroughs of late eighteenth and nineteenth century astronomy was the extension of the easily-mapped two-dimensional distribution of stars on the celestial sphere to a gradual understanding of their three-dimensional distribution in space. The term 'double star' had been in use for millennia, Ptolemy, for example, using it to describe Nu Sagittarii, two fifth-magnitude stars about 14 minutes of arc apart. The numbers of know double stars increased considerably with the introduction of the telescope. Until the work of John Michell, they had all been thought of as optical pairs, their close proximity on the sky being simply a matter of chance. Michell changed this with his pioneering introduction of statistics to the field of astronomy, and specifically to star groupings. He presented his findings to the Royal Society on 7 and 14 May 1767. In the published paper, Michell (1767: 243) wrote:

Figure 1: The village of Eakring, the birth place of John Michell, can be seen in the centre of Nottinghamshire on this contemporary map (Cary, 1787). The area is known as the Dukeries. The main thoroughfare is the Great North Road, this passing though Newark, East Retford and Bawtry.

> … from the apparent situation of the stars in the heavens, there is the highest probability, that, either by the original act of the Creator, or in consequence of some general law (such perhaps as gravity) they are collected together in great numbers in some parts of space, whilest in others there are either few or none.

John Michell's probability arguments rested on the following contentions:

(1) The surface area of the three-dimensional celestial sphere, assuming that this sphere has a diameter D, is equal to πD^2. A sufficiently small circle, of radius r (and $r << D$), drawn on the surface of the celestial sphere will be approximately plane, and will therefore have an area of πr^2. Thus the ratio of the area of the small circle to the total area of the sphere is $(r/D)^2$. In the case of the sky, r and D are measured in angular

units. So consider a small circle of radius 1^o, i.e. 60 minutes of arc. The area of this circle is $\pi 60^2$ square minutes of arc, whereas the area of the surface of the whole celestial sphere is 4π square radians = $4\pi\ (57.29578)^2 = 41252.96$ square degrees = 1.485×10^8 square minutes of arc = $\pi(6875.5)^2$ square minutes of arc.

(2) If stars are randomly distributed on the celestial sphere, the supposition, as Michell (1767: 243) puts it, being "… that they had been scattered by mere chance …", then the probability that a particular star is located in a particular circle of radius r, this circle being centred on the specified star, is simply the aforementioned ratio of areas, $(r/D)^2$. And the probability that it is *not* so located is the complement, $1 - (r/D)^2$, or equivalently $(D^2 - r^2)/D^2$. So the probability, C, of a specified star being in a specified area, 60 arc min in radius, is given by

$$C = \left(\frac{60}{6875.5}\right)^2 = 0.00076154. \tag{1}$$

Taking the reciprocal of the number given in equation (1) indicates that the chance is 1 in 13,131.

(3) The probability of two independent events both occurring is the product of their individual probabilities.

John Michell then argues that if there be n stars visible on the celestial sphere, brighter than a given limiting magnitude, the probability, P, that none of them should lie within a distance r of a given reference star is given by

$$P = ((D^2 - r^2)/D^2)^{\mathrm{n}}. \tag{2}$$

As, however, he is interested in the probability that *no* star lies within distance r of *any* other star, he writes (1767:244) that "… we must again repeat the last found chance n times …", leading to the probability

$$P_n = ((D^2 - r^2)/D^2)^{\mathrm{n}\times\mathrm{n}}. \tag{3}$$

The first stellar example that Michell chose to illustrate his argument was the visual double star β Capricorni (R.A. (2000.0) $20^h\ 21^m\ 00.5^s$, Declination (2000.0) $-14^o\ 46'\ 53''$). In order to calculate the probability that this was a chance pairing he needed to know

(i) the separation of the two stars, which Michell (1767: 246) took to be "… something less than 3⅓ …" arc minutes (in good agreement with the modern value of 205 arc seconds); and
(ii) the number of stars at least as bright as either of the components of β Cap, which he took to be "… about 230".[2]

Using the logarithmic tables of the day, and equations (1) and (2) above, Michell calculated that $6{,}875.5^2$ divided by $(3⅓)^2$ equals 4,254,603.[3] He then logarithmically calculated the value of the fraction 4,254,602/4,254,603 and (by multiplication) raised this fraction to the power of 230 × 230, obtaining a final answer of 0.987653. As 1/(1 – 0.987653) is very close to 81, Michell concluded that the probability of the two stars being that close by chance was about 80 to 1 against. The inference is that their close proximity is not produced by a chance alignment and that the system is a double star, in which both members are orbiting a common centre of mass.

This result is clearly critically dependent on the estimate of there being 230 stars in the sky as bright as the components of β Cap. In contrast to his very careful explanation of the mathematical foundations of his probability calculations, Michell offers us no justification for this number. In fact, the two components of β Cap are very different in brightness. β Cap is a 'telescope' double, the primary having apparent visual magnitude 3.08 and the secondary only 6.10. A magnitude distribution fit to a modern whole-sky catalogue of stars brighter than fifth magnitude (see, for example Ochsenbein and Halbwachs, 1987) indicates that the number of stars, N_m, brighter than apparent visual magnitude m_V, where $2 < m_V < 5$, is given by

$$\log_{10} N_m = (0.494 \pm 0.004)\, m_V + (0.735 \pm 0.015). \tag{4}$$

This equation indicates that there are 180 stars in the sky brighter than β_1 Cap, but (extrapolating to magnitude 6.1) fully 5,570 brighter than β_2 Cap. Using these numbers in the calculation gives a chance probability of 21% (i.e. 1/4.8) rather than Michell's 1/81.

Michell did not have access to a good star catalogue, but he comments that

> … it seems to be an object worth the attention of Astronomers, to enquire into the exact quantity of light, which each star affords us separately, when compared with the Sun; that, instead of distributing them, as has hitherto been done, into a few ill defined classes, they may be ranked with precision both according to their respective brightness, and the exact degree of it. (Michell, 1767: 241).

So it is surely inconceivable that he had failed to observe the difference between the third and sixth magnitudes of the two components of β Cap. Given, however, that he does not make any further use of the result that he obtained, one may charitably assume that he intended the calculation for β Cap to stand as a simple example before tackling the more complicated arithmetic needed for the Pleiades.

It is, however, ironic that he chose β Cap for this demonstration, since it is in fact a far more convincing exemplar than he could have known. Both components are spectroscopic and occultation binaries; in addition, there is a magnitude 9 visual companion which was discovered by William Herschel. So the total number of known components of this complex system is eight (see Hoffleit and Warren, 1991).

3 THE STATISTICS OF THE PLEIADES

Michell then turns his attention to one of the most obvious groupings of stars in the sky, this being the Pleiades galactic cluster in Taurus (see Jones, 1991: 168 and 394). The Pleiades (or Messier 45) has the honour of being mentioned three times in the Bible (Job 9:9, Job 38: 31 and Amos 5:31). Also the observation of the heliacal rising of the Pleiades in the month of May was regarded by the calendrically-minded Julius Caesar as indicating the start of summer. Six of the Pleiades stars are clearly visible to the naked eye, these being Alcyone, or η Tau (apparent visual magnitude, $m_V = 2.96$); Atlas, or 27 Tau ($m_V = 3.8$); Electra, or 17 Tau ($m_V = 3.81$); Maia, or 20 Tau ($m_V = 4.02$); Merope, or 23 Tau ($m_V = 4.25$); and Taygeta, or 19 Tau ($m_V = 4.37$); all of the afore-mentioned apparent magnitudes being taken from Hoffleit and

Warren, 1991. These stars occupy a region of the sky that is about 60 minutes of arc across, the actual interstellar separations quoted by Michell being shown in Figure 2. Michell concluded that the odds against this celestial grouping occurring randomly were about 496,000 to 1, and he went on to suggest that the Pleiades were an actual physical group in space, held together by the influence of Newtonian gravitation.

His methodology was as follows. Michell estimated that there were 1,500 stars visible in the sky brighter than Taygeta (19 Tau, $m_V = 4.37$). Our modern estimate, based on data in Ochsenbein and Halbwachs (1987) and Hoffleit and Warren (1991), would be about half this number, namely 722. This discrepancy indicates the parlous state of astronomical photometry and magnitude estimation in the later half of the eighteenth century. Michell should have at least realised that each magnitude class contains about three times as many stars as the one preceding (see von Humboldt, 1851: 275), and that most contemporary star catalogues showed that there were about 20 first magnitude stars, 65 second, 190 third, 425 fourth, 1100 fifth, and so on.

Mitchell (1767) proceeded by considering the six brightest Pleiades as five pairs. Here he related Taygeta, Electra, Merope, Alcyone and Atlas to the star Maia, the later presumably being selected as it is the closest to the centre of the visible grouping (see Figure 2). Using the β Capricorni approach for the Maia-Taygeta pair (but now assuming a separation of 11 mins arc and a value of $n = 1{,}500$) Michell calculated P (see equation 2) to be 0.996173. Similar calculations for Maia-Electra, Maia-Merope, Maia-Alcyone and Maia-Atlas yielded P values of 0.988018, 0.982506, 0.977148 and 0.926766 respectively. The number n has not been modified to take account of the differing magnitudes. Wanting to calculate the chance that the grouping *will* occur, as opposed to *will not*, Michell then calculated the complements of these quantities to unity, i.e. 0.003827, 0.011982, 0.017494. 0.022852 and 0.073234. As all these pairings occur simultaneously in the Pleiades, these numbers must be multiplied together, giving 1.3424987×10^{-9}. The reciprocal of this number, i.e. 744,880,000, then represents the odds of this grouping occurring at random. Michell then took his readers step by step through a calculation similar to the β Capricorni calculation discussed above. The combined probability is

$$[(744{,}880{,}000 - 1)/744{,}880{,}000]^{1500} = 0.99997984.$$

And $1/(1 - 0.99997984) = 496{,}000$.

Repeating this calculation on a modern pocket calculator gave 1 in 458,000; the difference can be ascribed to rounding errors in taking the logarithms of numbers that are very close to 1. Using the more realistic estimate that there are 720 stars at least as bright as the star Taygeta, this calculation would give even more impressive odds of 1 in 36 million.

Michell (1767: 249) contends that the value 496,000

> … is smaller than it ought to be upon two accounts; for, in the first place, this method of computation gives only the probability, that no five stars would be within the distances above specified from a sixth, if they occupied the largest space, they possibly could do, under that limitation; and secondly, we have made no allowance upon account of the different magnitudes, which, if it had been attended to, would have given a somewhat greater result. These considerations, however would have made the reasoning a good deal more intricate; and we have no need to descend to minutiae, a difference in the pro-portion of 10 to 1 not at all affecting the general conclusion.

Michell, our pioneer astronomical statistician, quite rightly points out that his conclusion—that the Pleiades group cannot possibly be a chance near-alignment—would have been the same even if the number that he calculated turned out to be ten times larger or ten times small that the figure 496,000.

This approach is most refreshing considering that Michell was working in the days when most astronomers quoted numbers to as many places of decimals as were given by their logarithmic calculation or there was room for on the piece of paper that they were using. Michell's astronomical breakthrough was in recognising the fact that some of the stars in the heavens were not like the Sun, i.e. both single, and hundreds of thousands of astronomical units distant from their nearest neighbours. Michell showed that some stars were in gravitationally-controlled groups, and his work pioneered and encouraged the search for stellar binaries by William Herschel, whose first and second catalogues of double stars were published in the *Philosophical Transactions of the Royal Society* in 1782 and 1785.

Historians of statistics such as Boole (1854: 364-367) and Hald (1998: 70-74) have investigated the statistical contributions of Michell, but they mainly concentrated on determining whether he was a Bayesian or not, or whether his work was an example of direct or inverse probability theory. Here we concentrate on Michell's Pleiades investigation, and why he was specifically interested in whether this grouping was a chance association of unrelated stars (at a range of distances from Earth, but all in a similar direction), or whether they were a cluster of stars kept together by gravitational forces

A modern statistical astronomer would use the Poisson distribution to calculate the probability of a chance celestial assemblage similar to the Pleiades. Taking the diameter of the Pleiades cluster to be 60′ (the distance from Taygeta to Atlas in Figure 2), we may calculate that there are 52,524 'pixels' of this size on the celestial sphere. The average number of stars brighter than Taygeta per pixel is thus 0.0286, assuming we use Michell's figure of 1500 such brighter stars on the celestial sphere, or 0.0137 using the more realistic estimate of 720. The Poissonian probability of actually observing r objects in a specific region when the expected number is μ is

$$P(r;\mu) = (e^{-\mu}\mu^{r})/r! \tag{4}$$

yielding a probability of 7.3×10^{-13} for 1,500 stars, and 9.1×10^{-15} for 720. Since we do not care which particular pixel contains the assemblage in question, we must multiply these numbers by 52,524, thus obtaining overall odds of 1 in 26 million for 1,500 stars, and 1 in 2.1 billion for 720 stars.

Since the famous French mathematical physicist Siméon-Denis Poisson (1781–1840) was twelve years old when John Michell died, Michell can be forgiven for not using Poissonian probabilities. However, we might reasonably ask why these probabilities are so much smaller than those calculated using Michell's method. The reason is simple. Michell made a mistake. In a Poissonian distribution the probability of observing two objects is not, as Michell assumed, the square of the probability of observing one object. Michell's algebra corresponds to assuming that $P(1;\mu) = \mu$; and since μ is so much less than unity, this is not a bad approximation. Unfortunately he also assumed that $P(n;\mu)/P(1;\mu) = \mu^{n-1}$, whereas in a true Poissonian distribution $P(n;\mu)/P(1;\mu) = \mu^{n-1}/n!$. The latter is this factor of 6! = 720 less than the former, this explaining the difference between the two calculations.

Conceptually, this can be understood by recognising that the five binary pairs considered by Michell (Alcyone-Maia, Atlas-Maia, Electra-Maia, Merope-Maia and Taygeta-Maia) are only a subset of the fifteen possible binary pairings of a set of six stars. So requiring that Taygeta be 11′ from Maia and that Atlas be 49′ from Maia *also* constrains the distance between Atlas and Taygeta, and this is not taken into account in Michell's calculation. There may be a hint of recognition of this problem in Michell's statement (1767: 249) that

> … this method of computation gives only the probability, that no five stars would be within the distance above specified from a sixth, if they occupied the largest space, they possibly could do, under that limitation.

4 DISCUSSION

Briefly returning to his county of birth (a county much loved by one of the authors (DWH), who was born in East Retford), we would like to tentatively suggest that Michell deserves the title of Nottinghamshire's greatest astronomer. He was the pioneering astronomical statistician, using statistics to show that nearly all double stars were actually gravitationally-bound systems, and not merely chance couplings of two stars close to the same line of sight. This proved that the attractive force of gravity was also a stellar phenomenon and not just a property of our Solar System. Michell was also the first astronomer to discuss black holes and the effect of gravity on light. His estimates of the expected interstellar distances were reasonable in that he intimated (Michell, 1767: 237) that stellar parallaxes were definitely less than 2 arcsec and probably less than 1 arcsec. He suggested that stellar twinkling was due to turbulence in the atmosphere and not eye motion, and his attempts to actually measure radiation pressure were commendable. His construction of the first 'Cavendish torsion balance' led eventually to the measurement of the constant of gravitation, G, and thus the mass of astronomical bodies. His role as a telescope-maker is also worthy of mention. To construct a 10-foot focal length, 30-inch diameter speculum mirror reflector that was so good that William Herschel was willing to pay 30 pounds sterling for it surely underlines Michell's skill.

In ranking Michell as Nottinghamshire's first astronomer, we should briefly mention the 'runners up'. Another prominent Nottinghamshire astronomer was John Russell Hind, FRS (1823–1895), Royal Astronomical Society President and Gold Medallist, Superintendent of the Nautical Almanac (1853-1891), discoverer of ten asteroids, cometary astronomer and celestial mechanician. Maybe Hind deserves second place. Third place might go to Norman Robert Pogson (1829–1891), photometrist, meteorologist, definer of the stellar magnitude scale, discoverer of six asteroids, superintendent of the Madras Observatory (1861-1891) and discoverer (in 1881) of a relationship between sunspot numbers and the price of Indian cereals!

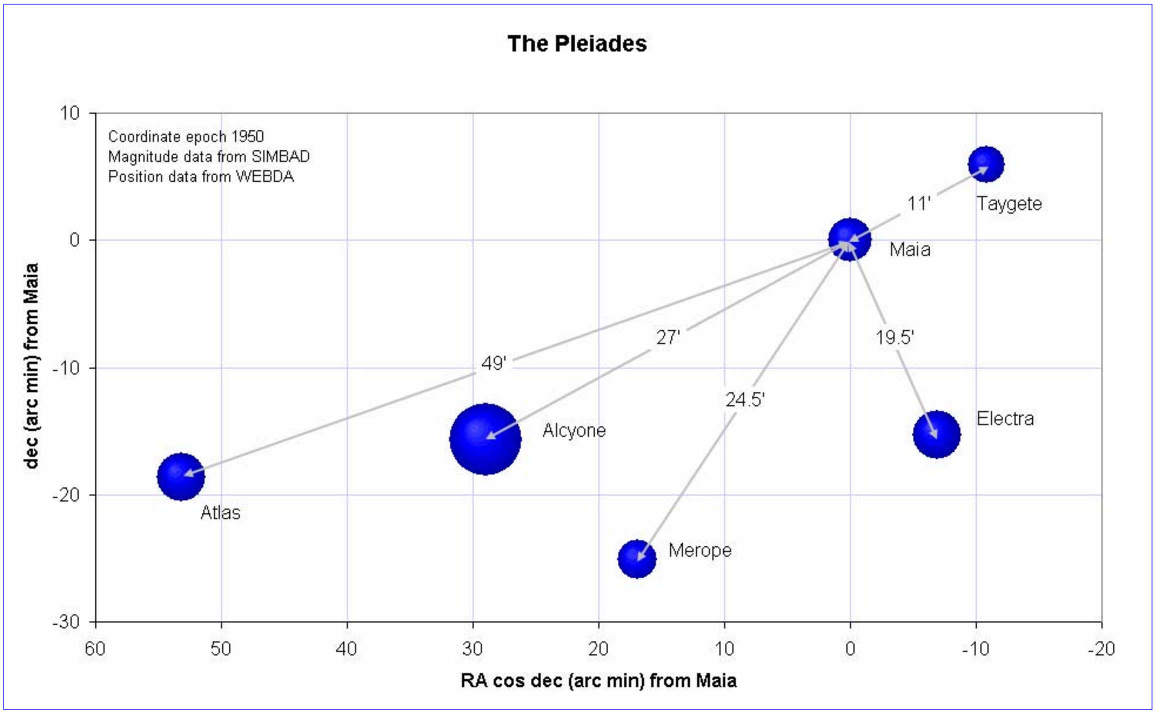

Figure 2: The positions of the six brightest stars of the Pleiades, showing the separations assumed by Michell. The sizes of the points represent the brightnesses of the stars, according to the apparent visual magnitudes taken from the astronomical online database SIMBAD (http://simbad,u-strasbg.fr/Simbad, maintained at the Centre de Données Astronomiques de Strasbourg); the positions are based on epoch 1950.0 coordinates taken from the open cluster database WEBDA (see Jean-Claude Mermilliod, http://obswww.unige.ch/webda/).

5 CONCLUSIONS

Apart from heralding the usefulness of statistics in the study of astronomy, Michell's work on the Pleiades and β Capricorni were important steps in the expansion of the realm of Newtonian gravitation, and in the search for stellar parallax (see, for example, Hoskin, 2003: 68; Hirshfeld, 2001: 186-188). Newton had claimed that his law of gravitational attraction applied throughout the *whole* universe, and to all the individual bodies within it. Evidence for this universality had at the time been only gleaned from the nearby Solar System. The predicted 1758/1759 return of Comet 1P/Halley (see Hughes, 1987), occurring as it did eight years before Michell wrote his statistical astronomy paper, had extended the known 'Newtonian' region well beyond the orbit of Saturn, to a distance of around 35 AU from the Sun. The possibility that the distant stars were influenced by gravity was still, however, a matter of supposition. Michell's insistence that the two components of β Capricorni were actually mutually interacting companions, strengthened the resolve of people investigating double stars. William Herschel started to hunt for them in earnest, presenting a catalogue of 269 doubles to the Royal Society in 1782, and an additional list of 434 three years later (see Herschel, 1782a; 1785). Similar searches were also carried out on the Continent by, for example, the German astronomer Father Christian Mayer. In 1784, Michell (page 56) noted:

> … it is not improbable, that a few years may inform us, that some of the great number of double, triple stars, &c. which have been observed by Mr HERSCHEL, are systems of bodies revolving about each other.

Nearly two decades later Herschel (1803 and 1804) had the proof that many of his double stars were actually binary companions "… intimately held together by the bond of mutual attraction." As the nineteenth century progressed the orbits of these stars about their common centres of mass were carefully plotted (see, for example, Herschel, 1833) and soon, to quote Agnes Clerke (1885: 24), "… the fundamental quality of attractive power was proved to be common to matter so far as the telescope was capable of exploring."

At the time of Michell's work double stars were also playing a part in the hunt for stellar parallax (see Herschel 1782b). Galileo Galilei (1632) had suggested that double stars would be useful in this endeavour. For this to be the case, however, the double had to be a chance alignment, with one of the stars close to the Sun, and the other extremely distant. As the Earth orbited the Sun the movement of the close star measured accurately with respect to the more constant position of the distant star should lead to an estimation of the parallactic distance of the nearer of the double. Michell's statistical analysis led him to conclude that the majority of observed double stars were actually binary stars. The fact that the two stars, in this case, were gravitationally attached companions, made their observation from any position useless when it came to measuring stellar distance with the imperfectly-mounted instruments of the day.

One may also ask if Michell understood the significance of his findings. To this the answer is an unqualified 'yes'. In the footnote on page 238 in his 1767 paper (when he was discussing the question of whether the colour of the light from a star is correlated with its brightness—itself a fascinating anticipation of Wien and Stefan), Michell argues:

> If however it should hereafter be found, that any of the stars have others revolving about them (for no satellites shining by a borrowed light could possibly be visible), we should then have the means of discovering the proportion between the light of the Sun, and the light of those stars, relatively to their respective quantities of matter; for in this case, the times of the revolutions, and the greatest apparent elongations of those stars, that revolved about the others as satellites, being known, the relation between the apparent diameters and the densities of the central stars would be given, whatever was their distance from us: and the actual quantity of matter which they contained would be known, whenever their distance was known, being greater or less in proportion to the cube of that distance.

In other words, Michell foresaw one of the vital attributes of binary stellar systems, this being the way their orbits can be used to determine stellar masses. This led to one of the early twentieth century's foundation stones of modern astrophysics, the relationship between stellar luminosity and stellar mass.

6 NOTES

1. I would like to thank Eric Hutton for informing me (private correspondence, 2006) that Michell's daughter, Mary, married Sir Thomas Turton in 1786, and that they had a daughter, Anna, in 1787. Around 1810 Anna married Henry Peterson, and one of their six children was the famous Yorkshire eccentric and concrete pioneer, Judge Andrew Thomas Turton Peterson, who used to publish under the nom-de-plume *Khoda Bux*.
2. Associate Professor Graeme L. White from James Cook University interestingly points out (private correspondence, 2006) that a more obvious choice for a suitable naked eye double star would have been the nearby Alpha Capricorni. This beautiful naked eye optical double consists of star α^1 of visual magnitude 4.24 and star α^2 of magnitude 3.57, separated by 378 sec. arc. Beta Capricorni, on the other hand is a true visual double but the two stars are more disparate in brightness, being of magnitude 3.4 and 6.2. The possibility of Michell having made a mistake is, however, discounted by the fact that the separation of the components of Beta Capricorni is 205 sec arc, i.e. 3.42′ (see Kaler, 2006a) as opposed to the 378 sec arc, 6.3′ separation of the Alpha Capricorni components (Kaler, 2006b).

   Perhaps Beta Capricorni was selected because of the greater contrast in the brightness of its two components. Michell might have been considering the possibility of using a binary like Beta Capricorni in order to investigate the prospect of using such a pair as a (nearby) target and (more distant) reference for a measurement of trigonometric parallax, as first suggested in 1632 by Galileo in the *Dialogue Concerning the Two World Systems* (see the Stillman Drake translation on pp. 383-384). If this was indeed the initial source of his interest in visual doubles, the choice of a strongly-contrasting pair is eminently sensible, since it suggests (if we assume that all stars are similar to the Sun) a substantial difference in distance.
3. There are rounding errors in his calculation: Michell quoted 2 × log 6,875.5 as being equal to 7.6746086,

whereas today even a simple electronic calculator would give 7.674608572.

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8 REFERENCES

Boole, G., 1854. *An Investigation of the Laws of Thought on which are Founded the Mathematical Theories of Logic and Probabilities*. London, Walton and Maberly.

Cary, J., 1787. *Cary's New and Correct English Atlas: Being a New Set of County Maps from Actual Surveys. Exhibiting all the Direct & Principal Cross Roads, Cities, Towns and Most Considerable Villages, Parks, Rivers, Navigable Canals, etc*. London, John Cary.

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

Crossley, R., 2003. Mystery at the Rectory: some light on John Michell. *Yorkshire Philosophical Society, Annual Report for the year 2003*, 61-69.

Davison, C. C., 1927, *Founders of Seismology*, Cambridge, Cambridge University Press.

Galileo, G., 1632. *Dialogo di Galileo Galilei Linceo Matematico Sopraordinarion,* Fiorenza, Gio Batista Landini; see the translation by Drake, S., 1953. *Dialogue Concerning the Two Chief World Systems*. Berkeley, University of California Press.

Geikie, A., 1918. *Memoir of John Michell M.A., B.D., F.R.S.* Cambridge, Cambridge University Press.

Hald, A., 1998. *A History of Mathematical Statistics from 1750 to 1930*. New York, John Wiley & Sons Ltd.

Hardin, C.L. 1966. The scientific work of the Reverend John Michell. *Annals of Science*, 22, 27-47.

Herschel, F.W., 1782a. Catalogue of double stars. *Philosophical Transaction of the Royal Society,* 72, 112-162.

Herschel, F.W., 1782b. On the parallax of the fixed stars. *Philosophical Transaction of the Royal Society*, 72, 82-111.

Herschel, F.W., 1785. Catalogue of double stars. *Philosophical Transaction of the Royal Society*, 75, 40-126.

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 Transaction of the Royal Society*, 93, 339-382.

Herschel, F.W., 1804. Continuation of an account of the changes that have happened in the relative situation of double stars. *Philosophical Transaction of the Royal Society*, 94, 353-384.

Herschel, J.F.W., 1833. On the investigation of the orbits of revolving double stars. *Memoirs of the Royal Astronomical Society*, 5, 171-222.

Hirshfeld, A.W., 2001. *Parallax: The Race to Measure the Cosmos*. New York, W. H. Freeman & Co.

Hoffleit, D., and Warren, W., Jr., 1991. *Bright Star Catalogue, 5th Revised Edition*. Astronomical Data Center, NSSDC/ADC; available also from http://vizier.u-strasbg.fr/vis-bin/Cat?V/50

Hoskin, M., 2003. *The Herschel Partnership, as Viewed by Caroline*. Cambridge, Science History Publications.

Howse, D., 1989. *Nevil Maskelyne: The Seaman's Astronomer*. Cambridge, Cambridge University Press.

Hughes, D.W., 1987. The history of Halley's Comet. *Philosophical Transaction of the Royal Society*, A323, 349-367.

Humboldt, A. von, 1851. *Cosmos: A sketch of a Physical Description of the Universe. Volume III.* London, Henry G. Bohn.

Jones, K.G., 1991. *Messier's Nebulae and Star Clusters*. Cambridge, Cambridge University Press.

Kaler, J., 2006a. http://www.astro.uiuc.edu/~kaler/sow/dahib. html

Kaler, J., 2006b. http://www.astro.uiuc.edu/~kaler/sow/algedi.html

Kopal, Z., 1986. *Of Stars and Men: Reminiscences of an Astronomer*. Bristol, Adam Hilger.

McCormack, R., 1968. John Michell and Henry Cavendish: weighing the stars. *British Journal for the History of Science*, 4, 126-155.

Michell, J., 1750. *A Treatise of Artificial Magnets; in Which is Shown an Easy and Expeditious Method of Making them Superior to the Best Natural Ones*. Cambridge, J. Bentham.

Michell, J., 1760. Conjectures concerning the cause, and observations upon the phaenomena of earthquakes; particularly that of the first November 1755 which proved so fatal to the City of Lisbon, and whose effects were felt as far as Africa, and more or less throughout almost all of Europe. *Philosophical Transaction of the Royal Society*, 51, 566-634.

Michell, J., 1767, An inquiry into the probable parallax and magnitude of the fixed stars from the quantity of light which they afford us, and the particular circumstances of their situation. *Philosophical Transaction of the Royal Society*, 57, 234-264.

Michell, J., 1784. On the means of discovering the distances, magnitudes, etc. of the fixed stars, in consequence of the diminution of the velocity of their light … *Philosophical Transaction of the Royal Society*, 74, 35-57.

Ochsenbein, F., and Halbwachs, J.L., 1987. Le Catalogue des Étoiles Les Plus Brillantes, *Bulletin d'Information du Centre de Données Astronomiques de Strasbourg*, 32, 83; a revised version including HIPPARCOS parallaxes has been taken from http://vizier.u-strsbg.fr/vis-bin/Cat?V/53A

Schaffer, S., 1979. John Michell and black holes. *Journal for the History of Astronomy,* 10, 42-43.

Sheynin, O., 1995. The introduction of statistical reasoning into astronomy: from Newton to Poincaré. In Taton, René, and Curtis Wilson, (eds). *The General History of Astronomy. Volume 2B. Planetary Astronomy from the Renaissance to the Rise of Astrophysics. Part B: The Eighteenth and Nineteenth Centuries*. Cambridge, Cambridge University Press, pp. 191-197,

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SOLAR PHOTOGRAPHY IN THE NINETEENTH CENTURY: THE CASE OF THE INFANTE D. LUIZ OBSERVATORY IN LISBON (1871-1880)

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Abstract: The Infante D. Luiz Observatory, located in Lisbon, was one of the leading Portuguese meteorologic and magnetic research institutions in the second half of the 19th century. Following the distribution of the equipment bought by the Portuguese government for the total solar eclipse expedition of 1870 December 22, the D. Luiz Observatory acquired an equatorial telescope. João Carlos de Brito Capello, one of the two Infante D. Luiz chief observers, seized this opportunity and decided, in early 1871, to embark in a programme of daily solar photography to study the relationship between the solar activity, in particular the sunspots, and the terrestrial magnetic field. The programme was active between 1871 and 1880, albeit intermittently, having been well received by the international community and led to a couple of publications. For a time the Infante D. Luiz Observatory solar photographs not only kept a record of the sunspot activity complementing similar work done elsewhere but were amongst the best available everywhere. This article proposes to give an account of its implementation and development in the context of the solar photography of the period.

Keywords: Portuguese Astronomy, Solar Photography, 19th century, Infante D. Luiz Observatory, João Carlos de Brito Capello

1 INTRODUCTION

In this paper we start by giving a short account of the early history of the Infante D. Luiz Observatory, before proceeding with an overview of the status of solar photography prior to 1871 and the techniques then in use. This will provide the background against which we will compare the research programme implemented at the Infante D. Luiz Observatory and the decisions made by João Carlos de Brito Capello, its main driving force. A detailed section concerning the execution of the programme from its beginnings in 1871 to its end approximately ten years later then follows. Finally, we will track the impact of this research on contemporaneous scientists and in the nineteenth century specialised literature.

2 THE INFANTE D. LUIZ OBSERVATORY

On 21 July 1853, sixteen years after its foundation (in 1837), staff at the Lisbon Polytechnic School decided to create Portugal's first meteorological observatory. From the beginning the observatory strategy was clearly defined. The new institution would have a research component in which an uninterrupted series of observations, as complete as possible, would be performed in a proper environment (Malaquias et al., 2005). Construction of the Observatory was concluded by the end of the summer of 1854, and the first published observations date from 1 October of that same year. Magnetic readings were taken from 1857 onwards. The original building was replaced in 1863 by a new one (see Figure 1), thanks to generous financial support from the Portuguese King, D. Luiz (Peixoto, 1987: 220). New equipment was purchased in England, specifically from the Kew Observatory, and one of the observers from the Lisbon Observatory, João Carlos de Brito Capello, went to London in 1863 where he was instructed in its use (Capello and Stewart, 1864).

Figure 1: The 1863 Infante D. Luiz Observatory building (after *Annaes do Observatório do Infante D. Luiz*, *Volume 1*, 1863).

The creation of the Infante D. Luiz Observatory must be viewed in the context of a wider international push for the study of meteorological and magnetic phenomena in the nineteenth century, which included the 1834 'Göttingen Magnetic Union', the 1853 Maritime Meteorological Conference in Brussels and the 1857 Paris International Meteorological Service (led by LeVerrier). Portugal was represented at all of the earliest meteorological conferences (Brussels 1853, Viena 1873, London 1874, and Rome 1879), and contributed, via the Infante D. Luiz Observatory, to the International Meteorological Service from 1857 onwards, just as soon as the telegraphic connection between Lisbon and Paris was established; it also joined the Magnetic Union in 1857. The *Annaes do Observatório do Infante D. Luiz* was published regularly beginning in 1863. From this date, João Carlos de Brito Capello (Figure 2) published several papers on the analysis of geomagnetic observations in the *Proceedings of the Royal Society* (of London) (see Capello, 1869; Capello and Stewart, 1864), and he also contributed to the 1865 and 1885 annual meetings of the British Association for the Advancement of Science (see Capello, 1876b; 1886).

Figure 2: Portrait of João Carlos de Brito Capelo (1831–1901) (after *O Occidente*, *Volume 24*: 100 (1901)).

Meteorological and magnetic studies are intrinsically co-operative sciences, if a global data analysis is to be successfully performed. Consequently, international meteorological observatories were involved in a network of data collection, analysis and exchange, mainly via observatory publications (see Malaquias et al., 2005). From the start, the Infante D. Luiz Observatory in Lisbon played an active role in these networks, primarily because of the quality of the instruments, the work performed, and the geographical location of the country. In 1878 the Director of the *Bureau Central Météorologique* in Paris wrote to Capello:

> I have written today to the Director General of the French telegraphic lines asking him to speed up the transmission of the Portuguese messages. These telegrams have for the meridional Europe the same importance as the English ones for northern Europe. (Mascard, 1878).

Thus, the Infante D. Luiz Observatory was in an ideal position to exchange ideas and information with other observatories, and surviving correspondence from this period (1870-1880) illustrates this: there are letters from scientists or institutions in Japan, Russia and the United States of America, as well as from several European countries. This situation turned out to be very useful during the development of the solar photography programme, as we shall see later.

3 SOLAR PHOTOGRAPHY FROM ITS BEGINNINGS TO 1870

The application of photography to the study of the natural sciences has a long history. On 19 August 1839, Arago made a public presentation about the daguerreotype at the French Academy, and *Comptes Rendus* contained the main passages of a report previously presented by Arago to the 'Chambre des Députés' in which he predicted the future use of the photographic technique in the fields of selenography, photometry and spectroscopy (Arago, 1839). The inevitability of this application was later recognised by Arago in the third volume of his *Astronomie Populaire*:

> The idea of applying the photographic processes of Nièpce and Daguerre to the reproduction of some scientific subjects was a natural one; it is then difficult to conceive that the persons that have published their projects in this respect may consider them something to be proud of. Claiming the priority of obtaining photographic images of the Sun and Moon, seems to me childish. (Arago, 1867: 469).

The first astronomical photographers concentrated, not surprisingly, on the two brightest objects in the sky, the Sun and the Moon, even though these involved quite different technical problems. Lunar photographs were constrained by the slow speed of the early photographic plates and therefore required long exposure times and good tracking mechanisms, while the Sun's brightness demanded very short exposures, i.e. fast shutters (see de la Rue, 1860). John William Draper obtained the first successful lunar photograph in 1840 (Brothers, 1866), and correctly-exposed daguerreotypes of solar features were obtained in the early 1840s. According to Arago (1858: 247), Fizeau and Foucault "… took a large number of solar photographs in 1844 and 1845 …", one of which, taken on 2 April 1845 at 9h 45m, still survives. The first photographs of the partial phase of a total solar eclipse and of the solar spectrum were also obtained in the 1840s. From this point on, the range of celestial objects exposed to photography gradually increased (e.g. see Bajac et al., 2000; de Vaucouleurs, 1961; Lankford, 1984; Mouchez, 1887; Pasachoff et al., 1996; Rayet, 1887).

In an 1849 communication to the French Academy, Hervé Faye implicitly proposed the continuous photographic observation of the Sun:

> If a solar image is formed in the daguerrotype plate ... the same measurements can be repeated later on and compared with contemporaneous ones ... The same procedure may be applied to the determination of the heliocentric sunspot co-ordinates ... (Faye, 1849).

Then on 24 April 1854 John Herschel wrote in a letter to Edward Sabine that

> I consider it an object of very considerable importance to secure at some observatory, and indeed at more than

> one, in different localities, daily photographic representations of the sun, with a view to keep up a consecutive and perfectly faithful record of the history of the spots. (Herschel, 1855).

This idea was expressed again later in the year at the Liverpool meeting of the British Association for the Advancement of Science. Herschel's solar observing plan was seized upon by Sabine and a grant was allocated to Warren de la Rue for the construction of the necessary photographic equipment for the Kew Observatory (Rothermel, 1993). After several trials, successful solar photographs were taken in 1858 (Selwyn, 1864), and the work was continuously executed from 1862 onwards. In all, 2,778 solar photographs were obtained with the Kew photoheliograph between 1862 and January 1872 (RAS Council Report, 1872). A series of papers dealing with the data analysis was subsequently published, most notably about a possible planets-sunspot connection (see Charbonneau, 2002).

Following the Kew example, other solar photographic programmes were started. At least as early as 1860, the Ely helioautograph operated by Professor Selwyn was taking daily solar photographs (Solar Physics Committee, 1889: 38), a work which was classified by Warren de la Rue (1863) as being "… extremely valuable." The Imperial Academy of Sciences of St. Petersburg also became interested in solar photography and ordered a photoheliograph from England. Although de la Rue provided the design—which was basically an improvement on the Kew instrument—the photoheliograph was actually built by the London firm of Dallmayer. The instrument was unpacked in Pulkowa in August 1864, coinciding with a visit by de la Rue to Russia (see de la Rue, 1864), and became operational, in Vilnius, in 1868. During the period 1868-1876, about 900 photographs of the photosphere were obtained, and the series was only terminated in 1876 because of a fire (Vilnius University - Astronomical Observatory, 2007).

The continuous study of the photosphere required observatories located at different longitudes and latitudes around to globe, in order to minimize the effect of unfavourable weather at any particular locality, and this scenario was outlined by Herschel in the aforementioned 1854 letter to Sabine:

> Three or four observations in tropical climates, distant several hours in longitude (suppose 3, at 8h distance in longitude), each recording at, or nearly at noon, would, when the results were assembled, keep up a continuous history of the solar disk. (Herschel, 1855).

We have an analogous situation today:

> The Global Oscillation Network Group (GONG) is a ground-based set of telescopes that are positioned so that, at one station or another, they are continuously monitoring the Sun. (Golub and Pasachoff, 2001: 70).

The hope of increasing the number and location of daily solar observatories appears in several occasions in the *Monthly Notices of the Royal Astronomical Society* during the 1860s. For example, in the Council Report of the Forty-sixth Annual Meeting we read:

> In addition to the establisment of a photoheliograph at Wilna [Vilnius], there is a prospect of the erection of a third at Quebec. If this hope is realized, there will then be a station in England, in Russia, and in America, by means of which, on account of the difference of longitude, we may hope to have an almost uninterrupted self-register of solar phenomena. (RAS Council Report, 1866).

Two years later, upon referring to the Kew photographs, de la Rue states:

> ... certainly a better climate would be desirable. There is a photoheliograph at Wilna [currently Vilnius], but it is not yet at work, and one at Melbourne would be very valuable. (RAS Council Report, 1868b).

There were only four observatories involved in taking daily solar photographs by the end of 1870: Kew and Ely in England; Vilnius in Lithuania and Harvard College in the United States of America. Elsewhere solar photographs were taken from time to time—for instance in France by M.L. Sonrel (Capello, 1871h) and in the United States by Lewis Rutherfurd (Rees, 1906)—but not on a regular basis.

3.1 Solar Photographic Apparatus

As is well known, a thin converging lens of focal length f_0 produces a linear image size, h_0, in the focal plane equal to

$$h_0 = 2f_0 \tan(\alpha/2) \qquad (1)$$

where α is the angular diameter of the object and the distance between the object and lens is infinite. From this equation it is clear that increasing the focal length, f_0, increases the focal plane image size. Consequently, a long focus telescope is preferable if large images are required. During the solar eclipse of 15 March 1858, which was visible from Paris, Porro obtained photographs of the Sun with a 15m focal length equatorially-mounted telescope (Faye, 1858).

A different approach, proposed by Herschel in 1854, was to use a medium-power equatorial telescope and photograph the Sun's image after amplification by a secondary lens. This was the system used in the Kew photoheliograph where the secondary amplification was provided by a Huyghenian eyepiece. The solar image produced by the first lens underwent an 8 times linear amplification by the secondary, and the final solar photograph that was obtained was 10 cm in diameter (de la Rue, 1860: 150).

A third option would involve securing a small prime focus image followed by a posterior enlargement, but:

> Even the portraitists tried to dissuade their costumers from having carte de visite and cabinet negatives enlarged, unless they were going to be painted over. The definition was low, the contrast even lower, and the potential profit hardly worth the time involved. (Hannavy, 1997: 54).

That is, if the initial solar image was not 'big enough', any enlargement was an unpractical proposition. Enlargements were nevertheless made to magnify solar surface characteristics like individual sunspots or if short exposures were needed.

During the 18 July 1860 solar eclipse Laussedat utilised a new approach to solar photography by using a fixed horizontal telescope combined with a Silberman heliostat (Laussedat, 1860b), and several photographs of the partially-eclipsed Sun were obtained. On 6 March 1867 there was an annular solar eclipse, and Laussedat tried again a similar apparatus in Italy but without success (Laussedat, 1868). In the papers that were presented to the *Académie des Sciences de Paris* (Faye, 1870; Laussedat, 1860a; Laussedat 1860b), there is no evidence that the telescope used was a long focus one. What is certain is that at Harvard on 4 July 1870 four photographs were taken with a forty foot focal length and a 4-in aperture lens made by A. Clark & Sons placed horizontally with independent supports for the movable unsilvered plane mirror, lens and photographic apparatus. In 1870, a newer lens corrected for the 'chemical rays' from the same maker accompanied the solar eclipse expedition to Spain (Searle, 1876: 40).

During the 1870s the main photographic apparatus used in solar photography was either a medium focal length equatorially-mounted telescope with amplifier (as proposed by Herschel and used at Kew, Vilnius and later on in Lisbon and Greenwich) or a horizontal long focus configuration without amplification (as used in Harvard College). Both approaches were used in the 1874 transit of Venus observations. The British, German, Dutch and Russian preferred the Kew model, while the French and Americans used the horizontal long focus telescopes (see Sheenan and Westfall, 2004: 245).

3.2 Photography Versus Drawing

The debate about the advantageous use of astronomical photography over drawing was not solved when the first celestial images were obtained with this new technology. Each new technical advance had to be thoroughly tested before it could be accepted, but there were early converts for whom photography will "… suppress the unfaithful eye of the observer." (Faye, 1849). Solar physics is a particular good example of a field in which the two different techniques coexisted for several decades. While some important early scientific results were obtained using photography—like limb darkening in the 1840s and the nature of prominences in 1860 (Meadows, 1970)—visual observations and drawings also contributed major advances, like Schwabe's 1843 sunspot cycle period and Carrington's (1863) investigation of differential solar rotation. In the 1868 "Statement of the Work Done at the Kew Observatory with the Heliograph" which was presented at the Forty-eighth Annual General Meeting of the Royal Society we find two examples of this ambiguity. In the first example, the heliographical elements obtained from the Kew photographs "… may in a measure be regarded as a continuation of Mr. Carrington's results …", and in the second one, area measurements of Schwabe's solar drawings will be used to establish a more trustworthy curve of periodicity (see RAS Council Report, 1868a).

Following observations in Sicily of the solar eclipse of 1870, steps were taken the following year to found the Società degli Spettroscopisti Italiani (Bònoli, 1998: 21). In the list of tasks to be performed, drawings of prominences and of the chromosphere were listed—as tasks 2, 6 and 9 respectively (Tacchini, 1872b). Nowhere are photographs mentioned in connection with the proposed solar program, a stance that draws criticism from Faye in his report on the new society to the Paris Academy:

> Concerning the relationship that might exist between the faculae, sunspots and the chromosphere that the Italian Society rationally plans to study, I believe that simple drawings executed by projection onto a screen are not enough today. (Faye, 1872a).

These comments provoked a reply from one of the Society's founders, Pietro Tacchini:

> That photography might be employed in an establishment with advantage is something I believe myself, but to study the relation between the faculae and the prominences, I believe that the results I have obtained by comparing the measured position angles and the positions of the prominences would be the same if I had used solar photographs, assuming that the photographs can reproduce exactly what is visible by projection, something I am not sure about … (Tacchini, 1872a).

In a counter reply Faye states that

> ... photographic observation which does not forget and does not exclude the visual observation is infinitely preferable in all situations. (Faye, 1872b).

This declaration somewhat conceals Faye's true position. In the previously-quoted paper (Faye, 1872a), a more pragmatic approach concerning the possible roles played by the two techniques is presented. Drawing as the only available solution would be used to register the spectroscopic images of the chromosphere while the record of the photosphere would be pursued photographically. The situation of solar photography versus drawing is, we believe, clearly summarised by Young (1881: 57) in his book *The Sun*:

> The character of the picture produced depends very greatly upon the proper timing of the exposure ... This circumstance detracts considerably from the value of the photographic method. The skillful draughtsman can show in the same picture details differing to any extent in intensity, while the photograph is, so to speak, limited to the reproduction of only one certain class of details at a time. Still we can always be sure that, whatever a photograph does show, is an autographic representation of fact, and not a figment of the imagination. This is not the case with drawings; for it is remarkable how widely two conscientious artists will differ in their representations of the same object, seen by both with the same telescope, and under the same circumstances. As an accurate record of the number, position, and magnitude of the solar spots at any given time, the photograph is, of course, unexceptionable.

In a letter to the French Academy, Father Angelo Secchi (1872) states:

> I am actually engaged in discussing the relationship between these two phenomena (sunspots and prominences) using the drawings made during the year. The comparison of these drawings with the fine photographs of Mr. Capello has convinced me that our drawings, without attaining the perfection of the photographic images, might be useful to science.

This judgement was expressed by a renowned scientist with practical experience in astronomical photography. Secchi photographed the Moon in the 1850s, obtained photographs of the partial solar eclipse of 28 July 1851 and was involved in the famous 18 July 1860 solar eclipse photographs obtained in Spain by José Monserrat (Gasparini, 1999).

A good example of the quality and detail attained using the drawing technique is the remarkable drawings of sunspots made by Samuel Pierpoint Langley (see Figure 3). George Ellery Hale is quoted saying that "… in the best views of sunspots he has ever had, the better they were seen, the more nearly they appeared as shown in Langley's drawings …" (Abbot, 1906). Meanwhile, Wittmann (2000: 86) presents an enlightening comparison between an 1873 Langley sunspot drawing and a modern CCD image.

Not surprisingly, the new technology—photography—did not immediately replaced the 'old' one—drawing. Instead both techniques co-existed for a long time, either in 'competition' or by complementing one another (see Pang, 1995, 1997; Tucker, 2005).

4 SOLAR PHOTOGRAPHY AT THE INFANTE D. LUIZ OBSERVATORY

During the nineteenth century the meteorological observatories kept expanding their measuring capabilities as time went by. At the Infante D. Luiz Observatory, for instance, magnetic measurements were introduced in 1857 and quantification of the atmospheric electrical potential in 1877 (Peixoto, 1987). Following the establishment of a relationship between solar activity and the Earth's magnetic field in the 1850s by John Lamond, Edward Sabine, Richard Carrington and Richard Hodgson (Dewhirst and Hoskin, 1997: 265), several observatories included the study of solar activity alongside that of geomagnetism and meteorological phenomena. Two well-known cases were the Kew Observatory (between 1858 and 1872) whilst operated by the British Association for the Advancement of Science, and the Collegio Romano in Rome, which was under the directorship of Father Secchi (Proverbio and Bufoni, 2004).

Those at the Infante D. Luiz Observatory were aware of these trends, for the importance of studying the relationship between sunspots and terrestrial magnetism was stressed in a 1861 report about magnetic work done at the Observatory and presented to the *Academia Real das Sciencias de Lisboa* (Lisbon Royal Academy of Sciences; see Silva, 1861). Photography was also performed at the Observatory, with self-registering instruments. In fact, "... Senhor Capello resided there [at Kew Observatory] for some time [in 1863] in order to become acquainted with the photographic processses." (Capello and Stewart, 1864). It is highly probable that during his visits to Kew Observatory, Capello became acquainted with the Kew solar programme. Close scientific links existed between the Portuguese Observatory and the Kew Observatory from the early 1860s onwards, and the Infante D. Luiz Observatory was one of the first to install Kew magnetographs, in 1863 (Malaquias et al., 2005). Following this event, research papers were published in collaboration with Balfour Stewart, who at the time was the superintendent of the Kew Observatory, and the scientific collaboration between Capello and Stewart survived Stewart's move to Owens College in Manchester in 1870, and continued throughout his life. For instance, in his 1885 report "Suggestions for the Committee [of the British Association for the Advancement of Science] on Magnetic Reductions", Stewart (1886: 68) writes:

> The following suggestions are founded on the methods proposed by several magneticians ... To Senhor Capello I am especially indebted for the trouble he has taken in explaining his views, with which these suggestions are almost identical.

Nevertheless, while they were aware of the new photographic and spectroscopic techniques, no Portuguese scientists tried to apply them in an astronomical context prior to 1870. The breakthrough only occurred in 1870, thanks to the solar eclipse of 22 December (see Bonifácio et al., 2006a).

4.1 The 22 December 1870 Total Solar Eclipse

The path of totality of the total solar eclipse of 22 December 1870 crossed the southern part of the Portuguese continental territory. Local scientists seized upon this opportunity, and an eclipse expedition was prepared with Government support and involving all of the Portuguese meteorological and astronomical observatories (Bonifácio et al., 2006a). Unfortunately, bad weather thwarted the sizeable effort made in preparing and equipping the expedition, and no results were obtained from the Portuguese station located at Tavira (Algarve). Nevertheless, this eclipse facilitated the acquisition of new equipment, and it also introduced Portuguese scientists to astronomical photography and spectroscopy. Both of these techniques were learned and experimented with prior to the eclipse. Following the eclipse, and in accordance with a recommendation by the eclipse commission, the Government decided to distribute the new equipment among some of the scientific institutions involved in the expedition (Folque, 1871).

Figure 3: Langley's drawing of the 23-24 December 1873 sunspot (after Young, 1881).

The 1870 eclipse also led to several attempts to start up solar physics in Portugal, but an important unsolved point remains: were these later aspirations a direct consequence of the eclipse experience and the possibilities opened up by the new equipment, or was the entire eclipse endeavour planned from the start with the objective of broadening the scope of astronomical research in Portugal? Doubts arise because the main protagonists, Luiz Albano and Brito Capello, both belonged to the eclipse planning commission and were observers at the Algarve station (Freire, 1872).

Albano was based in Coimbra, and was Professor of Practical Astronomy in the Faculty of Mathematics and second astronomer at the Coimbra Observatory. Although he received a photoheliograph and started teaching astrophysics, the implementation of a serious scientific research programme was not possible in a cramped understaffed Observatory constrained by its duties in classical astrometry (i.e. elaboration of the Coimbra Ephemerides), the complex rigid organisational structure and the economic difficulties the country found itself in at that time (see Bonifácio et al., 2006b).

Following Capello's involvement in the solar eclipse expedition, in early 1871 the Infante D. Luiz Observatory received a 12 cm aperture, 1.98 m focal length refractor by Repsold, with optics and a clockwork drive by Merz.

4.2 The Daily Solar Photography Research Project

In the beginning of 1871 Capello stated that in order to

> ... study the relationship between the sunspots and the magnetic perturbations we will perform both visual and photographic observations of the sunspots especially during the strong perturbations. (Capello, 1871e).

This statement was repeated during 1871 to different correspondents, including Father Secchi and Warren de la Rue.

Photography was preferred as the chosen image-recording medium even if initially that was before experimentation, but a mixed approach was considered best:

> I will try to photograph enlarged sunspots directly using the strongest eyepieces of the telescope ... If I am not able to do so I will try to draw the different sunspots and prominences while their positions will be established from full-disk photographs. (Capello, 1871f).

Later, in a letter to Faye, the choice of the photographic medium over drawing is explicitly made:

> You are right the use of photography is incomparably more exact and less tiring than drawings made by hand. (Capello, 1872i).

As far as equipment was concerned, the equatorial telescope provided by the Government implied that an approach similar to the one used at Kew Observatory would be adopted. This involved making minor alterations to the Repsold refractor and converting it into a photoheliograph.

4.3 The Beginning: 1871

In February 1871 the Director of the Infante D. Luiz Observatory, Fradesso da Silveira, submitted requests to the Government for funding (Pereira, 1871) and to the Polytechnic School for authorisation to construct a modest building to house the photoheliograph on the grounds of the school's planned Botanical Garden (Corvo, 1871). Simultaneously, Capello was busy gathering information from his network of correspondents. Scientific queries and requests for publications went far afield, and in February 1871 he wrote to Kew:

> I am asking you if at Kew you have tried to enlarge the sunspots seen in the solar photographs and if you were successful. I would like to know some details concerning Mr. Carrington's sunspot drawings: what was the diameter of solar drawings where all the groups of sunspots were represented & did Mr. Carrington make separate drawings of the outstanding sunspots and in that case what was the size of these sunspots in comparison with those in first drawing which contained all of the sunspots? ... Would it be possible to send me an original or a facsimile? Another question: I would like to have all the publications about the Kew sunspots by Mr W. de la Rue, Stewart and Loewy. (Capello, 1871c).

In an April 1871 letter to Father Secchi, Capello (1871j) presents his research plan, asks several technical questions and requests printed materials. Later that same year his plans and queries, coupled with requests for solar photographs, were sent to de la Rue in London (Capello, 1871g) and to M.L. Sonrel in Paris (Capello, 1871h).

At the same time technical tests were being performed. Initially Capello was using grey density filters to reduce the excessive solar radiation, but their frailty (they tended to break) led him to experiment with a glass with parallel surfaces (Capello, 1871c). Finally, he ordered a Herschel eyepiece from Kew (Capello, 1871b). In August a photographic workshop was installed near the photoheliograph building, and photographs of the Moon and the Sun were obtained. While poor tracking hampered the lunar photographs, the solar efforts were more successful, but they still needed improvement, which Capello thought could be obtained by using shorter exposure times. Shutter apertures of 1.5 mm and 12 mm were used for the whole-disk photographs and for enlarged sunspots, respectively. Exposure times were not indicated (Capello, 1871a). September was not very suitable for photography owing to adverse weather conditions, but by October the 'chemical' (or photographic) focus was established to be 6-7 mm longer than the visual one, and the first results began to appear:

> The 13 October image of the entire Sun seems very sharp and it is hardly possible to make the sunspots better defined, even though the amplifying lenses were not specially made; one belongs to a microscope and the other which is achromatic is a short focus eyepiece, 12 cm, from a small telescope. (Capello, 1871a).

The quality Capello attributed to the 13 October photograph may be inferred from the fact that he sent it to Secchi, de la Rue and the Paris and Kew Observatories (Capello, 1871a; 1871d; 1871g; 1871i). Capello's earliest known published photograph dates from 30 December 1871, and it appeared in the 1870-1871 Infante D. Luiz Observatory Service Report (Observatorio de Infante D. Luis, 1872). Subsequently, complimentary replies were received from Secchi ("The full Sun's image is of an admirable precision. It will be difficult to do better …") and from Marié Davy, head of the Paris Observatory's Meteorological section (ibid.).

4.4 The Year of Confidence: 1872

According to Capello by March 1872 the definition of the Portuguese Observatory's solar photographs was better than ever before, allowing the observation of the 'willow leaves' or 'rice grains' (granulation) on the solar surface (Capello, 1873b). The difficulty of this achievement may be ascertained by the fact that when Janssen succeeded in photographing these same features in 1877 he announced them at a meeting of the *Académie des Sciences de Paris* (Janssen, 1877a). In a later communication, Janssen (1877b) claimed the 'rice grains' had not previously been photographed, a statement that was promptly contested by Lewis Morris Rutherfurd. He stated that his solar photograph of 11 August 1871 that was presented to the Royal Astronomical Society showed the 'rice grains' (Rutherfurd, 1878), although it was of a poorer quality than Janssen's result. So it would seem that Capello's photographs can be compared favourably with the best contemporaneous efforts available elsewhere, even if some information was lost in the negative-to-positive 'translation':

> The nucleus is crossed by filaments seen perfectly on the negative; they are difficult to print on paper ... we have the same difficulty in printing the faculae. (Capello 1872e).

In April, stimulated by the description of the newly-formed *Società degli Spettroscopisti Italiani*, Capello (1872i) writes to the Secretary of the French Academy and reports "… our modest solar physics progresses …", even if "… the service is not totally organised. It lacks personnel and funds to function regularly." Several photographs accompanied the two letters sent to Hervé Faye. As a consequence, a note describing the Infante D. Luiz Observatory solar photographic programme subsequently appeared in the *Comptes Rendus*, where Faye took the liberty of elaborating upon Capello's comments about petitioning the Portuguese Government:

> We hope that the Portuguese government will want to take advantage of the fair Lisbon weather as well as of the skills already acquired by the Infante D. Luiz observers and will allow the scientific world to count with a vast harvest of important documents for the history of the solar physics. (Capello, 1872i).

From the start, the Lisbon climate was a distinct advantage:

> Since April 1st I took photographs of the solar disk in all clear sky days; during May, I only missed four or five days and none in June. (Capello, 1872e).

In July, the wet collodion response to the summer heat, plate manipulation and possible distortions of the optical apparatus were discussed with Secchi (Capello, 1872a), and the following month a Dallmayer eyepiece was ordered from London—through the good will of Warren de la Rue (Capello, 1872c). Meanwhile, Carrington's book, *Observations of Spots on the Sun from November 9, 1853 to March 24, 1861 Made at Redhill*, was bought via George Whipple (Capello, 1872b). Capello would later analyse the observations following Carrington's method (Capello, 1872h). Having seen a 9 August drawing of the solar photosphere, published in the 19 August issue of *Comptes Rendus* (Cheux, 1872), Capello (1872h) quickly sent the French Academy solar photographs for 8, 9, 10 and 11 August, and these were published in the 23 September issue (e.g. see Figure 4).

During this period Capello increased the definition and amplification of his photographs. Table 1 reveals his growing confidence in successfully enlarging photographs of sunspots.

In April Capello considered the possibility of obtaining "… an enlarged sunspot from the small photographs." (Capello, 1872g; 1872f), and by November results had been obtained (Capello, 1872j). This search for better and more enlarged positives might explain the existence of four unsigned sunspot photographs, corresponding approximately to an 88 cm diameter Sun and dating 9 and 11 November 1872, 17 December 1872 and 27 January 1873 in the Library of the *Instituto Geográfico Português* (Portuguese Geographic Institute, hereafter IGP) (see Figure 5). The IGP was the continuation of the 'Direcção Geral dos Trabalhos Geodésicos Topográficos, Hidrográficos e Geológicos do Reino' (the nation's General Direction of the Geodesic, Topographic, Hydrographic and Geological works) whose Photographic Section, run by José Júlio Rodrigues, was in the 1870s a centre of excellence in the field of photographic reproduction. Rodrigues was a Professor at the Lisbon Polytechnic School and we know that in 1875 Capello reproduced an engraving through the Photographic Section. If the identification of these photographs is correct, they are the only surviving unpublished enlarged sunspot photographs known that were taken at the Infante D. Luiz Observatory.

From his experiments Capello concluded:

> The principal problem affecting the sunspot enlargements and one, which I believe is beyond solution, is due to the agitation of the hot air; the sunspots are quite good if the image on the screen is stable; if the image is dancing the photograph will be blurred. It will take a very sensitive collodion. During winter the air is less agitated and I took several sunspots with lots of detail. (Capello, 1872d).

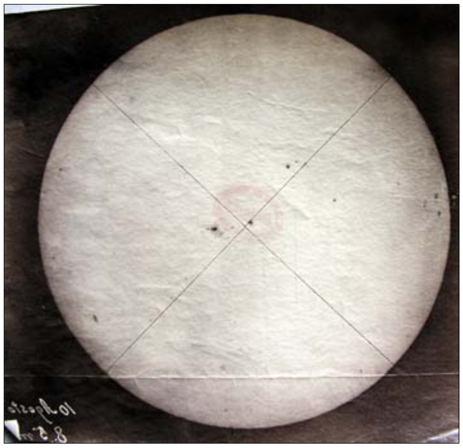

Figure 4: Capello's 10 August 1872 solar photograph (courtesy: *Académie des Sciences de l'Institut de France*; Note manuscrite de M.J. Capello conservée dans la pochette de séance du 23 Septembre 1872: "Sur l'aspect du Soleil vers le 9 Août").

Table 1: The Sun's equivalent diameter, *D*, as a function of time, derived from different sunspot photographs. *D* represents the Sun's diameter if the entire solar disk is photographed with the same magnification as the sunspots.

Date of photographs	*D* (cm)	Reference
August 1871	41.2-63	(Capello 1871a)
August 1871	32-65	(Capello 1871g)
October-November 1871	38.2-57.5	(Capello 1871d)
December 1871-January 1872	89	(Capello 1872k)
February 1872	72	(Capello 1872f)
April 1872	77	(Capello 1872l)
May 1872	90	(Capello 1872e)

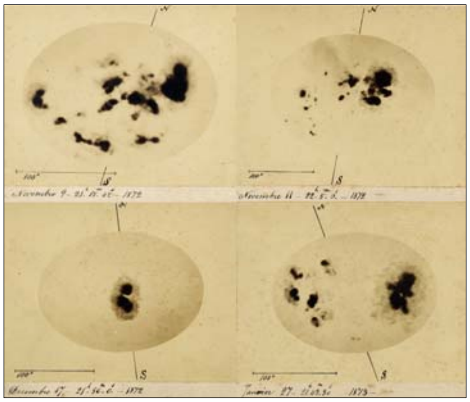

Figure 5: Four sunspot groups photographed on 9 and 11 November 1872, 17 December 1872 and 27 January 1873 (courtesy: *Instituto Geográfico Português*, F101F, F102F, F103F, F104F).

Once again the proposed solution involved a faster collodion i.e. shorter exposure times avoiding the Earth's atmospheric blurring effect. The photographic work continued throughout 1872, even though a compelling distraction—the 1874 transit of Venus—started to divert some of Capello's attention.

4.5 The Transit of Venus Hiatus: 1873-1874

An event that led to a surge of interest in solar photography was the 1874 transit of Venus. Astronomers waited for decades for the possibility of making this observation. Not surprisingly preparations started several years before 1874. At the time there was a widespread consensus amongst the astronomical community that the photographic observation of the transit would provide the necessary unbiased data for the exact determination of the Astronomical Unit.

The precision required demanded answers to very specific questions concerning the properties of the different photographic media, their corresponding techniques and the stability of the exposed plates. Wet, dry collodion and even the old daguerreotype were all tested with regard to durability, practicability and, most importantly, if any deformation occurred between the exposure and the final image. In his 1872 address to the British Association meeting in Brighton, de la Rue (1873) stated that "… in such observations as that of the transit of Venus, no refinement or correction ought to be neglected.", a sentence we know now to be prophetically true. The precision of the plate measuring turned out to be the weak link of the transit data analysis, even when care was taken with the machines that were employed for the purpose. While theoretically the idea seemed feasible, the lack of results from the 1874 transit of Venus is a stark reminder of the difficulty of carrying out experimental work in extraordinary circumstances.

The Portuguese astronomers did not manage to escape the transit of Venus 'fever' and as early as 1872 were planning a national expedition. In the summer, Capello was already writing to de la Rue asking for information concerning the British transit of Venus programme and sharing the still vague idea of a possible Portuguese observing station in the Far East, probably in Macao (de la Rue, 1873). We believe that some of the photographic experiments done in 1872, like the previously-mentioned photoheliograph distortion tests, were connected with the planned transit of Venus observations. This trend continued in 1873. For instance, Capello (1873a) tried the 'Uranium dry plates' developed by Colonel Stuart Wortley. The Infante D. Luiz Observatory's photoheliograph had to be altered to prepare it for the transit observation. With only one instrument available, this meant that the daily solar photographic work was interrupted from September 1873 (Capello, 1873a). Despite an initially favourable Government response to the Portuguese observation plans, the promised funds did not materialize and the expedition had to be cancelled (Campo, 2005).

4.6 The Decline: 1874-1880

After this disappointment, the solar photographic work resumed in February 1875 (Capello, 1875), but several factors contributed to the programme slowly fading away. The sunspot cycle was progressing from its 1870 maximum to its 1878 minimum (der Linden and the SIDC team, 2007) and Capello (1875) noted: "In this part of the year [i.e. February to August] I have seen only one remarkable sunspot." Later, on 10 March 1876, Capello (1876a) wrote to Secchi: "In these last times, 1875 and 1876, the sunspots and faculae are extremely rare." Since Capello seemed to be particularly interested in the problem of sunspot enlargements, the absence of sunspots or interesting sunspot groups for days at a time was far from encouraging.

From the documents consulted we conclude that the Portuguese solar photography project depended very heavily on Capello's efforts. From the start we find complaints concerning the need for more personnel and financial assistance. In 1871 Capello wrote of "My plan (if I am able to carry it out ... due to all the varied works I have) …" (Capello 1871a), and while the increased workload was taken over by Capello sometimes the strain involved appears in his correspondence. In an 1872 letter to Secchi he wrote: "… we have not yet obtained the necessary funds for the work and we lack personnel. At present I am alone in this need and I dedicate one hour and a half every morning to the work and I prepare the [photographic] baths during the night." (Capello, 1872g). We could not find any evidence of an increase in the staff of the Infante D. Luiz Observatory during the 1870s, to alleviate this problem. To make matters worse, on 30 April 1875 Capello was appointed Director of the Observatory, following the untimely death of Silveira (Ferreira, 1940). Obviously, this new position brought with it increased responsibilities.

Nor did the instrumental set-up at the Observatory change significantly during the 1870s, as the same 'old' equatorial was used, albeit with different eyepieces. If the main interest was to keep a consistent daily record of the solar photosphere this would not be a problem. For instance, the Kew photoheliograph was kept in the same state to avoid possible instrumental variations. But we have seen that Capello was very interested in experimenting, with the goal of obtaining the highest possible definition and magnification of the sunspot groups. In this sense, his equipment was slowly becoming outdated.

During this period at least two potential developments could have changed this *status quo*. In 1872, a 15-inch equatorial by Merz—still large by world standards—was to be installed in the Real Observatorio Astronomico de Lisboa (Royal Lisbon Astronomical Observatory) and Capello (1872a) wrote: "I possibly will have the opportunity to take a photograph with that large objective." In fact, the installation of the equatorial took longer than expected, and the instrument was not operational until 1876 (Raposo, 2006). Then in 1877, an 11-inch Alvan Clark refractor specifically dedicated to photography—the largest of this kind in Europe—was expected at the new Observatório Astronómico da Escola Politécnica de Lisboa (Lisbon Polytechnic Astronomical Observatory), founded in 1875 following the closing down of the Observatório da Marinha (Navy Observatory) in 1874, but for some unknown reason it never arrived in Lisbon (Silva, 1996). It therefore became impossible for Capello to compete in solar photography, especially after the breakthrough attained by Jules

Janssen in 1876 (see Janssen 1876). According to Young (1881: 59), Janssen "... has carried solar photography to a point far beyond any previous attainment …", a statement with which Capello agreed: "I have seen M. Janssen's fine work at Meudon I am much in doubt as to its being worth the trouble to take pictures as I did in former years." (Solar Physics Committee, 1882: 239).

We believe that the combination of these different factors contributed to the end of the solar photography programme at the Infante D. Luiz Observatory. In 1881, when responding to a circular from the Solar Physics Committee, Capello wrote:

> With reference to the collection and publication of sunspots, I would acquaint you that I possess a certain number of negative plates of the sun, about 4 inches diameter, taken during the years 1872 (the end), 1873, 1874 [we believe this is a printing error and that the correct date is 1875—see Capello (1875)], and some which are more recent. (Solar Physics Committee, 1882: 239).

While we know that these photographs cannot represent the entire output of the programme since there is no doubt that successful photographs were taken in 1871 and that several were damaged over the years (Capello, 1883), one might be tempted to conclude that no significant solar photography occurred at the Infante D. Luiz Observatory in the years 1876-1879. We did not find any reference to solar photographs being taken after 1880, and we consider this year to mark the end of the programme.

5 THE INFANTE D. LUIZ OBSERVATORY'S SOLAR PHOTOGRAPHY PROGRAMME AND THE NINETEENTH CENTURY LITERATURE

To our knowledge only two scientific research papers, both published in the widely-read *Comptes Rendus*, resulted directly from the Infante Infante D. Luiz Observatory's solar photography programme (see Capello, 1872i; 1872h). The real impact of the Observatory's photographs is difficult to assess but we believe its profile was increased by two factors. Firstly, there is the extensive network of contacts that Capello established (see Figure 6), which allowed the photographs to become known to a large number of important scientists and institutions.

Secondly, we believe that the quality of the photographs made them more noticeable, as can be ascertained by the flattering remarks published by Father Secchi in *Comptes Rendus* and from the medal of merit that was awarded at the 1873 Vienna Universal Exhibition (Silveira, 1874: 157). This visibility also led to an invitation from the *Societé Française de Photographie* for Capello to exhibit "… the interesting work done at the observatory …" in the 1874 Paris *Exposition Universelle de Photographie* (Koziell 1874), but Capello (1874) was forced to decline because he was busy preparing for the 1874 transit of Venus. We believe it significant that the Solar Physics Committee (1882: 231) included Capello in their shortlist in 1880 when they were seeking "… to communicate with men eminent in solar inquiry, with a view of obtaining suggestions and ascertaining to what extent they might hope for help …"

In a non-exhaustive search of the nineteenth century astronomical literature, we found the following references to the Infante D. Luiz Observatory's solar photography programme:

- 1872, photographs published in the *Comptes Rendus de l'Académie des Sciences* (Capello, 1872h).
- 1877, Secchi, *Le Soleil*, second edition, first volume: "In the observatories where it is possible to sacrifice a telescope to this work one adopts solutions that make it easier: for example, one can enclose the eyepiece in a dark chamber fixed to the end of the telescope: that is, the method used at Kew, Lisbon and elsewhere." (Secchi, 1875a: 42).

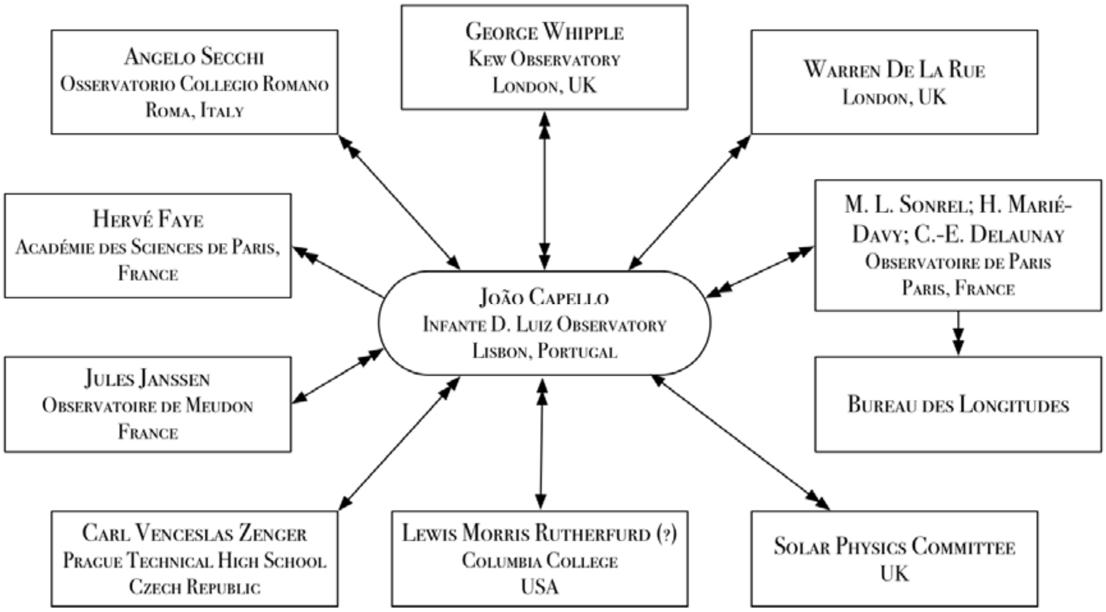

Figure 6: Diagram constructed from the *Biblioteca do Instituto Geofísico* correspondence volumes illustrating Capello's solar photography communications. Single arrows indicate letters that were sent, double arrows show that photographs were also exchanged. None of the photographs sent to Lisbon seem to have survived. Note: The identification of Lewis Rutherfurd is based on a single ambiguous Capello draft letter. This is the reason why we use a question mark in front of his name.

- 1877, Secchi, *Le Soleil*, second edition, second volume reproduces the sunspot group photographed on 23 April 1872 (Secchi, 1875b: 188)—see Figure 7, overleaf.
- 1878, Radau, *Revue des Deux Mondes*: "Mr. Dallmayer was responsible for building the Wilna [Vilnius] and Lisbon photoheliographs, where the instruments are functioning regularly." (Radau, 1878).
- 1887, Rayet, *Notes sur L'histoire de la Photographie Astronomique*: "Photoheliographs similar to the Kew one were several years ago installed in Vilna [Vilnius] and Lisbon." (Rayet, 1887: 879).
- 1896, Janssen, *Annales de l'Observatoire d'Astronomie Physique de Paris*: "The Kew solar photographic work was the starting point for similar programmes organised in Lisbon, Wilna [Vilnius], and after that in several other places." (Janssen, 1896: 32).
- 1897, Scheiner, *Die Photographie der Gestirne*: "Instruments build in a similar fashion [to the Kew photoheliograph] exist in Wilna [Vilnius] and Lisbon." (Scheiner, 1897: 268).

Not surprisingly, the Portuguese mathematician, Rudolfo de Guimarães (1909: 96) wrote that Capello's "… sunspot studies granted him a universal reputation."

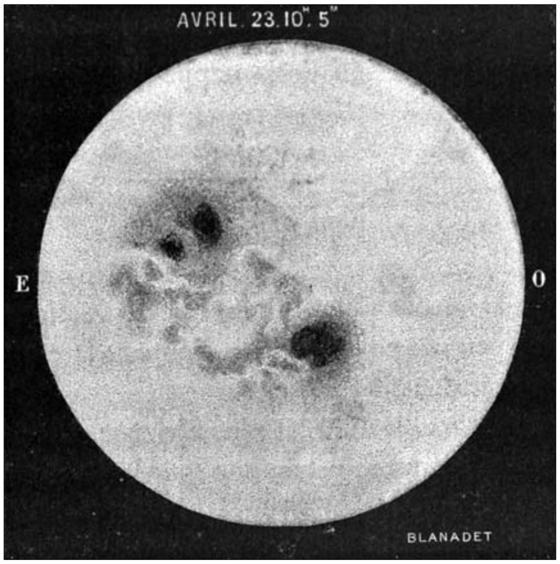

Figure 7: Capello's enlargement of a sunspot group, taken on 23 April 1872 (after Secchi's *Le Soleil*, *Volume 2*, 1878: 879).

6 CONCLUSIONS

Taking advantage of the Government's gift of a quality equatorial telescope to the Infante D. Luiz Observatory, the institution's prior involvement in geomagnetic work and the support of his Director and the Polytechnic School, João Carlos de Brito Capello decided to embark on a 'hot' research topic, the relationship between solar activity and the Earth's magnetic field. The choice of photography as the medium to register the solar photosphere placed the Observatory in a very restricted club of institutions. The analysis that Capello wanted to perform on the collected data is not completely clear. We have not found statements concerning the planetary-sunspot hypothesis pursued at Kew in the 1860s and early 1870s or the possible physical causes behind the solar activity-terrestrial magnetism connection. Rather, the information we can deduce from the available documents indicates, on one hand, that Capello did not expect a short-term response to the complex Solar-terrestrial connection problem, which is not surprising for someone with a meteorological background, while on the other hand, his pursuit of high-definition enlarged photographs of the solar surface, and in particular of sunspots, may have been connected with the hope of inferring information not available to naked-eye observers. This is a plan that was not dissimilar to one followed by Jules Janssen a few years later, which did produce well-known results.

The fate of the Portuguese solar photography project rested on the shoulders of just one man, João Carlos de Brito Capello, and the programme met its demise in about 1880 after he experienced ten years of increased responsibilities, stress, equipment short-falls and limited financial support. When at its peak, the Infante D. Luiz Observatory solar programme produced first-class results, and was recognised by its peers as a leading institution in this field. Later references in the works of several important scientists, including Angelo Secchi and Jules Janssen, provide a sparkling reminder of the programme's overall achievements.

7 ACKNOWLEDGEMENTS

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8 REFERENCES

Abbot, C.G., 1906. Samuel Pierpont Langley. *Astrophysical Journal*, 23, 271-283.

Arago, F., 1839. Le daguerréotype. *Comptes Rendus de l'Académie des Sciences de Paris*, 9, 250-267.

Arago, F., 1858. *Oeuvres de François Arago, Volume10*. Paris, Weigel.

Arago, F., 1867. *Astronomie Populaire, Volume 3*. Paris, L. Guérin.

Bajac, Q., and Saint-Cyr, A. (eds.), 2000. *Dans le Champ des Étoiles. Les Photographes et le Ciel 1850-2000*. Paris, Éditions de la Réunion des Musées Nationaux.

Bonifácio, V., Malaquias, I., and Fernandes, J., 2006a. The 1870 Portuguese solar eclipse expedition - a preliminary report. In Afonso, J., Santos, N., Moitinho, A., and Agostinho, R. (eds.). *2005: Past Meets Present in Astronomy and Astrophysics, Proceedings of the 15th Portuguese National Meeting*. Singapore, World Scientific. Pp. 89-92.

Bonifácio, V., Malaquias, I., and Fernandes, J., 2006b. Trying to start up the astrophysics research in Coimbra in the 1870's. The difficult move from the classroom to the Observatory. In Breda, A., Duarte, R., and Martins, M. (eds.). *Proceedings of the International Conference in Mathematics, Sciences and Science Education*. Aveiro, Universidade de Aveiro. Pp. 7-15.

Bònoli, F. (ed.), 1998. *L'astronomia in Italia*. Napoli, Arte Tipografica.

Brothers, A., 1866. Celestial photography. *Astronomical Register*, 4, 25, 34-38, 62-66.

Campo, A., 2005. Campos Rodrigues. *Revista da Armada*, 383, 19.

Capello, J., 1869. On the reappearence of some periods of declination disturbance at Lisbon during two, three, or several days. *Proceedings of the Royal Society*, 17, 238-239.

Capello, J., 1871a. Brouillon de lettre à Angelo Secchi. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1871b. Brouillon de lettre à G. Whipple, écrit Mars. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1871c. Brouillon de lettre à l'Observatoire de Kew, écrit Février. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1871d. Brouillon de lettre, probablement envoyé à l'Observatoire de Paris. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1871e. Brouillon de lettre, 1871. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1871f. Brouillon de lettre, écrit Mars. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1871g. Brouillon de lettre à Mr. de la Rue, sans date. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1871h. Brouillon de lettre à Mr. Sonrel. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1871i. Brouillon de lettre à Samuel Jeffreys, 1871. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1871j. Lettre à Père Secchi, écrit 14 Avril. Archivio della Università Pontificia Gregoriana, Roma (Cartelle 12 - N. 185).

Capello, J., 1872a. Brouillon de lettre à Angelo Secchi, écrit 10 Juillet. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1872b. Brouillon de lettre à Mr. Whipple, 1872. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1872c. Brouillon de lettre à Mr. Whipple, écrit 30 Août. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1872d. Brouillon de lettre à Warren de la Rue. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1872e. Lettre à Angelo Secchi, écrit 15 Juin. Archivio della Università Pontificia Gregoriana, Roma (Cartelle 12 - N. 190).

Capello, J., 1872f. Lettre à Angelo Secchi, écrit 26 Avril. Archivio della Università Pontificia Gregoriana, Roma (Cartelle 12 - N. 188).

Capello, J., 1872g. Lettre à Angelo Secchi, écrit 8 Avril. Archivio della Università Pontificia Gregoriana, Roma (Cartelle 12 - N. 187).

Capello, J., 1872h. Sur l'aspect du Soleil vers le 9 Août. *CRAS*, 75, 729-730.

Capello, J., 1872i. Sur les études photographiques du Soleil récemment entreprises à l'Observatoire de l'Infant don Luiz. *Comptes Rendus de l'Académie des Sciences de Paris*, 74, 1082-1083.

Capello, J., 1872j. Lettre à Angelo Secchi, écrit 16 Novembre. Archivio della Università Pontificia Gregoriana, Roma (Cartelle 12 - N. 190).

Capello, J., 1872k. Brouillon de lettre, probablement envoyé à Kew, écrit 14 Janvier. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1872l. Brouillon de lettre à Mr. Faye, écrit 26 Avril. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Nacional e Estrangeira 1872-1875).

Capello, J., 1873a. Brouillon de lettre à Colonel Stuart Wortley. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Nacional e Estrangeira 1872-1875).

Capello, J., 1873b. Lettre à Angelo Secchi, écrit 8 Mars. Archivio della Università Pontificia Gregoriana, Roma (Cartelle 12 - N. 184).

Capello, J., 1874. Brouillon de lettre à Mr. E. Koziell. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1875. Brouillon de lettre à Angelo Secchi, écrit 13 Août. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Nacional e Estrangeira 1872-1875).

Capello, J., 1876a. Brouillon de lettre à Angelo Secchi, écrit 10 Mars. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1876b. Brouillon de lettre à Mr. Andrews. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Capello, J., 1883. Lettre à le Solar Physics Committee, écrit 7 Mars. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Estrangeira 1882-1883).

Capello, J., 1886. Remarks on magnetic reductions. *Report of the Fifty-fifth Meeting of the British Association held at Aberdeen in September 1885*. Aberdeen, British Association for the Advancement of Science. Pp. 77-78.

Capello, J., and Stewart, B., 1864. Results of a comparison of certain traces produced simultaneously by the self-recording magnetographs at Kew and Lisbon. *Proceedings of the Royal Society of London*, 13, 111-120.

Carrington, R.C., 1863. *Observations of Spots on the Sun from November 9, 1853 to March 24, 1861 made at Redhill*. London, Williams and Norgate.

Charbonneau, P., 2002. The rise and fall of the first solar cycle model. *Journal for the History of Astronomy*, 33, 351–372.

Cheux, M.A., 1872. Aurore boréale blanche observée à la Baumette, près Angers le 8 août 1872. *Comptes Rendus de l'Académie des Sciences de Paris*, 75, 503-504.

Corvo, J., 1871. Carta a Fradesso da Silveira. Biblioteca do Instituto Geofísico, Lisboa (Correspondência 1868-1871).

de la Rue, W., 1860. Report on the present state of celestial photography in England. *Report of the Twentieth-ninth Meeting of the British Association, held at Aberdeen in September 1859*. London, Taylor and Francis.

de la Rue, W., 1863. Warren de la Rue exhibited to the Society a diagonal eyepiece. *Monthly Notices of the Royal Astronomical Society*, 24, 185.

de la Rue, W., 1864. The President's address. *Memoirs of the Royal Astronomical Society*, 25, 1-17.

de la Rue, W., 1873. *Report of the Forty-Second Meeting of the British Association for the Advancement of Science; held at Brighton 1872*. Chapter 'Address by Warren de la Rue. President of the Mathematics and Physics Section.' London, John Murray.

de Linden, R.V., and the SIDC team, 2007. Online catalogue of the sunspot index. URL: http://sidc.oma.be/html/sunspot.html

de Vaucouleurs, G., 1961. *Astronomical Photography*. New York, The Macmillan Company.

Dewhirst, D., and Hoskin, M., 1997. The message of starlight: the rise of astrophysics. In Hoskin, M. (ed.). *Cambridge Illustrated History of Astronomy*. Cambridge University Press. Pp. 256-343.

Faye, H., 1849. Sur les observations du Soleil. *Comptes Rendus de l'Académie des Sciences de Paris*, 28, 241-244.

Faye, H., 1858. Observations photographiques de l'éclipse, faites avec la grande lunette de M. Porro. *Comptes Rendus de l'Académie des Sciences de Paris*, 46, 507.

Faye, H., 1870. Sur l'observation photographique des passages de Vénus et sur un appareil de M. Laussedat. *Comptes Rendus de l'Académie des Sciences de Paris*, 70, 541-548.

Faye, H., 1872a. Note sur l'Association nouvellement fondée en Italie sous le titre de Società degli Spettroscopisti italiani. *Comptes Rendus de l'Académie des Sciences de Paris*, 74, 913-918.

Faye, H., 1872b. Réponse à la letter précédente de M. Tacchini. *Comptes Rendus de l'Académie des Sciences de Paris*, 74, 1240-1243.

Ferreira, H.A., 1940. *O observatório do Infante D. Luiz*. Lisboa, Sociedade Industrial de Tipografia, Lda.

Folque, F., 1871. Portaria do Ministerio do Reino de 4 Janeiro 1871. Arquivo da Universidade, Coimbra (Correspondência do Ministerio do Reino 1843 - 1910 cx 35).

Freire, F.d.C., 1872. *Memoria Historica da Faculdade de Mathematica nos cem annos decorridos desde a reforma da Universidade em 1772 até o presente*. Coimbra, Imprensa da Universidade de Coimbra.

Gasparini, L., 1999. Fotografia e astronomia. Gli esperimenti di Padre Angelo Secchi. In *Fotografia e astronomia. Nelle immagini degli Archivi Alinari e dell'Osservatorio Astrofisico di Arcetri*. Firenzi, Alinari. Pp. 9-18.

Golub, L., and Pasachoff, J.M., 2001. *Nearest Star*. Cambridge, Harvard University Press.

Guimarães, R., 1909. *Les Mathématiques en Portugal*. Coimbra, Imprensa da Universidade.

Hannavy, J., 1997. *Victorian Photographers at Work*. Princes Risborough, Shire Publications.

Herschel, J., 1855. On the application of photography to astronomical observations. *Monthly Notices of the Royal Astronomical Society*, 15, 158-159.

Janssen, J., 1876. Présentation de photographies solaires de grandes dimensions. *Comptes Rendus de l'Académie des Sciences de Paris*, 82, 1363-1365.

Janssen, J., 1877a. Note sur la reproduction par la photographie des "grains de riz" de la surface solaire. *Comptes Rendus de l'Académie des Sciences de Paris*, 85, 373.

Janssen, J., 1877b. Sur la constitution de la surface solaire et sur la Photographie envisagée comme moyen de découverte en Astronomie physique. *Comptes Rendus de l'Académie des Sciences de Paris*, 85, 1249-1255.

Janssen, J., 1896. Introduction historique. *Annales de l'Observatoire d'Astronomie Physique de Paris*, 1.

Koziell, E., 1874. Lettre à Brito Capello, écrit 8 Fevrier. Biblioteca do Instituto Geofísico, Lisboa (Correspondência 1872-1875).

Lankford, J., 1984. The impact of photography on astronomy. In Gingerich, O. (ed.) *Volume 4A - Astrophysics and Twentieth-century Astronomy to 1950*. Cambridge, Cambridge University Press. Pp. 16-39.

Laussedat, A., 1860a. Éclipse du 18 juillet: observations faites à Batna (Algérie). *Comptes Rendus de l'Académie des Sciences de Paris*, 51, 441-445.

Laussedat, A., 1860b. Rapport sur l'observation de l'éclipse du 18 juillet 1860, faite en Algérie par MM. Laussedat, De Salicis, Mannheim, Bour et Girard. *Comptes Rendus de l'Académie des Sciences de Paris*, 51, 990-999.

Laussedat, A., 1868. Réflexions à l'occasion de la note de M. H. Sainte-Claire Deville sur le sidérostat de Foucault. *Comptes Rendus de l'Académie des Sciences de Paris*, 66, 487-488.

Malaquias, I., Gomes, E.V., and Martins, D., 2005. The genesis of geomagnetic observatories in Portugal. *Earth Sciences History*, 24, 113-126.

Mascard, E., 1878. Carta a Brito Capello, 27 Junho 1878. Biblioteca do Instituto Geofísico, Lisboa (Correspondência Internacional 1876-1879).

Meadows, A.J., 1970. *Early Solar Physics*. Oxford, Pergamon Press.

Mouchez, E., 1887. La photographie astronomique à l'Observatoire de Paris et la Carte du Ciel. In *Annuaire pour l'an 1887 par le Bureau des Longitudes*. Paris, Gauthier-Villars. Pp. 755-842.

Observatorio de Infante D. Luis, 1872. *Relatorio do Serviço do Observatorio do Infante D. Luiz no Anno meteorologico de 1870-71*. Lisboa, Imprensa Nacional.

Pang, A.S-K., 1995. Victorian observing practices, printing technology, and presentations of the solar corona. *Journal for the History of Astronomy*, 25, 249-274.

Pang, A.S-K., 1997. 'Stars should henceforth register themselves': the rhetoric and reality of early astrophotography. *British Journal for the History of Science*, 31, 177-201.

Pasachoff, J.M., Olson, R.J.M., and Hazen, M.L., 1996. The earliest comet photographs: Usherwood, Bond, and Donati 1858. *Journal for the History of Astronomy*, 27, 129-145.

Peixoto, J.P., 1987. O Instituto Geofísico do Infante D. Luís e a Ciência em Portugal. In *Faculdade de Ciências da Universidade de Lisboa : passado / presente perspectivas futuras*. Lisboa: Faculdade de Ciências da Universidade de Lisboa. Pp. 215-244.

Pereira, A., 1871. Carta ao director, 24 Fev 1871. Biblioteca do Instituto Geofísico, Lisboa (Correspondência 1868-1871).

Proverbio, E. and Buffoni, L., 2004. La meteorologia dei fenomeni estremi alla metà del XIX secolo e il contributo di Angelo Secchi allo studio dei rapporti tra fenomeni meteorologici, magnetici e solari. *Physis*, XLI, 125-165.

Radau, J-C.R., 1878. Les applications scientifiques de la photographie. *Revue des Deux Mondes*, 25, 872-890.

Raposo, P., 2006. Instrumentos com história: o grande Refractor Equatorial e o Círculo Meridiano do OAL. *O Observatório*, 12(7), 4-5.

RAS Council Report, 1866. Report of the Council to the Forty-sixth Annual General Meeting of the Society. *Monthly Notices of the Royal Astronomical Society*, 26, 149-150.

RAS Council Report, 1868a. Report of the Council to the Forty-eighth General Meeting of the Society - Statement of the work done at the Kew Observatory with the heliograph. *Monthly Notices of the Royal Astronomical Society*, 28, 84-85.

RAS Council Report, 1868b. Royal Astronomical Society. *Astronomical Register*, 6, 33-42.

RAS Council Report, 1872. Report of the Council to the Fifty-second General Annual Meeting of the Society - Statement of the work done at the Kew Observatory with the heliograph. *Monthly Notices of the Royal Astronomical Society*, 32, 155-156.

Rayet, M.G., 1887. *Notes sur l'Histoire de la Photographie Astronomique*. Paris, Gauthier-Villars.

Rees, J.K., 1906. A Catalogue of Rutherfurd's Photographic plates of the Sun, the Moon and the stars. *Contributions from the Rutherfurd Observatory of Columbia University New York*, 2.

Rothermel, H., 1993. Images of the sun: Warren de la Rue, George Biddell Airy and celestial photography. *British Journal for the History of Science*, 26, 137-169.

Rutherford, L.M., 1878. Extract from letter on a solar photograph. *Monthly Notices of the Royal Astronomical Society*, 38, 410.

Scheiner, J., 1897. *Die Photographie der Gestirne*. Leipzig, Verlag von Wilhelm Engelmann.

Searle, A. (ed.), 1876. *Annals of the Astronomical Observatory of Harvard College, Volume VIII*. Cambridge, John Wilson and Son.

Secchi, A., 1872. Résumé des observations des protubérances solaires du 1er janvier au 29 avril. *Comptes Rendus de l'Académie des Sciences de Paris*, 74, 1315-1320.

Secchi, A., 1875a. *Le Soleil, Volume 1*. Paris, Gauthier-Villars, 2 edition.

Secchi, A., 1875b. *Le Soleil, Volume 2*. Paris, Gauthier-Villars, 2 edition.

Selwyn, W., 1864. Photographs of the Sun. *Monthly Notices of the Royal Astronomical Society*, 24, 183-184.

Sheenan, W. and Westfall, J., 2004. *The Transits of Venus*. Amherst, Prometheus Books.

Silva, J., 1861. Noticia dos trabalhos magneticos executados no Observatorio do Infante D. Luiz na Escóla Polytechnica. *Memorias da Academia Real das Sciencias de Lisboa -*

Classe de Sciencias Mathematicas, Physicas e Naturaes, 2, 1-22.

Silva, V.R., 1996. Do Antigo Observatório Astronómico da Escola Politécnica e da sua Musealisação. M.Sc. Thesis, Universidade Nova de Lisboa, Lisboa.

Silveira, F., 1874. *Relatorio do Serviço do Commissariado Portuguez em Vienna de Austria na Exposição Universal de 1873*. Lisboa, Imprensa Nacional.

Solar Physics Committee, 1882. *Report*. London, Eyre and Spottiswoode.

Solar Physics Committee, 1889. *Second Report*. London, Eyre and Spottiswoode.

Stewart, B., 1886. Suggestions for the Committee on Magnetic Reductions. *Report of the Fifty-fifth Meeting of the British Association held at Aberdeen in September 1885*. Aberdeen, British Association for the Advancement of Science. Pp. 68-71.

Tacchini, P., 1872a. Lettre de M. Tacchini à M. Faye à propos de sa note présentée à l'Académie le 1er avril dernier, sur l'organisation de la Société des Spectroscopistes italiens. *Comptes Rendus de l'Académie des Sciences de Paris*, 74, 1237-1240.

Tacchini, P., 1872b. Nuova Società di Spettroscopisti Italiani. *Memorie della Società degli Spettroscopisti Italiani*, 1.

Tucker, J., 2005. *Nature Exposed*. Baltimore, The Johns Hopkins University Press.

Vilnius University - Astronomical Observatory, 2007. History of the Astronomical Observatory of the Vilnius University. URL: www.astro.ff.vu.lt

Wittmann, A.D., 2000. Did Struve observe the nucleus of Halley's comet in 1835? In Hentschel, K. and Wittmann, A.D. (eds.). *The Role of Visual Representation in Astronomy: History and Research Practise*. Thun, Verlag Harri Deutsch. Pp. 79-89.

Young, C.A., 1881. *The Sun*. New York, Appleton and Company.

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THE NORWEGIAN NAVAL OBSERVATORIES

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Abstract: Archival material has revealed milestones and new details in the history of the Norwegian Naval Observatories. We have identified several of the instrument types used at different epochs. Observational results have been extracted from handwritten sources and an extensive literature search. These allow determination of an approximate location of the first naval observatory building (1842) at Fredriksvern. No physical remains exist today. A second observatory was established in 1854 at the new main naval base in Horten. Its location is evident on military maps and photographs. We describe its development until the Naval Observatory buildings, including archives and instruments, were completely demolished during an allied air bomb raid on 23 February 1945. The first Director, C.T.H. Geelmuyden, maintained scientific standards at the Observatory between 1842 and 1870, and collaborated with university astronomers to investigate, develop, and employ time-transfer by telegraphy. Their purpose was accurate longitude determination between observatories in Norway and abroad. The Naval Observatory issued telegraphic time signals twice weekly to a national network of sites, and as such served as the first national time-service in Norway. Later the Naval Observatory focused on the particular needs of the Navy and developed into an internal navigational service.

Keywords: naval observatory, transit instrument, universal instrument, chronometer, sextant, time-service

1 INTRODUCTION

In 1817 a Naval Academy was established at Fredriksvern Naval Base in Norway. The cadets there were trained in the theory and practise of astronomical navigation following basic mathematical training in arithmetic, planar geometry, and spherical trigonometry and geometry. Annual cruises at sea allowed observational training and included computational tests to demonstrate that required skills in positioning were individually fulfilled by the cadets. On land the cadets exercised time-keeping and chronometer control. These topics occupied 10 hours in a weekly total of 46 hours of teaching. Most cadets needed 5-6 years to complete the Naval Academy training and pass the final exam; they were then appointed junior officers in the Norwegian Navy (Kvam, 1967).

Captain Søren Lorents Lous (Figure 1)[1] was the first lecturer in mathematics and navigation at the Naval Academy, and held this post from 1817 to 1841. On 19 November 1816 he attempted to determine the longitude of the Naval Academy by observing a solar eclipse, and in a letter to Professor Christopher Hansteen at the University of Oslo he described his effort throughout the month of November to determine local time whenever weather conditions permitted (Lous, 1816). He observed corresponding elevations of the Sun on either side of the local meridian to determine the acceleration and bias of his clocks. During the eclipse, clouds prevented observation of first contact, but Lous did observe the occultation of a large sunspot by the Moon before clouds prevented further observation. The total eclipse phase, which he estimated to last 49 seconds, was noted only from the darkness at the site. Three minutes later the skies cleared and the Sun remained visible until half its diameter had emerged from the eclipse. Then it remained overcast for the rest of the day. The following day Lous observed corresponding solar elevations in order to verify the clock bias. He derived a latitude of 58° 57′ N.

Hansteen (1816) remarked that clouds prevented observation of the solar eclipse elsewhere in Norway, and in Copenhagen, even though observers had been ready in Oslo, Bergen, Trondheim, and Kongsberg. Thus, no longitude differences could be determined.

Figure 1: Søren Lorents Lous (courtesy: Naval Museum).

The first successful determination of the geographical coordinates of Fredriksvern was made by Hansteen (1823) in July 1819, when he arrived by sailing ship from Oslo after a voyage of six days. Using the Naval Academy's sextant (Figure 2) he observed circum-meridian elevations of the Sun in the Academy gardens and found the latitude to be 58° 59′ 54.9″. The results from observing the upper and lower solar limb on 7 July 1819 were listed as 53.8″ and 56.0″, respectively, i.e. 54.9″ ± 1.6″ (rms). The drift and bias of his Arnold No. 132 pocket chronometer (Figure 3) was determined in Oslo before the voyage, and in Fredriksvern upon his arrival. The longitude difference between Oslo and Fredriksvern was found to be 2 minutes 41.6

seconds. However, the time determinations in Oslo suffered from lack of levelling control of the transit instrument because a thief had broken into the interim University Observatory at Akershus and had stolen the levelling device (see Pettersen, 2002). This incident and the long duration of the voyage suggest the presence of systematic effects in the result, which may be uncertain by several seconds.

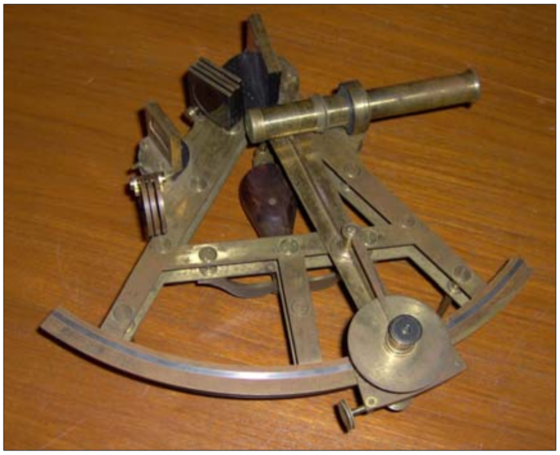

Figure 2: A Troughton sextant similar to the one used at the Naval Academy.

Figure 3: The Arnold No. 132 pocket chronometer, which is now in storage at the University of Oslo.

Another chronometer expedition took place in September 1824, involving a steamship (Hansteen, 1828a), and on this occasion the Arnold No. 132 pocket chronometer also was used. Hansteen (Figure 4) made observations in Oslo before the voyage, and Lous observed at Fredriksvern upon his arrival two days later. They derived a longitude difference between Oslo and Fredriksvern of 2 minutes 45.68 seconds.

To improve opportunities for travel, freight transport and postal deliveries the state operated the steamship *S.S. Constitutionen* between the capital, Christiania (now Oslo), and Kristiansand. It connected on a regular basis with the *S.S. Prinds Carl* in Fredriksvern, which then continued on to Gothenburg and Copenhagen. This allowed chronometer expeditions to Fredriksvern, as well as Gothenburg and Copenhagen, to be conducted more conveniently, and with a shorter travel time (Hansteen, 1828b). At this time, Hansteen was frequently corresponding with the Altona clockmaker Heinrich Johan Kessels in order to acquire chronometers for the University and the Geographical Survey of Norway. In 1826 he requested the first chronometer for the Norwegian Navy (Schulz, 1938), and was offered a Kessels No. 1257 for 200 Dutch ducats (Kessels, 1826a). This box chronometer (Figure 5) was made to run for 36 hours, but Kessels (1826b) recommended rewinding every 24 hours. It had a two-axis suspension for sea voyage, which could be locked for use on land. Hansteen accepted the offer on behalf of the Navy and the chronometer left Altona on 21 August 1826 on a ship bound for Oslo. It was promptly paid for and Kessels' receipt was in Hansteen's hands by 22 September (Kessels, 1826c). For a period of five months from 3 September 1826, Hansteen compared the Kessels No. 1257 chronometer with three other chronometers and with a pendulum clock made by Urban Jørgensen (Copenhagen). Local mean time was episodically determined by observations in the Observatory at Akershus. The Kessels No. 1257 chronometer showed a monthly acceleration of 1 second (Hansteen, 1827).

In May 1827 Hansteen (1828c) made repeated time observations in Oslo to determine the bias and drift of the Kessels chronometer. He then sent the box chronometer by *S.S. Constitutionen* to Fredriksvern, where it arrived the next day. Lous then made time observations with a Troughton sextant at Fredriksvern over the next four days. The longitude difference between Oslo and Fredriksvern was found to be 2 minutes 45.43 seconds. Recent GPS observations at the two locations produced geodetic longitudes (east of Greenwich) of 10° 44′ 28″ for Oslo and 10° 02′ 08″ for the Academy gardens, a difference of 42′ 20″, which corresponds to 2 minutes 49.3 seconds. The geodetic latitude of the Academy gardens is 58° 59′ 54″.

Figure 4: Christopher Hansteen, portrait from 1826.

Lous continued to work with small field instruments throughout his tenure at the Naval Academy. No documents reveal the acquisition of any larger instruments for scientific use. A doubling of the Naval budget from 6.5% to 14.5% of the national budget in 1836 (see Einang et al., 1934: 285) allowed a series of new box chronometers to be acquired. An archival note (Preus 1928) lists the acquisition of four new Kessels chronometers: No.1333 in 1835, No.1349 in 1837, No.1372 in 1839 and No.1391 in 1841. According to an archival card at the Norwegian Naval Museum, a Dent No.2067 box chronometer (Figure 6) was acquired in 1839, but in 1847 Hansteen remarked that it was a recent acquisition (see Hansteen and Fearnley, 1849: 56). The two-year periodicity of the acquisition program suggests that the Dent chronometer may have been acquired in 1843. A box chronometer by Kessels with suspension was then priced at 240 Dutch ducats (Hagerup, 1840). The chronometer investment program thus represented a considerable financial outlay, and led to the establishment of the Navy's *Instrument and Chart Collection* in 1835.

When C.T.H. Geelmuyden (Figure 7)[2] succeeded Lous as Naval Academy lecturer in 1841, plans were immediately prepared to expand the Instrument and Chart Collection with a Naval Observatory equipped with permanent instrumentation. In addition to its educational function, its role would include the accurate control of Navy chronometers.

Figure 5: The Kessels No. 1257 box chronometer (courtesy: Naval Museum).

Figure 6: The Dent No. 2067 box chronometer (courtesy: Naval Museum).

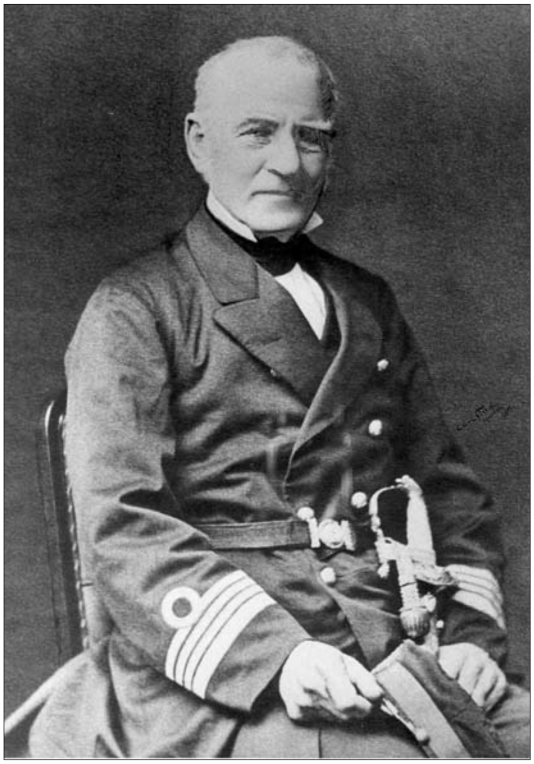

Figure 7: Christian Torber Hegge Geelmuyden (courtesy: Naval Museum).

2 THE NAVAL OBSERVATORY IN FREDRIKSVERN 1842-1864

The Norwegian Naval Observatory was set up in 1842 on the campus of the main naval base, which was then located at Fredriksvern (Oppegaard, 1928). It was equipped with a transit instrument and two pendulum clocks. No vestige of this facility remains today.

The transit instrument has a peculiar history. It was ordered from A. & G. Repsold by Hansteen (1838) for the Geographical Survey of Norway (which he directed), and was intended as a field instrument for mountaintop stations; thus it had to be small and transportable. During a visit to continental Europe in the summer of 1839 Hansteen discussed the details directly with Repsold in his workshop in Hamburg (Hansteen, 1839). Four years later he had still not received it (Hansteen, 1842), but Repsold (1842a) responded that it was almost ready for shipment to Norway. During this time priority at the Repsold workshops had been given to several large and complex instruments for the observatories at Pulkovo, Königsberg, and Oslo (see Repsold, 1914; 1927). The transit instrument left Hamburg at the end of November 1842 (Repsold, 1842b; 1842c) and arrived in Oslo two months later (Hansteen, 1843), delayed by transport irregularities between Germany and Norway caused by winter conditions. It was promptly paid for, and Repsold (1843) acknowledged this with a signed receipt.

Figure 8: The Repsold universal instrument at the Utrecht Observatory (courtesy: Dr Roland Wittje).

The peculiarity is that this transit instrument was never included in the instrument inventories of the Geographical Survey of Norway, and the receipt is not in their Archives. I suggest that Hansteen offered the instrument to the Naval Observatory and that it was paid for by the Navy. A key motivation for Hansteen was the involvement of Geelmuyden, who was the driving force behind the setting up of the Naval Observatory at Fredriksvern. Geelmuyden's academic skills had impresssed Hansteen, who had good reason to expect that the young lieutenant would develop a scientific career and become a useful future collaborator. In addition to his Professorship at the University, Hansteen lectured in advanced mathematics, mechanics, astronomy and navigation at the Military College in Oslo between 1826 and 1849, and upon entering the College in 1839 Geelmuyden had attracted attention by requesting permission to skip the first year and enter directly into the senior class (Oppegaard 1928). He was given an individual mathematics exam by Hansteen and passed it brilliantly. When he was appointment lecturer at the Naval Academy in 1841, Geelmuyden promptly wrote a textbook on navigation which was to remain a standard for the next fifty years. A transit instrument in Geelmuyden's hands thus seemed an excellent investment for the future. Unfortunately, no photographs or drawings exist of this instrument, and no information of its dimensions have survived. Presumably it was destroyed in the 1945 bombing raid (see below).

Once the Naval Observatory was set up, Geelmuyden (1850) attempted to determine its longitude by observing the end of a partial solar eclipse on 6 May 1845. He used a refractor ('Seefernrohr') by Utzschneider & Fraunhofer (e.g. 1816), for which he improvised a tripod. Unfortunately the telescope did not focus sharply, and Geelmuyden considered the result erroneous by several seconds. At the University Observatory in Oslo the start of the eclipse was observed, but the end was missed due to clouds (Hansteen 1847), so no longitude difference emerged from this effort.

In 1850 Geelmuyden organised a chronometer expedition with Dent No. 2067 between the Naval Observatory and the University Observatory in Oslo. The latter institution had been established by Hansteen in 1833 and was located 5.5 seconds west of its interim predecessor at Akershus (Hansteen and Fearnley 1849; Pettersen 2002), which was the datum reference point prior to 1830. The longitude difference was found to be 2 minutes 40.55 seconds (Hansteen 1853), while previous chronometer expeditions conducted in 1824 and 1827 had yielded a value of 2 minutes 45.56 seconds between the Academy gardens and the Observatory at Akershus. Thus the Academy gardens were 2 minutes 40.1 ± 0.2 seconds west of the University Observatory. The Naval Observatory and the Academy gardens were almost on the same meridian. The resulting longitude of the Naval Observatory is 10° 03′ 14.2″ east Greenwich. By comparison, its geodetic longitude was derived to be 10° 02′ 01″ east Greenwich using a GPS observation in the Academy gardens.

The latitude of the Naval Observatory was listed by Hansteen (1852; 1853) as 58° 59′ 33.9″, obtained by combining unpublished observations by Geelmuyden and Hansteen's results from July 1819, transferred from the Academy gardens to the Naval Observatory. This locates the Naval Observatory 650 meters south of the Academy gardens. A search in the area revealed no physical remains of the Observatory.

The total solar eclipse on 28 July 1851 offered opportunities for further longitude observations. Geelmuyden (1850; 1851) was on duty on the corvette *Ellida* that summer (Einang et al., 1934), while his deputies, Knud Geelmuyden Smith and Hans Iver Andreas Hiorth, observed from the Naval Observatory. They were accompanied by several foreign visitors (e.g. John Couch Adams (1852) from Cambridge University), who observed the eclipse from locations near the Observatory building. The Naval Observatory results are discussed by Hansteen (1852; 1853).

A new Naval Observatory was established in Horten in 1854. A remark by Geelmuyden (1857) reveals that the first observatory at Fredriksvern continued to be used for chronometer control as late as 1857. The Naval Academy was transferred to Horten in 1864, and it is likely that all activity ceased at the Naval Observatory at Fredriksvern on this occasion.

3 THE NAVAL OBSERVATORY IN HORTEN 1854-1945

A major new naval base had been under development in Horten for several decades when the Naval Observatory was transferred there in 1854. A two-story brick building for the Instrument and Chart Collection was set up south-east of the outer perimeter of the base citadel. The collections of navigation instruments and naval charts were housed on the ground floor, while an observation room occupied the top floor. The Repsold transit instrument was transferred from Fredriksvern to Horten, and in 1858 a new pendulum clock was acquired (Geelmuyden, 1858).

In 1860 wear and tear justified a replacement for the transit instrument, and Geelmuyden sought the advice of the new Director of the University Observatory in Oslo, Carl Fredrik Fearnley (1860a). He ended up ordering a large universal instrument from Repsold (Fearnley 1861a), and Repsold (1861a; 1861b) responded by offering a copy of the one he was making for the observatory in Utrecht (Figure 8). This was accepted by Geelmuyden (Fearnley, 1861b), and the instrument left Hamburg on the *S.S. Sanct Olaf* on 29 August 1863, bound for Oslo (Repsold, 1863). At the Naval Observatory a small octagonal building with a rotating roof was set up in 1864 to house the new instrument. This same year the Naval Academy was transferred from Fredriksvern to Horten, and the Observatory was used for educational purposes. In 1876 an administrative reorganisation changed the name of the unit from the Instrument and Chart Collection to the Navigation Services of the Navy (Einang et al., 1934: 225). The two Observatory buildings were connected in 1878 by a one-story east wing to the original observatory, adding a magnetic observing room and offices (Karl-Johansværns Ingeniør-Detachement, 1882). The Observatory buildings remained the home of the navigation services until the entire naval base (then occupied by the German Navy) was destroyed in an allied bombing raid on 23 February 1945. By that time, the astronomical time-service had been surpassed by more modern technology.

In 1856 the national geodetic survey had reached Horten. The garrison church had been consecrated the previous year, and the church spire was thus available as a surveyor's datum point. Using the geodetic coordinates determined for the church (Næser, 1856) and my own GPS observations of the official national reference point at the time, I have derived coordinates in a modern reference frame (ϕ = 59° 25′ 34.2″ and λ = 10°29′ 22″ east of Greenwich). These compare well with GPS observations made at the church (φ = 59° 25′ 37″ and λ = 10° 29′ 23″). Photogrammetric measurements on historical military maps of the naval base locate the Naval Observatory 142m north of the church and 280m west of it. The derived geodetic coordinates are thus 59° 25′ (39-42)″ north and 10° 29′ 04″ east of Greenwich for the location of the transit instrument. GPS observations at the site yielded φ = 59° 25′ 41″ north and λ = 10° 29′ 06″ east Greenwich. The University Observatory in Oslo has a geodetic longitude (as measured repeatedly by GPS) of 10° 43′ 05″. This yields a geodetic longitude difference of 13′ 59″ (arc) = 55.9 time seconds between Oslo and Horten.

Horten and Oslo were connected by a telegraph line on 23 June 1855, and in November of that year Fearnley (Figure 9) and Geelmuyden began testing the line for a transfer of time signals. Astronomical observations of meridian transits of stars established local sidereal time as recorded on the Observatory's pendulum clocks. Time was then transferred to chronometers, which were carried to the local telegraph office. In Horten the telegraph office was located in barrack A on the naval base (Hansen, 1993). The observers received exclusive access to the telegraph line at midnight on predetermined dates, and exchanged time signals according to an agreed upon scheme. Several attempts were made to determine the longitude difference between the Naval Observatory and the University Observatory, but were thwarted by broken lines, noisy signals and other operational difficulties. Repeated tests in 1855 and 1858 revealed variable time delays and changing observational precision.

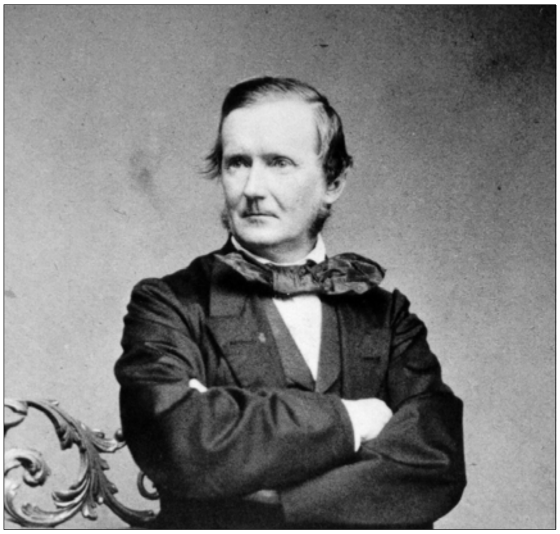

Figure 9: Carl Fredrik Fearnley.

By 1859 the telegraph system had expanded to include other major cities in Norway, and the operational reliability had improved. A project was carried out between March and July 1859 to exchange telegraphic time signals between three observatories in the country (Pettersen, 2006). Repeated experiments between Oslo and Horten yielded a longitude difference of 54.0 seconds (Fearnley, 1860b). My reanalysis of the data from twelve different days gave a value of 53.9 ± 0.5 seconds. The astronomical longitude of the University Observatory, 10° 43′ 22.5″ east of Greenwich (cf. Hansteen and Fearnley, 1849; Fearnley et al., 1890; Lous, 1926), thus implies that the Naval Observatory in Horten was located at 10° 29′ 52.5″ east of Greenwich. The latitude was listed by Fearnley (1860b) as 59° 25′ 55″ north.

The numerical differences between the astronomical (Φ, Λ) and geodetic (φ, λ) coordinates for the Naval Observatory are about 15″ in the meridian and about

45″ along the parallel. The astronomical value is larger than the geodetic. The deflection of the vertical at an observation site some 20 km to the southwest (on the Tønsberg baseline) is $\xi = \Phi - \varphi = -3.9''$ in the meridian and $\eta = (\Lambda - \lambda) \cos \varphi = 12.6''$ in the prime vertical. For the University Observatory (about 55 km to the northeast) these parameters are $\xi = \Phi - \varphi = -3.3''$ and $\eta = (\Lambda - \lambda) \cos \varphi = 8.8''$. It appears that the errors of both astronomical coordinates may be 10″ or more for the Naval Observatory.

In Bergen an observatory was established in 1855 (Pettersen, 2005), and it was equipped with a transit instrument by Repsold (f.l. = 60 cm), a pendulum clock with Mercury compensator by C. Höeg obtained from Bergen, and pocket chronometers derived from Altona. Johan Julius Åstrand was appointed City Astronomer on 1 July 1857, just weeks before the local telegraph office opened. In 1857 he used lunar distances to derive a longitude of 5° 18′ 00″ east of Greenwich. In 1859 Åstrand (1859) observed on six days in May and June and exchanged telegraphic time signals with Geelmuyden in Horten. Åstrand was concerned about the accuracy of the time transfer since the walking distance between the Observatory and the telegraph office in Bergen was considerable. He determined Bergen to be 20m 46.1s west of Horten, with an uncertainty of 2s, i.e. at longitude 5° 18′ 21″ (± 30″) east of Greenwich.

At the Naval Observatory in Horten time determinations were made routinely for chronometer control. Each Sunday a cadet transferred time from the Observatory's pendulum clock to a box chronometer, and the latter was carried to the Telegraph Office. Starting in November 1855, telegraphic time signals were issued every Sunday for the next thirteen years. In 1858 signals were transmitted also on Wednesdays. Telegraph offices in local communities across the country were thus updated continually and so could show correct time. This service continued until it was taken over by the University Observatory in Oslo in 1869. Geelmuyden had announced that he would leave the Naval Observatory the following year in order to accept the Directorship of the Technical School in Trondheim.

4 CONCLUDING REMARKS

The Naval Observatory in Fredriksvern was established as a calibration facility for the collection of chronometers acquired by the Navy between 1835 and 1845. The improved accuracy of navigation at sea put the safety of naval ships far ahead of commercial shipping at the time. The navigational skills and experience of naval officers, with six years of basic training at the Naval Academy, made them attractive also as teachers and examiners in the public nautical schools.

The Naval Observatory in Horten initiated the use of telegraphy for time transfer in Norway, and developed the first national time-service by issuing weekly telegraphic time signals to other coastal cities during C.T.H. Geelmuyden's Directorship. His personal interest in modern technology was a driving force. Unfortunately, his successors did not continue his collaborative and national approach, and so the Naval Observatory developed into an internal navigation service for the Navy.

5 NOTES

1. Søren Lorents Lous was born in Copenhagen on 29 June 1785, where his father (and grandfather before him) was Director of Navigation. He attended the Naval Academy in Copenhagen and began a military career in the Danish-Norwegian Navy. He was transferred to the main naval base in Norway (Fredriksvern) in 1812, and married in that same year. When Denmark handed Norway to Sweden in 1814 following the Napoleonic wars, Lous remained in Norway and became a Captain (and later a Commander) in the Norwegian Navy. When the Norwegian Naval Academy was established at Fredriksvern in 1817, he was appointed to lecture in mathematics and navigation. He held this position until 1841, when he was appointed head of military enrolment and chief of the pilot service in Bergen. He retired in 1857 and died in Bergen on 14 January 1865 (Anon., 1872).
2. Christian Torber Hegge Geelmuyden was born in Trondheim on 16 October 1816. He passed through the Naval Academy (1837) and the Military College (1840) in record time with remarkable academic success, and was sent to Sweden to study iron production and cannon design. In 1841 he was appointed lecturer of mathematics and navigation at the Naval Academy in Fredriksvern. He wrote a textbook on navigation which was used for more than fifty years. In 1842 he established the Naval Observatory. He also married that same year, and one and a half years later became father to a future university professor of astronomy. A decade later he transferred to the naval base in Horten, which replaced Fredriksvern as the main naval station. He also transferred the Naval Observatory to Horten, and founded a technical school in 1855 (which he directed until 1870). For some years he then directed a new technical school in Trondheim before returning to Horten as commander of the naval base. He died in Horten on 13 May 1885. He was a member of science academies in Oslo, Stockholm and Trondheim.

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7 REFERENCES

The following abbreviation is used:

UO = Original in the astronomy archives, University of Oslo.

Adams, J.C., 1852. The observations of the total solar eclipse of July 28, 1851; Frederiksværn. *Memoirs of the Royal Astronomical Society*, XXI (I), 101-107.

Anon, 1872. *Stamtavle over familien Lous.* Bergen, J.D. Beyers bogtrykkeri.

Åstrand, J.J., 1859. Letter to C. Hansteen, dated 12 June, UO.

Einang, E., Gløersen, E., and Sundt, T., 1934. Sjømilitære Samfunds Marinekalender 1814-1934. Sjømilitære Samfund.

Fearnley, C., 1860a. Letter to C.T.H. Geelmuyden, dated 1 February, UO.

Fearnley, C., 1860b. Sonnenfinsterniss am 15ten März 1858. *Astronomische Nachrichten*, 52, 285-288.

Fearnley, C., 1861a. Letter to A. & G. Repsold, dated 11 September. Original in Staatsarchiv Hamburg.

Fearnley, C., 1861b. Letter to A. & G. Repsold, dated 23 October. Original in Staatsarchiv Hamburg.

Fearnley, C.F., Schellerup, F.C., and Lindhagen, D.G., 1890. *Längenbestimmungen zwischen den Sternwarten in Stockholm, Kopenhagen und Christiania*. Kungl. Svenska Vetenskaps-akademiens handlingar, Bandet 24, No. 4.

Geelmuyden, C.T.H., 1850. Letter to Christopher Hansteen, dated 16 December, UO.

Geelmuyden, C.T.H., 1851. Letter to Christopher Hansteen, dated 15 May, UO.

Geelmuyden, C.T.H., 1857. Letter to Christopher Hansteen, dated 1 January, UO.

Geelmuyden, C.T.H., 1858. Letter to Carl Fredrik Fearnley, dated 5 December, UO.

Geelmuyden, H., 1888. *Observasjoner i første vertikal*. Forhandlinger i Videnskabs-Selskabet i Christiania i året 1888, No. 10.

Hagerup, H.S., 1840. Om Chronometeret og dets Anvendelse tilsøes, p. 51. Christiania, Forlagt af Johan Dahl.

Hansen, J.I., 1993. *Da telegrafen og telefonen kom.* Borre-minne.

Hansteen, C., 1816. Letter to H. C. Schumacher, dated 27 December. Original in Staatsbibliothek zu Berlin.

Hansteen, C., 1823. Geographiske Bestemmelser i Norge. *Magazin for Naturvidenskaberne*, 2, 266-288.

Hansteen, C., 1827. Astronomiske Uhre, Chronometre. *Magazin for Naturvidenskaberne*, 7, 327-330.

Hansteen, C., 1828a. Geographiske Bestemmelser i Norge af forskjellige Iagttagere*. Magazin for Naturvidenskaberne*, 8, 280-298.

Hansteen, C., 1828b. Beitrag zur Bestimmung des Längenunterschiedes zwischen Christiania, Gothenburg and Kopenhagen vermittelst drei Chronometer. *Astronomische Nachrichten*, 6, 469-472.

Hansteen, C., 1828c. Længdeforskjel imellem Christiania og Fredriksværn. *Magazin for Naturvidenskaberne*, 8, 161-163.

Hansteen, C., 1838. Letter to A. &. G. Repsold, dated 30 July. Original in Staatsarchiv Hamburg.

Hansteen, C., 1839. Letter to H.C. Schumacher, dated 24 June. Original in Staatsbibliothek zu Berlin.

Hansteen, C., 1842. Letter to A. & G. Repsold, dated 2 May. Original in Staatsarchiv Hamburg.

Hansteen, C., 1843. Letter to A. & G. Repsold, dated 2 February. Original in Staatsarchiv Hamburg.

Hansteen, C., 1847. Beobachtungen auf der Sternwarte in Christiania. *Astronomische Nachrichten*, 26, 145-148.

Hansteen, C, 1852. Beobachtungen der totalen Sonnenfinsterniss 1851 Juli 28 auf der Sternwarte in Christiania und verschiedenen anderen Puncten in Norwegen. *Astronomische Nachrichten*, 34, 367-378.

Hansteen, C., 1853. Den totale Solformørkelse den 28de Juli 1851. *Nyt Magazin for Naturvidenskaberne*, 7, 93-106.

Hansteen, C., and Fearnley, C., 1849. *Beschreibung und Lage der Universitäts-Sternwarte in Christiania*. Universität-Programm.

Karl-Johansværns Ingeniør-Detachement, 1882. Beskrivelse over Bygninger og Befæstninger m.m. henhørende under Karl-Johansværns Værft, pp. 47-48. Christiania, Carl C. Berner & Co.

Kessels, H.J., 1826a. Letter to Christopher Hansteen, dated 21 July, UO.

Kessels, H.J., 1826b. Letter to Christopher Hansteen, dated 22 August, UO.

Kessels, H.J., 1826c. Letter to Christopher Hansteen, dated 22 September, UO.

Kvam, K., 1967. *Beretning om den norske sjøkrigsskoles virksomhet 1817-1967.* Oslo, Sjøkrigsskolen.

Lous, K., 1926. A preliminary determination by wireless timesignals of the longitude of the University Observatory at Oslo. *Astronomische Nachrichten*, 229, 25-28.

Lous, S.L., 1816. Letter to Christopher Hansteen, dated 30 November, UO.

Næser, F., 1856. Observing log book and computational note book for 1856. Original in the archives of the Norwegian Mapping Authority, Hønefoss (D.G.O. II. 97).

Oppegaard, S. 1928. C.T.H. Geelmuyden. In Bull, E., and Jansen, E. (eds.). *Norsk Biografisk Leksikon*. Oslo, A. Aschehoug & Co. Pp. 415-416.

Pettersen, B.R., 2002. Christopher Hansteen and the first observatory at the University of Oslo, 1815-1828. *Journal of Astronomical History and Heritage*, 5, 123-134.

Pettersen, B.R., 2005. Astronomy in service of shipping: documenting the founding of Bergen Observatory in 1855. *Journal of Astronomical History and Heritage*, 8, 123-128.

Pettersen, B.R., 2006. Astronomy in a shipping nation: harbour observatories in Norway 1850-1900. *Proceedings of a Conference on Scientific Instruments and their Users*. Leiden, Museum Boerhaave (in press).

Preus, M, 1928, Note from the Director of the Naval Navigation Services, dated 19 May. Original in the archives of the Norwegian Naval Museum, Horten.

Repsold, A. &. G., 1842a. Letter to Christopher Hansteen, dated 3 June, UO.

Repsold, A. &. G., 1842b. Letter to Christopher Hansteen, dated 22 November, UO.

Repsold, A. &. G., 1842c. Letter to Christopher Hansteen, dated 30 December, UO.

Repsold, A. &. G., 1843. Letter to Christopher Hansteen, dated 10 February, UO.

Repsold, A. & G., 1861a. Letter to C. Fearnley, dated 20 September, UO.

Repsold, A. & G., 1861b. Letter to C. Fearnley, dated 2 November, UO.

Repsold, A. & G., 1863. Letter to C. Fearnley, dated 29 August, UO.

Repsold, J.A., 1914. *Zur Geschichte der Astronomischen Messwerkzeuge von 1830 bis um 1900*. Leipzig, Verlag von Emmanuel Reinicke.

Repsold, J.A., 1927. Verzeichnis der aus der Repsoldschen Werkstatt hervorgegangenen Instrumente. *Vierteljahrsschrift der Astronomischen Gesellschaft*, 62, 251-257.

Schulz, F., 1938. Litt om kronometeret og problemet lengdebestemmelse til sjøs. *Bergens Sjøfartsmuseums Årshefte 1938*, 5-23.

Utzschneider, J., and Fraunhofer, J., 1816. Verzeichniss der optischen Werkzeuge, welche in dem optischen Institute zu Benedictbeurn Utzschneider & Fraunhofer für nachstehende Preise verfertigt werden. *Zeitschrift für Astronomie*, Zweiter Band, pp. 173-179.

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DEMETRIOS EGINITIS: RESTORER OF THE ATHENS OBSERVATORY

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Abstract: Demetrios Eginitis (1862–1934), one of the most eminent modern Greek astronomers, directed the National Observatory of Athens for 44 years (1890-1933). He was the fourth director since its founding, and was responsible for the restoration and modernization of the Observatory, which was in a state of inactivity after the death of Julius Schmidt in 1884. Eginitis ordered the purchase of modern instruments, educated the personnel, enriched the library with necessary and up-to-date books and arranged for new buildings to be built to house new telescopes and accommodate the personnel. Moreover, he divided the National Observatory of Athens into three separate Departments: the Astronomical, the Meteorological and the Geodynamic. D. Eginitis' contribution to Greek society went beyond his astronomical accomplishments. He was instrumental in the adoption of the Eastern European time zone for local time in Greece, and he succeeded in changing the official calendar from the Julian to the Gregorian. Having served twice as Minister of Education, he created many schools, founded the Academy of Athens and the Experimental School of the University of Athens. Eginitis was fluent in French, German and English, and therefore was the official representative of his country in numerous international conferences and councils.

Keywords: Athens Observatory, Greece, Gregorian Calendar, Academy of Athens

1 THE FOUNDING OF THE ATHENS OBSERVATORY – ITS FIRST DIRECTORS

The National Observatory of Athens (Figure 1) was founded in 1842 and the main building was built in the period 1843-1846 on top of the Hill of the Nymphs (*Lofos Nymphon*) opposite the Acropolis, at geographical latitude φ = 37° 58′ 27″.42 N, longitude λ = 23° 43′ 07″.3 and altitude 107 m above sea level. The cost of the construction exceeded 500,000 drachmas, a very large amount for the period, of which 300,000 were donated by the rich merchant Baron George S. Sinas, then General Consul of Greece in Vienna.

The main building was designed in 1842 by the architects Theophil Hansen (1813–1891) of Denmark and Edward Schaubert (1804–1860) of Germany, based on the suggestions of the Danish astronomer Heinrich Christian Schumacher (1780–1850). Schumacher is best known for establishing the Altona Observatory and publishing the *Astronomische Nachrichten* journal (1822). The building is in the shape of a cross, with its sides aligned in the four cardinal directions, and at the centre is a small domed tower (see Plakidis and Kotsakis, 1978).

The National Observatory of Athens was officially inaugurated by King Otto I (Otto von Wittelsbach I, 1815–1867) on the morning of 8 July 1842 (using the current calendar system), during the partial solar eclipse of that day. The first (meridian) astronomical observations were performed on the evening of 21 September 1846 by the Observatory's founder and first Director, Georgios K. Vouris (1790–1860), Professor of Mathematics and Physics at the University of Athens. Vouris (Figure 2) also taught astronomy after 1844. As Director of the Observatory he organized its offices and conducted mainly meteorological obser-vations. After his retirement, due to health problems in 1858, the Director's duties were temporarily carried out by Professor I.G. Papadakis of the University of Athens. Later that year, Baron Simon Sinas (son of George) proposed that the Directorship be given to the German astronomer Johann Friedrich Julius Schmidt (1825–1884).

Schmidt (Figure 2) was distinguished as an Assistant to Friedrich Wilhelm August Argelander (1799–1875), the creator of *Bonner Durchmusterung* catalogue (1859-1862). In 1858 Schmidt accepted the offer by Baron Simon Sinas and became the second Director of the Observatory, with a monthly salary of 1,000 drachmas, paid by Baron Sinas (Plakidis, 1960).

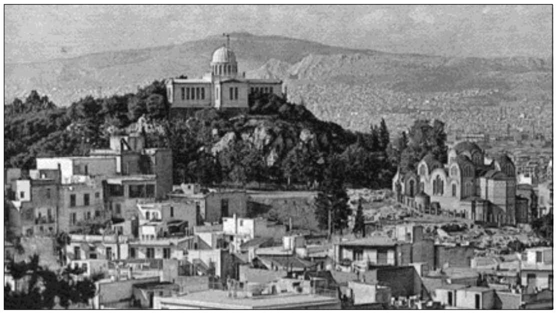
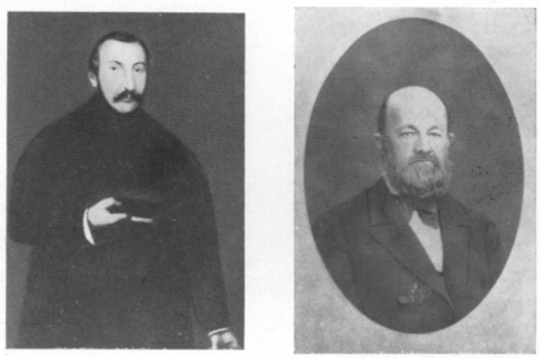


Figure 1: The National Observatory of Athens.

The scientific work of Schmidt in Athens included observations of nebulae, comets, sunspots, meteors and the zodiacal light. His main accomplishment, however, was the compilation and drawing of a large topographical map of the Moon, the result of 34 years of lunar observations (between 1840 and 1874). This exquisite map was bought by the Prussian Academy of Sciences, which financed its printing and publishing in 1878, along with the accompanying volume of explanatory notes. It is mainly for this work, which shows 32,856 distinct characteristics of the lunar surface, and is its most detailed representation before the era of photographic atlases, that Julius Schmidt won a place in the history of astronomy. A capable observer of the skies, Schmidt discovered a comet in 1862, five variable stars and a nebula (1865-1866, 1872-1873), and two novae: Nova Coronae Borealis on 13 May

1866 and Nova Cygni 1876. His papers were written in German and published in two series entitled *Publications of the Observatory of Athens*. This publication was paid for by Baron Simon Sinas (ibid.).

After the death of Julius Schmidt, the directorship was given to his collaborator Demetrios Kokkidis (1840–1896), who occupied it for six years, until 1890 (see Figure 2). In that year a young astronomer, educated in France, returned from Paris. His name was Demetrios Eginitis (Figure 2), and he was destined to play a key role at Athens Observatory.



G. VOURIS
(1790 - 1860)

J. SCHMIDT
(1825 - 1884)

D. KOKKIDIS
(1840 - 1896)

D. EGINITIS
(1862 - 1934)

Figure 2: The first four directors of the National Observatory of Athens (courtesy: Institute of Astronomy and Astrophysics, National Observatory of Athens).

2 THE EDUCATION AND CAREER OF EGINITIS BEFORE THE ATHENS OBSERVATORY DIRECTORSHIP

Demetrios Eginitis (Archer, 1935) was born in Athens on 22 July 1862, and graduated from the famous Varvakeio School of Athens in 1879. In the same year, he began his studies in the Faculty of Physics and Mathematics in the Philosophical School at the University of Athens (Figure 3). He graduated in 1886 with a Doctor of Philosophy degree in mathematics (Stefanides, 1948). The Athens University's Council for post-doctoral studies awarded him a scholarship so that he could take astronomy and mathematics classes at the Sorbonne in Paris. The following year, on 1 November 1887, he was accepted as an apprentice astronomer (*élève astronome*) at the meteorological observatory of Montsouris and, somewhat later, at the Paris Observatory, where he finally became a staff astronomer, in 1889.

When in France, Eginitis also worked at the Laboratory for Stellar Spectra in Salet, at the Physics Laboratory of Cornu, at the meteorological centre of Parc Saint Maur and at the Meudon Observatory. In addition, he worked outside Paris for a while, at the Observatory of Nice, and even outside of France, in Lockyer's astronomical laboratory in England.

At the Paris Observatory, Eginitis worked diligently for two years with the meridian circle carrying out regular equatorial observations (i.e. measurements of the culmination of stars for mapping of the northern skies and determinations of the proper motion). He also observed asteroids and variable stars with the meridian telescope located in the western dome (Makris, 1975).

Eginitis became internationally known for his classic treatise *Sur la Stabilité du Système Solaire* (*On the Stability of the Solar System*), in which he studied the secular variations (anomalies) of the semi-major axes of the planetary orbits. He submitted this in 1889 to the Paris Academy, where it was presented by Rear-Admiral Mouchez (the Director of Paris Observatory). In the same year, his treatise on celestial mechanics was published in the *Annales de l'Observatoire de Paris* (Eginitis, 1889), where for the first time Eginitis is referred to as a staff astronomer (*astronome*); this was an important career step for such a young man.

2.1 The Treatise *Sur la Stabilité du Système Solaire*

Astronomy prior to 1900 was based on the assumption of the invariability of the semi-major axes of the planetary orbits. It was assumed that if, due to the mutual attractions among the planets, their distances from the Sun were secularly changing, even by an imperceptible amount every year, then after several thousands of years their approach to or recession from the Sun would be such that the Solar System would be destroyed in either case. Therefore, many eminent scientists attempted to prove the constancy of the axes of the planetary orbits, including the mathematicians and astronomers J.L. Lagrange (1736–1813), P.S. Laplace (1749–1827) and S.D. Poisson (1781–1840). Moreover, this hypothesis was casually taken as a fact by many astronomers of the nineteenth century. The problem had been solved, albeit incompletely, with respect to first-order anomalies by Laplace, with respect to first-order and second-order anomalies by Lagrange and Poisson, and with respect to third order anomalies by S.C. Haretu (Eginitis, 1951: 16).

Eginitis proved in a mathematically-elegant way that the average distances of the planets from the Sun do not remain constant but instead are constantly changing: He proved that the semi-major axes of the planetary orbits are subject to anomalies of the third order, which are periodic but with large durations that equal the periods of the anomalies of the orbital eccentricities and inclinations. Eginitis verified his work in the cases of Earth and Saturn, planets that were currently approaching the Sun due to these anomalies. The reason for these anomalies is the attraction by Venus in the case of the Earth, and the attraction by Jupiter in the case of Saturn.

Eginitis refuted the basic question that was asked in the Academy, which is whether our world will be destroyed. The Earth would approach the Sun for 20,000 years and then the semi-major axis of its orbit will start to increase for a corresponding period of time. After that, the cycle will continuously repeat. To the next question, whether the Earth would ever approach the Sun to the point where the heat would be unbearable and life on Earth would be impossible, Eginitis answered that the variations of the average distance to the Sun will be so small that no special effects will be observable on Earth. With this work Eginitis won the appreciation and respect of the French astronomical community and especially of Camille Flammarion (1842–1925), who congratulated him in a letter.

During his period in France, Eginitis (Figure 4) published more than one thousand astronomical observations in the *Annales de l'Observatoire de Paris* and in the *Comptes Rendus* of the French Academy of Sciences (Eginitis, 1951: 14).

2.2 The Invitation by the Greek Government

Eginitis' accomplishments quickly became known in Greece, and in 1889 the Greek Prime Minister, Charilaos Trikoupis, notified him through Petros Lykoudis, the Military Attaché at the Greek Embassy in Paris, that the Government wanted him to return to Greece and apply his knowledge to the National Observatory of Athens as its Director. Although moved by the honour of this invitation, Eginitis hesitated because he knew that following the death of Schmidt, the Observatory had been largely inactive. Indeed, the bad economic situation of the nation and the death of Baron Simon Sinas on 27 April 1876 had left the Observatory in a difficult position.

Nevertheless, after repeated pressure by Trikoupis, Eginitis decided to abandon his successful career in Paris and return to Greece, but only after the Greek Government promised that it would fulfil the following four conditions (see Eginitis, 1951: 22):

a) The purchase of new scientific instruments;
b) The construction of adequate new buildings;
c) Appointment of sufficient scientific and supporting staff of Eginitis' choice; and
d) Payment to him of a salary equal to what Schmidt had received.

So the Greek Government promised to supply Eginitis with the necessary means and staff to establish astronomical research at a viable level in Greece, and even agreed to pay him Schmidt's salary (although that had been paid by Baron Sinas, and was far beyond what any other public sector employee was receiving at the time). Fortunately, King George I of Greece had shown a special interest in Eginitis, and so the Minister of Education, G. Theotokis, was able to submit a special bill to the Parliament which explicitly mentioned the conditions relating to the new Director of the Observatory, and his elevated salary 'honoris causa'. Despite the peculiarity of the bill, on 17 May 1890 it was approved unanimously by all the parties in the Parliament, and on 19 June 1890, at the age of just 28, Demetrios Eginitis was appointed Director of the National Observatory of Athens (see Makris, 1975).

3 THE FIRST STEPS AS A DIRECTOR

Eginitis immediately began developing and applying his program, slowly but systematically working his way through the many obstacles imposed by the political strife of the period and the usual bureaucracy.

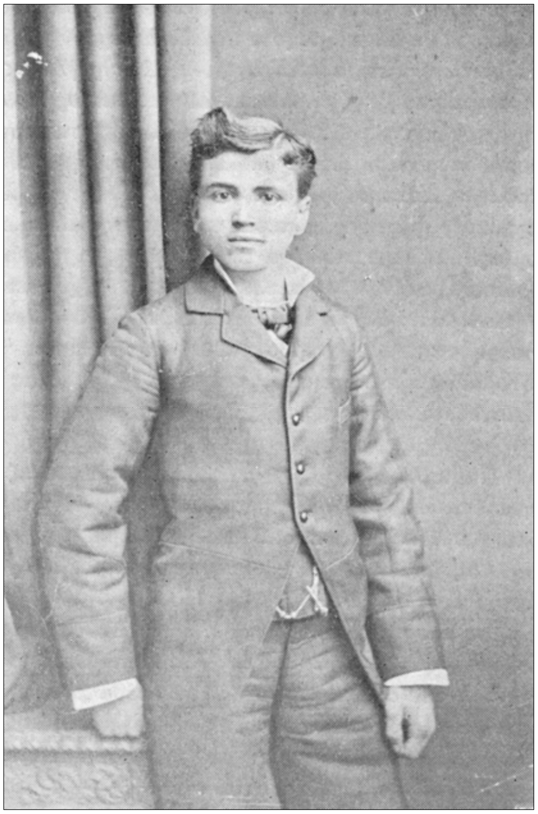


Figure 3: Eginitis as a student (courtesy: Institute of Astronomy and Astrophysics, National Observatory of Athens).

Figure 4: Eginitis as an astronomer in Paris (courtesy: Institute of Astronomy and Astrophysics, National Observatory of Athens).

The Observatory building was the original one on the hilltop opposite the Acropolis. Eginitis provided guidelines for the construction of a new building for the observers, and he also arranged for the planting of a cluster of pine-trees around the main building. After that, domes were erected for a meridian telescope and an equatorial telescope. The latter instrument was sited on an adjoining hill, which was specifically bought by Eginitis for this purpose. These activities and the purchase of the new instruments were financed by rich donors, whom Eginitis knew personally and had persuaded to provide the necessary funds (ibid.).

In 1892, two years after his appointment as Director of the Observatory, Eginitis was also appointed Professor of Astronomy and Geodesy at the Military School of Army Officers (which was called the *Evelpidon School*), a post he retained until 1902. In 1895 the next Government, under Theodoros Deligiannis, submitted a bill to Parliament proposed by Eginitis "… about the organization of the Observatory and its stations." When this bill became a law, the National Observatory of Athens was divided into separate Astronomical, Meteorological and Geodynamic Departments. With the separation of these activities in place, Eginitis proceeded to make following organizational changes:

a) In the Astronomical Department he organized the chronometer regulation service for ships of the Greek Navy and commercial ships based on the accurate determination of local time made from regular meridian observations with the Starke-Fraunhofer Transit Circle. That is, Eginitis essentially created the official time service for all maritime calculations.
b) Through the Meteorological Department, he founded the Greek Meteorological Service (today known by its Greek initials as EMY), and organized a network of stations covering most of Greece. The EMY started issuing daily weather bulletins that were sent to the major agricultural centers and port authorities of the nation. Eginitis connected the EMY with the rest of the world through the telegraphic network of the Eastern Company; thus, the EMY received free meteorological telegrams from 55 foreign meteorological stations.
c) For the study of earthquake activity in Attica by the Geodynamic Department, he established seismographic stations in cooperation with the geologist S. Papavassileiou (Plakidis, 1960: 7).

In 1896 Eginitis succeeded Demetrios Kokkidis as Professor of Astronomy and Meteorology at the University of Athens, a position he retained until his death. In 1903 he became Dean of the Philosophical School of the University, and it was during his tenure that a separate School of Physics and Mathematics was formed. In 1908-1909, Eginitis would become Dean of this School as well.

The first period of Eginitis' Directorship ended on 18 July 1910 when he submitted his resignation from his positions at the Observatory and the University. His resignation was a protest against the political 'cleansing' of the University by the Government, which was expelling its political opponents from public institutions. However, after the Minister of Education promised that all the expelled professors would return to their previous posts, and Prime Minister, Eleftherios Venizelos, insisted on this happening, Eginitis resumed his duties as Director of the Observatory (on 17 November 1910) and as a Professor at the University (on 24 January 1912).

Beyond his administrative and scientific work, Eginitis was essentially the permanent representative of Greece at international conferences on astronomy, seismology, geography and even of archaeology, because of his multiple interests and the fact that he was fluent in French, German and English. For example, in 1905 he participated in the international archaeological conference held in Athens, where he presented his paper on "The climate of Athens in the ancient times" Kotsakis, 1979: 27). In another such conference (in Cairo, in 1909) he presented his work on "La brise de mer et la bataille de Salamine" (i.e. "The sea breeze and the naval battle of Salamis") (Eginitis, 1951: 52).

4 REFORM OF LOCAL TIME AND THE CALENDAR IN GREECE

In August 1908, Eginitis represented Greece at the International Geographical Conference in Geneva, which was held to commemorate the 350 years of the city's University. As a Vice-president of the relevant section he submitted a proposal "… for the regular wireless broadcasting at certain times daily of the accu-rate local time of each place, in order to determinate the geographical latitudes at both sea, for the benefit of navigation, and on land, for the advancement of astronomy, geography and of science at large." This proposal was unanimously accepted by the participants and became an international agreement among the nations of the Earth (Eginitis, 1951: 49).

Greece's adoption of the international time system and time zones was the direct result of Eginitis' persistence on this matter. The official time in Greece was, up to then, the mean local time of Athens. However, when the railroad connected Greece with the rest of Europe there was an urgent need to link the national time system with the European one. Eginitis' intense efforts persuaded the Greek Government to adopt the hour zone of Eastern Europe on 28 July 1916, and it is still in force in Greece today. Eginitis wrote an article on the great importance of the time change under the title "The unity of time". This article was published in two parts by the newspaper *Estia* on 12 and 13 June 1916, and in it he explains why Greece adopted the eastern European hour:

> Greece lies mostly in the section [hour zone] of Eastern Europe and partly in the one of the Central Europe, as well; therefore, according to the rule of the sections system, we must take the time of the Eastern Europe, which is closer to the local time for the largest part of the country. By adopting the time of Eastern Europe, we shall turn our clocks 25 minutes ahead, while if we adopted the time of Central Europe we should turn them 35 minutes back. Therefore, if instead of the central European time we adopt the eastern European one, all our occupations will start one hour earlier.

In addition to the local time reform, Eginitis introduced the measure to adopt the Gregorian Calendar as the civil one in Greece. The Gregorian Calendar replaced the Julian Calendar in 1923, along with the International System of weights and measures. On the important subject of the calendrical reform Eginitis gives us interesting information through his announcement, "The question of the reform and of the unity of the calendar", which was presented to the Academy of Athens at its meeting on 24 February 1927:

> In December 1918, realising that not only the difficulties on which the unification of the calendar was stumbling had ceased to exist, but also important international and national reasons supporting its immediate application had appeared due to the World War, I considered that I had to seize the opportunity and propose the correction of our calendar. Therefore, I submitted, in December 1918, to the Greek Government a lengthy memorandum in which I was proposing: a) The addition of 13 days to the civil calendar's date, leaving the date of the religious festivals, i.e. of the religious calendar, intact, until an agreeing decision of our Church. b) The introduction in Greece of the International System of weights and measures.

Eventually this memorandum was communicated from the Ministry of Education to the Holy Synod of the Church of Greece. The Synod formed a committee, which voted in its last meeting, on 6 March 1919, that the State could change the calendar, while the Church, in agreement with the Ecumenical Patriarchate, would proceed to the compilation of a new calendar. The Greek Government, after a suggestion by Eginitis, proceeded to make the change. In the announcement mentioned above, Eginitis reports:

> By our suggestion, with a legislative decree issued on 18 January 1923 [old calendar] the Julian Calendar, being in force in Greece since two millennia, was replaced by a civil calendar according to which was accepted the Gregorian chronology since 16 February 1923 [old calendar], a date that was named 1 March 1923, without any change of the religious festivals.

Eginitis proposed at the first conference of the International Astronomical Union (IAU), held in Rome in May 1922 that the League of Nations should establish a Committee for calendrical reform. This Committee was indeed established by the League of Nations; in it Eginitis was a member representing the Ecumenical Patriarchate and introducer of the issue. The League of Nations appointed Eginitis as a member of its permanent Commission for the calendrical issue, while the Ecumenical Patriarchate awarded him a higher honorary office (officium) for his distinguished services to the Church. In parallel, the International Association of Scientific Academies established a Committee to study the disadvantages of both the Julian and the Gregorian Calendar and suggest a reformed calendar, free of these disadvantages. Eginitis was again appointed as a member of this Committee (*Comité- d'étude pour la Réforme du Calendrier*). After the decoupling of the civil calendar from the Orthodox Church and the canon of the religious holidays, the Greek Government accepted the Gregorian Calendar as the civil one. The Church of Greece initially retained the Julian Calendar, but in 1924 it adopted the New Rectified Julian Calendar, which was identical to the Gregorian one for the next few centuries (Theodossiou and Danezis, 1995: 145f).

5 MATURITY AS DIRECTOR AND SCIENTIST

It can be said that Demetrios Eginitis (Figure 5) was the one scientist who contributed in an exemplary way with his studies to the promotion of astronomy, meteorology and seismology in Greece, while at the same time helping considerably the development of the national economy, especially in the fields of agriculture and navigation. During the forty-four years in which he was Director of the National Observatory of Athens, through his scientific work in astrometry, his international publications, his presence at international conferences, his books and articles, and by sending young astronomers to study abroad, he introduced the Observatory to the international scientific community. In the later years he participated in the Madrid Conference on Geodesy and Geophysics. The last international conference he attended was the IAU meeting at Cambridge (Massachusetts) on 2 September 1932, where he also represented the Academy of Athens (Eginitis, 1951: 90).

Figure 5: A sketch of Demetrios Eginitis as Director of the National Observatory of Athens (courtesy: Department of Astrophysics-Astronomy and Mechanics, National and Kapodistrian University of Athens).

Eginitis became a Fellow of many foreign scientific societies, such as the Royal Astronomical Society in London, the French Astronomical Society (Société Astronomique de France), the German Astronomical Society (Astronomische Gesellschaft), the Portuguese Institute of Coibra and the International Meteorological Committee (Comité Météorologique International). He was also a member of the IAU Committee on Meridian Observations. The director of the Paris Observatory, Dr Loovy, in a speech to the Academy of Sciences under the title "Nouvelle organisation des études d'Astronomie et Physique du Globe à l'Observatoire National d'Athènes" (i.e. "New organization of the astronomical and geophysical studies at the National Observatory of Athens") emphasized that

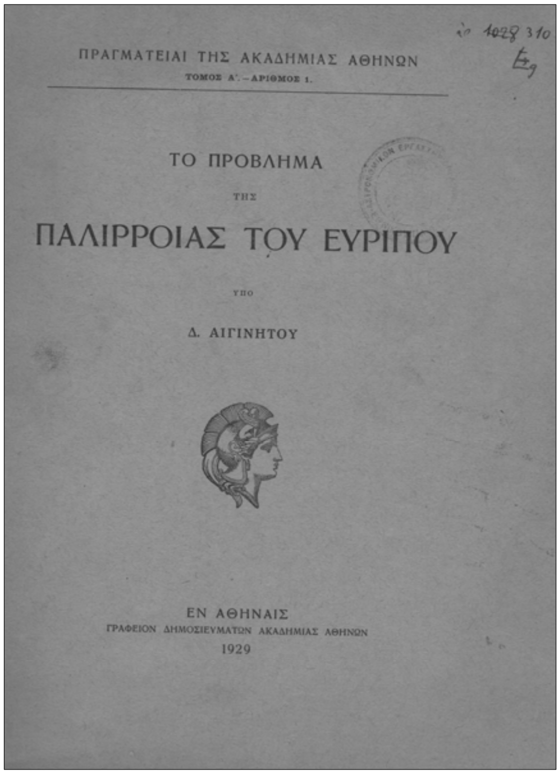
> Since D. Eginitis assumed the direction of the Athens Observatory in 1890, a considerable development took place in the scientific activity of this institution, which had already been distinguished by the publications of Julius Schmidt, the famous predecessor of the present Director. The Paris Observatory can lay a claim upon the honor that in D. Eginitis it created one of its best students. (Xanthakis, 1975: 3).

Eginitis was honoured several times by the Academy of Athens, while France also honoured him with the medal of the Legion of Honour (Arthur, 1935: 12).

5.1 The Explanation of the Evripos Phenomenon

Aristotle and Eratosthenes (ca 200 BC) tried to explain the phenomenon of the anomalous strong tidal current observed in the Evripos Straits in Chalkis, central Greece. In modern times, especially during the nineteenth century, other scientists endeavoured to do the same. A complete explanation was finally provided by Demetrios Eginitis.

According to Eginitis (1926), the main tidal bulge begins in the Mediterranean Sea and travels to the open Aegean Sea and the South Euboean Gulf (Notios Evoikos Kolpos). Because of the different length of the path, the tide coming from the south arrives at the narrowest point, where a bridge separates the South from the North Euboean Gulf (Voreios Evoikos Kolpos), seventy-five minutes earlier than the tide that arrives from the north, which has travelled around the long island of Euboea (Evoia), the second largest of the Greek islands. So the level of the water to the south of the bridge rises 30 to 40 cm relative to that of the water to the north of the bridge, thus creating a northward current. Six hours later the conditions are reversed and the waters move southward. This 'normal' situation prevails when the lunar phase is near Full Moon or New Moon. However, for several days around the first and last quarter phases, that is, when the tides are the weakest, the currents are mainly irregular. One must remember that in a small restricted sea like the Aegean, the tides are much weaker than in any ocean. Therefore, during the 'irregular days', other factors come into play, such as the morphology of the sea floor, the current atmospheric pressure, the winds, etc. (see Kotsakis, 1977).



ΠΡΑΓΜΑΤΕΙΑΙ ΤΗΣ ΑΚΑΔΗΜΙΑΣ ΑΘΗΝΩΝ
ΤΟΜΟΣ Α'.–ΑΡΙΘΜΟΣ 1.

ΤΟ ΠΡΟΒΛΗΜΑ
ΤΗΣ
ΠΑΛΙΡΡΟΙΑΣ ΤΟΥ ΕΥΡΙΠΟΥ
ΥΠΟ
Δ. ΑΙΓΙΝΗΤΟΥ

ΕΝ ΑΘΗΝΑΙΣ
ΓΡΑΦΕΙΟΝ ΔΗΜΟΣΙΕΥΜΑΤΩΝ ΑΚΑΔΗΜΙΑΣ ΑΘΗΝΩΝ
1929

Figure. 6: The front page of Eginitis' treatise about 'The problem of the Evripos Tide' (1929).

The study of the 'Evroipos Phenomenon' by Eginitis was published in a separate 128-page volume of large format, under the title "To provlema tis palirroias tou Evripou" ("The problem of the Evripos Tide") in the series *Pragmateiai tis Academias Athinon* (*Treatises of the Academy of Athens*), Volume 1, Number. 1 (Publications Bureau of the Academy of Athens, 1929) (see Figure 6 above). The same treatise was published in French under the title "Le problème de la marée de l'Europe", as an excerpt of the *Annales de l'Obser-vatoire National d'Athènes*, Volume XI (Athens, 1931).

As is evident from this study, Eginitis worked not only in astronomy, meteorology and seismology, but also in the science of tidal studies. At the same time he was delving into geography, and proposing to the State solutions on irrigation and on the exploitation of the Greek natural resources. On this topic, Eginitis had already developed numerous initiatives by 1908, and in his work on the Athenian climate he discussed the provision of water to Athens. In this work he suggested that the problem could be solved with the construction of dams in Attica and the creation of reservoirs where rainwater and the flow from melting snow from the Parnitha Mountain could be stored. He also proposed the construction of trenches on the slopes of Attica Mountains for the controlled channeling of water into the sea. The suggestions put forward by Eginitis became reality with the construction of the Marathon Dam in 1931, which solved Athens water problems for the next decades.

6 EGINITIS AS THE MINISTER OF EDUCATION

On 21 April 1917 Eginitis was appointed Minister of National Education in the new Government of Alexandros Zaimis, and he remained in this post until 14 June of that year. His participation in the Government was a result of the esteem by King Alexander I and of the trust by the French and British during World War I. Negotiations with the French General, Maurice Sarrail (1856–1929), Chief of the Allied Forces in the region, on how King Constantine (who resigned following pressure from the Allies) would leave Greece, took place with the initial participation of D. Eginitis, who later collaborated with Pericles Argyropoulos on the same issue. In fact, King Constantine's farewell address to the Greek people was written by Eginitis and Fokion Negris.

Even after his departure from the Ministry, Eginitis continued to have almost the final word on educational matters: in 1918 he was the President of the Committee that wrote the law for the functioning of the University of Athens, then the sole university in Greece. Eginitis was also sent by the Greek Government on several missions to other countries. Probably his most important mission was in the summer of 1920, when he went to Paris in the hopes of influencing the French press, which was then maintaining a pro-Turkish stance. One month after Eginitis arrived, the French Prime Minister and Foreign Minister, Alexandre Milleran (1859–1943), called the editors of the newspapers and urged them to support the Greek interests, which essentially were aligned with the French ones. Also, through an inspired three-hour speech and discussion in the French senate, Eginitis was able to convince the senators that their pro-Turkish policy would bring a collapse of French influence in the East.

On 6 March 1926, Eginitis was once more appointed Minister of Education, this time in the dictatorial Government of Theodoros Pangalos. Although this second period would be only slightly longer than the first one, lasting until 16 June 1926, it resulted in some major accomplishments. It was Eginitis' decision that led to the construction of five large educational buildings in Athens. In one of these the Experimental School of the University of Athens was created, which filled a large gap in the Greek educational system. In the countryside, he arranged for more than 300 public schools to be built, contributing considerably to the upgrade of primary education throughout the nation.

In Athens he bought the Heinrich Schliemann mansion (Iliou Melathron) to use as the National Gallery; he restored and renovated the archaeological museum of ancient Olympia; and he arranged for the purchase by the State of the Duchess of Placentia's mansion in downtown Athens, where the Byzantine Museum found a better residence after the inadequate basements of the Academy of Athens.

Eginitis also initiated the building of the Philekpaideftiki Etaireia (the Philo-educational Society) and, as the executor of the will of Marinos Koryialenos, he gave the required amount for the establishment of a microbiological laboratory at the University of Athens. He created the Organization of the Educational Council in the Ministry of Education and restored to operation the second Greek university, the University of Thessaloniki, and opening its Philosophical School. He published the statutes of operation of the Rizareios Ecclesiastical School, the Athens Eye Hospital, the Pediatric School of the University of Athens and various other institutions. He submitted bills for the protection of the intellectual property and compulsory education, while he reintroduced technical courses in the school curricula. He fenced in Marathon's Tomb and shaped its area, also adding a statue at its base. He negotiated with American archaeologists the issue of large-scale excavation of the archaeological sites in Athens, so that the whole project would be accomplished with the equal participation of Greek archaeologists (who had largely been ignored in previous agreements). In addition to all these, Eginitis succeeded in persuading Queen Olga to donate the heirlooms of the Kings of Greece, and these treasures are now exhibited in a special section of the Ethnological Museum.

However, the single most important act of Eginitis as the Minister of Education is almost certainly the founding of the Academy of Athens (Figure 7), in 1926. He had started to prepare the ground much earlier. Prime Minister Eleftherios Venizelos had succeeded in having Greece accepted into the International Union of Academies, and had promised that a Greek Academy would be established. Nevertheless, for various political reasons, the Academy had not been founded, and Greece was therefore in danger of being evicted from the Union. Eginitis considered the Academy a necessity regardless of the position in the International Union, so he proceeded with this issue immediately upon beginning his second ministerial term of office. The inaugural ceremony and session of the Academy of Athens took place on 25 March 1926 in the central hall of the estate built for it many decades earlier by Baron Simon Sinas during the reign of King Otto I. The session was declared open by Theodoros Pangalos, and those present were then addressed by Eginitis as the Minister of Education and by Fokion Negris, its first President. The founding of the Academy of Athens is considered by most reviewers as Eginitis' greatest gift to Greece. Demetrios Eginitis became Vice-President of the Academy in 1928, and was President from 10 January 1929.

7 THE LAST YEARS AND POST-MORTEM HONOURS

On 24 January 1931, in a special meeting of the Academy, the forty years (1890-1930) of scientific activity of Demetrios Eginitis were celebrated, and a special issue of the Academy's Proceedings was published. The then President of the Republic, Alexandros Zaimis, attended the celebration, while the Prime Minister, Eleftherios Venizelos, sent a message with his sincere wishes. From 1931 till his death, in 1934, Eginitis continued to offer his services to the Academy, and was its General Secretary in 1933-1934. He also continued his scientific work both in the Academy and at the Observatory. He consumed much of his time presiding over the Academy's Committee for the Greek language issue. Actually his very last text was the report on the conclusion of this Committee. This report, entitled "The language issue: The Modern Greek language", was published finally in 1958, in the "Treatises of the Academy of Athens" series (Volume 23, Number 4).

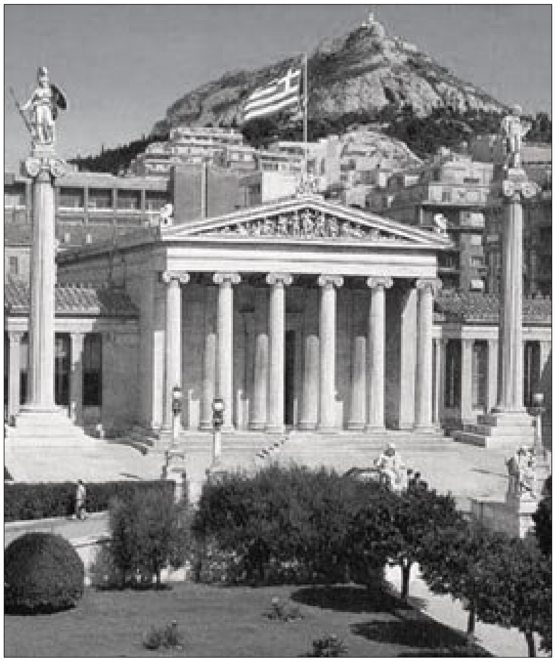


Figure 7: Academy of Athens.

Demetrios Eginitis died on 13 March 1934, and his State Funeral took place the next day in the church of St. George Karytsis in downtown Athens. He was buried in the First Cemetery of Athens, in a small mausoleum (see Figures 8 and 9) which was donated by the Athens municipality. The main funeral oration was given by the then Dean of the School of Physics and Mathematics at the University of Athens, Professor of Chemistry, Konstantinos D. Zeghelis (1870–1957). On the day of Eginitis' death, the University of Thessaloniki held a scientific memorial meeting, in which his work was described by Professor Petros Kontos.

Figure 8: Eginitis' mausoleum in the First Cemetery of Athens.

After Eginitis' death, various biographical texts were published, with emphasis in his scientific activities and work. Most important among these are the one written by Stavros Plakidis (Astronomy Professor at the University of Athens) and published in the *Bulletin of the Secondary Education Mathematicians' Association* (volume of 1934, pp. 445-446) and the one published in the March 1935 issue of the *Journal of Calendar Reform*, an American edition of the World Calendar Association, written by Laird W. Archer under the title "Passing of a Pioneer", which fully represents the personality of D. Eginitis. Another contemporary biographical sketch can be found in the *Who is Who in Central and Eastern Europe* (Zurich 1933-1934).

Figure 9: On the east side of his mausoleum it is written in Greek: 'Religion-Astronomy-Education'.

In 1974, on the occasion of the 40th anniversary of Eginitis' death, relatives, students and his surviving friends and collaborators formed a committee (comprising University of Athens Professors L. Karapiperis and D. Kotsakis, and the Lecturer K. Makris) to organize a scientific memorial meeting and the publication of a volume with scientific papers from all of the sciences to which Eginitis contributed. On 26 February 1975 this meeting was held in the large hall of the Evgenides Foundation, and in the associated scientific tome published *in memoriam*, thirty-four papers on astronomy, mathematics, meteorology seismology, and physics, written by 39 authors, found their place.

8 PUBLISHED WORKS OF DEMETRIOS EGINITIS

8.1 Publications of the Academy of Athens

The very first scientific announcement at the Academy of Athens, in its April 8, 1926 meeting, was the talk "On variable stars" by Eginitis. This was the first of a long series of announcements by Eginitis at the Academy, including the following:

a) In Greek (titles translated into English): "The droughts and the necessary watering and irrigation works in Greece." (meeting of 2 December 1926); "The calendar reform in the League of Nations." (24 February 1927); "The transits of Mercury in front of the solar disc." (8 December 1927); "The Corinth earthquake of 22 April 1928 and its consequences." (3 May 1928); "The problem of the Evripos Tide." (1928 December 8; this was included, also, in the *Treatises* series—see below); "The 'blue coal' and the industrial exploitation of the Evripos current." (17 January 1929); "On the climatological adequacy of Egyptian cotton cultivation in Greece." (16 May 1929); and "The evolution of the worlds." (26 December 1929).
b) In French: "La contribution des géographes de l'antiquité à la découverte de l'Amérique." (16 April 1931); "Les marées dans la science ancienne." (12 May 1932); and "La longitude de l'Observatoire d'Athènes." (20 May 1932).

In particular, in the series *Pragmateiai tis Academias Athinon* (*Treatises of the Academy of Athens*) were published, as already mentioned, "The problem of the Evripos Tide." in Volume 1, Number 1 (1929) and "The language issue: The Modern Greek Language." in Volume 23, Number 4 (1958).

8.2 *Annales de l'Observatoire National d'Athènes*

The Annals of the National Observatory of Athens were published in French, totaling 12 volumes up to 1932. Among other papers, Eginitis published the following important contributions:

"Le tremblement de terre de Constantinople." *Annales de Géographie*, IV, 151-165 (Athènes, 1895).
"Le climat d'Athènes.", *Annales de l'Observatoire National d'Athènes*, I, 1-220 and 391-395 (Athènes 1898b).
"Le climat de l'Attique.", *Annales de Géographie*, XVII, 98-115 (Athènes 1908).
"Sur la question du calendrier dans l'Europe Orientale.", *Annales de l'Observatoire National d'Athènes*, IX, 7-17 (Athènes 1926).
"Le problème de la marée de l'Euripe.", *Annales de l'Observatoire National d'Athènes*, XI (Athènes 1931).

Other titles of papers in the *Annales* by Eginitis are:

"La latitude de l'Observatoire d'Athènes"; "L'équatorial Doridis"; "L'éclipse solaire de 30 Août 1905"; "La Comète Morehouse"; "Les grands sismes du 28 Décembre 1908 et du 23 Janvier 1909"; "Les éléments du magnétisme terrestre à Athènes pendant les années 1904-1908"; "Etude des sismes survenus en Grèce, pendant les années 1904-1908"; "Les éléments magnétiques à Athènes, pendant les années 1904-1908"; "La comète de Halley"; "Saturne, 1910-1911 et 1911-1912"; "Jupiter, 1911"; "La Nova Lacertae"; "Le tremblement de terre du golf de Corinthe du 30 mai 1909"; "Les sismes survenus en Grèce, pendant les années 1909-1911"; etc.

Unfortunately, due to lack of adequate funding, the publication of the *Annales* ceased after 1932, thus leaving valuable observational material in the archives of the Observatory of Athens unpublished.

8.3 The *Analecta* of 1920

The daughter of D. Eginitis, Egli Eginitis-Botsaris, collected some works of her father, written in French, and published them in 1920 as a separate volume under the general title *Analecta* (Athens 1920). The most important of these works are:

"Mémoire sur la Stabilité du Système Solaire"
"Résultats des observations d'étoiles filantes"
"Résultats des observations sismiques, faites en Grèce, pendant l'année 1899"
"Observations météorologiques, faites aux stations départementales, pendant les années 1894-1899"
"Anciennes observations de pluies d'étoiles filantes"
"L'agrandissement des disques du soleil et de la lune à l'horizon"
"Résultats des observations sismiques, faites en Grèce de 1893 à 1898"
"La politique turcophile équivaudrait à l'effondrement le l'influence de la France en Orient"

8.4 Books

The titles of the following Greek books by Eginitis have been translated into English:

Practical Meteorology: Guide for the Meteorological Stations in Greece (Athens, Ethniko Typographeio, 1892).
Lessons in Geodesy (Athens, Evelpidon Military School, 1895-1896).
General Astronomy (Athens, Evelpidon Military School, 1897).
The Sky (translation & adaptation of C. Flammarion's book) (Athens, Syllogos pros diadosin ofelimon vivlion, 1900).
The Climate of Greece, two volumes (Athens, Vivliothiki Marasli series, D. Sakellariou, 1908).
Elements of Cosmography, for 4th Grade of Gymnasium Schools (Athens, Organismos Ekdoseos Didacticon Vivlion, 1910).
Astronomy Lessons, Taught at the National University in the Academic Year 1914-1915 (Athens, Panepistimiakon Typographeion, 1917).
Stars (Athens, Syllogos pros diadosin ofelimon vivlion, 1918).
Meteors (Athens, Syllogos pros diadosin ofelimon vivlion, 1927).

8.5 Other Scientific Papers in French

"Sur la Stabilité du Système Solaire." *Annales de l'Observatoire de Paris*, Mem. 19, H1 (1889).
"Sur l'agrandissement des disques du Soleil et de la Lune à horizon." *Comptes Rendus des Séances de l Académie des Sciences*, 126, 1326-1329 (1898a).
"Radiants observés à l'Observatoire National d'Athènes." *Astronomische Nachrichten*, 159, 361 (1902).
"Le climat d'Athènes en Grèce ancienne." *Comptes Rendus du Congrès International d'Archéologie, Ire session,* Athènes, imprimerie 'Hestia', C. Meissner et N. Kargadouris (1905).

8.6 Miscellaneous Articles

The titles of the following articles in Greek by Eginitis have been translated into English:

"Cosmology", the text of his talk to the 'Society of the Friends of the People' on 8 April 1891, printed in the printing-office of the newspaper *Asty*, Athens, May 1891.
"Shooting stars and bolides." *Estia* newspaper, Athens, 9 January 1894.
Two announcements on the climate of Athens in the *Bulletin of the Industrial and Commercial Academy*, Athens, December 1895 issue.
"The Universe", his inaugural lesson at the University of Athens on 23 October 1896, was published in an issue of the *Athena* magazine, Volume 9, Athens, 1897.
"The Nature", *Athinai* newspaper's monthly inset no. 4, February 1908, pp. 17-20.
"The forecast of weather." *Melete* magazine monthly publication of the 'Syllogos pros diadosin ofelimon vivlion', issues 8-10, Athens, 1907.
"The National Observatory in the twenty years 1890-1910." Athens, 1910.
"Halley's Comet and the scientific struggle", the text of his talk to the 'Archaeological Society' on 8 December 1910, printed as a booklet, Athens, January 1911. (Of special interest are his notes on the large curvature of the comet's tail.)
"The latest appearance of Halley's Comet." *Melete* magazine (monthly publication of the 'Syllogos pros diadosin ofelimon vivlion'), issue 37, Athens, January 1911.
"Activities and needs of the National Observatory." Athens, 1916.
"The unity of time." *Estia* newspaper, Athens, 12 and 13 June 1916.
"The work of 25 years: activities and needs of the Observatory." Edition of the Observatory of Athens, Athens, 1916.
"The official time." *Imerologion tis Megalis Ellados*, Athens, G Drosinis, 1922.
"Is the Universe infinite?" *Imerologion tis Megalis Ellados*, Athens, G. Drosinis, 1923.
"On the change of the calendar", the memorandum he submitted at the IAU's session in Rome in 1922, published in the *Epistimoniki Icho* magazine, Spyridon N. Papanikolaou editions, Athens, 1923 February-March issues.
"The unknown forces." *Imerologion tis Megalis Ellados*, Athens, G. Drosinis publ, 1926.

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10 REFERENCES

Archer, L.W., 1935. Passing of a pioneer. *Journal of Calendar Reform*, 5(3), 12-16 (published by the World Calendar Association, New York).

Eginitis, D., 1889. Sur la Stabilité du Système Solaire. *Annales de l'Observatoire de Paris*, Memoires 19, H1.

Eginitis, D., 1898a. Sur l'agrandissement des disques du Soleil et de la Lune à horizon. *Comptes Rendus des Séances de l'Académie des Sciences*, 126, 1326-1329.

Eginitis, D., 1898b. Le climat d'Athènes. *Annales de l'Observatoire National d'Athènes*, I, 1-220, 391-395.

Eginitis, D., 1895. Le tremblement de terre de Constantinople. *Annales de Géographie*, IV, 151-165.

Eginitis, D., 1902. Radiants observés à l'Observatoire National d'Athènes. *Astronomische Nachrichten*, 15, 361.

Eginitis, D., 1908. Le climat de l'Attique. *Annales de Géographie*, XVII, 98-115.

Eginitis, D., 1916. Les tremblements de terre de Leucade et d'Ithaque des 23 et 27 Novembre 1914 et du 27 Janvier 1915. *Annales de l'Observatoire National d'Athènes*, VII, 27-32.

Eginitis, D., 1920. La politique turcophile équivaudrait à l'effondrement de l'influence de la France en Orient. In *Analecta*, Volume II. Athens, published by the author.

Eginitis, D., 1926. Sur la question du calendrier dans l'Europe Orientale. *Annales de l'Observatoire National d'Athènes*, IX, 7-17.

Eginitis, D., 1931. Le problème de la marée de l'Euripe. *Annales de l'Observatoire National d'Athènes*, XI.

Eginitis, Demetrios, 1951. Bibliographike Bibliotheke. Mega Hellenic Biographicho Lexicon. 2nd Volume. *Biomechanike Epitheorisis*. Athens [in Greek].

Haretu, S.C., 1877. Sur l'invariabilité des grands axes de l'orbites planétaires. *Comptes Rendus des Séances de l'Académie des Sciences*, 85, 504-506.

Karapiperis, C., Kotsakis, D., and Makris, K. (eds.), 1975. *Special Volume in Memorium Demetrii Eginitis (1862-1934)*. Athens, Chr. Zisoulis & Co. O.E. [in Greek].

Kotsakis, D., 1975. I kosmotheoria tou Demetriou Eginiti. In Karapiperis et al., 26-34.

Kotsakis, D., 1977. *Neoi Orizontes stin Astronomia*. Athens, editions *Zoe*, p. 33 [in Greek].

Makris, K., 1975. Mia megali morphi tis neoteras ellinikis astronomias. In Karapiperis et al., 16-25.

Plakidis, S., 1960. *Astronomy in Modern Greece*. Athens, G.S. Christou & Son.

Plakidis, S., and Kotsakis, D., 1978. *Astronomy in Modern Greece*. Athens, Chr. Zisoulis & Co.

Stephanides, M., 1948. *Istoria tis Physikomathematikis Scholis*. Athens, Ethniko Typographeio [in Greek].

"The Resolution for the organization of the Academy of Athens". *Ephemeris tis Kyverniseos*, No. 96a, 1926 March 18 and No. 104a, 1926 March 26, Athens [in Greek].

Theodossious, E., and Danezis, E., 1995. *The Odyssey of the Calendars. Astronomy and Tradition*. Athens, Diavlos Publications.

Xanthakis, I., 1975. Omilia gia tin prosfora kai to ergo tou D. Eginiti. In Karapiperis et al., 2-8.

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F.X. VON ZACH AND THE FIFTH CONTINENT

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Abstract: In his journals which were devoted mainly to astronomy, Franz Xaver von Zach also presented geographical and geodetic information about Australia. Initially these accounts were in German, but towards the end of his life he wrote them in French for a more general audience.

Keywords: geography and geodesy, Australia, ca. 1800

1 INTRODUCTION

Franz Xaver von Zach (see Brosche, 1998; 2001) was born in Pest (Hungary) in 1754 and died in Paris in 1832. So his life can be attributed to the 'Goethezeit', more so since his most active years were spent in Gotha, near Weimar, from 1786 to 1807, where he installed and used the new observatory with Duke Ernst II (1745–1804).

However, the kind of activity most relevant to this paper relates to his founding of several scientific journals, the *Allgemeine Geographische Ephemeriden* (or *AGE*) in 1798[1] and the *Monatliche Correspondenz zur Beförderung der Erd- und Himmelskunde* (or *MC*) in 1800.[2] Although their titles suggested the focus was more on terrestrial issues, in fact both were intended as astronomical journals. It was in the afterglow of the peak period in Gotha that Zach was able to found a third journal. This occurred in Genoa in 1818, with the launch of the French-language *Correspondance Astronomique* … (or *CA*).[3] Unfortunately, an index of this last one has yet to be produced, so we are not in a position to make any statistical statements about its papers.

The use of geography as a means of promoting astronomy—as in the case of Zach's first two journals—was partially a 'camouflage', hence Zach (1798: 157) admitted in a letter that the 'light' content with the human touch was a kind of 'Korkholz' (= cork wood) that helped him keep his journal (= ship) afloat. First of all, astronomy was (and even today partially is) intimately related to geodesy, and Zach's own research work spanned both disciplines. Second, Zach was personally interested in including 'soft' (i.e. non-astronomical) topics in his journals, from adventurous discoveries in distant lands, to mercantile products and even drinking habits and linguistic digressions on names of towns etc.

2 ZACH'S JOURNALS AND THE 'FIFTH CONTINENT'

In eighteenth century Europe, Australia was referred to as the 'Fifth Continent'. Although the earliest European exploration of the coastal regions of Australia occurred long before 1800, when Zach's first two journals were launched, the interior of the fifth continent provided scientists and explorers with a seemingly endless series of discoveries. Consequently, Australia (but not under this name) is well represented in Zach's journals. Given the competition for space from other regions of the globe, the reasons for this may be manifold, but one seems to be obvious: as a result of his birth in Hungary, a country that was largely on the edge of, rather than fully immersed in, scientific developments at the time, Zach was happy to welcome contributions to his journals from and about other 'fringe areas', be they in Europe (e.g. Poland, Portugal and Spain) or much further afield, like Australia.

Formally, contributions were more often than today reviews, therefore publications of second order. At that time, however, there was a greater desire and a greater necessity for such enlarged abstracts. Books were expensive, and foreign books were difficult to obtain. Consequently, famous journals existed which contained nothing but reviews, and Zach's journals, although not of this kind, did contain elaborate reviews and articles detailing the historical development of various topics. Very often these articles and reviews included detailed comments or notes by Zach, who relied on other sources for his information.

The keywords 'Neu-Holland' and 'Sydney' appear for the first time, but only very briefly, in the first volume of the *AGE* (on page 580) in a detailed review of a map of travels around the world. But characteristically Zach notes the absence of Sydney Cove on the otherwise precise map of Neu-Holland.

Continuing to search for 'Neu-Holland', we find that it occurs five times in the index of the second volume of the *AGE*. The most substantial contribution is a review of David Collins' new book about New South Wales and New Zealand which was published in London in 1798 (see *AGE*, 2: 349-362 (1798)). As the reviewer, Zach admits that initially he had been afraid to review this massive tome, but he was pleasantly surprised and ended up reading the entire book! He then describes the contents in such detail, that the reader not only encounters a short history of the foundation of the first colony, but also learns about the flora, the climate, and the indigenous people and their customs. The founding of an astronomical observatory at Sydney Cove, furnished with instruments by the British Board of Longitude, was noted with pleasure.[4] One is led to wonder about the accuracy of the geographical co-ordinates mentioned by Zach (a longitude of 151° 19′ 30″ E, and latitude of 33° 52′ 30″ S).

The next two volumes of the *AGE* only refer to Neu-Holland within the context of the heights of mountains everywhere in the world.

Since 28 volumes of Zach's second journal, *Monatliche Correspondenz* … (or *MC*) were produced, one can expect even greater Australian coverage in this journal, and this is indeed the case, for the following names appear more than once in the indexes: 'Neu-Holland' (five times), 'Van-Diemens-Land' (seven times), 'Sydney' (three times), 'Neu-Süd-Wales' (twice), Lieutenant James Cook, in connection with our topic (three times) and 'Neuseeland' (four times).

The first article in the *MC* (2, 599-624, 1800) is a large one with a rather distinctive title: "Über eine neuentdeckte Durchfahrt oder Meer-Enge, welche van Diemen's Land von Neu-Holland trennt" (or On the recently-discovered strait between Van Diemen's Land, i.e. Tasmania, and New Holland, i.e. the mainland of Australia). Once again, Zach begins with a comprehensive historical introduction about the discovery of Australia from the time of Pedro Fernandez de Quiros in 1606 onwards, then of van Diemen's Land and the long-held general belief that it was only a peninsula of the mainland. Finally, in 1798 Governor Hunter sent Lt. Flinders and the physician Bass on an expedition and they announced the true nature of Van Diemen's Land. Then Zach returns to the history and sightings made prior to 1798. The most recent statement on this topic was presented in a Letter to the Editor by Sir Joseph Banks, dated 10 February 1802, which appeared in the fifth (April 1802) volume of the *MC* (on pages 356ff). A map of Bass Strait based on information in this letter and in the earlier 1800 article was then published in the May 1802 issue of the Journal, and this is included here as Figure 1.

A little piece of political history appeared in the 16th volume of the *MC* in 1807 starting on page 34: an article in the (French) *Moniteur* (No. 42, of the same year) insisted that Cook's discovery of the east coast of Neu-Holland was based on earlier Portuguese and French efforts. Since Zach had left Thuringia in the summer of 1807, it is unclear whether the inclusion of these pages in the *MC* was due to him or to his disciple and follower, B.A. von Lindenau. The word 'Australien' appears in this article, but is used in context of the old meaning, namely the large mythical Southern land mass.

A unique item is present in the fourth volume of the *MC* (373ff, 1801), namely a letter from the Australian Aborigine, 'Bannolong', to his host in London, written after Bannolong returned to Australia. Although the original letter was in English, a German translation was included in the *MC*. In a footnote, Zach compares the content with the simplicity of the Homerian style.

A long paper dealing with Neu-Holland appears in the 17th volume of the *MC* (pp. 439-463, 1808) and relates primarily to the voyage of M.F. Péron (Paris 1807). The reviewer considers this a continuation of Collins' book (see above), and this connection suggests that Zach was the author. Pages 447 seq. provide information on what was then called Port Jackson, and on its surroundings. Following page 483 an original French map dating to 1802 is reproduced and this is shown here in Figure 2. Note that the observatory mentioned by Collins is not marked.[5]

The geographical determination of a northern and a southern port in Van Diemen's Land by D'Entrecasteaux's expedition is reviewed in the 19th volume of the *MC* (pp. 388 and 394, 1809), and these points are connected with others, e.g. the Cape of Good Hope.

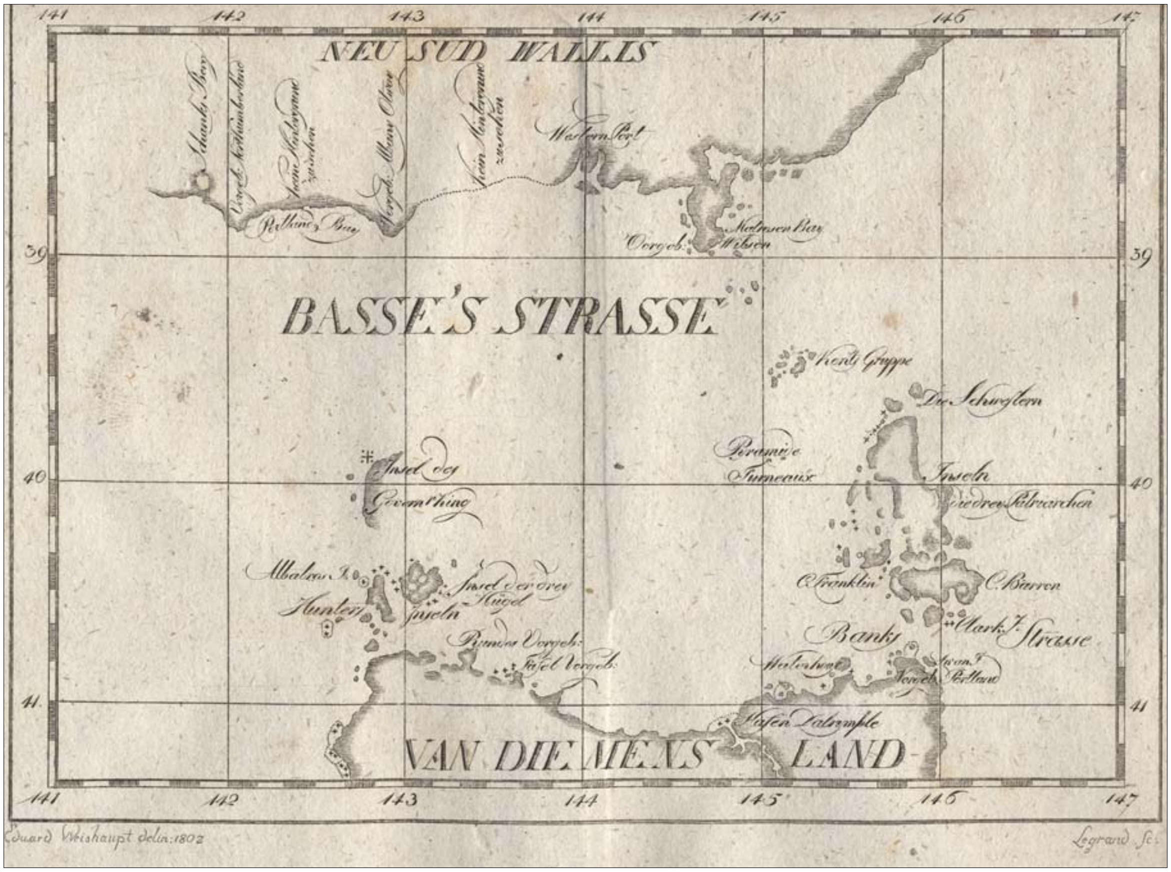

Figure 1: Basse's Straits (after *MC*, 5, 1802)

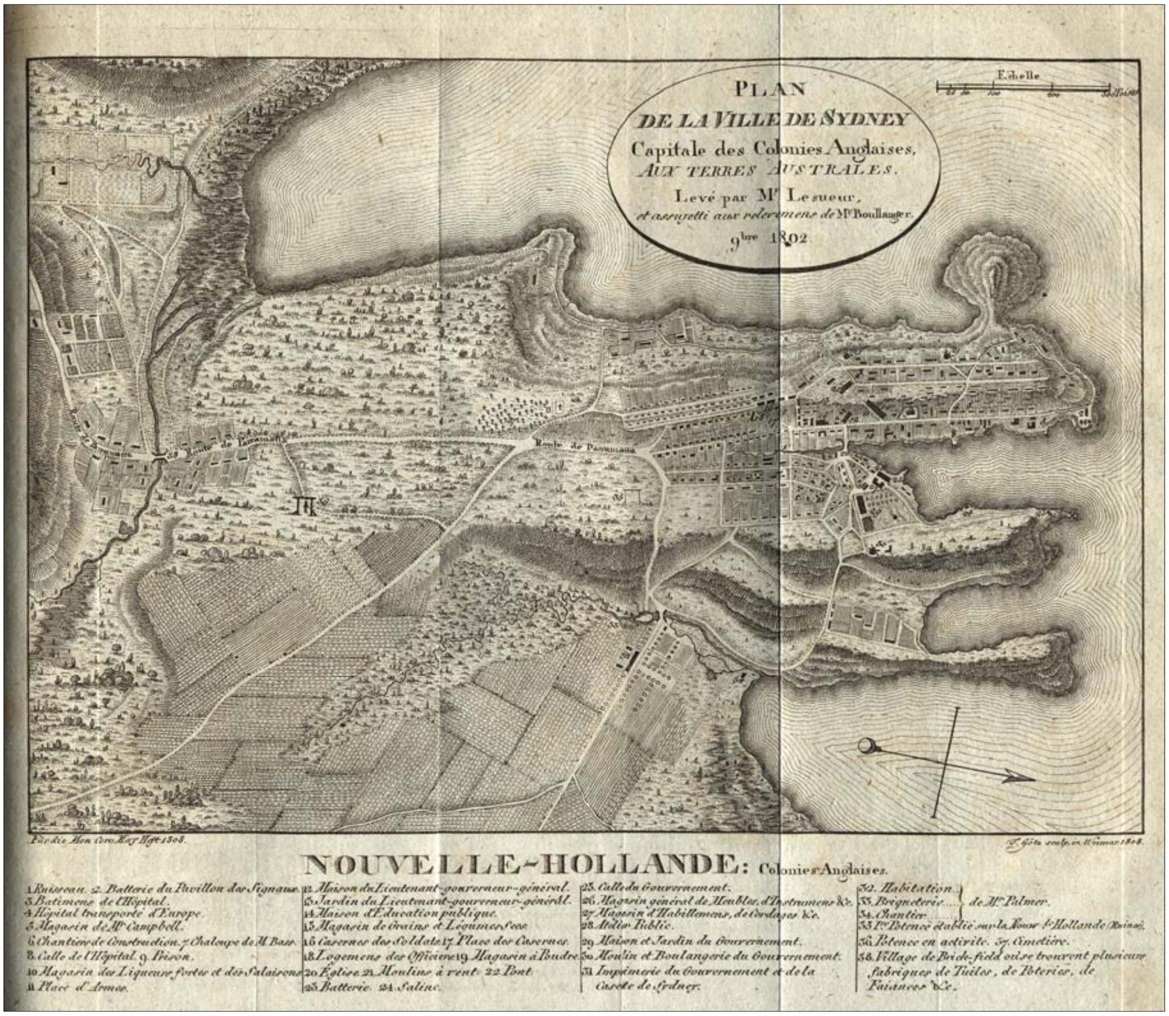

Figure 2: Map of Sydney (after *MC*, 17, 1809).

Zach was not able to produce a general index for his last journal, the *CA*, but it is safe to assume that his location at Genoa—one of the busiest ports in the Mediterranean Sea—meant that he was involved in maritime matters, and therefore was in contact with seamen and officers. In this way he would continue to have received news about the 'Fifth Continent'. One outstanding example of such a navigator was Captain W.H. Smyth (1788–1865), the future Admiral and astronomer, although he seemingly only discussed Mediterranean matters with Zach.

A look into the first and the last complete volumes of the *CA* confirms this expectation. Already in the first volume (*CA* 1, 303, 1818), Zach compares reports on deviations of the compass for the islands of Elba and Van Diemen's Land, and in one of his extensive notes he explains the substitution of the name 'Australia' for 'New Holland': "Les anglais, qui sont proprement les maitres dans cette partie du monde ... attachent le nom d'*Australia*, de préférence à la nouvelle Hollande, et îles adjacentes ..."

And the last complete volume of the *CA* is full of 'Australiana'. Monsieur Nell de Bréauté[6] communicates an excerpt of a voyage by two Englishmen (*CA* 14, 46ff., 1826), while in the same volume Zach reports on Admiral Krusenstern's analysis of his map of the Australian coast (which is still 'Nouvelle-Hollande'), and he also includes some historical information (*CA* 14, 201ff., 1826). In this same issue of the *CA*, on pages 305ff. and 418ff., a melange of topics on and around Australia is touched on, including Papua, the Coral Sea, Cook, Flinders, etc. So it is clear that the Fifth Continent and the surrounding region was under regular observation by the old astronomer-journalist and 'arm-chair discoverer', who, by the way, had stimulated real long-distance voyagers, not only morally but also by teaching them the astronomical methods of geographical position-finding. Amongst those were A. von Humboldt (by correspondence), Horner and Rüppell.

Kaspar Horner (1774–1834) was one of Zach's disciples, and was recommended by Zach to Krusenstern when he was seeking an astronomer. Although Krusenstern's expedition did not go to Australia, we owe to Horner a sketch of a celestial object which has been intensively researched by Australian astronomers: the Large Magellanic Cloud. This was published in the tenth volume of the *MC* in 1804, and is reproduced here as Figure 3.

Finally, Zach was in contact with C.L.Ch. Rümker (1788–1862), the German astronomer who, after a period in Australia, went back to Europe and finally obtained a position in Hamburg, thanks to support from an elderly ailing Zach in Paris.[7]

Figure 3: The Milky Way and the Large Magellanic Cloud, as drawn by Horner (after *MC*, 10, 288, 1804). The original caption "Capsche Wolken" is confusing. One might add that Horner reports photometric experiments with attenuation glasses on the Milky Way and the Magellanic Clouds (same volume, p. 220).

3 DISCUSSION AND CONCLUDING REMARKS

Above are a few examples of Australian material presented to an international audience in languages other than English. The journals in which the articles appeared were produced by a Hungarian-born astronomer in German, and later in French while he was domiciled at an Italian seaport. I cannot say if these journals contained real news for Australians, but at least they carried information that was of general interest to those in the Non-Anglo-Saxon world.

As far as Zach himself is concerned, he did not conceal his general opinion on Australia and its future:

> A new and big step in the knowledge of our globe in a continent has been made, which will very likely bear consequences for latter generations; these expectations are the more founded because of the rapid progress in the English settlement at Botany Bay … *Themis* and *Urania*, *Thalia* and *Melpomene* have erected their thrones in this place already. (Translated into English, the original German text appeared in the second volume of the *MC*, 617f., 1800).

4 NOTES:

1. *Allgemeine Geographische Ephemeriden*. Weimar, at Bertuch; two volumes per year; Volume 1 (1798) to Volume 4 (1799) were edited by F. von Zach.
2. *Monatliche Correspondenz zur Beförderung der Erd- und Himmelskunde*. Gotha at Becker; two volumes per year; Volume 1 (1800) to Volume 28 (1813) edited by F. von Zach.
3. *Correspondance Astronomique, Géographique, Hydrographique et Statistique du Baron de Zach*. Génes chez A. Ponthenier; two volumes per year except for Volumes 1, 4, and 15; Volume 1 (1818) to Volume 15 (1826).
4. This is the well-documented Dawes Observatory which was located on the present-day site of the southern pylon of the Sydney Harbour Bridge (see, e.g. Laurie, 1988; McAfee, 1981; Orchiston, 1989).
5. This is hardly surprising in that the Observatory was abandoned in 1791 when Dawes returned to England.
6. This gentleman was really a man, even though his Christian names were Éléonore Suzanne—a case of 'gender mainstreaming' avant la lettre?
7. On 15 February 1831 Zach wrote a letter to Olbers asking him to support Rumker (this is the English spelling used by Zach) in Hamburg (Brosche, 1990), but in fact the full story is much more complex (see Schramm, 1996: 92-110).

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6 REFERENCES

Brosche, P., 1990. Konjunktionen. Briefe Franz Xaver von Zachs. *Mitteilungen Gauß-Gesellschaft*, 27, 3-27.

Brosche, P. (ed.), 1998. *Astronomie der Goethezeit = Ostwalds Klassiker 280*. *Second Edition*. Thun, Harri Deutsch Verlag.

Brosche, P., 2001. *Der Astronom der Herzogin*. Frankfurt, Harri Deutsch Verlag.

Laurie, P.S., 1988. William Dawes, and Australia's first observatory. *Quarterly Journal of the Royal Astronomical Society*, 29, 469-482.

McAfee, R.J., 1981. *Dawes's Meteorological Journal*. Canberra, Australian Government Publishing Service (Bureau of Meteorology Historical Note No 2).

Orchiston, W., 1989. William Dawes: Australia's first meteorologist. *Quarterly Journal of the Royal Astronomical Society*, 30, 359-360.

Schramm, J., 1996. *Sterne über Hamburg*. Hamburg, Kultur- und Geschichtskontor.

Zach, F. von, 1798. Letter to Father David, dated 18 February 1798. In Seydl, O. 1938. Briefe Franz Xaver Freiherrn von Zachs ... *Publications de l' Observatoire National de Prague*, 11.

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CAVEAT LECTOR: COMMENTS ON DOUGLAS J. KEENAN, 'ASTRO-HISTORIOGRAPHIC CHRONOLOGIES OF EARLY CHINA ARE UNFOUNDED'

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Abstract: The author offers a point-by-point rebuttal of Douglas J. Keenan's criticism of the use of astronomical records in dating the earliest Chinese dynasties in the 2nd millennium BCE. Detailed references are provided to the abundant historiographical and archaeological evidence bearing on the early chronology which Keenan ignored in his critique.

Keywords: ancient Chinese chronology, planetary conjunctions, astronomical dating, ancient Chinese history

1 INTRODUCTION

Douglas J. Keenan (2002) criticizes recent efforts to establish the chronology of ancient China's earliest dynasties. He begins by asserting that China's five-year long (1995-2000), national research program, the *Xia-Shang-Zhou Chronology Project* (hereafter the *Project*), produced a chronology "… relying on a record of a solar eclipse." In this way he leaves the reader with the impression that the results of five years of intensive research by hundreds of Chinese scholars working collaboratively rests on the slender reed of one bit of astronomical evidence, the so-called 'double-dawn' solar eclipse of 899 BCE. Keenan then points to disagreement (comparatively minor) between the chronology for the Chinese Bronze Age presented in the preliminary report of the *Xia-Shang-Zhou Chronology Project* (2000) and that found in the previously-published *Cambridge History of Ancient China* (Loewe and Shaughnessy, 1999). In a second questionable claim, he asserts (Keenan, 2002: 61) that the *Cambridge History* dating also "… is based on records of conjunctions of the five visible planets." Once again, according to Keenan, it all comes down to a single astronomical reed, which he aims to snap, thereby bringing down the entire scholarly edifice.

Despite the 'historiographical' in his title, neither here, nor anywhere else in Keenan's article is there any mention of the historical evidence from a variety of disciplines that has been brought to bear on the problem of the early chronology. This includes archaeological evidence (stratigraphy, ^{14}C dating, ceramic and bronze vessel typologies, paleography, etc.) as well as extensive historiographical evidence (ancient histories, chronicles, inscriptional records, etc.), whose comprehensive analysis serves to narrow down the chronological range of benchmark dates to within just a few decades in some cases. In fact, the 'double-dawn' eclipse and the planetary conjunctions play supporting roles in this research enterprise, rather than being the principal pillars Keenan makes them out to be. This is not to say that the astronomical evidence is unimportant, but rather that the *Project's* overall chronology does not stand or fall based on that evidence. In a comprehensive discussion and evaluation of all aspects of the *Project* scientists' methodology (including evaluation of ^{14}C dating techniques) Yun Kuen Lee (2002: 30) characterized the collaborative effort this way:

> A noteworthy point is that the thorough investigation of these data requires expertise in several different fields and a number of skills that are almost impossible for any single individual to fully comprehend. It is only through the collaborative effort of specialists in different fields that such a high quality chronological scheme can be achieved.

The astronomical evidence is important in pinning down certain benchmark dates, especially the precise date of the Zhou Conquest of Shang in mid-11th century BCE, so it is Keenan's discussion of that evidence that I will mainly focus on. Liu Ciyuan, the astronomer responsible for coordinating and evaluating the astronomical research for the *Chronology Project*, has already responded to Keenan's critique of the analysis of the 'double-dawn' solar eclipse (Liu, 2002a; 2002b; also Liu, Liu & Ma, 2003), so I need not take up that issue here.

2 FIVE-PLANET CONJUNCTIONS

Apart from solar eclipses, Keenan's discussion focuses on the records of three planetary massings and a lunar eclipse, which have been discovered in ancient Chinese sources. Let me cite some of the key points in Keenan's critique by way of illustration. Following are quotations from Keenan's article, to which my own discussion and corrections are appended. Insertions in brackets are my own, provided where necessary to clarify the context.

2.1 First Quotation

> It is unclear how close planets would have to be in order for the ancient Chinese to have considered them to be in conjunction ... some researchers have suggested that the planets only had to be within an arc of 30º (i.e., spanning 30º of the sky). Conjunctions of all five planets that span ≤30º occur, on average, every 40 years. Thus, if the suggestion is correct, conjunctions would tend not to be useful in chronology. There are seven historical texts from after the Han 漢 period (ended AD 220) that record five-planet conjunctions: three of these refer to occasions when the planets spanned >30º. There is no evidence that ancient observers considered differently from Han ones. (Keenan, 2002: 63).

Keenan's source for these arguments is Huang Yilong (1990). However, Huang's assumption that pre-Han Chinese considered the operative definition of *wu xing ju yu yi she* 五星聚于一舍 "… the five planets gathered in one lodge …" to be within an arc of 30º is, in fact, nothing more than a guess. Huang assumes that if *yi she* can refer to an army's progress of 30 Chinese *li* on the ground, then by analogy it ought to denote 30º in the sky. But this presumption is based on no pre-Han astronomical source, or even post-Han source, for that matter. Indeed, it is directly contradicted by those sources. There exists another conventional term for the stages of an army's march—*ci* 次—which was borrowed to denote the so-called 'Jupiter stations' *sui ci* 岁次 of 30º, which correspond roughly to that planet's annual progress through the lunar mansions. But *ci* and *she* are not the same. Keenan, following Huang, ignores the textual evidence from the early 2nd century BCE *Mawangdui* silk ms. *wu xing zhan* 五星占 "Prognostications of the five planets" that directly contradicts Huang's assumption as to the operative definition of *yi she*. In that early Han Dynasty silk manuscript, *yi she* "one lodge" is used to express the range in longitude, for example, of Venus's retrograde motion of ~15º degrees (*ni xing yi she* 逆行一舍). This is consistent with the conventional practice in period texts of using *she* "lodging" as a synonym for *su/xiu* 宿 "lodge for the night; lunar lodge" to refer to the moon's daily progress of about 13º. For example, in Ho Peng-yoke's discussion of specialized terminology in the astronomical treatise of the *Jin shu* (ca. 635), he has the following to say in regard to planetary groupings:

> The term *chü* 聚 (assembly) refers to celestial bodies found within the same lunar mansion ... and according to Li Shun-feng at least three celestial bodies must be involved before the term is applicable ... When the rays of the celestial bodies concerned seem to extend towards each other, the condition is described by the term *hui* 會 (meet) ... (Ho, 1966: 38).

So here we have a very early excavated manuscript as well as the most authoritative source for early astronomy, the *Jin shu*, and both explicitly refute Huang's supposition as well as Keenan's assertion that "…there is no evidence ..." The *Mawangdui* ms. evidence is discussed in detail in Pankenier (1995: 123), which Keenan cites as his source for research on planetary conjunctions in Chinese history. In the same location in that article, I also point out that, "… when this narrower definition is applied, only four of twenty-four clusters from the first two millennia BC computed by Huang are found to qualify, for an average of one every 500 years." In fact, the spectacular massings of 1953 and 1059 BC were much denser, spanning about 4º and 7º, respectively.

Apart from the conjunction records under discussion it is not known how early the ancient Chinese began paying attention to the movements of the planets, though there is suggestive evidence from the Shang divination records that the planets were considered spirit minions of the high god. However, a solar observation platform was recently discovered at the late Neolithic site of Taosi in Shanxi, which was used to observe the rising sun at certain dates during the year, including the solstices (Liu et al., 2005). The site dates from about 2100 BCE. According to the preliminary analysis, sightlines pointing to the Sun's rising points would have permitted construction of a calendar based on horizon observations, a method hitherto unknown from early China. More relevant to the present discussion is that if regular sunrise observations were being conducted this early, whether for ritual or calendrical purposes, it is unlikely those astrologer-priests could have missed the spectacular pre-dawn planetary massing of 1953 BCE, which persisted for days. This discovery also places in a new light recent analysis pointing to the even greater antiquity of the Chinese lunar lodge system (Schaefer, 1999).

2.2 Second Quotation

> There was, however, no five-planet conjunction in 1576 BC, only a four-planet conjunction: at the time of the 'conjunction,' Venus was over 40º away from the other four planets ... attempts to promote the [*chronological*] proposals have essentially ignored this. (Keenan, 2002: 63).
>
> And in the caption to Figure 1: "… the claim of a conjunction is false." (Keenan, 2002: 63).

Keenan's criticism misrepresents the case. In referring to this planetary event, Pankenier (1995: 132) says:

> With the help of the *Bamboo Annals* relative date placing the Shang founding 517 years before the Zhou event [*i.e. 1059 BCE conjunction*], the curious behavior of the planets recorded at that juncture, *wu xing cuo xing* 五星错行, became comprehensible as a description of planetary behavior in the fall of 1576 as the planets reversed horizons and times of visibility from dusk to dawn and dawn to dusk.

Nowhere is the claim made that this event qualifies as a five-planet 'conjunction' by the same definition as those of 1953 and 1059 BCE, nor does the original text record it as a conjunction. In addition to ignoring the chronological context in which all three planetary events are embedded (see below), Keenan also disregards the lengthy discussion in another article he cites (Pankenier, 1981-1982: 19), which marshalls linguistic evidence to show that the term *cuo* 錯, in *wu xing cuo xing* "the five planets criss-crossed," in the Shang Dynasty language probably originally referred to the Sun's nightly disappearance in the west and reappearance in the east, so that its use in this planetary context is strikingly apt. The lapse is all the more inexplicable in that Keenan actually corresponded with me about this very event, and in a response to his August, 1998 e-mail I wrote:

> Your last question still confuses me a bit, but the attached charts should clarify ... No. 1 shows four of the planets (excl. Venus) clustered above the SE horizon at 5:31 local time in Xi'an on 20 Dec 1576 BCE. No. 2 shows the location of the planets at the same hour in relation to the sun's position. The planets were just emerging after being invisible, in some cases for many weeks, while located within the 15º circle surrounding the sun. They would have last been observed after sunset just above the NW horizon, rather than just before dawn in the SE.

I then referred him to Pankenier (1981-1982) for detailed discussion of this 1576 BCE phenomenon and the linguistic analysis of *wu xing cuo xing*.

2.3 Third Quotation

> The above text is like the main text cited as recording a 'conjunction in 1576 BC,' and the proposals adduce this likeness as demonstrating that the two texts record like events. (Keenan, 2002: 64).

Keenan is confused. The 'text' he refers to is from *Mozi* 墨子 (4th century BCE) and relates the myth of the founding of the Xia Dynasty. The likeness asserted in regard to this passage is between the accounts of the conjunctions of 1059 and 1953 BCE, *not* 1576 and

1953 (Pankenier, 1995: 132ff). The passage in *Mozi* is of particular interest precisely because it is the earliest to refer to both major conjunctions (the two densest massings in the past 5,000 years) using imagistic language to describe the supernatural means by which the transfer of Heaven's mandate was accomplished. The literature on cultural astronomy is replete with similar examples of myths and legends that encode astronomical information from around the world. Indeed, the first hexagram in the *Book of Changes* (*Yijing*: *qian gua*), encodes in its six line texts the seasonal behavior of the huge Dragon constellation (Virgo to Scorpius) in precisely the same kind of imagistic language.

In *Mozi*, on both occasions the auspices are said to have occurred in the form of a marvelous bird or bird-like creature, which conferred a jade scepter of authority on the dynastic founder. The term for the jade scepter *gui* or *gui zhang*, refers not to just any jade ornament, but to one that symbolized the delegation of authority in the archaic period. In the later of the two accounts in *Mozi* the scepter is said to have carried the actual text of the appointment. In that case, in 1059 BCE, the parallel account of the behavior of the Red Bird in the *Bamboo Annals* differs only in being conjoined with explicit mention of *wu xing ju* 五星聚 "… the five planets gathered." When the location of the actual conjunction at its densest is plotted, this is found to be just west of the reference star that traditionally marked the 'beak' of the huge Vermilion Bird constellation (Pankenier, 1995). It has also been shown (Pankenier, 1981-1982: 12) how in late May of 1059 BCE the Bird constellation with the planetary formation at its beak would have set in the northwest in the direction of the Zhou ancestral homeland at Zhouyuan, as seen from King Wen's location at the eastern end of the Wei River valley in Shaanxi. This corresponds to the account in the *Bamboo Annals* "… clasping a jade scepter (in its beak) it alighted on the Zhou altar to the soil …" which altar was located among the ancestral temples at Zhouyuan. The date of this event is deeply embedded in the *Bamboo Annals* relative chronology for the Dynastic founding period, since we know from other textual evidence that it must have occurred in the founder, King Wen's, 41st year. This historiographical evidence and the constraining chronological context, as well as the ^{14}C results that confirm this dating in the *Project's* preliminary report, Keenan does not mention.

2.4 Fourth Quotation

> Records of a five-planet conjunction that have been proposed to refer to the conjunction in 1059 BC ... claim that the conjunction occurred at the time of the succession of the long-lived Zhou 周 dynasty (the succession is usually dated to 1200-1000 BC). Five-planet conjunctions were believed to portend very beneficial times, so the veracity of the records should be considered inherently doubtful. That the conjunction is recorded as occurring in the lodge of *Fang* has been attributed to 'portentological revisionism.' Such revisionism, though, would seem to be at least as likely to affect the conjunction's recorded historical timing as its location in the sky. As to the supposed record of a lunar eclipse, it is from a text that is suspected of being fabricated. (Keenan, 2002: 66).

First, Keenan does not mention the series of five lunar eclipses in the Shang divination records, which have been dated to a brief span from 1201-1181 BCE during the reigns of the first two kings of the late-Shang. Eight more kings followed before the Dynasty's fall. The eclipses are discussed in the *Project* report (*Xia Shang Zhou duandai gongcheng*, 2000: 55). Prior to the *Xia-Shang-Zhou Chronology Project*, proposed dates for the Shang-Zhou Dynastic transition meriting serious consideration ranged between 1122-1027 BCE, although 1122 has been thought to be too early for many years. As a result of the refined ^{14}C and new stratigraphical analyses of important Western Zhou sites completed by the *Project*, that window was narrowed to between 1050-1020 BCE (*Xia Shang Zhou duandai gongcheng*, 2000: 43-44; Lee, 2002: 33). Since Keenan cites the preliminary report on the *Project's* results as a principle source, he should know this. Concerned with sowing doubt, Keenan proposes a 200-year window for the date of the Zhou Conquest for which there is no support in authoritative historical or archaeological research on the period, and which would leave no room for the last eight kings of the Shang Dynasty. Then, too, there is also the fact that even the supposedly unreliable *Bamboo Annals* chronology is only off by four years in dating the Zhou Conquest to 1050, and by only twelve years in dating the planetary conjunction to 1071 BCE. Analysis has shown this relatively minor misdating arises from understandable errors on the part of the scholars who reconstructed the damaged bamboo slips after their recovery from a tomb in the 3rd century CE (Pankenier, 1992a; 1992b).

Second: five-planet conjunctions as signs of heavenly approbation became *de rigueur* first in the early imperial period in the late 3rd century BCE after reunification of China's warring kingdoms. Of course, such a portent was highly beneficial only to the usurper, not to the incumbent Dynasty. This is why, beginning with the founding of the Han Dynasty in 206 BCE, the need to prove the new Dynasty's legitimacy made it inevitable subsequently that less impressive groupings of planets (like that of May 205 BCE) might occasionally be pressed into service, qualified sometimes as instances of the five planets appearing 'like linked pearls' rather than 'gathering'. Records of massings of the five planets in the imperial period are comparatively rare, but rarer still are the actual instances officially recognized as having Dynastic implications such as occurred in 750, 967, 1006 and 1524 CE. Even though by the Ming Dynasty astrology had long been domesticated, and despite the conjunction's being unobservable, the 1524 CE massing caused a stir at court precisely because it appeared rather ominously in mid-Dynasty (Pankenier, 1995: 512).

The *Bamboo Annals*, whose record of the 1059 BCE conjunction is embedded in a year-by-year chronology for the Conquest period, was buried in a tomb in the early 3rd century BCE and only rediscovered about 281 CE, six centuries later (Nivison, 1993). In the interim, during the mid-Han Dynasty, astrological and portentological speculation based on five-phases/*yin-yang* correlations came into vogue. In the process, the elemental force (phase) thought to govern the Han Dynasty was officially changed, with the result that the phase and official color governing the preceding Qin and Zhou Dynasties also had to be revised, in the case of the Zhou from Fire/Red to Wood/Green. As a consequence of the change of the official color of Zhou to Green, and by the logic of the correlative cosmology of the time, the Zhou Dynasty's correlated quadrant in the heavens of necessity had also to change from summer (Red Bird) to spring (Green Dragon). These revisions came about in mid-Han Dynasty and were institutionalized in the very influential scholar Liu Xin's (d. 23 CE) new chronological and calendrical scheme. The evidence documenting this transformation is overwhelming and indisputable (Wang, 2000: 137), as is the evidence that Zhou had previously been identified with Red and the Red (or Vermilion) Bird. The astronomical location of the 1059 BCE conjunction (near Alpha Hydrae) encoded by implication in the reference to the Red Bird (i.e., red ~ summer ~ summer solstice palace dominated by that constellation), being no longer recognized for what it was but taken simply as a reference to the auspicious phoenix, there was no obstacle to placing the Zhou Dynastic portent in lunar lodge *Fang* 'House' in Scorpius at the heart of the Green Dragon constellation. Indeed, not only was there no obstacle, astrological imperatives would have dictated that it must be so! We now know this introduced an obvious contradiction into the recorded location of the phenomenon, but the 3rd century CE court scholars who reconstructed the *Bamboo Annals* did not know, and so they 'helpfully' introduced this new location *yu fang* 于房 "in Room" into the reassembled text of the annals, possibly as an interlinear note which, as so often happened, subsequently became incorporated into the main text by a copyist.

Third: as regards the record of the lunar eclipse, the source text comes from chapter 23, *Xiaokai* 小開, one of the 'core' chapters of the *Yi Zhou shu*, which date from the late 4th or early 3rd century BCE (Shaughnessy, 1993). It is impossible to come away from a reading of the discussion of this work in the authoritative bibliography *Early Chinese Texts* with the impression that the scholarly consensus is that the *Xiaokai* chapter "… is suspected of being fabricated."

Fourth: the best approximation for Jupiter's period achieved in the mid-Han Dynasty was 11.92 years (present figure = 11.86 years), and for Jupiter-Saturn conjunctions was 20 years (actually 19.53 years). It is a simple matter to demonstrate using either of those figures that it would have been utterly impossible at a remove of some eight to ten centuries to retrospectively compute the location of an 11th century BCE conjunction of planets with sufficient accuracy to place it in the correct location in the sky. For example, for every supposed 11.92-year Jupiter cycle the computed result would be long by 0.6 years per cycle. Retrospectively computing over 1,000 years, or eighty-three cycles, would produce a cumulative error of some 5 years. On this point, consider the remarks by astronomer Zhang Peiyu (2002: 350):

> It is particularly important to point out that starting from the circulation of the *Santongli* 三统历 calendar, compiled in the first century AD, ancient scholars began to show great interest in the retro-calculation of the exact dates and cycle of planetary conjunctions. However, the computation of planetary trajectories is a complex exercise, and so those early computations contain many inaccuracies: calculating the exact locations of conjunctions of over 1,000 years in the past would have been unthinkable for those early astronomers. Because of this difficulty, I would argue that it would not have been possible for scholars of the Warring States or Han period (when the received classical texts containing reference to those astronomical events were first recorded) to have been able to accurately retro-calculate the exact time and location of the planetary conjunction that correlates to the Shang Conquest. Since this event can be shown by modern calculations to have actually occurred, and because it was recorded in the historical traditions, we can thus eliminate the possibility of a falsification of records of this conjunction by later hands.

The true location and absolute dates of the planetary phenomena are two sides of the same coin, neither could possibly have been generated during the Eastern Han Dynasty when portentological speculation and outright fabrication of omens (mainly contemporary and infrequently astral) were at their height. This is equally true of the *Bamboo Annals* relative chronology, which the planetary omens punctuate. Keenan does not attempt to explain how a motivated Han period or later forger accomplished the impossible by accurately computing the behavior of the planets a millennium (or two) earlier, not least the 1576 BCE planetary 'horizon-switching' phenomenon, or how said forger could have encoded the information in obscure language and then insinuated it into a well-known passage in the *Mozi* already several centuries old, not to mention inserting the records of the planetary phenomena into exactly the right place in the erroneous *Bamboo Annals* chronology while it was buried in a tomb.

2.5 Fifth and Sixth Quotations

> Additionally, there is supposedly a record of a lunar eclipse, near the time of the conjunction [*of 1059 BCE*], on (cyclic) day *bingzi* 丙子 in the first (lunar) month of the year, and there was a total *lunar* eclipse that matches this on 12 March 1065 BCE." (Keenan, 2002: 64).
>
> The eclipse record reads thus: on day *bingzi* in the first month, at the ceremony paying homage to the full moon ... the king announced, 'The many ... eclipse(s) is/are untimely; you shall begin planning succession.' (Keenan, 2002: 67; "Excursus").

In both locations Keenan is at pains to show how common lunar eclipses are, how uncertain we are about when the day and the year were thought to begin at the time, concluding with anachronistic assertions about how many calendars might have potentially been operative. The one crucial fact he does not mention is that he has selectively quoted only a portion of the eclipse record from *Yi Zhou shu*. He failed to include the relative date in the reign of King Wen of Zhou that prefaces the reference to the lunar eclipse: "It was the King's 35th year ..." (Li, 1981: 21; Pankenier, 1981-1982: 7; Pankenier, 1995: 129). Given year, month, and precise day, we can be a great deal more confident about the dating of this eclipse than if only the month and day had appeared. In Keenan's own words in another context (Keenan, 2002: 66): "That an actual eclipse would match the record's date just by chance is very improbable." Note that if 1065 BCE was King Wen's 35th year, then 1059 BCE, the year of the conjunction of the five planets, would have been King Wen's 41st year, precisely the result referred to above which derives from completely independent historical evidence. So the records of two astronomical phenomena and a variety of independent textual sources corroborate each other, incidentally also fixing the absolute dates of King Wen's reign.

3 CONCLUDING REMARKS

I could go on, but the above examples should suffice to make the point that Keenan's critique of "astro-historiographic chronologies" is not to be relied upon.

4 REFERENCES

Aihe Wang, 2000. *Cosmology and Political Culture in Early China*. Cambridge, Cambridge University Press.

Huang Yilong, 1990. A study of five-planet conjunctions in Chinese history. *Early China*, 15, 97-112.

Ho Peng-yoke, 1966. *The Astronomical Chapters of the Chin Shu, with Amendments, Full Translations and Annotations*. Paris, Mouton & Co.

Keenan, Douglas J., 2002. Astro-historiographic chronologies of early China are unfounded. *East Asian History*, 23, 61-68.

Li Changhao 黎昌顥 (ed.), 1981. *Zhongguo tianwenxue shi* 中國天文學史. Beijing, Kexue chubanshe.

Liu Ciyuan 劉次沅, 2002a. Tian zai dan rishi de genju—dui Keenan zhiyi de zhushi, 天再旦日食的根据 - 對 Keenan 质疑的注释. *Shaanxi tianwentai tai kan* 陕西天文台台刊 (*Publications of the Shaanxi Astronomical Observatory*), 25(1), 70-73.

Liu Ciyuan, 2002b. Astronomy in the Xia-Shang-Zhou Chronology Project. *Journal of Astronomical History and Heritage*, 5, 1-8.

Liu Ciyuan, Liu Xueshun, and Ma Liping, 2003. Examination of early Chinese records of solar eclipses. *Journal of Astronomical History and Heritage*, 6, 53-63.

Liu Ciyuan, Liu Xueshun, and Ma Liping, 2005. An exciting new discovery: a 4,000 year old Chinese observatory site. *Journal of Astronomical History and Heritage*, 8, 129-130.

Loewe, Michael, and Shaughnessy, Edward L. (ed.), 1999. *The Cambridge History of Ancient China*. Cambridge, Cambridge University Press.

Nivison, David S., 1993. Chu shu chi nien. In Loewe, M. (ed.). *Early Chinese Texts: A Bibliographical Guide*. Berkeley, Institute of East Asian Studies. Pp. 39-47.

Pankenier, David W., 1981-1982. Astronomical dates in Shang and Early Zhou. *Early China*, 7, 2-37.

Pankenier, David W., 1992a. The *Bamboo Annals* revisited: problems of methodology in using the chronicle as a source for the chronology of early Zhou, Part 1. *Bulletin of the School of Oriental and African Studies*, *University of London*, LV(2), 272-297.

Pankenier, David W., 1992b. The *Bamboo Annals* revisited: problems of methodology in using the chronicle as a source for the chronology of early Zhou, Part 2, The congruent mandate chronology in *Yi Zhou shu*. *Bulletin of the School of Oriental and African Studies*, *University of London*, LV(3), 498-510.

Pankenier, David W., 1995. The cosmo-political background of Heaven's Mandate. *Early China*, 20, 121-76.

Schaefer, Bradley E., 1999. Date and place of origin of the Asian lunar lodge system, *Oxford VI and SEAC 99: Astronomy and Cultural Diversity* (Proceedings of the International Conference 'Oxford VI and SEAC 99'), La Laguna, Canary Islands, Museo de la Ciencia y el Cosmos. Pp. 283-287.

Shaughnessy, Edward L., 1993. I Chou shu. In Loewe, M. (ed.). *Early Chinese Texts: A Bibliographical Guide*. Berkeley, Institute of East Asian Studies. Pp. 229-233.

Xia Shang Zhou duandai gongcheng: 1996-2000 nian jieduan chengguo baogao—jianben 夏商周斷代工程: 1996-2000 年階段成果報告—簡本. Beijing, Shijie tushu chuban gongsi. (2000).

Yun Kuen Lee, 2002. Building the chronology of early Chinese history. *Asian Perspectives*, 41(1), 15-42.

Zhang Peiyu, 2002. Determining Xia-Shang-Zhou chronology through astronomical records in historical texts. *Journal of East Asian Archaeology*, 4(1-4), 347-57.

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DEFENCE OF PLANETARY CONJUNCTIONS FOR EARLY CHINESE CHRONOLOGY IS UNMERITED

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Abstract: Pankenier (2007) has responded to the critique by Keenan (2002) of astro-historiographic chronologies of early China. The arguments of Pankenier are assessed herein; the conclusion is that the arguments have negligible scholarly merit. Some problems with radiocarbon dates are also considered.

Keywords: astro-historiography, carbon-14, eclipse, planets, Xia-Shang-Zhou

1 INTRODUCTION

Pankenier (2007) (hereinafter 'P07') presents a response to my critique of astro-historiographic dating of early China (Keenan, 2002). The present work considers the points raised in P07. It uses the same section names and numbers as P07.

P07 begins by quoting my critique: the Xia-Shang-Zhou Chronology Project produced a chronology "… relying on a record of a solar eclipse." (Xia, Shang, and Zhou are the first three Dynasties recorded in Chinese history.) P07 seems to suggest that the quote is inaccurate. In fact, as even a cursory reading of the Chronology Project report shows, the eclipse is relied upon. Additionally, the Director of the Chronology Project described the eclipse as being "… a key point." (Li, 2002: 328). Similarly, Liu (2002a: Section 1), cited by P07, describes the eclipse as "... one of the fulcrums …" of the Chronology Project. All scholars of early Chinese chronology should know this.

P07 then makes a similar suggestion with regard to the *Cambridge History of Ancient China* (Loewe and Shaughnessy, 1999). Following is a quote from this volume:

> The attempt to establish the first year of the Shang has benefited from new initiatives in archaeo-astronomy. Entries in Zhou texts have been taken as mythologized memories of the five-planet conjunction that occurred in Sagittarius in 1576. Other records of what appear to be similar conjunctions, understood as symbolizing Heaven's transferral of the Mandate, at the time of the founding of the Xia and the overthrow of the Shang, support this view. (Keightley, 1999: 248).

Elsewhere the *Cambridge History* … cites five-planet conjunctions as one piece of evidence for chronology, whilst adding that all dates "… should be regarded as provisional." (Shaughnessy, 1990: 23). Compare those two quotes with this statement from my critique: "[the *Cambridge History* …] … has provisionally adopted a chronology based on five-planet conjunctions." (Keenan, 2002: 67). The statement in my critique is fair.

P07's second paragraph begins as follows:

> Despite the "historiographical" in his title, neither here, nor anywhere else in Keenan's article is there any mention of the historical evidence from a variety of disciplines that has been brought to bear on the problem of the early chronology. This includes archaeological evidence (stratigraphy, ^{14}C dating, ceramic and bronze vessel typologies, paleography, etc.)….

The assumption here is that 'historiographical' is defined to include ^{14}C dating, vessel typologies, etc. That is obviously untrue, as Pankenier, who has been working in the field for decades, surely knows. For a discussion of the differences between historiography and archaeology in the context of ancient Chinese chronology, see Lee (2002b).

P07's second paragraph continues by stating that archaeological and other evidence "… serves to narrow down the chronological range of benchmark dates to within just a few decades in some cases." This is a statement of the obvious. The chronologies proposed by the Chronology Project and the *Cambridge History* …, though, claim to be accurate to within about a year, and those claims rely on astro-historiography. A similar point is made by Lee (2002a: 16, 19).

To summarize, my critique does not consider archaeological chronologies, let alone criticize them. P07 invents this claim, then attacks my critique for it. Having said that, I take the opportunity here to make some remarks about one aspect of archaeology: the use of ^{14}C in the Chronology Project—see Appendix 1.

P07's first section ends by citing Liu (2002a; 2002b) for a discussion of a solar eclipse in 899 BC, which was considered by my critique. Liu raises two valid issues.

First, my critique's discussion of eclipse brightness reduction refers to the reductions as percentages (Keenan, 2002: 62). The brightness reductions considered in the cited studies of Liu and co-workers, though, used absolute (rather than relative) reductions; thus the use of percentages in my critique could mislead. To rectify this, the six percentages on page 62 should be changed to decimal fractions, e.g. '25%' to '0.25'. This error in the critique is obviously tiny.

The other valid issue raised by Liu concerns the eclipse record of 776 BC from the Bamboo Annals. Liu points out that the record is only in the *jinben* version of the Annals, not the *guben* version, and so that makes the record *a priori* less reliable than the record of the double dawn (which is in both versions). (The *jinben* is also called the 'current text' and the *guben* is also called the 'old text'; for a discussion of the versions, see Nivison (1993).) This oversight in the critique is obviously not crucial.

Otherwise, the points raised in my critique remain valid. In particular, Liu's treatment of Earth's rotation rate, eclipse magnitude, etc.—which Liu (2002a: section 5) rightly describes as "… the essential issues …"—is incorrect; I will not discuss those issues further here, but instead defer to Stephenson (2008), who presents a detailed rejoinder.

Finally, I take this opportunity to fix another error in my critique. On page 66, the critique refers to a record of the eclipse of 776 BC as the sole *Bamboo Annals* astronomical record from after 841 BC. The record is actually just the earliest eclipse record in the *Annals*. This error is of negligible consequence in the context.

2 FIVE-PLANET CONJUNCTIONS

2.1 First Quotation

P07 begins by quoting from my critique: “It is unclear how close planets would have to be in order for the ancient Chinese to have considered them to be in conjunction … some researchers have suggested that the planets only had to be within an arc of 30º (i.e. spanning 30º of the sky).” (Keenan, 2002: 67). P07 next correctly notes “Keenan’s source for these arguments is Huang Yilong (Huang, 1990) …”, and it then argues against the analysis of Huang.

I showed the argument of P07 to Huang, who replied as follows (private communication, May 2007):

> 1. No ancient texts so far gave an explicit definition for 五星聚合. My estimate is based on actual usages in ancient observational records.
>
> 2. If she 舍 is a synonym for su/xiu 宿, we will find some lunar mansion extends more than 30 degrees.

I am not competent at analysing ancient texts, but that would seem to largely rebut P07’s argument. At a minimum, it can be said that there is scholarly dispute, thereby justifying my critique’s statement that “... some researchers have suggested …”

It is also worth noting that P07’s main argument against Huang’s analysis is based on a *Mawangdui* silk manuscript from the second century BC (Han period); so even if the manuscript had given an explicit definition, it would hardly be definitive for how conjunctions were perceived by pre-Han peoples many centuries earlier. Considering both that and Huang’s reply, the central point made here by my critique has obviously not been rebutted: it is unclear how close the planets would have to be in order for the ancient Chinese to have considered them to be in conjunction.

P07’s final remark in this section is as follows: “… it is unlikely those astrologer-priests could have missed the spectacular pre-dawn planetary massing of 1953 BCE….” My critique, however, never claims otherwise. That is, P07 is again accusing my critique of saying something that it does not say.

2.2 Second Quotation

My critique observes that “There was … no five-planet conjunction in 1576 BC, only a four-planet conjunction.” (p.63). Regarding this, P07 acknowledges that there was no five-planet conjunction at that time. P07 further acknowledges that there is no record of a planetary conjunction at that time (saying “… nor does the original text record it as a conjunction.”). P07 then tries to argue that none of this matters. This certainly does matter, though.

Pankenier’s central proposal has been that planetary conjunctions induce dynastic transitions. The lack of a five-planet conjunction for the transition from the Xia to the Shang is fatal for such a proposal, because four-planet conjunctions (i) occur quite frequently (defined via any reasonable span of arc; for some examples, see Zhang (1990: 147)) and (ii) have the opposite astrological connotations of five-planet conjunctions (Huang, 1990: 110).

P07 further claims that my critique “... misrepresents the case …” concerning the conjunction of 1576 BC. I disagree; consider how others have presented the case. The quotation in the Introduction from D.N. Keightley explicitly refers to a five-planet conjunction, and Pankenier’s works are the sole source for that. Keightley is a leading scholar in this area, and the quote is from the article on the Shang in the *Cambridge History of Ancient China* (the standard English-language reference for ancient China), which was edited by two other leading scholars, M. Loewe and E.L. Shaughnessy. Thus Keightley, and presumably also Loewe and/or Shaughnessy, believed that Pankenier’s proposals were based on five-planet conjunctions. (Additionally, at the Second Worldwide Conference of the Society for East Asian Archaeology (in 2000), I met other respected scholars who had the same belief.) As this evidences, Pankenier’s publications have led people into believing that the chronological proposal was based on a five-planet conjunction in 1576 BC.

Given some of the statements in Pankenier’s publications, that might be expected. For instance, the following is from Pankenier (1981-1982: 19):

> … the original account of the conjunction of 1576 B.C. was an extraordinarily apt characterization of this planetary event: “The five planets regressed …”

Another example, from Pankenier (1983-1985), is detailed in the next section. And Pankenier (1995) repeatedly discusses the conjunction of 1576 BC together with the conjunctions of 1953 BC and 1059 BC in a way that would likely induce (and evidently has induced) many people to believe that the three were alike. In other words, although Pankenier might never have explicitly stated that there was a five-planet conjunction in 1576 BC, each of Pankenier’s three main papers on the topic has been written such that it could be readily interpreted as describing a five-planet conjunction then.

One other point deserves mention. Pankenier (2007: Section 2) correctly states that I contacted him in August 1998, because of technical issues with his proposals, and that he afterwards sent me a reply. P07 faults my critique for not discussing that. For my critique to discuss that, however, would mean criticizing an unpublished private communication, which seemed to me to be unfair.

The private communication that Pankenier and I had in August 1998 was about a figure in his 1995 paper (Pankenier, 1995: Figure 2). The figure displays the sky on 27 December 1576 BC, when there was a four-planet conjunction. The figure has some obvious problems, e.g. it shows the Sun high in the sky, but lists the time of day as 23:00. So I asked Pankenier, “This isn’t the sky that you would see in China at the time?” Pankenier replied, “No”. When I then tried to ascertain why he would publish such a figure, Pankenier responded, “I’m not quite sure why.”

2.3 Third Quotation

P07 begins as follows (first quoting my critique):

> The above text is like the main text cited as recording a 'conjunction in 1576 BC,' and the proposals adduce this likeness as demonstrating that the two texts record like events. (Keenan, 2002: 64).
>
> Keenan is confused. The 'text' he refers to is from *Mozi* 墨子 (4th century BCE) and relates the myth of the founding of the Xia dynasty. The likeness asserted in regard to this passage is between the accounts of the conjunctions of 1059 and 1953 BCE, *not* 1576 and 1953 ….

The Mozi passage of relevance contains an account of the founding of three Dynasties: Xia, Shang, and Zhou. The passage is translated below (Watson, 1967: Mo Tzu 19; Keenan, 2002: 64). It begins as a question put to Mozi about engaging in warfare:

> Now those rulers who delight in offensive warfare attempt to put a pleasing façade upon their doctrines and criticize Mozi, saying "Do you claim that offensive warfare is an unrighteous and unprofitable thing?—in ancient times Yu launched an expedition against the ruler of the Miao, Tang attacked Jie, and King Wu attached Chou, and yet all three are regarded as sage kings; why is that?"
>
> Mozi replied: You have failed to examine the terminology that I employ and do not understand the reasoning behind it. What those men did was not "attack" but "punish".
>
> In ancient times the three Miao tribes were in great disorder and Heaven decreed their destruction. The sun came out at night and for three days it rained blood. A dragon appeared in the ancestral temple and dogs howled in the market place. Ice formed in summertime, the earth split open until springs gushed forth, the [cereal crops] grew differently, and the people were filled with a great terror. Kao Yang gave the command in the Dark Palace, and Yu [the Xia founder] … grasped the jade staff of authority and set out to subdue the ruler of the Miao. Amidst the din of thunder and lightning, a spirit with the face of a man and the body of a bird came bearing a jade baton to wait upon Yu. The general of the Miao was felled by an arrow and the Miao army thrown into great confusion … This is how Yu launched an expedition against the ruler of the Miao.
>
> In the case of King Jie of Xia [the last king of the Xia], Heaven likewise sent down its direst command. Sun and moon failed to appear at the proper time, hot weather and cold mingled in confusion and [cereal crops] were seared and died. Spirits wailed throughout the land and cranes shrieked for more than ten nights. Heaven gave its command to Tang in the Biao Palace, ordering him to take over the solemn mandate from the Xia, for the Xia had fallen into grave disorder … Only then did the Tang dare to lead forth his troops in obedience to the command … After a while a spirit appeared and reported to Tang: "The virtue of the Xia is in great disorder; go and attack it, and I will surely cause you to win victory over it, for I have already received the command from Heaven." Then Heaven ordered Zhuyong to send down fire on the northwest corner of the city of Xia, and Tang, leading the army of Jie, conquered it. … This is how Tang punished Jie.
>
> In the case of King Chou of Shang [the last king of the Shang], Heaven would not sanction his power. His sacrifices were untimely; for ten days and ten nights it rained earth at Bo, and the nine cauldrons moved about. A woman turned into a man, flesh rained down from Heaven, and brambles grew on the state roads. A red bird holding in its beak a baton of jade alighted on the altar of the Zhou state in the city of Ch'i and proclaimed, "Heaven orders King Wen of Zhou to attach Shang …" Tai Dian journeyed to pay his respects to the Zhou ruler, the river cast up its chart, and the land brought forth the "riding-yellow" beast. King Wu ascended the throne and in a dream he saw three spirits who said to him this: "We have already drowned Chou of Shang in the power of wine; go and attack him, and we will surely cause you to win victory over him!" So King Wu went and attacked him, and replaced the state of Shang with that of Zhou, and Heaven presented King Wu with the yellow bird pennant … This was how [King Wu] carried on the labours of Tang.
>
> Thus, if we examine the cases of these three sage kings, we see that what they did was not to "attack" but to "punish".

(My critique quoted the third paragraph, on the founding of the Xia.) It is apparent that the three paragraphs on the foundings of the Xia, Shang, and Zhou have similarity and are intended to be considered together. Moreover, there seems to be roughly as much similarity between the paragraph on the founding of the Shang (fourth paragraph) and each of the paragraphs on the foundings of the other two Dynasties (third and fifth paragraphs) as there is between the paragraphs on the foundings of the other two Dynasties.

The conjunction of 1576 BC is the event that Pankenier's proposals associate with the passing of the Mandate of Heaven from the Xia to the Shang (i.e. the transition from one dynasty to the next). Compare the quote from P07 at the start of this section with the following from Pankenier (1983-1985: 176-178):

> … the earliest mythicized versions of the Mandate conjunctions are found in Mozi … I have suggested that this Mozi account … derives from oral traditions … and that couched in the mythical language in which they are written there is much valuable information bearing on archaic cosmological, astronomical, and religious conceptions of the Chinese. The most obvious example of this is of course the planetary conjunction of 1059 B.C. …
>
> Mozi's account in the same context of the founding of the Shang some five hundred years earlier follows a similar pattern ... Here, too, I would suggest that "Biao Palace" does not refer to a terrestrial edifice … but to one of the constellations … The untimely appearance of the sun and moon … parallels the motive given for the overthrow of Shang …
>
> Mozi singles out three instances of the Mandate's conferral; namely, the founding of the "Three Dynasties"—Xia, Shang, and Zhou. The parallels between the latter two events have already been discussed, so let us now turn to the earliest historical precedent … Here Mozi again reports seasonal dislocations …

Thus, Pankenier compares the three Mozi accounts of what he proposes are descriptions of conjunctions and says that they are similar and have parallels. Moreover, the article from which these quotes are taken is entitled "Mozi and the dates of the Xia, Shang, and Zhou". This thus falsifies what P07 claims (as quoted at the start of this section).

Finally, Huang (1990), Keenan (2002), and others have argued that the interpretation of the three Mozi paragraphs as records of planetary conjunctions is impressionistic and not reliable enough to form the basis of a chronology. Really, I think that is clear.

2.4 Fourth Quotation

P07 begins by claiming "… Keenan does not mention the series of five lunar eclipses in the Shang divination records …" Compare that with what my critique said (Keenan, 2002: 67): "The Late Shang chronology was claimed to be verified by records of five lunar eclipses; those records, though ..." The claim of P07 is thus an invention.

P07 next faults my critique for (parenthetically) stating that the succession of the Zhou Dynasty "… is usually dated to 1200-1000 BC …", arguing that the dates given in the statement are inaccurate. In fact, I wrote the dates as round numbers, as should be obvious. Exactness was unneeded, and possibly distracting, because the purpose was merely to give some idea of the date for those readers without background knowledge of ancient China. P07 claims that the exact range is 1122-1027 BC, but that claim ignores some proposals. Other authors given different ranges; e.g. in 1997, an extensive review by the Sinological Institute of Beijing Normal University gave 1130-1018 BC (cited by Li, 2003: 482). It is difficult for me to state what the true exact range is, but I agree that it would have been better if my critique had stated the range as 1150-1000 BC. How important is this, given the context?

P07 next claims "… there is also the fact that even the supposedly unreliable *Bamboo Annals* chronology is only off by four years in dating the Zhou Conquest to 1050, and by only twelve years in dating the planetary conjunction to 1071 BCE." This supposed 'fact' presupposes that the astro-historiographic date proposed by Pankenier is valid. The claim is thus circular. It also illustrates how Pankenier 'amends' the years in ancient texts in order to obtain his chronology; this point is discussed further in the next section.

P07 then discusses the texts that purportedly describe the five-planet conjunction (in 1059 BC) proposed to be linked with the transition to the Zhou Dynasty. Those texts described the conjunction as occurring in the astronomical lodge of *Fang*, whereas the conjunction actually occurred 120° away from *Fang* (Huang, 1990: 105-106; Keenan, 2002: 64). Pankenier (1995: n.17) ascribes that discrepancy to 'portentological revisionism'. My critique points out, though, that such revisionism "… would seem to be at least as likely to affect the conjunction's recorded historical timing as its location in the sky" (page 66). In response to that, P07 largely just repeats the arguments of Pankenier (1995). As my critique discusses, those arguments are plausible speculation, but they "… are not reliable enough to form the basis of a chronology."

P07 further makes the following claim:

> … as regards the record of the lunar eclipse, the source text comes from chapter 23, *Xiaokai* 小開, one of the 'core' chapters of the *Yi Zhou shu*, which date from the late 4th or early 3rd century BCE (Shaughnessy, 1993). It is impossible to come away from a reading of the discussion of this work in the authoritative bibliography *Early Chinese Texts* with the impression that the scholarly consensus is that the *Xiaokai* chapter "… is suspected of being fabricated."

The statement being quoted from my critique ("… is suspected of being fabricated.") has two references (Keenan, 2002: n.44). One of those references is by E.L. Shaughnessy (whom P07 relies upon); here is what Shaughnessy (1991: 222-223) says: "… the record itself is somewhat suspect since the 'Xiao kai' chapter belongs to what I have elsewhere termed the 'Jizhong' stratum of the Yi Zoushu, which I have suggested may have been composed in the fourth century A.D." The other reference is by N. Barnard (one of the most esteemed scholars of ancient Chinese texts); here is what Barnard (1993: n.17) says: "Chang Hsin-ch'eng's survey of the accounts and critical analyses of the *Yi-Zhou-shu* results in the impression that it is, for the most part, a forgery, if not entirely so. Liang Ch'ich'ao, for instance, is of the opinion that 'no less than eleven of the chapters are faked, while of the remainder, many have been tampered with or falsified; but it is not easy to determine which ones are authentic'". P07 thus ignores the references in my critique and misrepresents Shaughnessy.

Additionally, even if the text were reliable, it is far from clear that the text records an eclipse on the specified day. This point was made by my critique (p. 67) and is ignored by P07.

The last claim in this section of P07 is that the *Bamboo Annals* record of a planetary conjunction in 1059 BC must be reliable because "… it would have been utterly impossible … to retrospectively compute the location of an 11th century BCE conjunction of planets with sufficient accuracy to place it in the correct location in the sky." As noted above, the location given in the *Bamboo Annals* is actually in error by 120°, which obviously greatly weakens the claim to be utterly impossible. Pankenier's proposals argue that the error was due to portentological revisionism; they then give an interpretation of the bird bearing a jade baton (mentioned in the quoted ancient texts) to relocate the conjunction, but the interpretation is plainly impressionistic and less than certain.

2.5 Fifth and Sixth Quotations

P07 faults my critique for its discussion of lunar eclipses, saying the "… one crucial fact …" that the critique does not mention is "… the relative date in the reign of King Wen of Zhou that prefaces the reference to the lunar eclipse [in the eclipse record]." The eclipse was indeed recorded as occurring during the 35th year of the king's reign. P07 then argues: "Given year, month, and precise day, we can be a great deal more confident about the dating of this eclipse than if only the month and day had appeared."

We are not, however, given a certain month and day; rather, there is some uncertainty in both, especially the month (Keenan, 2002: 67). Furthermore, we are not given an absolute (i.e. calendar) year, but rather a relative year; the argument of P07 appears to mix absolute and relative years.

P07 further claims "… if 1065 BCE was King Wen's 35th year, then 1059 BCE, the year of the conjunction of the five planets, would have been King Wen's 41st year, precisely the result referred to above which derives from completely independent historical records." The "… result referred to above …" is that the five-planet conjunction of 1059 BC occurred during Wen's 41st year. The 'result' is unreferenced, but Pankenier (1995: n.10) claims the same result, citing Pankenier (1992: part 2). Pankenier (1992) generally argues for dates by making numerous revisions to the ancient texts. Here are some selections to illustrate that:

> … there is a sixteen-year error in the *Bamboo Annals* for the beginning of Di Xin's reign …
>
> … The events of [King Wu's] actual five years of rule … have been redistributed among the seventeen years allotted to him in the *Bamboo Annals* …
>
> … this contradiction between *Yi Zhou shu* and reconstructed *Bamboo Annals* is the result of the same confusion …
>
> … these events could not really have taken place four years after …
>
> … reconciling the *Yi Zhou shu* record of Kind Wen's death in the 9th year with the contradictory account in [*Records of the Grand Historian*] which has Wen dying in the 7th year.
>
> … allowing only for the commonest of copyist's errors (writing '23' for '13', and '3' for '1') …
>
> … [*Records of the Grand Historian*] states that King Wen died six years after attacking the Quan Yi barbarians … it appears, therefore, that [the *Records of the Grand Historian* was incorrect] about the timing of that campaign …
>
> … the *Bamboo Annals* figures can be shown to be unreliable; for example, Di Xin is assigned fifty-two years, though we now know that he actually only reigned for forty years; Di Yi is assigned only nine years even though … he ruled more than fifteen.
>
> … the *Bamboo Annals* … misplaced a reference to a Phoenix augury alluding to the planetary conjunction by entering it under the year of King Wen's ascension in Zhou many years before.

As the selections indicate, Pankenier ignores some texts and revises others. (The selections give only some examples; there are several more.) This is a game that allows matching the texts to almost any feasible chronology.

Additionally, the claim about King Wen's 41st year assumes that the transition from the Shang to the Zhou was synchronous with the five-planet conjunction of 1059 BC. That introduces some circularity into the argument. Furthermore, it requires revision of the *Bamboo Annals*. Indeed, Pankenier (1992) relies on a revision of the *Bamboo Annals* that was shown to be unsound (Barnard, 1993); this unsoundness was noted by my critique (page 65), but is ignored by P07.

The last fault claimed by P07 is the following:

> In Keenan's own words in another context (Keenan, 2002: 66): "That an actual eclipse would match the record's date just by chance is very improbable."

The other context (page 66) concerns *(near-)total solar* eclipses. The discussion here concerns *partial lunar* eclipses. The error in the argument of P07 is plain.

3 CONCLUDING REMARKS

To summarise, P07 has no significant points that are valid. Moreover, Pankenier surely knows that many of the points raised by P07 are untrue. Additionally, it is noteworthy that the Chronology Project considered using planetary conjunctions for its work, but ultimately decided to reject this approach, because the records were considered too unreliable (see, for example, Liu, 2002b: 2 and Liu, 2002a: section 2).

In conclusion, the present work, together with that of Stephenson (2007), affirms my 2002 critique: astro-historiographic chronologies of early China are unfounded.

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5 REFERENCES

Xia-Shang-Zhou duandai gongcheng 夏商周斷代工程 [Xia-Shang-Zhou Chronology Project] Beijing, Shijie Tushu Chuban Gongsi (2000).

Barnard, N., 1993. Astronomical data from ancient Chinese records: the requirements of historical research methodology. *East Asian History*, 6, 47-74.

Guo Zhiyu, Lin Kexin, Lu Xiangyang, Ma Hongji, Li Kun, Yuan Jinglin, Yuan Sixun, Wu Xiaohong, and Liu Xu, 2001. AMS radiocarbon dating of Tianma-Qucun site Shanxi, China. *Radiocarbon*, 43, 1109-1114.

Huang Yi-long, 1990. A study of five-planet conjunctions in Chinese history. *Early China*, 15, 97-112.

Keenan, D.J., 2002, Astro-historiographic chronologies of early China are unfounded. *East Asian History*, 23: 61-68.

Keenan, D.J., 2004. Radiocarbon dates from Iron Age Gordion are confounded. *Ancient West & East*, 3, 100-103.

Keightley, D.N., 1999. The Shang. In Loewe, M., and Shaughnessy E.L. (eds.). *Cambridge History of Ancient China*. Cambridge, Cambridge University Press. Pp. 232-291.

Kromer, B., Manning, S.W., Kuniholm, P.I., Newton, M.W., Spurk, M., and Levin. I., 2001. Regional $^{14}CO_2$ offsets in the troposphere. *Science*, 294, 2529-2532.

Lee, Y.K., 2002a. Building the chronology of early Chinese history. *Asian Perspectives*, 41, 15-42.

Lee, Y.K., 2002b. Differential resolution in history and archaeology. *Journal of East Asian Archaeology*, 4, 375-386.

Li Xueqin, 2002. The Xia-Shang-Zhou Chronology Project. *Journal of East Asian Archaeology*, 4, 321-333.

Li Yong, 2003. A query on the year of King Wu's conquest over King Zhou determined by the Xia-Shang-Zhou Chronology Project. *Science in China G*, 46, 481- 491.

Liu Ciyuan, 2002a. Tian zai dan rishi de genju—dui Keenan zhiyi de zhushi 天再且日食的根据 - 對 Keenan 质疑的注释. *Shaanxi tianwentai tai kan* 陕西天文台台刊 [*Publications of the Shaanxi Astronomical Observatory*], 25, 70-73. [English translation available at http://www.informath.org/PSAO02i.pdf.]

Liu Ciyuan, 2002b. Astronomy in the Xia-Shang-Zhou Chronology Project. *Journal of Astronomical History and Heritage*, 5, 1-8.

Liu Kexin, Guo Zhiyu & 14 others, 2000. Improvements of PKUAMS for precision ^{14}C analysis of the project of Xia–Shang–Zhou Chronology. *Nuclear Instruments and Methods in Physics Research B*, 172, 70-74.

Loewe, M., and Shaughnessy E.L. (eds.), 1999. *The Cambridge History of Ancient China*. Cambridge, Cambridge University Press.

Manning, S.W., Kromer, B., Kuniholm, P.I. and Newton, M.W., 2001. Confirmation of near-absolute dating of east Mediterranean Bronze-Iron Dendrochronology. *Antiquity*, 77, Project Gallery — http://antiquity.ac.uk/projgall/manning/manning.html. [Data at http://www.arts.cornell.edu/dendro/antiquity.html.]

Nivison, D.S., 1993. Chu shu chi nie. In Loewe M. (ed.). *Early Chinese Texts: A Bibliographical Guide.* Berkeley, Society for the Study of Early China. Pp. 39-47.

Pankenier, D.W., 1981-1982. Astronomical dates in Shang and Western Zhou. *Early China*, 7, 2-32.

Pankenier, D.W., 1983-1985. Mozi and the dates of Xia, Shang, and Zhou. *Early China*, 9-10, 175-183.

Pankenier, D.W., 1992. The *Bamboo Annals* revisited: problems of method in using the chronicle as a source for the chronology of early Zhou. *Bulletin of the School of Oriental and African Studies*, 55, 272-297 and 498-510.

Pankenier, D.W., 1995. The cosmo-political background of Heaven's mandate. *Early China*, 20, 121-176.

Pankenier, D.W., 2007. *Caveat Lector*: Comments on Douglas J. Keenan, 'Astro-historiographic chronologies of early China are unfounded'. *Journal of Astronomical History and Heritage*, 10, 137-141.

Shaughnessy, E.L., 1991. *Sources of Western Zhou History* Berkeley, University of California Press.

Shaughnessy, E.L., 1999. Calendar and chronology. In Loewe, M., and Shaughnessy, E.L. (eds.). *Cambridge History of Ancient China*. Cambridge, Cambridge University Press. Pp. 19-29.

Stephenson, F.R., 2008. How reliable are archaic records of large solar eclipses? *Journal for the History of Astronomy*, 39, in press.

Watson, B. (translator), 1967. *Basic Writings of Mo tzu, Hsün Tzu, and Han Fei Tzu*. New York, Columbia University Press.

Wiener, M.H., 2007. Cold fusion: the uneasy alliance of history and science. In Manning, S.W. (ed.). *Festschrift for Peter Ian Kuniholm*. In press.

Zhang Peiyu, 1990. [Untitled]. *Early China*, 15, 133-150.

6 APPENDIX 1: RADIOCARBON DATING

For a short review of radiocarbon dating, see http://www.informath.org/Basic14C.pdf. This appendix presents a few remarks on the radiocarbon dating done in the Xia-Shang-Zhou Chronology Project. These remarks are brief, and nowhere near a comprehensive review of the project's radiocarbon dating. They do, though, demonstrate that there are some problems.

There are two aspects to radiocarbon dating, for any sample. The first is to obtain an accurate measurement of the ^{14}C in the sample (this includes sample preparation). The second is to 'calibrate' that measurement to a calendar date. (For a discussion of measurement accuracy versus dating accuracy in radiocarbon, see Wiener (2007).) Regarding the measurements made for the Chronology Project, the project's radiocarbon laboratory invested much effort in trying to obtain accurate measurements (Liu et al., 2000); I make no further comment on that aspect. Regarding the calibrations of the measurements, there are potential problems.

One problem is that appropriate confidence intervals for the calibrated dates have not always been cited. The standard in radiocarbon studies, and indeed most sciences, is to give a 95% confidence interval for a quantity (in this case, a calibrated date). Not all publications of the Chronology Project have followed that standard. For example, Guo et al. (2001: Tables 1, 2) give only 68% confidence intervals for calibrated dates. Such intervals are unrealistically narrow.

Another example of the problem is in a summary of the Chronology Project that was written by the project's Director (Li, 2002). The summary claimed that a certain ^{14}C measurement from an important tomb "… gave a radiocarbon age of 2640±50 BP, or 814-796 calibrated BC". The claim is incorrect: a date of 814-796 BC is obtained by calibrating 2640 BP and ignoring the measurement imprecision of ±50; when the radiocarbon age is calibrated in the standard way, the 95% confidence interval for the date is much wider: about 917-756 BC. (This interval can be reduced via statistical sequence analysis—see below.)

Another potential problem is that during times when solar output was fluctuating rapidly, ^{14}C measurements taken at a latitude near 40 N cannot be accurately calibrated by the standard international calibration curves (Kromer et al., 2001). The problem is believed to become more serious for locations closer to the equator. Ancient China lay at roughly 35 N; so the problem would be expected to be at least as serious there as at 40 N, although the inaccuracies are not constant at a given latitude, but vary somewhat with location (details are not known).

The problem was only discovered after the Chronology Project was completed. The discoverers claimed that the inaccuracy resulting from using the standard calibration curves could be a few decades; later work showed that for some samples (especially short-lived samples whose carbon came primarily from winter-time growth), the inaccuracy could be as much as a century (Keenan, 2004)—at least during part of 850-750 BC, when solar output is known to have been fluctuating very rapidly.

The Chronology Project relied heavily on bone samples for its ^{14}C dating. It is unclear to what extent those samples would be affected by latitudinal effects. The main source of carbon in the bones is believed to be millet in the diet (Guo et al., 2001: 1112); so, much would depend on the planting and harvesting schedule (see Keenan (2004) for some discussion of this issue). A comparison of bone samples of known date, from 841-781 BC (Guo et al., 2001: Table 2), very strongly suggests that the problem, if it exists, is not large.

There might have been other times in the past when solar fluctuations led to inaccuracy, albeit usually not large. This is currently an area of research. Some recent work suggests a possible inaccuracy of half a century around 1600 BC (Manning et al., 2003: data; Wiener, 2007: Figure 1); this is based on only a single sample (from tree rings, at 40 N) though, and it remains to be replicated.

The existence of the latitudinal problem indicates that some radiocarbon dates from the Chronology Project should be reassessed. A related issue is that the project used sequences of ^{14}C ages, which were then statistically combined to give highly-precise calibrations (i.e. dates). That poses a difficulty for the ^{14}C-dating of samples even during times when solar output was nearly constant. As an example, the cemetery of the Marquises of Jin was the source an important sequence of samples for ^{14}C dating (Guo et al., 2001). Many of those samples' dates are known to be from 850-750 BC, and so they can probably not be calibrated as accurately as would otherwise be expected. The dates for samples from earlier in the sequence might not be directly affected by solar fluctuations; yet the statistical analysis of those earlier samples is affected by the ^{14}C ages of samples from later in the sequence. In this way, even samples from the time when China's chronology is known (after 841 BC) can affect earlier ^{14}C-derived dates.

To conclude, the dates derived from radiocarbon are unlikely to have the accuracy that they have often been portrayed as having.

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STEPHEN J. O'MEARA AND RING SPOKES BEFORE VOYAGER 1

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Abstract: I consider why Stephen J. O'Meara's visual observations of spokes in Saturn's B ring were not acted upon by planetary scientists before Voyager 1.

Keywords: O'Meara, Saturn, spokes, Voyager

1 INTRODUCTION

My paper "E.E. Barnard and the eclipse of Iapetus in 1889" appeared in the March 2007 issue of this journal (Bryan, 2007). In it I offered an interpretation of Barnard's observation of Iapetus as it emerged from the planet's shadow, crossed the sunlit gap between the planet and rings, entered the shadow of the C ring, and disappeared in the shadow of the B ring. I suggested that changes in the eclipsed satellite's visual magnitude agreed with modern optical depths at some places in the rings but disagreed at other places. If the inner rings in 1889 were identical to their condition today, what Barnard saw might be explained by a combination of shadows from the normal rings and shadows from transitory ring spokes. In support of this, I described historical and modern visual observations that found spoke-like objects in the A and C rings. However, spokes have not been observed by Voyager, Hubble Space Telescope, and Cassini in any location on the rings other than the B ring. Given the credibility of space-based observations, reliance on visual results is problematic. Yet these results are relevant and ought to be considered. In respect of this, I relied extensively on observations of spokes by Stephen J. O'Meara to interpret what some of Barnard's contemporaries saw in Saturn's rings around the time of the eclipse of Iapetus.

O'Meara's experience is interesting history. He was not the first to see spokes in the B ring, but he was the first to report them to planetary scientists in 1976 and to observe them systematically thereafter. Voyager 1 arrived at Saturn in 1980 and obtained credit for discovery of spokes. Why did O'Meara's pre-Voyager reports receive interest but nothing more? This paper corrects an error in my Barnard paper. It goes on to consider what happened when O'Meara, a skillful visual observer of Saturn, reported a completely unexpected result.

2 BARNARD AND O'MEARA

Barnard and O'Meara had the same problem. Both observed extraordinary events, but no other person saw what they saw. They made different decisions about what to do. Barnard was confident about his skill as a visual observer, but he did not see strange things at Saturn that others claimed, and he recoiled from criticism. Further, he could not repeat his observation of Iapetus. O'Meara was also confident of his skill as a visual observer. Unlike Barnard, he was able to repeat his observation. Barnard reported what he saw but de-emphasized and finally ignored the strangest part of the eclipse. O'Meara reported B-ring spokes that exhibited non-Keplerian orbital motion. Responses from their respective audiences differed. Since Barnard's result conformed to what others already thought about Saturn's rings, there were few published reactions and none was critical. O'Meara's result was controversial because non-Keplerian orbital motion appeared to be inconsistent with particulate rings.

To consider how Barnard might have been received if he, too, had offered something radical, perhaps that he had seen the effect of an unknown and unseen ring interior to the C ring, I contrasted that hypothetical circumstance with what happened to O'Meara. I used a set of conditions that scientists historically relied on to evaluate the trustworthiness of testimony given by others about observations that scientists themselves did not make. The conditions, which prefer conservatism, passed Barnard's facts and failed O'Meara's. That is, the outcome implied that O'Meara's testimony represented more risk of error than did Barnard's. Yet nobody could have repeated what Barnard saw. By contrast, Voyager 1 successfully repeated O'Meara's observations. The point of this exercise was that conservatism is not always a reliable guide to recognizing strange reality.

3 AN ERROR

After publication of the Barnard paper, I learned from O'Meara about an error in my text. My statement that "... astronomers whom O'Meara consulted either did not know him or did not fully trust him." is incorrect (Bryan, 2007: 45). In fact, he was both known to and trusted by these astronomers. Valued friendships from that time, 30 years ago, continue today. He described the situation:

> ... as far as I know, the astronomers with whom I consulted did not have any trust issues with me personally. They knew I was a good observer; they knew that I believed in what I saw ... I had correctly identified 1/10-magnitude azimuthal (in four points) variations in Ring A visually over ... [a] period of weeks or months and ... my visual observations were confirmed with photometric observations with a 16-inch reflector at Oak Ridge (O'Meara, pers. comm., 2007).

O'Meara's congenial relationship with his professional colleagues raises the question of why that audience considered his spoke observations but did not pursue them.

4 WHO OR WHAT TO TRUST

The test of trustworthiness referred to in Section 2 relied on a set of considerations that date to the seventeenth century. Its conditions reveal what English scientists once thought about trust. Steven Shapin (1994: 211) identified seven 'prudential maxims of testimony' which I condensed into the test's five points. As Shapin ordered them, contributed testimony is trustworthy if it:

1. Conforms to what we know of the world.
2. Comes from several sources.
3. Is free of inconsistencies.
4. Is the account of an eyewitness.
5. Comes from a competent person.
6. Comes from a person whose manner inspires confidence.
7. Comes from a person who is honest and without agenda.

I do not wish to suggest that those who considered O'Meara's situation literally referred to seventeenth-century maxims. His acquaintances, being planetary scientists, certainly relied upon their extensive knowledge of Saturn and upon common sense to evaluate what he brought to them. In looking back upon events, O'Meara suggested that trust may not have been a factor in the outcome. That is, neither trust of him personally nor trust of his evidence influenced the outcome. He explained that everyone involved was most puzzled by his description of spokes. Uncertainty over what the observations might mean may have been so great that nothing was done. This explanation is both simple and plausible. It may be correct, but there is an alternative.

In carrying out research for the Barnard paper, I found that a majority of the planetary scientists whom I consulted were distrustful of visual results, especially when no independent confirmation existed. Multiple historical reports of spoke-like objects in the A and C rings were not persuasive and did not qualify as being independently confirmed. Similar objects have not been observed by spacecraft or with modern ground-based instruments. Nobody knows how the personal state of the observers affected what they saw or if what they saw was illusory. Finally, visual observation was abandoned by professionals long ago. All of this explains a general lack of enthusiasm among professsionals for visual results. Attitudes in the 1970s could not have been much different than they are today. O'Meara's audience consisted of planetary scientists. He presented them with a serious problem that required a decision. They decided to do nothing. Why was that? For their timeless common sense, the old maxims may provide clues about what his audience thought they could trust.

O'Meara was a collaborator and not an outside contributor as is anticipated by the maxims. However, in respect of the fact that he was an amateur among professionals, there is reason to consider him as an outsider. Four of the seven maxims purport to evaluate the person who testifies. This implies that if an audience has full confidence in the person, on the strength of this alone, they might be able to trust his or her evidence. The decision becomes especially difficult when the evidence offered by a trusted person has significant problems.

O'Meara had much in his favor. His audience knew him to be honest, competent, free of agenda, and to have seen spokes first-hand on several occasions. Further, and significantly, as described above, they had authenticated by photoelectric photometry his ability to see slight differences in the A ring's brightness. However, his evidence was very difficult to accept because it ran contrary to physics. Also, there was no independent confirmation.

5 EXPLAINING-AWAY SPOKES

As O'Meara (pers. comm., 2007) described it, the astronomers around him

> ... found the observations of spokes interesting but, based on the spokes['] defiance of Keplerian rotation, concluded that the atmosphere or conditions on the planet must have set up some sort of visual illusion -- making me see things that unfortunately were not.

His audience did not recognize the real issue. They characterized the problem as an irreconcilable conflict between a radical visual observation of the rings and the physics that govern the motion of bodies in the rings. Posed in this way, conservative scientists had no choice but to prefer physics over the observer and the evidence. Hindsight makes it possible to say that there was no irreconcilable conflict. However, at the time and in the middle of the problem, an indication of the right answer existed, but recognizing it required going beyond the obvious. On one hand, O'Meara was personally trusted. His ability was respected. His accuracy was proven. On the other hand, the existence of Keplerian orbital motion in Saturn's rings is undeniable. If there was no reasonable basis to object to either side of the dilemma, then there was no dilemma. Something else was wrong. That 'something' was a too-simple model of Saturn's rings. Since the audience knew that observations may sometimes correctly conflict with models, they had a basis to suspect the ring model. Either they did not do that, or, if they did, another factor outweighed this consideration.

O'Meara's audience distrusted the visual method. How can that be? They had verified, on their own terms, the accuracy of O'Meara's visual results in the A ring. I distinguish between the audience's perception of O'Meara and their perception of the method he used. The distinction is slight since the observations were a product of his vision and judgment. The audience's expectation was important. They anticipated variations in the A ring's brightness and employed O'Meara to detect those variations. When he found them, and his result conformed to photoelectric photometry of the ring, his skill with the visual method was evident. The audience did not anticipate spokes in the B ring. They expected that these objects did not exist. Spokes were unaccountable and physically implausible. What inducement did planetary scientists have to prefer the observations? Was there a weak link in the evidence for spokes? There was if the audience had the right predisposition. The observations came from a technique that planetary scientists did not use, prefer, understand, or probably trust. The clearest indication of their discomfort with how the evidence was obtained was that they invoked an optical illusion induced by an unknown cause to deceive their otherwise trusted and skilled observer. Instead of arranging for independent confirmation of O'Meara's strange B-ring observation, as they had done in the A ring, the audience allowed the matter to drop. As an optical illusion, there was nothing to observe, so nothing to confirm. O’Meara remained a trusted figure because even the best visual observer may see an optical illusion. If such reasoning occurred, his evidence became untrustworthy by association with a distrusted technique.

6 CONCLUSION

Inaction that followed O’Meara’s report of spokes in 1976 may have been caused by others’ distrust of the visual method he used. This explanation may be correct even though his audience had verified his ability to produce scientifically-valid results for Saturn’s rings.

Except in their dealings with O’Meara, the planetary scientists he consulted almost certainly had no other active involvement with visual observation. The consequence of the decision not to act was to postpone recognition of spokes until Voyager 1 arrived. Would his audience have responded differently if O’Meara had, instead of seeing spokes, measured them with an instrument and reduction process?

7 ACKNOWLEDGMENTS

I thank Steve O’Meara for sharing more details of his experience with spokes before Voyager 1. I also thank Wayne Orchiston for the opportunity to publish this short follow-up to my original paper.

8 REFERENCES

Bryan, J., 2007. E.E. Barnard and the eclipse of Iapetus in 1889. *Journal of Astronomical History and Heritage*, 10, 31-48.

Shapin, S., 1994. *A Social History of Truth: Civility and Science in Seventeenth-Century England*. Chicago, University of Chicago Press.

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CORRIGENDA

James Bryan noticed a misprint in his Barnard paper that was published in the previous issue of *JAH*^{2}. On page 39, near the end of the first paragraph, the uncertainty of Barnard’s estimated magnitudes is shown as ± 0.01 whereas it should be ± 0.1.

Journal of Astronomical History and Heritage, 10(2), 151-156 (2007).

OBITUARY: DONALD EDWARD OSTERBROCK (1924-2007)

On 11 January 2007 the *Journal of Astronomical History and Heritage* lost one of its foundation Editorial Board members and the international astronomical community lost a leading astrophysicist and historian of astronomy when Donald Edward Osterbrock died suddenly in Santa Cruz, California.

With sadness we note the death of Donald E. Osterbrock (Figure 1), one of the leading American astronomers of his generation as well as one of the most influential historians of twentieth century astronomy and astrophysics. Among many other accomplishments, he was a foundation member of the Editorial Board of this journal.

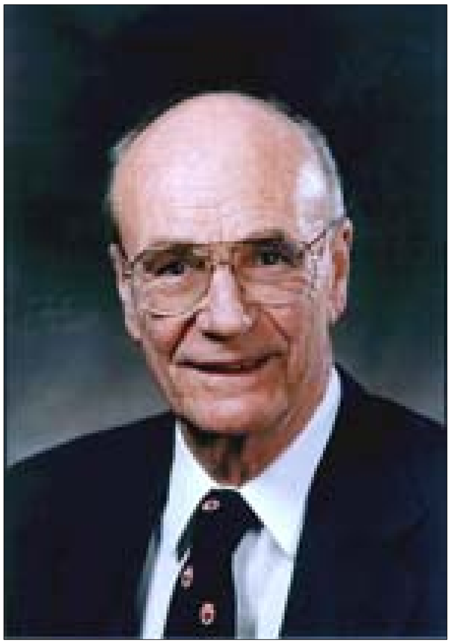

Figure 1: Donald Edward Osterbrock (1924-2007).

Donald E. Osterbrock—'DEO' to his students and 'Don' to his many professional colleagues and friends worldwide—was born on 13 July 1924 in Cincinnati, then a "… pleasant old southern Midwestern city on the bank of a beautiful river …" with a predominantly German culture. Don's father's own parents and his mother's grandparents were all German immigrants, and he recalled the culture in which he grew up as one "… in which hard work, education, science, poetry, music, and love of the outdoor life were all positive values."

Both of his parents worked full-time jobs during high school—his father as a stenographer, his mother as a laboratory assistant in a soap factory—but they completed their educations by attending night school. His father, a great role model for Don, later studied electrical engineering part-time at the city-funded University of Cincinnati while still working full-time in engineering-type jobs; because of his aptitude for physical sciences and mathematics he was hired as an Instructor in Electrical Engineering and later rose through the ranks to teach all the more mathematical engineering courses and to be Chairman of the Department.

Don (Figure 2) attended good public schools in Cincinnati and his interest in astronomy developed early—by the time he was in high school he was reading all the astronomy books he could lay his hands on in the high school library, including the semi-technical Harvard books on astronomy, which were beginning to appear at the time. His interest in astronomy was greatly fanned by the presence of the nearby Cincinnati Observatory, which had been founded by Ormsby McKnight Mitchel, a gifted popularizer whom Don later described as a nineteenth century 'Carl Sagan', as well as by lecturers at the local astronomy club, who, though many of them were quite mundane, included such luminaries as Harlow Shapley and Otto Struve. Don acquired a small second-hand telescope with which he observed the Moon, planets, double stars, and nebulae, and while still in high school became sure that he wanted to follow a career in astronomy.

Figure 2: Don Osterbrock as a young schoolboy.

On 7 December 1941 Don was a high school senior when the Japanese attacked Pearl Harbor. After a few months at the University of Cincinnati, he joined the U.S. Army and completed basic training and an Army/Air Force pre-meteorology school at the University of Chicago, which was a one-year course that included physics and mathematics (but no humanities courses) with the goal of training weather forecasters for the Air Force. After serving as a weather observer at an Army Air Field in California (Figure 3), Don shipped out to Hawaii for several months and was serving in Okinawa as the war ended. He admitted that he was never in much danger, but after the war he was able to continue his education at the University of Chicago, as a civilian student, on the G.I. bill.

Figure 3: Serving at the AAF Weather Station in Victorville, California.

The University of Chicago was still led by outstanding President Robert Maynard Hutchins, and Don received his B.S. in physics and M.S. on campus before heading to Yerkes Observatory in 1949 to begin working toward his Ph.D. Among his teachers was the nuclear physicist Enrico Fermi, whom he would always regard as the best teacher he ever had, and astronomer Thornton Page, who was "… almost the best." In his last year at Chicago, he attended astronomy courses taught by senior astronomers at Yerkes, including Otto Struve, Subrahmanyan Chandrasekhar, Gerard P. Kuiper and William W. Morgan.

Yerkes was then directed by Struve, and Chandrasekhar—'Chandra', as he was known to his students—became Don's thesis advisor. Of Chandra, Don later wrote: "All the graduate students who worked with him felt they had learned much from him, and had been fortunate to have been his students. A few thought of him as a god; most recognized him as an exceptional human being." Apart from Chandra, who was the outstanding theoretical astronomer of the day and a master of quantitative modeling of stellar atmospheres, the person who influenced him most during his Yerkes years was Morgan, an observer and the leading expert on stellar classifications. In contrast to Don, whose parents had always actively encouraged his interest in mathematics and science, Morgan's father beat him severely and discouraged him from a career in science. When Morgan was offered a job at Yerkes by then-Director, Edwin B. Frost, his father violently opposed him, telling him he would "… end up just in a laboratory working for somebody else, and that's nothing." That was the last time Morgan ever saw his father, who left soon afterwards, and Morgan went on to a distinguished career in astronomy.

Morgan's gifts were unique, and his methods were not always appreciated. They were sometimes criticized as being 'qualitative', and one critic even accused him of being nothing more than 'a celestial botanist'. Dimitri Mihalas, who was one of Morgan's later colleagues, noted that "Chandra and Morgan were like two mountain peaks; there was a chasm between them, and everyone else fell somewhere in between." Don was remarkable in that, though he was trained as a theoretical astronomer, he was always an eager observer as well, and he managed to bridge the methodological and personality gap and form close alliances with both of these eminent astronomers.

Because of his military service, Don was an older student, but he achieved success while quite young. He was a graduate student, still in his twenties, when he was involved in one of the outstanding discoveries of twentieth century astronomy: the spiral-arm structure of our Galaxy. Don and Morgan's graduate student, Stewart Sharpless, obtained photographs of HII regions of the Milky Way with the wide-angle Henyey-Greenstein camera, and these photographs contributed to Morgan's identification of the spiral arms. Don later wrote to Morgan: "Let me say that I have always felt a tremendous amount of gratitude to you for including Sharpless and myself as coauthors of that paper … It was a very generous thing for you to do, and I believe that it had a lot to do with the early recognition I received in astronomy. I will never forget it."

While a graduate student at Yerkes, Don met the love of his life, Irene Hansen, who was a native of Williams Bay and was working as a 'computer' for Morgan. Theirs was a very happy marriage, and they had three children together.

Figure 4: At the monastery, Mt. Wilson, in the spring of 1955.

After Chicago, Don was a Post-doctoral Fellow at Princeton and maintained a career-long association with that institution and the neighboring Institute for Advanced Study. He became an Assistant Professor at Caltech (Figure 4) and then relocated in 1958 to the University of Wisconsin, where he rapidly rose through the academic hierarchy (Figure 5). Although he had made a major contribution to understanding the internal structure of low mass stars while at Princeton, the work for which he was best known was in the field of gaseous nebulae, which are clouds of gas and dust that can be associated with both very young massive stars and the ejecta from stars like our Sun as they end their active life cycle, becoming White Dwarfs. Don had a superb understanding of quantum mechanics and worked with the University College London physicist, Michael J. Seaton, in establishing how observations of certain emission lines of gaseous nebulae could be used to establish their physical conditions of temperature and density. Later he applied these same techniques to the study of the most luminous objects in the Universe, the active galactic nuclei, developing the standard model for these objects. He summarized the tools and results of this approach in a textbook, *Astrophysics of Gaseous Nebulae and Active Galactic Nuclei*, which recently came out in its revised third edition with Gary J. Ferland as co-author. This text is the standard in the field and has served two generations of astrophysicists. In all, Don had twenty different graduate students and post-doctoral fellows, and he left both a personal and professional legacy as an astrophysicist.

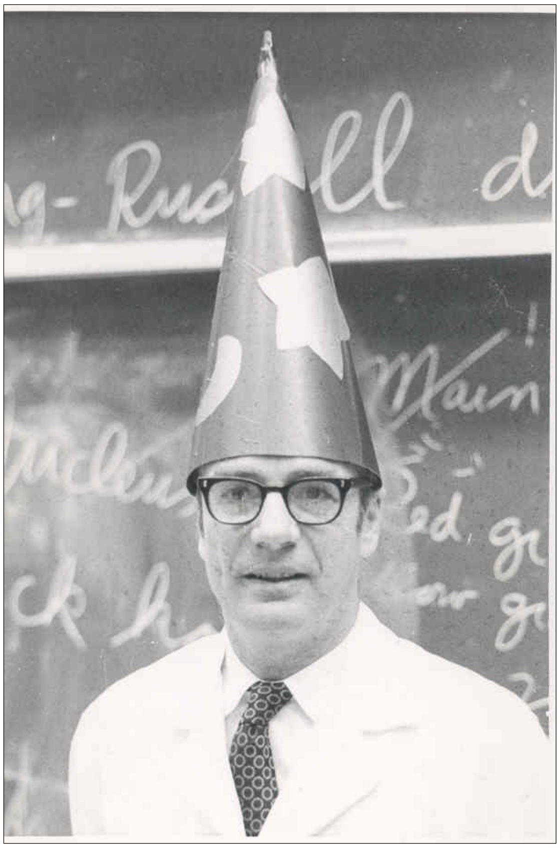

Figure 5: Presenting the Halloween Lecture at Madison in 1972.

Beginning in the mid-1970s, Don began devoting some of his time to the history of astronomy, which became his main focus after his retirement. He once explained that astronomers should work as hard as they possibly can on astronomy while they are young, but "… after you pass fifty, and start getting that nostalgic, family, searching-for-roots feeling, I hope that you will be sure to give your scientific correspondence to your university's, laboratory's, observatory's, or company's archives for the benefit of future generations of historians." Don's own career developed in this way.

According to his own account, in 1973, when he first came to Lick Observatory as its Director, Mary Lea Heger Shane showed him the Observatory's archives, collected and organized from old files and dusty letter books going back to the days of the Lick Trust which had built the Observatory in the 1880s. Don's astronomical history interest dated from that moment (Figure 6).

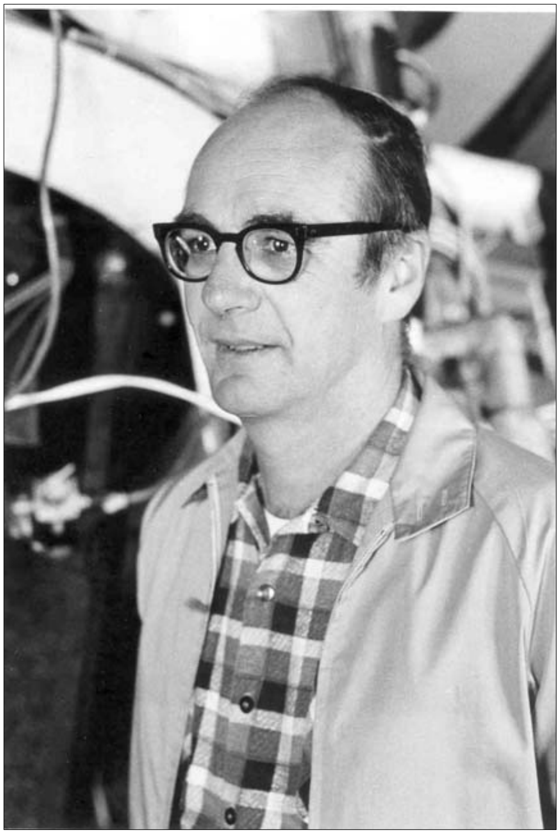

Figure 6: Shortly after becoming Director of the Lick Observatory.

His first major study in the history of astronomy, a series of articles on what he called the 'California-Wisconsin axis in American astronomy', appeared in 1976, when he was fifty-two. One sees the turning of his interests in a quantitative analysis of his bibliography. In the 1970s, he published 39 astrophysics papers and 7 historical papers; in the 1980s, the ratio was 40 to 20; in the 1990s he published 21 astrophysics papers and 29 historical papers; and in the 2000s, 5 astrophysics papers and 20 historical papers. His level of productivity in both areas was outstanding.

Don's knowledge of astronomy and instrumentation was, of course, unsurpassed. He once noted that "… immersion in a field like astronomy makes one better qualified to understand what others have done in that field, and to write about it." It was a necessary but not a sufficient prerequisite. In addition he brought to the job great literary skill, and was also possessed of remarkable psychological insights.

He had, of course, known many of the leading figures of twentieth century astronomy personally, and he also had participated, as a leader and a researcher, in many of its developments. But rather than writing impressionistic memoirs, as many astronomers have done, he adopted the rigorous methods of historians like Owen Gingerich, William Graves Hoyt, and LeRoy Doggett, all of whom he greatly admired, and did not write anything until he had thoroughly surveyed the primary sources—letters, personal papers, diaries, and other materials—pertaining to the figures or institutions he chose as his subjects. Though he valued oral histories, he realized how hazy or self-serving or selective the memory can be and preferred to anchor himself wherever possible in the facts provided by contemporary documents. He was greatly aided in this work by having ready access to the Mary Lea Shane archives of the Lick Observatory, and he noted that they were "… a tremendous advantage … I can look up almost any astronomer since 1880 and find letters to him or her there." He also consulted extensively the archival holdings of other institutions, not all of which are as well or lovingly organized and cared for as the Shane archives.

Figure 7: Don and Irene Osterbrock at their home in Santa Cruz in October 2005.

Don had a real flair for character and narrative. He once said that every story needs a hero and a villain (or villains). After writing his first biography about James Keeler—a supremely gifted and unappreciated Adonais-like figure, who accomplished great things and died tragically young—Don showed his versatility by taking up the tremendous figure of George Ellery Hale, who had already been subject of a splendid (if worshipful) biography by Helen Wright. Instead of following in the established paths, he revealed a somewhat darker side to Hale and pitted him as villain against the perfectionistic and persecuted hero of telescope-making, George Willis Ritchey. The result was a breathtaking and paradigm-shifting reappraisal of early twentieth century astronomy. His last book, a biography of Walter Baade, is a warm and intimate tribute to an astronomer he greatly admired, and whom in some respects he resembled. (Like Baade he was an 'astronomer's astronomer'—always generous with ideas and willing to support others in their researches.)

In addition to these towering biographies, Don's other astronomy books are comprehensive histories of the Lick Observatory (with John R. Gustafson and W. J. Shiloh Unruh) for its centennial celebration in 1988, and of the Yerkes Observatory, for its centennial celebration in 1997. A full list of his historical writings is included below.

Don received almost all the awards that it is possible for an astronomer to receive, including the Bruce Medal of the Astronomical Society of the Pacific, the Henry Norris Russell Lectureship of the American Astronomical Society, the Gold Medal of the Royal Astronomical Society (he was one of only a few American astronomers to receive this award), the Hans Lippershey Medal of the Antique Telescope Society and the LeRoy Doggett Prize for Historical Astronomy of the Historical Astronomy Division of the American Astronomical Society. His address on receiving this last award was entitled "History is too important to be left to the historians", which espouses his strong view that astronomers should write their own histories rather than leave them to the mercy of professional historians.

Don was a member of the National Academy of Sciences, a Past President of the American Astronomical Society, etc., etc.

He offered great encouragement and was a mentor to many. He astounded with his near-photographic memory and ability to call up, seemingly effortlessly, detailed information about the history of astronomy. At times this ability seemed almost beyond the possibility of what the human brain could be expected to accomplish. His intellect was formidable, but he also possessed a charming, down-to-earth manner; he was sociable, interested in learning details about his colleagues' families and personal lives, and was always warm and genuinely interested (Figure 7).

Although he wrote extensively about the history of astronomy, Don's published volumes contain only a fraction of the knowledge and wisdom that he carried in his head. He literally died in harness, working right up to the end. His death from a heart attack on 11 January 2007 was unexpected by all who knew him, and occurred as he walked across campus from his office where he had put in the usual morning's work.

Donald Osterbrock: Full List of History of Astronomy Publications in English

Books:

Osterbrock, D.E., 1984. *James E. Keeler, Pioneer American Astrophysicist: And the Early Development of American Astrophysics*. Cambridge, Cambridge University Press.

Osterbrock, D.E., Gustafson, J.R., and Unruh, W.J.S., 1987. *Eye on the Sky: Lick Observatory's First Century*. Berkeley, University of California Press.

Osterbrock, D.E, 1993. *Pauper & Prince: Ritchey, Hale, & Big American Telescopes.* Tucson, University of Arizona Press.

Osterbrock, D.E., 1997. *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*. Chicago, University of Chicago Press.

Osterbrock, D.E., 2001. *Walter Baade: A Life in Astrophysics*. Princeton, Princeton University Press.

Research Papers:

Osterbrock, D.E., 1976. The California-Wisconsin axis in American astronomy. *Sky & Telescope*, 51, 7-14, 91-97.

Osterbrock, D.E., 1976. The California-Wisconsin axis in American astronomy. *Wisconsin Academy Review*, 23(1), 2-8.

Osterbrock, D.E., 1977. Su-Shu Huang. *The Astronomy Quarterly*, 1, 261-264.

Osterbrock, D.E., 1978. First world astronomy meeting in America. *Sky & Telescope*, 56, 180-183.

Osterbrock, D.E., 1978. Edward S. Holden—the founder of the A.S.P. and the early days of the California-Wisconsin astronomical connection. *Mercury*, 7, 106-110.

Osterbrock, D.E., 1978. The California-Wisconsin axis in American astronomy. *Transactions of the Wisconsin Academy of Science, Arts & Letters*, 66, 1-24.

Osterbrock, D.E., 1979. James E. Keeler, pioneer astrophysicist. *Physics Today*, 32(2), 40-47.

Osterbrock, D.E., 1980. Lick Observatory solar eclipse expeditions. *The Astronomy Quarterly*, 3, 67-79.

Osterbrock, D.E., 1980. Graduate astronomy education in the early days of Lick Observatory. *Mercury*, 1, 151-155.

Osterbrock, D.E., 1980. America's first world astronomy meeting: Chicago 1893. *Chicago History*, 9, 178-185.

Osterbrock, D.E., 1981. Further links in the California-Wisconsin astronomical connection. *Transactions of the Wisconsin Academy of Science, Arts & Letters*, 69, 153-162.

Osterbrock, D.E., and Cruikshank, D.P, 1982. Keeler's Gap in Saturn's A ring. *Sky & Telescope*, 64, 123-126.

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.

Osterbrock, D.E., 1983. Rudolph Leo Minkowski, 1895-1976. *Biographical Memoirs of the National Academy of Sciences*, 54, 271-298.

Osterbrock, D.E, 1984. Mary Lea Shane, 1987-1983. *Journal for the History of Astronomy*, 15, 74.

Osterbrock, D.E., 1984. The rise and fall of Edward S. Holden. *Journal for the History of Astronomy*, 15, 81-127, 151-176.

Osterbrock, D.E., 1985. James E. Keeler's "prettiest application of Doppler's principle." *Mercury*, 14 (April-May), 46-62.

Osterbrock, D.E., 1985. Rudolph Minkowski: observational astrophysicist, *Physics Today*, 38(4), 50-57.

Osterbrock, D.E., 1985. The quest for more photons: how reflectors supplanted refractors as the monster telescopes of the future at the end of the last century. *Astronomy Quarterly*, 5, 87-95.

Osterbrock, D.E., 1985. The 1910 meeting of the International Union Cooperation in Solar Research. *Mercury*, 14, 152-153.

Osterbrock, D.E., 1985. The minus first meeting of the American Astronomical Society. *Wisconsin Magazine of History*, 68, 108-118.

Osterbrock, D.E., 1986. Failure and success: two early experiments with concave gratings in stellar spectroscopy. *Journal for the History of Astronomy*, 17, 119-129.

Osterbrock, D.E., 1986. Nicholas T. Bobrovnikoff and the scientific study of Comet Halley 1910. *Mercury*, 15, 46-50, 63.

Osterbrock, D.E., and Seidelmann, P.K., 1987. Paul Herget, 1908-1981. *Biographical Memoirs of the National Academy of Sciences*, 57, 59-86.

Osterbrock, D.E., 1988. Lick Observatory: the first century. *Mercury*, 17, 34-42.

Osterbrock, D.E., 1989. To climb the highest mountain: W.W. Campbell's 1909 Mars expedition to Mount Whitney. *Journal for the History of Astronomy*, 20, 77-97.

Osterbrock, D.E., and S. Vasilevskis, 1989. C. Donald Shane, 1895-1983. *Biographical Memoirs of the National Academy of Sciences*, 58, 489-511.

Osterbrock, D.E., 1989. Guido Münch: an appreciation. In Tenorio-Tagle, G., Moles, M., and Melnick, J. (eds.). *Structure and Dynamics of the Interstellar Medium*. Berlin Heidelberg, Springer-Verlag. Pp. xvii-xxi.

Osterbrock, D.E., 1990. The observational approach to cosmology: U.S. observatories pre-World II. In Bertotti, B., Balbinot, R., Bergia, S., and Messina, A. (eds.). *Modern Cosmology in Retrospect*. Cambridge, Cambridge University Press. Pp. 247-289.

Osterbrock, D.E., Brashear, R.S., and Gwinn, J.A., 1990. Young Edwin Hubble. *Mercury*, 19, 2-15.

Osterbrock, D.E., 1990. Armin O. Leuschner and the Berkeley Astronomical Department. *Astronomical Quarterly*, 7, 95-115.

Osterbrock, D.E., Brashear, R.S., and Gwinn, J.A., 1990. Self-made cosmologist: the education of Edwin Hubble. In Kron, R. (ed.). *Evolution of the Universe of Galaxies*. San Francisco, Astronomical Society of the Pacific. Pp. 1-18.

Osterbrock, D.E., 1992. The appointment of a physicist as director of the astronomical center of the world. *Journal for the History of Astronomy*, 23, 155-165.

Osterbrock, D.E., 1993. The Canada-France-Hawaii Telescope and George Willis Ritchey's great telescopes of the future. *Journal Royal Astronomical Society of Canada*, 87, 56-63.

Osterbrock, D.E., Gwinn, J.A., and Brashear, R.S., 1993. Edwin Hubble and the expanding universe. *Scientific American*, 269, 84-89.

Osterbrock, D.E., 1994. Fifty years ago: Astronomy; Yerkes Observatory; Morgan, Keenan and Kellman. In Corbally, C.J., Fray, R.O., and Garrison, R.F. (eds.). *The MK Process at 50 Years: A Powerful Tool for Astrophysical Insight*. San Francisco, Astronomical Society of the Pacific. Pp. 199-214.

Osterbrock, D.E., 1994. Getting the picture: wide-field astronomical photography from Barnard to the achromatic Schmidt. *Journal for the History of Astronomy*, 25, 1-14.

Osterbrock, D.E., 1994. William W. Morgan. *Physics Today*, 47(12), 82-83.

Osterbrock, D.E., 1994. Franklin E. Roach, 1905-1993. *Bulletin of the American Astronomical Society*, 26, 1608-1610.

Osterbrock, D.E., 1995. Founded in 1895 by George E. Hale and James E. Keeler: the *Astrophysical Journal* centennial. *Astrophysical Journal*, 438, 1-7.

Osterbrock, D.E., 1995. Walter Baade, observational astrophysicist (1): the preparation 1893-1931. *Journal for the History of Astronomy*, 26, 1-32.

Osterbrock, D.E., 1995. An infrared astronomer's early vision of airborne astronomy: Paul Merrill 1920. In Hass, M.R., Davidson, J.A., and Erickson, E.F. (eds.). *Airborne Astronomy Symposium on the Galactic Ecosystem: From Stars to Dust*. San Francisco, Astronomical Society of the Pacific. Pp. 619-622.

Osterbrock, D.E., 1995. Walter Baade's discovery of the two stellar populations. In van de Kruit, P.C., and Gilmore, G. (eds.). *Stellar Populations*. Dordrecht, Kluwer. Pp. 212-230.

Osterbrock, D.E., 1996. Nicholas Ulrich Mayall, 1906-1993. *Biographical Memoirs of the National Academy of Sciences*, 69, 189-212.

Osterbrock, D.E., 1996. Walter Baade, observational astrophysicist (2): Mount Wilson 1931-1947. *Journal for the History of Astronomy*, 227, 301-348.

Osterbrock, D.E., 1996. Chandra and his students at Yerkes Observatory. *Journal of Astrophysics and Astronomy*, 17, 233-268.

Osterbrock, D.E., 1997. William Wilson Morgan, 1906-1994. *Biographical Memoirs of the National Academy of Sciences*, 72, 289-313.

Osterbrock, D.E., 1997. Walter Baade, observational astrophysicist (3): Palomar and Göttingen, 1948-1960 (Part A). *Journal for the History of Astronomy*, 28, 283-316.

Osterbrock, D.E., 1997. Louis Berman, 1903-1997. *Bulletin of the American Astronomical Association*, 29, 1468-1469.

Osterbrock, D.E., 1998. Walter Baade, observational astrophysicist (3): Palomar and Göttingen, 1948-1960 (Part B). *Journal for the History of Astronomy*, 29, 345-377.

Osterbrock, D.E., 1998. Subrahmanyan Chandrasekhar, 19 October 1910-21 August 1995. *Proceedings of the American Philosophical Society*, 142, 658-665.

Osterbrock, D.E., and Briggs, J.W., 1998. The challenges and frustrations of a veteran astronomical optician: Robert Lundin, 1880-1962. *Journal of Astronomical History and Heritag*e, 1, 93-103.

Osterbrock, D.E., and Briggs, J.W., 1999. The first Alvan Clark & Sons largest refracting telescope in the world. *Journal of the Antique Telescope Society*, 16, 11-13.

Osterbrock, D.E., 1999. AAS meetings before there was an AAS: the pre-history of the Society. In DeVorkin, D.H. (ed.). *The American Astronomical Society's First Century*. Washington, American Astronomical Society. Pp. 3-19.

Osterbrock, D.E., 1999. The first West Coast meeting of the AAS. In DeVorkin, D.H. (ed.). *The American Astronomical Society's First Century*. Washington, American Astronomical Society. Pp. 37-39.

Duerbeck, H.W., Osterbrock, D.E., Barrera, S., and R.Leiva G., 1999. Halfway from La Silla to Paranal – in 1909. *ESO Messenger*, 95, 34-37.

Osterbrock, D.E., 1999. Seyfert Galaxies. *Astrophysical Journal*, 525, 337-338.

Sheehan, W., and Osterbrock, D.E., 2000. Hale's "little elf": the mental breakdowns of George Ellery Hale. *Journal for the History of Astronomy*, 31, 93-114.

Osterbrock, D.E., 2000. A view of the future as seen from the past. *Publications of the Astronomical Society of the Pacific*, 112, 869-872.

Osterbrock, D.E., 2000. A fortunate life in astronomy. *Annual Review of Astronomy and Astrophysics*, 38, 1-33.

Osterbrock, D.E., 2000. Dorothy N. Davis Locanthi, 1913-1999. *Bulletin of the American Astronomical Society*, 32, 1677-1678.

Osterbrock, D.E., 2000. Kandarpa Narahari Rao, 1921-2000. *Bulletin of the American Astronomical Society*, 32, 1684-1685.

Osterbrock, D.E., 2000. Herman Zanstra, Donald H.Menzel, and the Zanstra Method of nebular astrophysics. *Journal for the History of Astronomy*, 32, 93-108.

Osterbrock, D.E., 2001. Astronomer for all seasons: Heber D. Curtis. *Mercury*, 30 (May-June), 24-31.

Osterbrock, D.E., 2001. Don Hendrix: Mount Wilson and Palomar Observatories master optician of Schmidt cameras and large telescopes. *Journal of the Antique Telescope Society*, 21, 2-9.

Harris, D., Schaumberg, D., and Osterbrock, D.E., 2002. An early photograph of S.W. Burnham at the Clark 18.5-inch refractor of the old Dearborn Observatory. *Journal of the Antique Telescope Society*, 22, 4.

Osterbrock, D.E., 2002. Young Don Menzel's amazing adventures at Lick Observatory. *Journal for the History of Astronomy*, 32, 95-118.

Osterbrock, D.E., 2002. In memory of Albert E. Whitford, 1905-2002. *Mercury*, 31 (July-August), 8-9.

Osterbrock, D.E., 2002. Walter Baade: master observer. *Mercury*, 31 (July-August), 32-41.

Osterbrock, D.E., 2002. The view from the observatory: history is too important to be left to the historians. In Heck, A., (ed). *Organizations and Strategies in Astronomy*. Dordecht, Kluwer. Pp. 201-215.

Osterbrock, D.E., 2003. Albert Edward Whitford. *Physics Today*, 56 (1), 67-68.

Osterbrock, D.E., 2003. Don Hendrix: Mount Wilson and Palomar Observatories master optician. *Journal of Astronomical History and Heritage*, 6, 1-12.

Osterbrock, D.E., 2003. The California-Michigan Axis of American Astronomy. *Journal of Astronomical History and Heritage*, 6, 120-136.

Osterbrock, D.E., 2003. Ernest Hurst Cherrington, Jr., 1909-1996. *Bulletin of the American Astronomical Society*, 35, 1458-1459.

Osterbrock, D.E., 2004. Albert Edward Whitford, 1995-2002. *Biographical Memoirs of the National Academy of Sciences*, 85, 381-407.

Osterbrock, D.E., 2006. Early photographs of the distant Sierra Nevada Mountains taken from Lick Observatory. *Journal of Astronomical History and Heritage*, 9, 181-184.

Osterbrock, D.E., 2007. Frank Ross, his Ross Lens design, and the Lick Observatory 20-inch astrograph. *Journal for the History of Astronomy*, 38, 31-73.

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BOOK REVIEWS

Caroline Herschel's Autobiographies, edited by Michael Hoskin (Cambridge, Science History Publications, 2003). Pp. 147, ISBN 0 905193 06 7, £25.

Caroline Herschel is without doubt the most renowned and admired woman in the history of astronomy. She was for fifty years the indefatigable collaborator of her brother, the great William Herschel, and achieved fame in her own right as the most successful woman discoverer of comets of all time. Caroline's memoirs, written in her old age, are a valuable, indeed indispensable, first-hand witness to the rise of William Herschel's brilliant astronomical career, as well as being a fascinating record of her own extraordinary life. They are copiously quoted in *Memoirs and Correspondence of Caroline Herschel* (1876) and in *The Herschel Chronicle* (1933), key sources of information on the senior Herschels' lives. The original texts, however, were never published until Michael Hoskin, the leading Herschel scholar of today, collected and edited them in the present volume.

The memoirs consist in fact of two separate versions of her autobiography, the first written when Caroline was in her seventies and the second when she was in her nineties. The first autobiography, the longer of the two, stretches from her earliest childhood in Hanover to the day of William's marriage, sixteen years after she had come to England. That event had come as a deep shock to her at the time, and her autobiography, at least as committed to paper, went no further. Caroline had by then discovered her first comet, and was already known and admired in her brother's elevated scientific circle.

Caroline's original manuscripts are preserved in different collections and Dr Hoskin has meticulously assembled them and has published them in full for the first time. Here we read Caroline's words exactly as she wrote them, complete with erratic spelling and somewhat stilted language, which vividly evoke her personality in a way that is lost in second-hand accounts. She had an extraordinary memory, and her story, which also involves the lives of her four brothers and her own relationships with people outside the family, is of absorbing human interest, quite apart from its value in the history of astronomy. Caroline herself emerges as a woman of rigid principles, doggedly hardworking, who never spared herself in the interests of her beloved brother. Her recollections of childhood are unrelentingly grim, and the resentment she felt at her lack of education for which she blamed her hard-pressed uneducated mother, never left her.

The second autobiography, written twenty years after the first, with the encouragement of her nephew John and his wife, revisits the earlier periods. Her memory is as sharp as ever, and her old grievances are again recalled. However, the portrait of her mother, who surely suffered her own share of hardship, seems—at least to this reader—to be more understanding and more just than in the first version which was written at a low point in Caroline's life.

Dr Hoskin (2003) has already provided the definitive account of Caroline Herschel's career as an astronomer and collaborator of her brother William. With this complete edition of the autobiographies, he now gives us a rich source of enlightenment, previously only partially explored, on Caroline's mind and character. He has performed his task with immense thoroughness. There are copious explanatory footnotes and elucidations of Caroline's occasional German expressions. Caroline's original pagination is retained in the body of the texts, and cross-references in the margins allow the versions to be compared. The Introduction is particularly helpful to the reader by explaining the confusing background of war which profoundly affected the entire Herschel household in the early part of Caroline's story. There is a genealogy going back three generations and a detailed chronology of family events during the relevant years that guide the reader through the frequent comings and goings of its members. The book is elegantly designed and produced, with illustrations of Caroline's telescopes and samples of her handwriting from her observing book, including the drawing of her first comet.

This is a book which will be indispensable to future students of Caroline Herschel's life and work and will also be of value to historians of women in science. It is also warmly recommended to all lovers of astronomy as the remarkable and highly-readable story, told in her own words, of one of the great icons of science.

Reference:

Hoskin, M., 2003. *The Herschel Partnership. As Viewed by Caroline*. Cambridge, Science History Publications.

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Two Paths to Heaven's Gate, by Nan Dieter Conklin (Greenbank, National Radio Astronomy Observatory, 2006), pp. x + 195, ISBN 0-9700411-1-X (paperback), $13.00 (USA), $24.00 (Canada/Mexico), $30.00 (elsewhere), 152 x 227 mm.

Nan Dieter Conklin is a pioneering radio astronomer, and was the first American woman to complete a Ph.D. in this field, back in 1958, with a thesis on "Neutral Hydrogen in M33".

Dr Conklin was born as Nannielou Reier in 1926, and even in high school decided that science was to be her *forte*. She refined this upon entering Groucher College (Baltimore) and "After two weeks in that first astronomy course I knew I had found what I wanted." (page 18). Her inspiration was the well-known solar astronomer, Dr Helen Dodson. From Groucher, she secured a position with the U.S. Coast and Geodetic Survey, and in 1951 moved to the Naval Research Laboratory where (as Nan Hepburn) she became involved in solar radio astronomy and published her first research paper. Soon she was also doing H-line work, and this was to remain one of her research emphases throughout her career as an astronomer.

In 1955 she began studying for a Ph.D. at Harvard, and after graduating joined the Air Force Cambridge Research Laboratory as an astronomer. In 1965 she moved across the continent to the Radio Astronomy Laboratory at the University of California, Berkeley, and quickly expanded her research 'portfolio' to include OH and formaldehyde lines. She would remain there until she took early retirement in 1977, and her autobiography brings out the excitement she felt in actually 'doing' science—and making discoveries.

Although designed primarily for a non-astronomical audience, this book immerses its readers in some astronomy (particularly radio astronomy), but it does more than this; it also discusses the state of astronomy in the Soviet Union and in France, in 1973-1975, a time when few American astronomers, at any rate, could speak from first-hand experience about their experiences behind 'the Iron Curtain'. Nan and her third husband, Garrett Conklin, spent April-June 1973 there, visiting Moscow, the Crimean Astrophysical Observatory (although the radio astronomers she specifically went to see were mysteriously absent!), the remarkable RATAN-600 Radio Telescope and the 6m telescope in the Northern Caucasus Mountains (which, at that time, was the largest optical telescope in the world). A highlight of their visit to Moscow was the presentation of the 1972 Bruce Medal to I.S. Shklovsky at a ceremony held at the Sternberg Astronomical Institute.

What I also found particularly captivating was the way in which Nan Conklin managed to successfully weave non-astronomical threads into her autobiography, thereby providing us with a view of how a remarkable woman managed to combine an astronomical career with being mother to two daughters, whilst experiencing (and at times greatly enjoying) three marriages, and coping with multiple sclerosis from the age of just 33. In the course of the narrative we also find interesting perspectives on well-known friends:

> Perhaps the greatest benefit of having the astronomy department near us was the presence of graduate students—a talented, dedicated bunch. On my first day I met the young man who was to be my favorite of them all—Miller Goss. He was the sort of student teachers dream of; one only needed to stand back and watch him grow. (Page 62).

Then, reflecting upon her 30-odd years in astronomy, she writes:

> I had chosen [to work in] astronomy for a complex set of reasons, among them my feeling that science held a sort of security for me ... I thought I could depend on my intellect, but that I could not depend on other people … Like many young people I wanted to do something that mattered, something that would last. It soon became clear that my high-flown ambition would require not only very hard work but also a measure of luck. I also found that it would make my connecting with other people still more difficult—especially with women … For reasons that I still cannot fathom anyone who chooses science is thought to be somehow smarter, and all my protests to the contrary don't seem to make any difference. (Page 148).
>
> That is not to say that astronomy provides an easy life. Acquiring the background in physics and mathematics is hard … [and] Actually working in observational astronomy presents other problems … On the other hand, I have found in astronomy a career always satisfying and occasionally thrilling. One persists through times of routine, demanding hours with the possibility of an extraordinary reward. Make no mistake; the approval of colleagues, especially those not familiar with your work, is wonderful, but it does not hold a candle to the joy in realizing that you are seeing something for the first time. In my experience there are two ways in which real discoveries are made: stumbling on something totally unexpected while looking at something else, and searching for something because you think it might be there. In my own work I found one of each. (Pages 148-149).

The foregoing examples—and various others that I could have given—indicate that *Two Paths to Heaven's Gate* is, at times, captivating reading. Apart from Nan Conklin's own narrative, we are treated to Forewards by Moreton S. Roberts and Claire Hooker (the latter on "The Woman in the Woman Scientist"), and I found the 'Timeline' on pages 13-14 and the Endnotes and full list of her publications (pp. 181-191) invaluable. An extra bonus are the photographs and paintings by Nan Conklin scattered throughout the book, although it has to be said that some of these could have been a little sharper. However, this in no way detracts from a fascinating book by a remarkable scientist, and it belongs in the library of every astronomer with an interest in the history of radio astronomy or the roles that women have played in the overall development of science.

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***The Man Who Changed Everything: The Life of James Clerk Maxwell*, by Basil Mahon (Chichester, John Wiley & Sons, 2004), pp. xx + 226, ISBN 13-978-0470-86171-4 (paperback), £8.99, 129 x 197 mm.**

Number 14 India Street is part of an elegant Georgian terrace in Edinburgh's New Town. It is now the home of the James Clerk Maxwell Foundation. The Foundation acquired it in 1993 because it is the birthplace of James Clerk Maxwell (1831–1879). As well as a working centre for mathematicians and scientists which regularly hosts meetings and symposia there is also a small museum of Maxwell memorabilia which is well worth visiting if you are in Edinburgh. Maxwell, of course, is famous for being the physicist who is not famous. Amongst physicists his contributions are held to be broadly as fundamental as those of Newton or Einstein, but he is largely unknown to the wider public. The aim of Mahon's biography is to rectify this deficiency and introduce Maxwell to a larger audience.

The outline of Maxwell's life is simply told. Though born in 14 India Street, he spent his early years at the family estates at Glenlair, in Galloway, South West Scotland. He attended the Edinburgh Academy and later Edinburgh and Cambridge Universities. He held posts at Aberdeen (where he was made redundant when that city's two universities merged), King's College London and Cambridge. Throughout he divided his time between his university posts and Glenlair. He died at the tragically early at the age of forty-eight.

Maxwell is best known for two pre-eminent pieces of work. The equations of electromagnetism that now bear his name underpin all electrical and magnetic phenomena and describe one of the fundamental forces of nature. They also predicted electromagnetic radiation. Maxwell and Ludwig Boltzmann, working independently, formulated the kinetic theory of gases which explained the behaviour of gases in terms of molecules moving with a range of velocities, and in the process introduced statistical methods into physics.

However, Maxwell did much other important work. He made significant advances to the study of colour vision and took the first colour photograph, a feat which was not replicated for many years. He developed the modern understanding of Saturn's rings by showing that they must be composed of countless separate particles, each pursuing its own orbit. He did important early work on the standardisation of electrical and magnetic units and in the process developed the practice of decomposing all units into their basic constituent quantities, the familiar 'mass, length and time' which is now second-nature to all physicists. He was the first Director of the Cavendish, superintending its construction and early years of operation, and consequently one of the architects of Cambridge's rise to its current eminence in the physical sciences.

All told, Maxwell's achievements are rather impressive for a man whose nickname at his first school was 'Daftie' because he was thought to be slow on the uptake. He seems to have been an admirable man to boot: kind, modest, generous, helpful and with a weakness for jokes and humorous poems (some of which are reproduced in the book). As a child, Maxwell continuously pestered his parents and relatives to know "what's the go o' that", a curiosity to understand the working of things that stayed with him throughout his life and underpinned all this scientific work.

The book's author, Basil Mahon, is an engineering graduate with a long-standing interest in Maxwell which originated when he was a student. Now retired, he has been an officer in the Royal Mechanical and Electrical Engineers and a civil servant. His biography of Maxwell tells the story of his life and work. It is well-written and easy to follow, with a largely chronological treatment. Maxwell's physical ideas are simply and effectively explained in non-technical language and with virtually no mathematics. The text is not unduly burdened with references but there are extensive notes and a bibliography for further study. The book has an index, a *dramatis personae* detailing Maxwell's (sometimes confusing) relatives and colleagues and a chronology of important dates. I did not notice any typographic errors. There is an inset of black and white illustrations, well-reproduced on glossy paper. During 2006 I attended a lecture that Mahon gave about Maxwell and he spoke as well as he writes.

The Man Who Changed Everything can be strongly recommended as a general biography of Maxwell. Readers seeking a detailed, mathematical analysis of his work should look elsewhere (Mahon relates that scholars of Maxwell still argue about whether a minus sign omitted from one of the equations in Maxwell's paper "On Physical Lines of Force", published in 1861, and apparently corrected for by changing the meaning of a symbol in another equation, is a mistake or a deliberate part of the treatment). However, as a general account aimed at introducing Maxwell to a wider audience this book deserves to succeed.

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***Harrison in the Abbey*, edited by Arnold Wolfendale (Durham, Roundtuit Press, 2006). Pp. [vi] + 78. ISBN 1-904499-06-6 (paperback), £10.00 + postage, 148 x 211 cm.**

John Harrison (1693–1776) is a famous figure in maritime studies and horology; he is the man who invented an accurate portable chronometer which revolutionized the determination of longitude at sea. Some years ago, the former Astronomer Royal, Sir Arnold Wolfendale, noted that Harrison's name was not commemorated at Westminster Abbey, and with commendable energy—and invaluable support from The Worshipful Company of Clockmakers—set about rectifying this. As a result, on 24 March 2006 a memorial was unveiled at the Abbey, precisely 313 years after Harrison's birth.

The appearance of this little book (Figure 1) was linked to the unveiling at Westminster Abbey, and in it we are presented with thumbnail sketches of Harrison and his chronometers. After a Foreword by His Royal Highness, Prince Philip, and two short introductory chapters by Sir Arnold, we are introduced to Harrison's early wooden clocks by John Taylor, and this is followed by a biographical sketch on Harrison by Dava Sobel, "Harrison's Contributions in Perspective" by William Andrewes, chapters on The Worshipful Company of Clockmakers and on Harrison's association with this group and the Clockmakers' Museum (by Dianna Uff and George White, respectively), and finally, two further biographical perspectives on Harrison, one by Andrew King and the other by Jonathan Betts.

For those who have already enjoyed Sobel's *Longitude* and want to learn more about Harrison without wading into Quill's (1966) long biography, Wolfendale's little book is an ideal option. It is beautifully-produced on fine-quality paper, well-endowed with coloured images, reasonably-priced and is very readable. I recommend that you add it to your bookcase (copies can be purchased from The Clerk, The Worshipful Company of Clockmakers, Salters' Hall, Fore Street, London EC2Y 5DE).

Reference:

Quill, H., 1966. *John Harrison. The Man Who Found Longitude*. London, John Baker.

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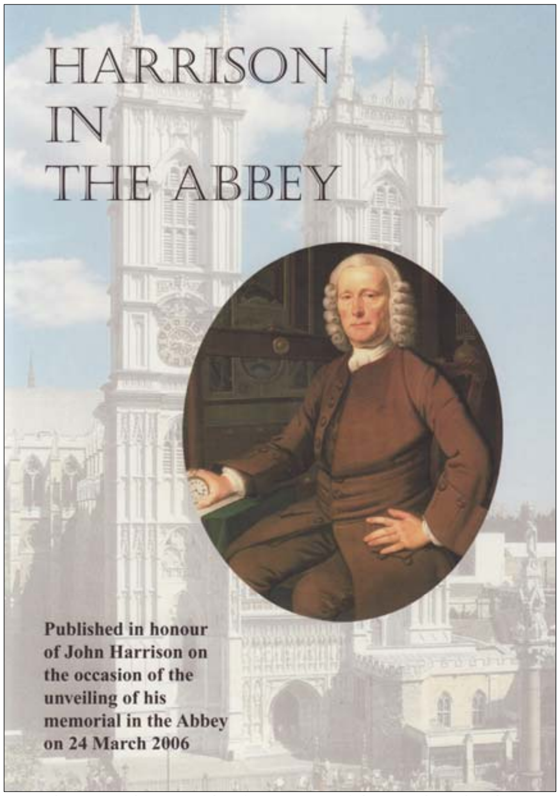

Figure 1: The attractive front cover of the Harrison book.

***Observatoires et Patrimoine Astronomique Français*, edited by Guy Boistel, 2005 (Lyons, ENS Éditions, Cahiers d'Histoire et de Philosophie de Sciences No. 54), pp. 220, ISBN 2-84788-015-1, €35, 150 x 210 mm.**

This volume contains the Proceedings of the Centre François Viète in University of Nantes colloquium, "Observatoires et Patrimoine Astronomique Français", which was held in Nantes on 8-9 June 2001. Professor Emeritus Jacques Gapaillard pointed out the three circumstances which triggered these two days dedicated to history of astronomy and observatories in France: first, a growing academic interest within France in its astronomical heritage, then the formation in Nantes, in the Centre François Viette, of a research team dedicated to historical studies in both astronomy and observatories; at last, the recent discovery in Nantes of the old observatory; this observatory, which was run by the Marine (Navy) and the City, closed in August 1887. In a sort of 'Tour de France' of observatories, this book contains a detailed and comprehensive presentation on eleven different observatories, although in a somewhat heterogeneous manner due to differing levels of available archives. For each of them, the connections between the local academic societies, hydrographic schools (when present) and town and/or public authorities are analysed since in most cases one or more of these institutions would support and fund the observatory, either in turn or simultaneously. The authors, who are astronomers and/or historians, report about the different observatories, and their successes and failures.

Laetitia Maison explores the *raison d'être* of the Bordeaux Observatory (which was created in 1878 and succeeded an earlier observatory established in 1772 by the Academy of sciences), and its activities during the early years (1879-1906). She analyses how the three objectives of the Observatory (astrophysical research using spectroscopy, the measurement of double stars, and the determination of stellar parallaxes), the economic utility of the Observatory, meteorological measures and predictions, the establishment and diffusion of a time service, the verification of Naval clocks and teaching activities (both in the University and in the City) were defined and how these objectives were fulfilled. All this occurred in spite of the interest of Georges Rayet—one of the two discoverers of the Wolf-Rayet stars—and fundamental research activity was limited to mathematical applications to celestial mechanics and astrometric observations. In fact, most time and energy was devoted to the Carte du Ciel project as Bordeaux Observatory was one of the participating observatories.

Guy Boistel examines Marseilles' Jesuit Observatory during Father Pézenas' era. Boistel first provides a detailed description of the different source materials used in his study. The many instruments available in the Jesuit Observatory (1750-1763), namely the clocks, mural quadrants and parabolic telescopes, are analysed, as are the scientific objectives and the relations between Father Pézenas and contemporary scientific communities. The presence of foreign Jesuits in Marseilles Observatory is emphasized by the author. James Caplan reviews the development of astronomy in Marseilles from 'prehistory' through to the founding of the Marseilles Observatory: Pythéas' time, the Middle Ages with Raymond de Marseilles, Guillaume l'Anglais (or de Marseilles?), the Thibbon family, then the foundation of the Observatory in 1685 when Chazelles came to Marseilles to educate sailors in hydrographic matters. The institutional history of the Observatory up till the present day is reported in an appendix, where some significant events and leading personalities are commented on.

Suzanne Débarbat describes more than the founding in 1667 of what is now l'Observatoire de Paris, soon after the creation of the Académie Royale des Sciences. She writes about the motivations, the working programs of the academicians and their astronomical measures. We find a description of the Observatoire from 1784, when Cassini IV received funds to restore Claude Perrault's edifice, to 1795, when the Observatoire was put under the supervision of the Bureau des Longitudes. A second section in Débarbat's presentation provides an insight into the relations and connections between the Bureau des Longitudes and the French observatories, and thanks to the minutes of the Bureau's meetings she is able to report on the various observatories, and the 'musical chairs' that occurred as new instruments replaced ancient ones, and how this affected the astronomers.

Françoise Le Guet Tully provides information about a special case: the creation in the nineteenth century of an observatory in France, *ab nihilo*. Her chapter, "From the reorganisation of the Bureau des Longitudes in 1854 to the creation of the Observatory of Nice in 1879: 25 crucial years to the French astronomy", contains three parts: (1) How the discovery of a new planet can influence the career of an astronomer; (2) Arago and Le Verrier, both astronomers and political key figures, but on opposite sides; and (3) October 1853-January 1854, a critical four-month period when the Observatoire de Paris and the Bureau des Longitudes were reorganized. Le Guet Tully's second part is based on the Report to the Ministère de l'Instruction Publique made by the Committee in charge of the modernization of astronomy in France. On 30 January 1854, the Observatoire de Paris retrieved its self-governing status from the Bureau des Longitudes. Later, after Le Verrier's death, according to Le Guet Tully, the Bureau may have seen the new observatory in Nice as the 'ideal observatory', as defined in the above-mentioned report.

Philippe Véron reports on the 'prehistory' of the Observatoire de Haute-Provence. He first paints a rather bleak picture of French astronomy from the time of the Revolution to the year 1920, with an evident lack of any modern observatory where stellar astronomy or astrophysics might develop. Thanks to internal reports of the Académie des Sciences and correspondence between key figures such as Danjon, Couder and Ferrié, the author details the track leading to the creation of the Observatoire de Haute-Provence. And it was not a quiet one, given the various conflicts between the different actors (Ritchey, Dina, Esclangon, Danjon and Couder) and the different institutions relating to the best site and the best instruments. In the end on 31 October 1936, and under a Front Populaire Government, a Service de Recherche d'Astrophysique was created, with a theoretical section in Paris and an observatory in the village of Saint-Michel (Basses-Alpes) in the south of France. This establishment with two sites depended on a new institutional structure (CNRS). Véron ends this story by questioning the future of the Observatoire de Haute-Provence: astronomers may be fond of large telescopes, but he points out that the first extra-solar planet was discovered in 1995 with the help of observations made with the comparatively-modest instruments at the Observatoire de Haute-Provence!

The tour of French observatories is not over! In his chapter, Jérome Lamy discusses the Observatoire de Toulouse from 1733 to 1908, emphasizing connections between knowledge and power. As in Paris, a Société des Sciences was created, but only in 1729, and an Académie des Sciences, Inscriptions et Belles-lettres, in 1746, and an astronomical observatory was given to the Académie by the City authorities as early as 1733. Garipuy, and then Darquier, were the main astronomers, participating fully in the academic and astronomical life of the eighteenth century. After the Revolution, in spite of support from the Bureau des Longitudes, scientific activity decreased, and a revival of astronomical work only occurred in 1839 when Frédéric Petit became Director. With help from the Bureau des Longitudes, and following the establishment of a new building in Jolimont that was more suitable for astronomical observations, the Observatoire de Toulouse was ready to perform a high-quality research. C. Delaunay, F. Tisserand and B. Baillaud were successive Directors of the Observatory, with each of them leaving some scientific legacy. From 1891, the main concern was the Carte du Ciel project. Lamy emphasizes that throughout its history the Observatory had three funding sources: initially the Académie (of Toulouse) and then the Capitole (City authority) and the République (either in turn or simultaneously).

Jean-Michel Faidit reports on the Observatoire de Montpellier, which started with the creation of the local Royal Society of Science in 1706. The first astronomical observation, of a 1706 solar eclipse, was reported from the Babotte Tower, the astronomical purpose of which was thereby initiated. The golden age of the Babotte Observatory ended around 1770 thanks to neighbouring conflicts, and there was then some rivalry between Toulouse and Montpellier about the formation of a provincial observatory. The revolution and subsequently coordinating activity of the Bureau des Longitudes did not favour Montpellier, and in 1810 the Babotte Observatory was assigned to the new Montpellier Faculty of Sciences. Later, astronomical observations were carried out in the observatory of the Porte du Peyrou created by Benjamin Valz in 1835, then, from 1837, at the top of the Cathedral Saint-Pierre tower. In 1862, Le Verrier succeeded in creating a modern new observatory in Montpellier, the main instrument being a reflector with a 80cm mirror made by Foucault. Faidhit's presentation ends with two appendices relating to the Babotte and Jardin des Plantes Observatories, and for both of them lists of observers and available instruments are listed.

Olivier Sauzereau writes about the different observatories in Nantes, the city at the mouth of the Loire River. Local archives reveal the early existence of astronomical activity and associated buildings in this important commercial town. To provide a good theoretical knowledge of navigation, a hydrographic school was created in 1672, the teaching being in the hands of the Jesuits. The lessons and some of the observations were carried out at the Hotel de Briord. The 1761 transit of Venus was an important event locally. Later, other observational facilities were used: the tower at the cathedral and the tower of the Maison-Graslin (which was at the highest point in the town). After a reorganization of the hydrographic schools was decreed in 1825 by Charles X, the City and the Navy collaborated and created a new Hydrographic School and associated observatory in Nantes. The 1860s proved to be the golden age of this Hydrographic School. The population then became quite interested in astronomy, thanks largely to newspaper articles, public observing nights and the creation in 1884 of the Société Astronomique de Nantes with Camille Flammarion as founding President. But in 1887, the School and the associated observatory were closed.

Two contributions concern the diffusion of astronomy in the public. Colette Le Lay presents a picture of the diffusion of astronomy, and the important contributions of Lalande (1732–1807) and Arago (1786–1853) are emphasized; for instance, Arago introduced soirées at the Observatoire de Paris and public visits. Everything changed drastically with Arago's death, when Le Verrier terminated Arago's initiatives and the popularization of astronomy moved to the hands of writers (except for Camille Flammarion, who worked for a time with Le Verrier). Le Lay examines how the public at large viewed the observatories and their professional astronomers. Danielle Fauque writes a contribution on "Observatoires Astronomiques Français et Diffusion de l'Astronomie à l'Association Française pour l'Avancement des Sciences (1872-1914) (AFAS)." First the AFAS is presented. Leading French scientists created this group after the 1870 defeat in order to promote French science, and it organized annual colloquia (with associated publications) and provided funding for scientific projects (including astronomical ones). Using the proceedings of these colloquia, Fauque analyses the relative importance of the observatories and the communications dedicated to astronomy (or presented by astronomers). On the basis of their outstanding contributions, she identifies two key astronomers, Émile Marchand, who was the first Director of the newly-created Pic du Midi Observatory, and Jules Janssen, who widely contributed on techniques and results related to astrophysics (and not just those relating to solar astronomy—which was his own main research interest).

To conclude: it is often claimed that in France everything derives from Paris, but this book shows that local contributions also have to be considered. The complex connections between institutions and actors are reported and analysed from different points of view; for instance, the actions of the Bureau des Longitudes may be seen quite differently according to the observer's situation. It would be naïve to suppose that the involvement of leading astronomers provided for the overall success of any one project. Thanks to the archives and ways in which they have been used by the different authors, readers are immersed in national and local history that has links to astronomy and to the development of various French observatories. This invaluable book also contains information about many of the astronomical instruments available during this period. Finally, the sources of the various archives, minutes of meetings and reports used in researching the chapters of this book are clearly indicated.

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