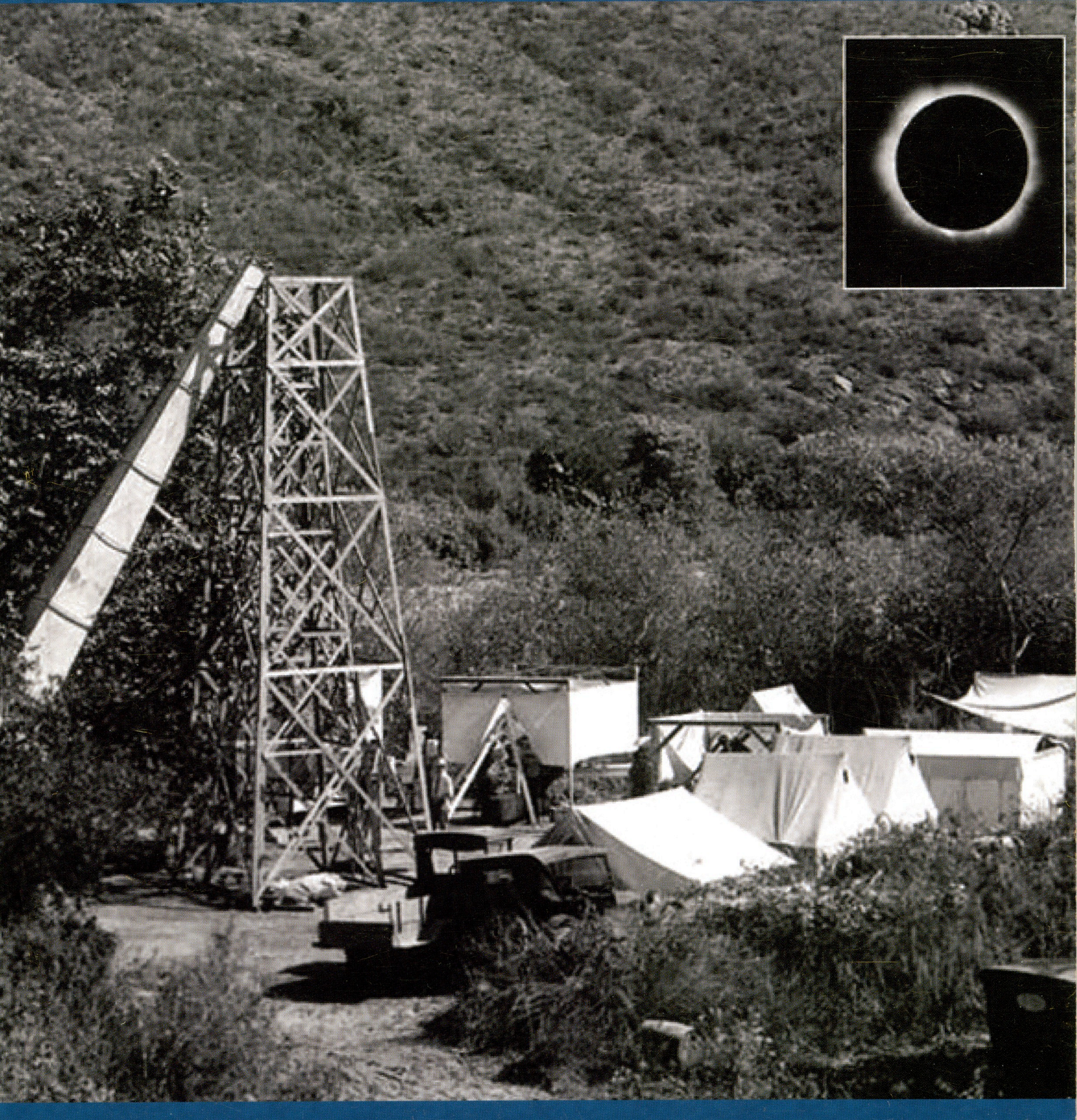
JOURNAL OF ASTRONOMICAL HISTORY AND HERITAGE

The Donald E. Osterbrock Memorial Symposium Issue



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

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

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COVER PHOTOGRAPH

This photograph shows the eclipse camp that was set up by the Lick Observatory at Ensenada, Mexico, for the 10 September 1923 total solar eclipse. The elaborate tower to the left of the 'tent city' supported the 40-ft Schaeberle Camera, which was used to take large-scale images of the solar corona and prominences. Meanwhile, the inset shows a photograph of the 16 April 1893 eclipse taken with this camera. A paper about the Schaeberle Camera appears on page 25 in this issue of the journal, and for information about the Plate Vault, which houses the original Schaeberle Camera plates (and other photographs taken over the years with various Lick Observatory telescopes and cameras), see page 22. Original versions of these two papers, and the lead paper on W.W. Morgan's discovery of the spiral arm structure of our Galaxy, were presented at the Donald E. Osterbrock Memorial Symposium that was held at the University of California, Santa Cruz, on 2-3 August 2007. Don Osterbrock was a founding Editorial Board Member of this journal and we are pleased to dedicate this issue to him.

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W.W. MORGAN AND THE DISCOVERY OF THE SPIRAL ARM STRUCTURE OF OUR GALAXY

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In memoriam—Donald Edward Osterbrock (1924-2007)

Abstract: William Wilson Morgan was one of the great astronomers of the twentieth century. He considered himself a morphologist, and was preoccupied throughout his career with matters of classification. Though his early life was difficult, and his pursuit of astronomy as a career was opposed by his father, he took a position at Yerkes Observatory in 1926 and remained there for the rest of his working life. Thematically, his work was also a unified whole. Beginning with spectroscopic studies under Otto Struve at Yerkes Observatory, by the late 1930s he concentrated particularly on the young O and B stars. His work on stellar classification led to the Morgan-Keenan-Kellman [MKK] system of classification of stars, and later—as he grappled with the question of the intrinsic color and brightness of stars at great distances—to the Johnson-Morgan UBV system for measuring stellar colors. Eventually these concerns with classification and method led to his greatest single achievement—the recognition of the nearby spiral arms of our Galaxy by tracing the OB associations and HII regions that outline them. After years of intensive work on the problem of galactic structure, the discovery came in a blinding flash of Archimedean insight as he walked under the night sky between his office and his house in the autumn of 1951. His optical discovery of the spiral arms preceded the radio-mapping of the spiral arms by more than a year. Morgan suffered a nervous breakdown soon after he announced his discovery, however, and so was prevented from publishing a complete account of his work. As a result of that, and the announcement soon afterward of the first radio maps of the spiral arms, the uniqueness of his achievement was not fully appreciated at the time.

Keywords: W.W. Morgan, spiral arms of our Galaxy, radio astronomy, Jan Oort, Yerkes Observatory, Edwin Frost, Otto Struve, stellar classification, MKK system, OB stars, OB associations, Walter Baade, Andromeda Nebula, HII regions, Jason Nassau, Stewart Sharpless, Donald Osterbrock, Local Arm, Perseus Arm, pattern-recognition, the nature of scientific discovery.

> “The things for this year are a deeper view and understanding of the character and depth of the mind and the completion of the first stage of the work on the evolution of galaxies. There is room for a more ordered picture in both areas; how beautiful are the vistas in each region—how beautiful and deep they are! And how similar are the aesthetic consideration and world-laws in both! The work of art; the world of the galaxies; the world of the mind; all – ALL – in the fundamental world of forms.” (W.W. Morgan, New Year’s resolutions, 1957).

1 INTRODUCTION

It is now common knowledge that our Galaxy is a vast spiral star system which we view edgewise from within one of the spiral arms. The first clear demonstration of the fact, however, by Yerkes Observatory astronomer William Wilson Morgan (Figure 1), occurred only in 1951. This was one of the grandest discoveries in the history of astronomy, and when Morgan presented it, in a fifteen minute talk at the American Astronomical Society meeting in Cleveland the day after Christmas 1951, he received a resounding ovation, that included not only clapping but stomping of feet.[1] But for various reasons—not least that Morgan suffered a nervous breakdown that led to hospitaliztion within months after the discovery—no definitive account of his discovery appeared at the time (but see Anonymous, 1952, and Morgan, Sharpless and Osterbrock, 1952: 3).

Morgan had used optical methods to detect the nearer spiral arms. When he left Billings Hospital at the University of Chicago, Morgan was determined to reconstitute himself and reorganize his psyche through a systematic program of self-help and psychoanalysis which he would document in a remarkable series of personal notebooks he kept for most of the rest of his life. By the time he returned to the Yerkes Observatory, Jan Oort and his Dutch and Australian collaborators had independently announced the discovery of the spiral-arm structure of our Galaxy on the basis of radio astronomical observations. At the time their results seemed more far-reaching, since whereas Morgan had only identified the nearby spiral arms, the radio astronomers were able to identify structures on the hidden far side of the Galaxy.

For a time the discoveries of Oort and his collaborators overshadowed Morgan’s work. Only later, in about 1970, was it realized that their distances were not as accurate as had been supposed because of large-scale systematic deviations from circular motion of the hydrogen gas clouds on which they had relied for their maps; thus, the radio maps turned out not to be very reliable, and the uniqueness of Morgan’s achievement began once more to be fully appreciated (see Burton, 1976: 279-281).

2 THE KAPTEYN UNIVERSE AND THE STRUCTURE OF OUR GALAXY

At the beginning of the twentieth century, the standard view of the Galaxy was that offered by the Dutch astronomer J.C. Kapteyn (Kruit and Brekel, 2000) who, much as William Herschel had done in 1785 in proposing his ‘grindstone model of the universe’, still regarded the Galaxy as a small disk of stars. Since Kapteyn ignored the effects of extinction of starlight by interstellar dust, his model, like Herschel’s, included only the nearer stars; as a result, his disk, measuring a mere 4,000 parsecs long by 1,000 parsecs wide, remained centered on or near the Solar System. It was this smallish star-system that George Ellery Hale set out to investigate—along lines defined by Kapteyn—with the 60-inch reflector at Mt. Wilson Observatory (which saw 'first light' in 1908). Kapteyn spent the summers between 1909 and 1914 as a Research Associate at Mt. Wilson, and was Hale's most influential adviser.[2]

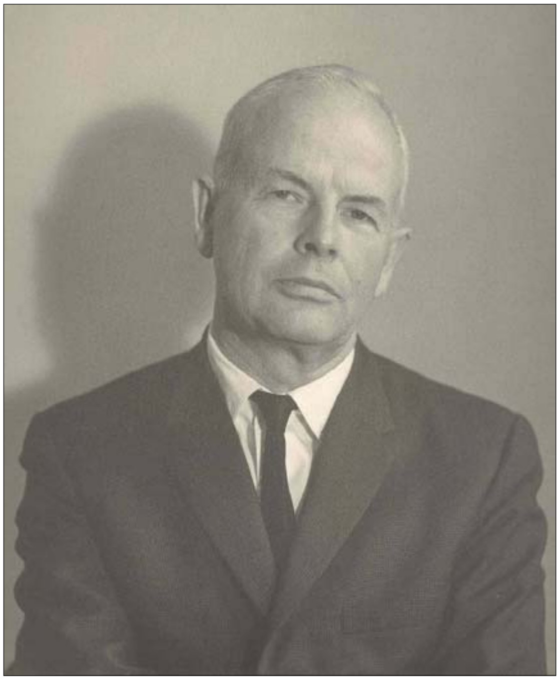


Figure 1: W.W. Morgan, at about the time he first documented the existence of spiral arms in our Galaxy (courtesy: Yerkes Observatory Archives).

Kapteyn's views about the structure and size of the Galaxy were eventually undermined by the work of Harlow Shapley, who also used the Mt. Wilson 60-inch reflector. In 1914 Shapley (then a 29-year-old Princeton Ph.D.) began making photometric measurements of the stars in globular and galactic clusters, little suspecting that this line of investigation would ultimately lead to unlocking the Sun's position in the Galaxy—to what he rather colorfully would call the 'galactocentric revolution'. He correctly surmised that extinction of starlight by interstellar dust was negligible in directions away from the Galactic Plane; however, his subsequent observations of stars in galactic clusters led him to erroneously extend that result to the Galactic Plane itself. And yet though he made mistakes, he was able, in 1917, to establish the main result. "In [his sixth paper]," wrote Allan Sandage (2004: 300), "… Shapley invented three powerful and (it turned out) highly reliable methods to determine cluster distances. It was a singular achievement." Armed with these tools and his 60-inch plates (as well as earlier plates taken by Solon Bailey at Harvard's Southern Station at Arequipa), Shapley succeeded in mapping the distribution of the globular clusters and showed that they form a framework located eccentrically to the Sun. Thus, Shapley deduced that the Sun was far removed from the center of the Galaxy and that the latter was much larger than Herschel, Kapteyn or anyone else had imagined. In the end, Shapley's model displaced the smaller, Sun-centered Kapteyn Universe.

Meanwhile, it was becoming clear, especially from the wide-angle Milky Way photographs taken by the American astronomer E.E. Barnard (Sheehan, 1995), that the dark regions in the Milky Way were not tubules or holes perforating a disk of stars, as earlier astronomers such as William and John Herschel had supposed, but dust clouds scattered along the Galactic Plane.

By this time many astronomers believed that the so-called 'spiral nebulae' were extragalactic star systems. Many had been discovered by the Herschels, while Lord Rosse and his assistants with the great reflector at Birr Castle, Ireland, had discerned a spiral structure in a number of them, most famously in the 'Whirlpool Nebula' in Canes Venatici, but also in eighty or so others. Faint and small spirals were later found by the thousands in deep plates taken by James Keeler with the 36-inch Crossley Reflector at Lick Observatory at the end of the nineteenth century, and appeared to be virtually numberless in the regions around the Galactic Poles (Osterbrock, 1984).

Although Keeler himself leaned toward the view that these spirals were planetary systems in formation, a later Lick astronomer, Heber D. Curtis, who studied the Crossley images more carefully, discerned a family resemblance in all the spiral nebulae; in other words, they appeared to form a class of similar objects that were distributed at different angles and at different distances. In each case where they were seen edge-on, dark rifts divided them, which Curtis recognized as similar to the dark dust clouds of the Milky Way that Barnard had photographed. By 1917, the same year Shapley published his paper on the globular clusters, Curtis was arguing that the spirals were star systems, or 'island universes', a result that seemed to receive confirmation with the discovery of novae in the spiral nebula NGC 6946 by G.W. Ritchey, again using the 60-inch reflector at Mount Wilson, followed by Curtis's discovery of additional novae in other spirals (Osterbrock, 2001a). Shapley himself remained unconvinced, and was misled by his overestimate of the size of the Galaxy into supposing that the spiral nebulae must be local, and also by his (and everyone else's) failure to grasp the sheer violence of the supernova explosion which had occurred in the Andromeda Nebula in 1885,[3] thus setting the stage for the famous Curtis-Shapley debate of 1920. The conclusive demonstration that the Andromeda Nebula was an extragalactic star-system finally came with Edwin Hubble's discovery of its Cepheid variables using the 100-inch Reflector on Mt. Wilson in 1923-1924; even Shapley accepted the implications at once.

Within a little more than a decade, Kapteyn's rather quaint model of the Galaxy had been completely discredited, largely owing to the pioneering work of the 60-inch telescope at Mt. Wilson, and our Galaxy became one of countless millions of 'star systems' strewn throughout the Universe. It might well be a majestic spiral in its own right (as suggested as early as 1900 by the Dutch amateur, Cornelis Easton), though it might also be a flattened elliptical. Determining its actual form proved to be one of the most daunting problems of twentieth-century astronomy.

3 TWENTIETH CENTURY STUDIES OF GALACTIC STRUCTURE

Kapteyn died in June 1922, but largely as a result of his influence Dutch astronomers continued to work on galactic structure. Among them were his student P.J. van Rhijn at the Kapteyn Institute in Groningen, Jan Oort (Figure 2) at Leiden, and Bart J. Bok (who studied under Oort at Leiden, received his doctorate from Groningen, and then did much of his work at Harvard).

The first to propose a rotating model of the Galaxy was the Swedish astronomer Bertil Lindblad. However, finding Lindblad's mathematical treatment impenetrable, Oort decided to devise his own approach to the problem. Realizing that there was much more interstellar extinction by dust than had been realized, Oort surmised that the best way of understanding galactic structure would be to study the motions of the stars and introduced the concept that because of galactic rotation there was a well-defined relationship between the radial velocities, distances and angles of stars in a rotating system. This meant that "… measured systematic radial velocities could be converted to approximate distances in a straightforward way." (Osterbrock, 2001b: 147). This was a very important idea that would underpin his later investigations into the structure of our Galaxy.

Unfortunately the Netherlands is one of the worst places imaginable for observational astronomy, and Oort did not have the telescopes to provide the kinds of data he needed. But he learned of the discovery of radio emission from the Galaxy by the American engineer, Grote Reber (Reber's first paper was published in 1940), and grasped the great potential of a new and powerful technique which would allow penetration of the interstellar dust clouds. He thus posed an important question to H.C. van de Hulst, a brilliant student in Utrecht who had written a noteworthy paper on interstellar dust: "Is there a spectral line at radio frequencies we should in principle be able to detect? If so, because at radio frequencies extincttion would be negligible, we should be able to derive the structure of the Galaxy. We might even be able to detect spiral arms, if they exist." (Katgert-Merkelijn, 1997).

After several months of study Van de Hulst found that there was indeed such a spectral line, the 21-cm line of the ground state of hydrogen (neutral hydrogen, HI). Given the vast abundance of hydrogen in the gas in the Galactic Plane, Oort at once realized that mapping of the interstellar atomic hydrogen would likely led to the discovery of our Galaxy's spiral arms. Other advantages of the radio technique lay in the fact that it gave very high velocity (frequency) resolution—which meant that the results could be immediately tested against Oort's rotation model for the Galaxy.

Unfortunately, this work was delayed by World War II, and afterwards there were further delays in getting the proper equipment. A particularly frustrating setback occurred when one of Oort's receivers was destroyed in a fire. Nevertheless, Oort persisted, and in collaboration with the radio engineer C.A. Muller he finally succeeded in detecting the 21-cm line with an antenna at Kootwijk, on 11 May 1951, just six weeks after the feat had been accomplished at Harvard by Edward M. Purcell and H.I. 'Doc' Ewen (for details see van Woerden and Strom, 2006). (In contrast to the financially-strapped Dutch, the Americans had been beneficiaries of a crash wartime radar research program.) Oort and his Dutch and Australian colleagues now began working on a systematic study of the structure of our Galaxy, including hitherto inaccessible regions on the far side of the Galactic Center. This seemed to make the mapping of the spiral arms by radio astronomers inevitable; indeed, the first such maps appeared within a year. But the radio astronomers were forestalled by the narrowest of margins, as it was optical astronomers who were the first to achieve this result.

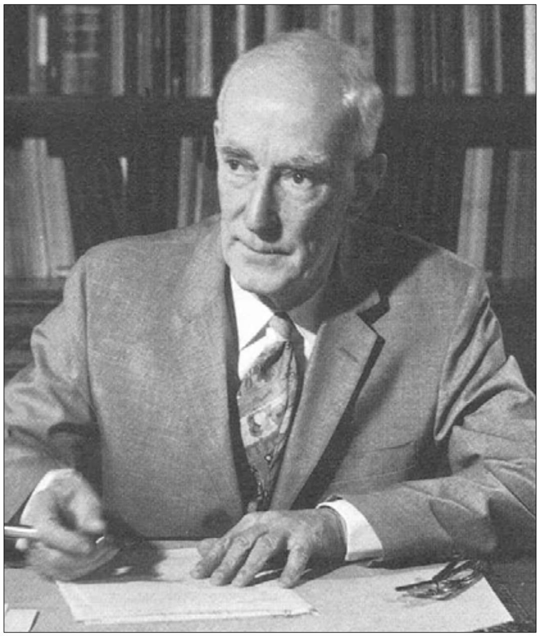


Figure 2: Jan Oort, 1900-1992 (after Oort, 1981: frontispiece).

For a long time, optical astronomers had been tackling the problem of galactic structure by means of brute-force star counts and methods of statistical analysis, such as those developed by Kapteyn. The basic idea was simple: as one counted stars, the number of stars would rise in the vicinity of a spiral arm and then drop off beyond it. No one applied these methods more diligently than Oort's former student Bok, but after ten years of hard slogging he had failed to find any expected stellar density concentrations that could be identified with spiral arms. By the late 1930s, he thought the task of tracing out the spiral structure of our Galaxy was almost hopeless: he later recalled that "In public lectures during that period I often said it was unlikely the problem would be solved in my lifetime." (cited in Croswell, 1995: 74).

Clearly a different approach was needed. By the middle of the twentieth century, optical astronomers had largely realized this and had regrouped around the idea of using the most luminous stars in their mapping efforts. The final breakthrough came when William Wilson Morgan, building on the brilliant work of Walter Baade at Mt. Wilson, put together a number of leads during the late 1940s and early 1950s and forged, from what others might have perceived as unrelated scraps, a technique that dramatically revealed the hitherto unglimpsed spiral arms of our Galaxy.

4 W.W. MORGAN: THE MAN[4]

Like two other important figures in galactic research, E.E. Barnard and Carl Seyfert, Morgan was a native of Tennessee. He was born on 3 January 1906 in the tiny hamlet of Bethesda, which no longer exists. His parents were William Thomas Morgan and Mary (née Wilson) Morgan, Southern Methodist Church home missionaries. During the first part of his professional career, Morgan always wrote his name as 'W.W. Morgan' rather than 'William Morgan', presumably in order to establish an identity that was independent from that of his father.

During W.W. Morgan's childhood, the Morgan family was constantly on the move in the South of the USA, and until the age of nine he was entirely home-schooled with his mother. A list of all the places he lived, recorded on a scrap of paper in the Yerkes Observatory Archives (Morgan, n.d.), shows that from Bethesda he moved to Crystal River, Florida, in December 1908; to Starke, Florida, in 1910, where he saw Comet 1P/Halley; to Punta Gorda in 1912; to Key West in December of that same year; to a farm 18 miles from Punta Gorda the following year; to Perry, Florida, in the latter part of 1914; to Colorado Springs, Colorado, in December 1915; to Poplar Bluff, Missouri, in October of the following year; to Spartanburg, South Carolina, in the summer of 1918; to Washington, D.C., the following summer; to Fredericktown, Missouri, in September 1919; and back to Washington in the spring of 1921.

The foregoing paragraph furnishes some insight into the basis of what became Morgan's obsession: a quest for permanence, the need to achieve a firm foothold and, above all, to find what the poet John Keats (1818: 302) once called "… certain points and resting places." In geographical terms, it would lead to his well-known clinging to Yerkes Observatory, where he lived and worked for more than sixty-eight years until his death in 1994. But perhaps just as significant was his need for conceptual fixed and secure resting places, which led to his attempt to develop a system of stellar classification that would be secure, and would not be overturned as a result of later revisions to the calibrations, as previous schemes of classification had been.

All his life, Morgan was haunted by his relationship with his domineering and unstable father, who seems to have been a man of great energy but who was also moody and given to dogmatic, intolerant views. He was possibly manic-depressive; there was no doubt he was sometimes emotionally and physically abusive toward his family, including Morgan. In later years, Morgan may have exaggerated the extent of the abuse; however, he did rather vividly recall being almost beaten to death at the age of two, only to be saved by the timely intervention of his mother. In an interview recorded in May 1993 (by which time he was suffering quite advanced Alzheimer's Disease) Morgan claimed that he was "… beaten up frequently …" by his father (see Croswell, 1995: 75).

As with others who have suffered from unhappy and abusive childhoods, Morgan found a refuge in the stars. In an interview he recalled:

> The stars gave me something that I felt I could stay alive with. The stars and the constellations were with me, in the sense that on walks in the evening, I was a part of a landscape which was the stars themselves. It helped me to survive. (Croswell, ibid.).

His father, William T. Morgan, seems to have started out as a fire-and-brimstone preacher who took a rigid and literal-minded interpretation of the Scriptures like that associated with the Scopes monkey-trial in Dayton, Tennessee. He interpreted the prohibition against working on the Sabbath literally, so that when young Morgan was in school he was forbidden to do any work on Sundays at all. As a result he was always falling behind:

> I remember late in high school, in Washington, D.C., I always dreaded Sunday night because I never was prepared for Monday. So it was a question of just survival. Just passing was all. And that's what it was like through these years. (Morgan, 1978).

Whereas William T. had become the same kind of man as his own father, a coal-miner from Warrior, Alabama, William W., the future astronomer, went about forming his personality-structure by what Freud called *reaction-formation*: the process of psychologically defining the self in opposition to, and outside of, the problematic person's perspective rather than by identifying with it. His father, who was awe-inspiring, powerful, capricious and terrifying, eventually found a career as an itinerant inspirational speaker and was absent for long periods of time. He wanted Morgan to follow in his footsteps, but Morgan wisely recognized that he did not have the same kind of personality as his father and that he would never be happy in such a role.

Morgan's first formal encounter with astronomy was "… as a refuge from an unhappy childhood, during the Influenza epidemic in the winter of 1918-19." (Morgan, 1987). His father had left for an extended period of time, and Morgan, with his mother and sister, moved to Fredrickstown, Missouri, where a Methodist junior college (Marvin College) and an attached high school were located in a cow pasture. Morgan entered high school there in the fall of 1919. He received his first astronomy book (a collection of star maps) from his Latin teacher, Alice Witherspoon, and she also arranged his first view through a telescope; it was of the Moon. Morgan (ibid.) later recalled the benevolent Miss Witherspoon's decisive influence on his development:

> In addition to the astronomical introductions, she presented the Latin language as a living thing, and helped me to realize that even a "dead" language could possess a vibrant, living form. My preoccupation with morphology (the science of form) probably began with this experience.

At the same time, Morgan discovered his father's set of the Harvard Classics—'Dr. Eliot's six-foot shelf of books', so-called because Harvard President, Charles W. Eliot, had selected them. One of these included the Elizabethan play *Doctor Faustus*, by Christopher Marlowe, and sixty-seven years later Morgan (ibid.) recalled the electrifying effect this had on him:

> The picture of the partially legendary Faustus, the man who longed to press outward toward the horizons of knowledge – and beyond to the stars – has been the ruling passion of [my] life.

Morgan finished his last two years of high school at Central High School in Washington D.C., then in the fall of 1923 he enrolled at Washington and Lee University in Lexington, Virginia. Although he was interested in astronomy, he had no idea at the time that this would become his profession. Instead, he decided to specialize in English, in preparation for a teaching career. However, he performed well in mathematics, physics and chemistry, and even talked his physics teacher, Benjamin Wooten, into acquiring a small astronomical telescope, so that he could observe sunspots.

In the summer of 1926, a year before Morgan was to finish his degree, Wooten made a summer trip to Yerkes Observatory. When he went up the stairs and rang the doorbell, the Director, Edwin Brant Frost, happened to be just inside. Wooten told Frost about the student who had pressed him to buy a telescope. It turned out that Frost was looking for an assistant to operate the Observatory's spectroheliograph and obtain daily images of the Sun, as the previous incumbent, Philip Fox, had just left to take a Chair at Northwestern University. Morgan was offered the job, but there were still difficulties; not least was the fact that Morgan's father was violently opposed, thinking that he would "… end up just in a laboratory working for somebody else, [and] that's nothing." (Morgan, 1978). That was the last time that Morgan talked to his father about anything; his father, who had been absent for long periods previously, now decided to abandon the family completely—Morgan never saw him again, and afterwards could not even find out what had happened to him or the year in which he died.

The rage and disappointment that Morgan felt towards his father would be reflected above all in one symbolic act. After his father left, Morgan appropriated his Harvard Classics, a coveted possession to anyone who valued literature and great ideas. Morgan savagely ripped his father's name plates out of all of the books bar one (where he missed the name plate because it had been accidentally affixed to the back rather than to the inside front cover of a book, like all the rest). One senses that with this act Morgan was symbolically attempting to tear his father out of his life.[5]

Though his father was gone, his image continued to cast a long shadow over Morgan's development. As a result of that problematical relationship, he always feared leaning on others and being 'devoured' by them. He was a sensitive, lonely introvert, who struggled with low self-esteem and feared being hurt by others. He wrote, characteristically, in a note from 1943: "Everything – objects and people – are shadows enduring for an instant. No one can come inside where I am. Where I live nothing can touch me." (Morgan, 1943: 25 July). He added further comments on his problematical relationships with his colleagues in one of his personal notebooks:

> January 5, 1957. Oort has stood for a number of years as a partial father figure for me … Until a few years ago I was afraid of men in general; they seemed to be a superior race to me and to women – with whom I felt much more at ease than with men … In the years 1953-7 I have made real friends – almost for the first time in my life. Of course, I did have friends earlier; but there was a sort of unstable equilibrium in connection with them because of my dependency leanings …
>
> January 19, 1957. 1 A.M. in bed. I am shedding [Bengt] Strömgren for the same general reasons that Freud shed Fliess: because I have been dependent on him; and I plan not to be dependent in that way anymore. (Morgan, 1957a).

Under the circumstances, Morgan was extremely sensitive to criticism, had difficulty feeling accepted by his professional peers, and often felt like an outsider even in his own family. After his breakdown, he entered into the only completely open and entirely comfortable relationship he ever really had, with his 'Dear Book', as he called the personal notebooks he kept compulsively for thirty years; these became his indispensable and constant companion, a confidante which was physically present at all times. It is clear that he personified these little volumes. They were the 'person' who was interested in everything in which he was interested, a sounding board which could be trusted to listen but never talk back and provided him with understanding and acceptance without qualification.[6]

5 THE MOVE TO THE YERKES OBSERVATORY

Yerkes Observatory Director, Edwin Brant Frost, had been born with congenital myopia, a condition predisposing to retinal detachments, and he was legally blind by the time Morgan arrived at the Observatory. (He once told Morgan, probably quite seriously, that the immediate cause of his blindness was the strain of correcting the young Edwin Hubble's first scientific paper!).[7] He was a humane and well-rounded sort of person who held great lawn parties (see Figure 3), but scientifically was not very productive. Nevertheless, in that pre-pension era, he tenaciously hung on until retirement age, courageously making his way everyday to his office from Brantwood, his residence, by means of a guy-wire strung along the footpath through the woods.

At first, Morgan lived in the basement of the Observatory (not in the often-unheated and sometimes damp attic-area known as the 'Battleship' because of its porthole-like windows), but soon after his arrival he married Helen Barrett, the daughter of Yerkes astronomer Storrs Barrett, and the couple moved into their own house, 100 yards east of the Observatory (Figure 4); Morgan would remain there for the rest of his life.

In 1931, Frost retired, and he was replaced by Otto Struve, a Russian immigrant from a very distinguished family of astronomers (e.g. see Batten, 1988). Struve (Figure 5) was an imposing, hulking man whose eyes were not quite congruent and who had a gruff, bearish manner. He was incredibly hard-working, and in those early days was a tremendous inspiration and father-figure to Morgan. Struve once remarked that he had never looked at the spectrum of a star, any star, where he did not find something important to work on. The remark made a lasting impression on Morgan and helped set the direction of his career. Although Struve was an astrophysicist and was interested in using stellar spectra as a tool to understand what was going on physically in stars, Morgan was temperamentally drawn to problems of stellar classification. As early as 1935 he produced his first paper on the subject, "A descriptive study of the spectra of A Stars." (Morgan, 1935).

6 MORGAN, THE YOUNG SUPERGIANT SLAYER

According to Struve (1953: 282), it was a series of lectures given by Bok at Yerkes one year later that first inspired Morgan "… to improve the distances of the hotter stars and to investigate the structure of the Milky Way with the help of these distances." These hotter stars included stars of spectral type B and their even brighter, but much rarer cousins, the O stars. None of the closer stars are B stars, and there are only a few of them within a distance of 300 light years. But because these stars are so intrinsically bright, they "… dominate the naked-eye sky all out of proportion to their true population." (Kaler, 2002: 183). The B stars include such admirable specimens as Rigel, Achernar, Beta Centauri, Spica, Alpha and Beta Crucis, and Regulus.

The B stars are young hot stars, prominent in the ultraviolet (Figure 6). They are rapidly rotating stars, and in some of them the velocities of rotation can be as high as 200 km/sec, so they eject matter into equatorial rings that radiate emission lines, characteristic of the so-called Be emission stars (Figure 7). They were first grouped together in a spectral classification in the Henry Draper Catalog, which was developed from spectra examined at Harvard College Observatory by Wilhelmina Fleming, Antonia Maury and Annie Jump Cannon, under the supervision of Edward C. Pickering. The Henry Draper Catalog introduced the familiar categories OBAFGKM, and was a one-dimensional classification which, as was eventually proved at Mt. Wilson in 1908, was keyed to temperature. In the hotter stars—those of classes A, B, and O—the spectral type is determined largely by the strength of the hydrogen lines (Balmer lines) and the increasingly dominant presence of lines of singly or doubly ionized helium (in O stars, these include not only absorption but also emission lines).

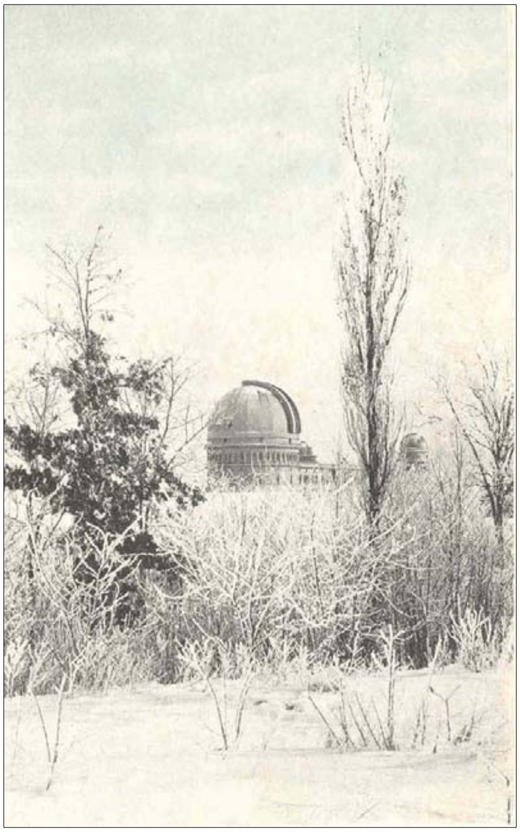


Figure 4: A picturesque postcard view of the Yerkes Observatory in the 1920s (courtesy: Yerkes Observatory Archives).

Figure 3: Yerkes Observatory garden party; Edwin Frost, with the cane, is in the foreground just left of centre, flanked by two women (courtesy: Yerkes Observatory Archives).

As early as 1897 Miss Maury had realized that there were distinctly different spectra for stars of a given temperature. In some stars of a given type—Miss Maury's c stars—the hydrogen lines were sharper, while in others they appeared broadened and more diffuse (showing 'wings'). Between 1905 and 1907, the stars in which the lines were sharp were shown by the Danish astronomer, Ejnar Hertsprung, to be much more luminous than the corresponding Main Sequence stars; in other words, they were supergiants. The stars whose lines are broadened into wings are those of the Main Sequence—dwarfs; the wings are produced by the effects of surface gravity and pressure.[8]

The Harvard catalog published by Annie Jump Cannon was based on the way the spectra appeared to her. When good high-resolution spectra of stars began to be obtained with the Mt. Wilson 60-in Telescope,[9] there was much more fine structure visible than had been the case in the Harvard spectrograms. Beginning in 1914, W.S. Adams and A. Kohlschütter at Mt. Wilson began to document in great detail the effects of luminosity on line strengths and line ratios in the spectra of these stars. It turns out that these effects are very sensitive to the precise physical conditions in stars and their atmospheres (as Cecelia Payne-Gaposchkin used to say, all stars, at high enough resolution, appear 'peculiar'). Naturally, the Mt. Wilson astronomers wanted to work out their own classification system in order to deal with all this additional level of detail they were finding, and they went on to develop the first two-dimensional classification system combining temperature and luminosity criteria. Of course, the classes they assigned differed markedly from those Annie Jump Cannon had assigned; inevitably, this produced some tension between the Harvard and Mt. Wilson groups.

Figure 5: Otto Struve posing outside the Yerkes Observatory (courtesy: Yerkes Observatory Archives).

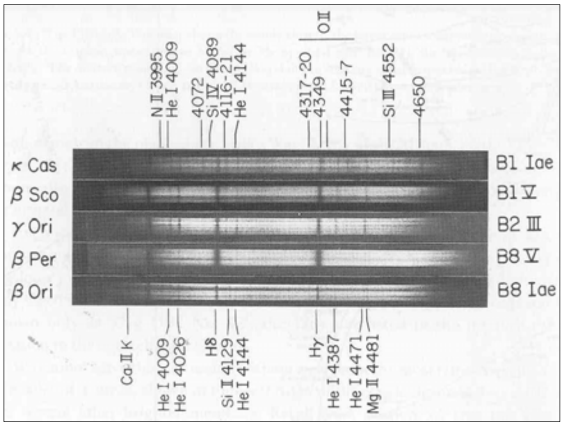


Figure 6: Spectra of five B stars in the MKK Atlas.

The O and B stars were especially problematic. They have weak spectral lines, and because of the great distances of these stars, the Harvard astronomers often had difficulty seeing them at all. Thus, Annie Jump Cannon had classified some heavily-reddened O and B stars as A or even as F. In 1936, when Morgan was beginning to work on spectral classification, he shifted his interest to these high-luminosity stars because they were precisely those where, as Donald Osterbrock notes,

> … the Harvard classification was so bad. Before Morgan, people were using spectral types out of the Henry Draper Catalogue that were not very good. If you take the spectral types as published in the HD and try to use them today, they're terrible. (cited in Croswell, 1995: 78).

The leaders in the classification of high-dispersion spectra of stars at this time were the groups at Mt. Wilson and the Dominion Astrophysical Observatory in Victoria (British Columbia), both of whom tried to relate spectral type to absolute values of luminosity. But when Morgan turned to their presumably more reliable classifications he found discrepancies, because the two groups had adopted different calibrations for their luminosities. It began to appear that the whole field of spectral classification might remain forever in a state of flux and confusion.

Morgan wanted to find a way around this. In the end, he decided to take "… the drastic step of abandoning the assignment of numerical values to absolute luminosities." (Sandage, 2004: 254). With his colleagues Philip Keenan and Edith Kellman (the latter providing clerical assistance), he began working on a new classification scheme. It was a huge undertaking, and was finally published in 1943 as the *Yerkes Atlas of Stellar Spectra, with an Outline of Stellar Classification*. It has became known as the MKK (and more recently as the MK) Atlas.

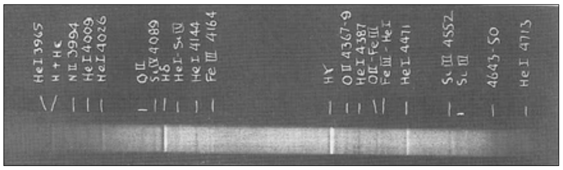


Figure 7: Spectrum of a Be star, taken from the MKK Atlas.

Instead of the continuous absolute magnitude numbers of the Mt. Wilson continuum, Morgan and Keenan sorted the Mount Wilson Main Sequence into discrete bins forming five luminosity classes: Ia and Ib supergiants, II bright giants, III giants, IV subgiants and V dwarfs (later adding VI, subdwarfs). The Mt. Wilson absolute magnitude numbers were based on measures of the intensities of specific spectral features. Inevitably, as new measures were made, the luminosity and hence classification of stars underwent constant recalibration and revision.

Morgan wanted his classification system to be secure—true for all time. In the MKK system, peculiar and exceptional stars were set aside. Instead the atlas emphasized 'ordinary stars', Main Sequence stars whose spectra were obtained using the same dispersion, depth of exposure on the photographic plate and method of development (which affects the contrast of bright and dark features). These stars were important statistically as they were the only ones that were suitable for large-scale studies of galactic structure. Morgan's strategy was to choose from among these ordinary stars a series of what he called 'specimens', standard stars defining what he later called a 'box' or reference frame; all other normal stars could then be classified by comparing them to these standard stars. C.R. O'Dell has described Morgan's method:

> The astrophysics was kept in the background. Morgan didn't directly try to relate the morphological spectral features to stellar temperature, luminosity, or gravitational effects at all. As far as the classification scheme was concerned, there was a sequence of boxes each having stars of a particular set of spectral signatures. The adjacent boxes held stars of similar but distinguishably different spectra. The astrophysicist could then come along and interpret these in terms of physical characteristics such as temperature, luminosity, and gravity. (Pers. comm., 31 March 2007).

Since only temperature (or color equivalent) and luminosity were needed to uniquely locate a star's spectrum, Morgan claimed that *by simple visual inspection* of a spectrogram these parameters could be determined and the star placed relative to the comparison stars. There was no need to measure anything. There would be no need—with new sets of measurements of the features of a spectrum, such as line width or intensity—to reshuffle the spectral classifications, since no quantitative value was put on any spectral feature. Morgan vigorously defended this qualitative approach. He admitted it was qualitative; but this did not mean it was indefinite or indeterminate. As he argued:

> The indefiniteness is … only apparent. The observer makes his classification from a variety of considerations – the relative intensity of certain pairs of lines, the extension of the wings of the hydrogen lines [Balmer lines], the intensity of a band – even a characteristic irregularity of a number of blended features in a certain spectral region. To make a quantitative measure of these diverse criteria is a difficult and unnecessary undertaking. In essence the process of classification is in recognizing similarities in the spectrogram being classified to certain standard spectra [those of standard stars]. (Morgan, Keenan and Kellmann, 1943: 4).

In a sense, spectral classification now became a true art-form and required, as Harvard historian of science, Peter Galison (1998: 340), points out,

> … the subjective, the trained eye, and an empirical art, an 'intellectual approach', the identification of 'patterns', the apperception of links 'at a glance', the extraction of a 'typical' sub-sequence with wider variations.

These were skills that defied simple or mechanistic algorithms; the judgments were far too complex for that. Though none of the features in the spectra that Morgan identified as the basis of his classifications is easily quantified—the line ratios (the relative intensities of lines in the spectra of different stars) are extremely variable when they differ appreciably from unity, and the appearance of spectra change greatly with resolution (Andrew T. Young, pers. comm., 14 January 2007)—Morgan insisted that the human eye-brain system (or at any rate *his* eye-brain system) is remarkably adept at just such pattern-recognition tasks. It excels in the discernment of similarities not unlike those involved in recognition of faces.[11] In his introduction to the MKK Atlas, Morgan concluded by specifically calling attention to the analogy between spectral classification and facial recognition tasks:

> It is not necessary to make cephalic measures to identify a human face with certainty or to establish the race to which it belongs; a careful inspection integrates all features in a manner difficult to analyze by measures. The observer himself is not always conscious of all the bases of his conclusion. The observer must use good judgment as to the definiteness with which the identification can be made from the features available; but good judgment is necessary in any case, whether the decision is made from the general appearance or from more objective measures. (Morgan, Keenan, and Kellman, 1943: 4).

This passage is vintage Morgan.[11] As a qualitative thinker in a field dominated by quantitative methods, Morgan could be savaged by insensitive colleagues who ridiculed his approach as 'celestial botany', but Morgan always considered himself as much an artist as a scientist. His method has proved the test of time. James Kaler (2002: 112), a leading expert on stellar classifications, has recently written:

> The standards become embedded in memory, and the typing of stars can proceed with impressive speed. There is a very important place for quantitative methods … Visual classification, however, is at present still useful in surveying in a reasonable amount of time the vast numbers of stars readily accessible to us.

One finds countless examples of Morgan's passion for visual pattern-recognition tasks in his publications, and especially in his personal notebooks. He had started to acquire art books in the 1930s and frequently commented on the works which captivated him. The following observations, written during the period when he was working on the Atlas, are typical:

> Sunset. May 19 [1942]. I want to be the man of the Rembrandt self portraits no. 40, 41, and 58. I want to look at women the way he looked at Hendrickje Stoffels and at the woman in no. 366. I want to look at the earth as Ma Yuan … and to feel like the sculptors of the Tang Bodhisattuas and to feel as does the head of Buddha. (Morgan, 1942; the numbers, above, refer to drawings in Dyke, 1927).

> Sunday afternoon. June 7 [1942]. Paraphrase of part of introduction of Cezanne book ... Elementary images can be created only by sacrificing the individual phenomena, the individual value of the human figure, the tree, the still-life subject. There is one characteristic of Cezanne's mode of representation which one may describe as aloofness from life, or better, as aloofness

> from mankind. In Cezanne's pictures the human figure often has an almost puppet-like rigidity, while the countenances show an emptiness of expression bordering almost on the mask. (Morgan, 1942).

Apart from the visual arts, Morgan's leisurely pursuits included putting puzzles together—he was famous for turning the colored sides of the pieces face down and assembling them from their shapes alone—and solving detective mysteries like G.K. Chesterton's Father Brown or Agatha Christie's Hercule Poriot stories. He was continually attentive to the patterns in the environment around Yerkes Observatory, which he documented in numerous photographs and paintings. The following passage, written at Walworth train station near Yerkes where he had gone to meet his daughter Emily (Tiki), is typical of countless passages. Awaiting her arrival he experienced—in a moment of Zen-like revelation—a world of profundities in the spare profiles of the telephone posts:

> Ah, another enchanted, cool-brilliant day; another communion; another sharpening of the senses, the vision, the physical response. How like delicate flower stems are these distant telephone posts. A progressive entrance into the world of reality – the World of the Self – during the past hour. Deeper and deeper, more and more removed from the ordinary. How far will it go? – how far can it go? There seems to be no limit in the Possibility – and no limit set by Time. (Morgan, 1963: 16 June).

7 ANOTHER CALIBRATION PROBLEM

In 1939, while using the 40-inch Yerkes refractor, Morgan realized that he could identify the different luminosity classes of B-type stars even from low-dispersion spectrograms. This was an important breakthrough since B-type supergiants, together with their brighter but rarer cousins, the O stars, are true stellar beacons, visible from relatively great distances. Morgan and others realized that these stars could, in principle, be used to map galactic structure provided one could calibrate the luminosities of the stars to their spectral types.

In principle, this is straightforward, but in practice difficult. The main problem is that, because there are so few of these stars—and none at all within a few hundred light-years of the Sun—they are all dimmed and reddened to some extent by interstellar dust, which is pervasive in the plane of the Galaxy. It exists as an omnipresent fog concentrated especially in the Galactic Plane in the dark clouds so well seen in Barnard's Milky Way photographs (as in Taurus, where the Pleiades illuminate some of the clouds at a distance of 400 light-years, and in Auriga and Perseus). Since the O and B stars, being young stars, are confined to the plane of the disk where the obscuration by dust is greatest, it turns out that even these luminous stars cannot be seen much beyond the nearest spiral features.[12]

It is possible to determine the amount of reddening of these stars even without knowing the detailed structure of the obscuring clouds. One must first calibrate the intrinsic color (the color of stars of a given spectral type independent of the effects of reddening). Then, since extinction of starlight by dust does not occur uniformly across the spectrum—it occurs about twice as efficiently at the blue end as at the red—by measuring the stellar magnitude in two different wavelength regions and taking the difference (i.e. the Color Index) one can, in principle, determine the degree of reddening of the star and so work out the effect of the dust.[13]

By correcting for the effects of extinction by interstellar dust and working out the distances of B stars in clusters, Morgan tried to calibrate the luminosity classes of his stars to their absolute magnitudes. His recognition, in 1939, that he could identify B stars even from low-dispersion spectra was important, because low-dispersion spectra could be obtained even for quite dim—thus far-away—stars. But he did not yet have any workable idea of how mapping these stars might lead to the discovery of the spiral arms. That recognition awaited developments from a totally unexpected line of research—Walter Baade's beautiful wartime work on the structure of the Andromeda Nebula.

Figure 8: Walter Baade (after Osterbrock, 2001b).

8 THE TWO STELLAR POPULATIONS

In 1944, Walter Baade (Figure 8) published his seminal work on the two stellar populations, which turned out to be young and old stars (see Osterbrock, 2001b). Baade had come to the United States in 1931 with the intention of applying for citizenship, but he had lost his paperwork and never followed up on this. During World War II, he was classified as an enemy alien, which precluded him from taking part in war work. His unintended reward was to be given free rein with the 100-inch reflector when the lights of Los Angeles and Hollywood were blacked out, and by using remarkably fastidious observational techniques he was able to obtain deep plates which resolved the faint red stars in the nucleus of the Andromeda Nebula and its elliptical companions, M32 and NGC 205.

Baade submitted a paper describing this work to the *Astrophysical Journal* in 1944, and Morgan—who was assisting Struve in editing the journal at the time—immediately recognized its importance. He also saw that Baade's plates would not reproduce well and, ever the artist, succeeded in talking Struve into allowing him to make actual prints from Baade's negatives. In a labor of love reminiscent of E.E. Barnard's fussing over every photographic print in his *Atlas of Selected Regions of the Milky Way*, Morgan personally produced and inspected prints of Baade's plates (Figure 9) and bound them into every individual copy of the *Astrophysical Journal* (which then enjoyed a circulation of between 600 and 800).

Baade's plates of the Andromeda Nebula showed clearly that in the spiral arms the hottest, most massive stars and open clusters were always associated with HII regions—diffuse nebulae of the Orion type, which had already, in 1939, been identified by Morgan's colleague, Bengt Strömgren, as regions of hot, ionized, interstellar hydrogen. The large complexes of nebulae and bright young O and B stars made up Baade's Population I. By contrast, the Galactic Nucleus and globular clusters were characterized by the fainter red giants of Population II. A crucial point, shown clearly in Baade's plates, was that concentrations of the O and B stars—the very same bright young hot stars that Morgan had been studying in our own Galaxy for several years—were the tell-tale markers that defined the spiral arms (e.g. see Figure 10). The reason they were concentrated in the arms was because they were young—necessarily so, since they were intrinsically bright, and would burn out before they had time to migrate very far from their place of formation.[14] This connection between stellar evolution and galactic structure was the essential clue that would ultimately produce the breakthrough leading to the recognition of our Galaxy's own spiral-arm structure.

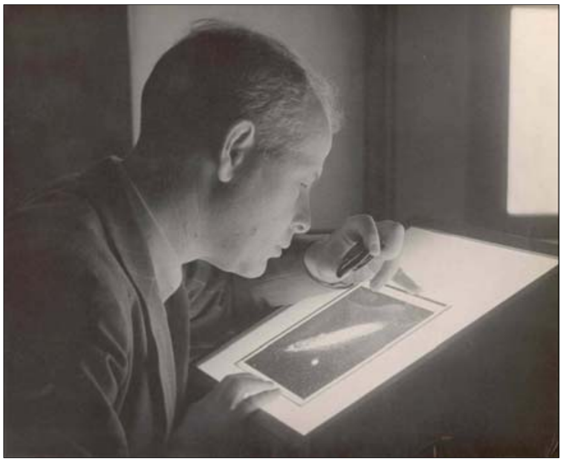


Figure 9: Morgan uses an ocular to inspect one of Baade's plates of the Andromeda Nebula (courtesy: Yerkes Observatory Archives).

As early as 1926, the two types of stars defined by Baade as Populations I and II had actually been recognized in our own Galaxy by Oort on the basis of their differing motions. Given their common interest in galactic structure, Baade and Oort began a vigorous correspondence shortly after the publication of Baade's 1944 paper. First Baade (1946) wrote to Oort on 23 September 1946:

> You mention in one of your remarks that the classical cepheids would be objects par excellence from which to determine the spiral structure. I think it is not certain yet that the longer period cepheids are especially concentrated in the spiral arms (they occur in the same regions in which the arms occur). But the B-stars of high luminosity are strongly concentrated in the spiral arms as my UV-exposures of the outer parts of M31 show most convincingly. I am therefore wondering, after reading Blaauw's fine paper about the Scorpius-Centaurus cluster, whether this extra-ordinary aggregation of B-stars is not in reality a short section of a spiral arm, the more so because in its orientation and motion it would fit perfectly into the expected picture (the arms trailing).

In the fall 1946 Oort gave a series of lectures at Yerkes Observatory which Morgan attended, and his pencil notes still exist, so that it is possible to follow Oort's reasoning on this important subject at just the time that Morgan was rapidly developing his own ideas on galactic structure.

Figure 10: Baade's photograph of Population I objects in the Andromeda Nebula (after Baade, 1944).

Oort focused on the high-luminosity B stars, although in his Yerkes lectures he observed that "… one of the great difficulties … was that one did not know the point of the color excess. This was a consequence of the fact that … there are not many B-type stars nearer than 100 [parsecs], and even those are slightly colored." (Oort, n.d.). Morgan had, of course, been working on this very problem. In early 1947, Oort responded to Baade's earlier letter:

> I quite agree that a study of the early B-type stars would be one of the most important steps for finding the spiral structure of the Galactic System. I have been discussing this subject with Van Rhijn [Kapteyn's successor at the Kapteyn Astronomical Laboratory at Groningen] for some time, and when Van Albada left Holland in order to pass a year at Cleveland we suggested to him that he should try to start a program with the Schmidt camera for finding faint B-type stars in the Milky Way ... This

> is a large programme, however, and I don't think the Warner and Swasey people are sufficiently interested yet to start it on a sufficiently big scale. How about future possibilities with the large Schmidt cameras on Mt. Palomar? (Oort, 1947).

Unbeknownst to Oort, Morgan had just teamed up with Jason Nassau from the Warner and Swasey Observatory on an ambitious survey to find B-type stars in our Galaxy. Morgan began to spend part of each year as a Visiting Professor of Astronomy in Cleveland, where he and Nassau identified the stars with the 24-inch Curtis-Schmidt camera at the Warner and Swasey Observatory. Later, Morgan used the 40-inch refractor at Yerkes Observatory to classify them rigorously by spectral type and luminosity. (This work would later be extended to more southerly regions by astronomers at Tonantzintla Observatory in Mexico.)

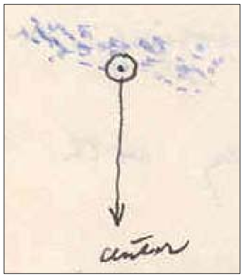
Morgan and Nassau's project was scarcely underway when, in December 1947, Baade spoke on the two stellar populations at an American Astronomical Society meeting at the Perkins Observatory in Ohio. By then it seemed increasingly likely that the spiral-arm structure of our Galaxy—if it existed—could best be mapped using the B stars, rather than by means of brute star-counts. Baade (1949a) later confided to Leo Goldberg that star-counts and statistical analysis had not led astronomers "… much beyond old William Herschel." Nassau and Morgan were of the same mind as everyone else, and fully expected that when they wound up their project of discovering the B stars they would have a good chance of working out the spiral-arm structure.

Nassau and Morgan were indeed finishing their project in the spring of 1949 when Morgan visited Pasadena and discussed with Baade the "… galactic survey for high-luminosity stars." (Morgan, 1949a). Shortly after their meeting, he wrote a long and important letter to Baade summarizing how far the project—and his thinking—had progressed by then:

> When I came out, I had a fairly definite idea of what the galactic spiral structure within a radius of 3000 pc of the Sun is like; after the description of your own work I found that many of my ideas were wrong.
>
> I regret very much that I did not take notes at the time of our discussion; as a consequence I do not remember the exact details of some important points …
>
> (1) Were the large new emission nebulae which you have recently found in the region of the dark rift projected against the galactic center? ... I can't remember the approximate galactic longitudes.
>
> (2) Have you also discovered similar nebulae [of] smaller dimensions in the anti-solar region? After thinking the matter over, it appears that the high luminosity stars which are observed within a fairly narrow range of true distance modulus[15] in the region of Cas[siopeia] and Per[seus] may well define a spiral arm located at a distance around 2-2.5 kpc. outside of the Sun. I have always been puzzled at the extent of the super giants surrounding the double cluster in Perseus; the concentration is probably explicable in terms of a spiral arm rather than as a physical cluster. In this respect, the region of Cepheus appears to be different in that high luminosity objects are observed over a greater range in the distance; this might be explained as a foreshortened effect for the outer spiral arm …
>
> (3) Could the nearby extended dark nebulosity in Ophiuchus and diametrical[ly] opposite in Perseus and Taurus be considered the tattered outer remnants of the general extinction stratum of the spiral arm immediately within the position of the Sun?
>
> It seems to me that within the next year it should be possible to reach a definite answer as to the location of the spiral condensations immediately within and without the position of the Sun. (Morgan, 1949b).

This letter has never been published, which is why I quote from it at length. It shows that Morgan now had the solution almost within his grasp. He had begun to pay attention to the distance moduli of the bright supergiant stars in Cassiopeia, Perseus, and Cepheus; moreover, he was already thinking of them as defining a spiral arm. He had also begun to pay attention to the distribution of the diffuse emission nebulae (HII regions). But though he was drawing close to the solution, it would be another two years before all those pieces, like the parts of a jigsaw puzzle or the characters and motives of a detective-novel, would finally and decisively fall into place.

Baade's response was delayed because he was then absorbed in trying to arrange the great sky survey using the 48-inch Schmidt telescope on Palomar Mountain (Edwin Hubble, who was originally charged with the program, had resigned owing to failing health). When Baade (1949b) did write, he agreed that Morgan was definitely on the right track:

> Your interpretation of the large number of supergiants surrounding the double cluster in Perseus would be in line with the findings in the Andromeda nebula. There supergiants of very high luminosity are always bundled up in large groups which stand out as prominent condensations in the spiral arms.
>
> The nearby extended dark nebulosities in Scorpius-Ophiuchus and Perseus-Taurus seem to be indeed manifestations of a single dark cloud ("streamer") which is tilted against the plane of the Milky Way and partly engulfs the solar neighborhood (both the Ophiuchus and the Taurus dark cloud are at a distance of only 100 parsecs).
>
> The distribution of the B stars which you first pointed out to me is like this:
>
> 
>
>
> which leaves no doubt that the Sun is either *in* or close to the *inner* edge of the nearest spiral arm … I still think that the B star program will be the first to lead to definite information about spiral structure in our neighborhood and that you will push it as far as you can.

In July 1950, a symposium on galactic structure, led by Baade, was held at the University of Michigan Observatory. Morgan and Nassau were both there, and reported on the progress of their survey of the high-luminosity stars. Within a galactic belt 10° wide, they had identified 900 O and B stars. For most of these stars, the distances had not been determined, but for 49 OB stars and 3 OB groups Morgan and Nassau had been able to estimate distances (see Figure 11).

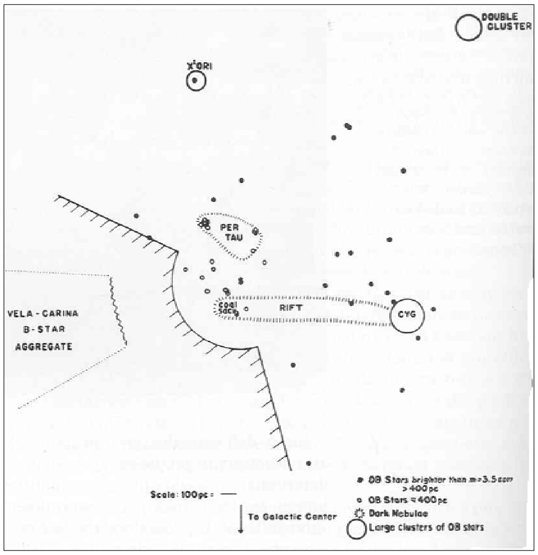


Figure 11: Plot showing the distribution of 49 OB stars and 3 OB groups with known distances (after Nassau and Morgan, 1951).

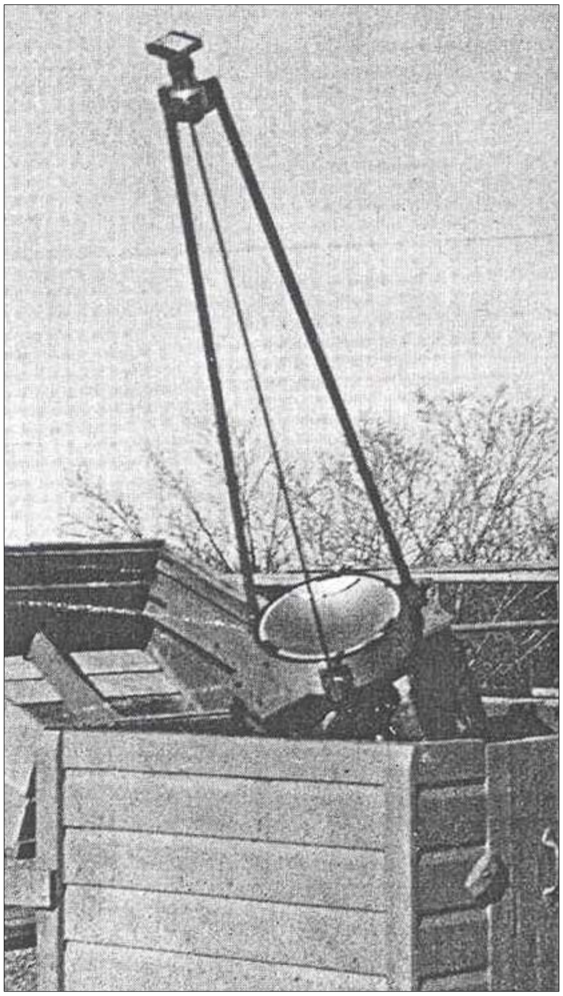


Figure 12: The Greenstein-Henyey Camera (courtesy Yerkes Observatory Archives).

According to a report on the symposium published in *Sky and Telescope* (1950), when Morgan and Nassau plotted these stars,

> Combining the results with already existing knowledge of many facts about the galaxy and other galaxies, these astronomers suggested that the sun is located near the outer border of a spiral arm. The arm extends roughly from the constellation Carina to Cygnus. The fact that many faint and hence distant OB stars are found toward Cygnus indicates that we are observing the stars in the extension of this arm beyond the clustering in that constellation, that is, beyond 3,000 light years.
>
> The part of the spiral arm near our sun contains a large cloud, or groups of small clouds, of interstellar dust and gas which obscures the distant stars and divides the Milky Way into two branches, easily visible to the naked eye. This obscuring cloud or rift is in the shape of a slightly bent cigar and is over 3,500 light years long. At one end of it is the southern Coalsack and at the other the brilliant group of OB stars of the Northern Cross … Dr. Nassau cautioned, however, that the evidence is insufficient to preclude the hypothesis that a great disorganization exists in the galaxy and that the star groupings do not trace definite spiral arms. (cf. Nassau and Morgan, 1951).

Nassau—and even Baade—had fully expected that a plot of B stars would furnish detailed information about our Galaxy's spiral structure, but they were deeply disappointed when nothing definite showed up from their plot, other than the well-known 'Gould belt', which represented the ring of bright hot stars close to the Sun that was originally mapped in the nineteenth century by the American astronomer, Benjamin Apthorp Gould.

9 EUREKA!

With the failure of this frontal assault on the spiral-arm structure, Morgan quickly regrouped. He now unfolded a grander strategy, which he hinted at in another paper presented at the same meeting. Innocently named "Application of the principle of natural groups to the classification of stellar spectra" (Morgan, 1951), its significance, indeed profundity, could hardly have been very apparent to anyone at the meeting. It was in fact as cryptic as the anagrams that earlier astronomers used to establish priority for their discoveries (yet at the same time effectively concealing them). In this paper, Morgan used the expression 'OB stars' to designate a category consisting of the O supergiant and early (young) B stars which formed what he called 'a natural group'. He noted that there was not much spread among the luminosity classes in type O, and even the early B stars showed only modest variations in luminosity. As he later explained later (Morgan, 1978), the significance was that it ought to be possible, "… by just a glance, [by looking] just a few seconds at each spectrum … to tell if a star was located …" in this rather narrowly-defined area of the Hertzsprung-Russell Diagram. Morgan (ibid.) felt that "… this was the crucial conceptual development." The stars in this region varied by only 1.5 or 2 magnitudes on either side of the means, which were around visual magnitudes –5 or –6.

Morgan was groping toward the concept of 'OB star associations' (although the term itself was not introduced until later by the Armenian astronomer, Victor Ambartsumian). The O and early B stars are found in loose aggregations typically of a few dozen stars (the majority of type B), which might be spread over a volume as small as an ordinary cluster or as much as a few hundred parsecs across. With a fair-sized group even of moderately discordant values of the luminosities, Morgan could pick the mean (around –5 or –6) and end up with a fairly reliable value for the group as a whole. Proceeding in this manner, he obtained good plots of their positions along the Galactic Plane, and this allowed him to reach out much further than Nassau had been able to do. Equally important, in Morgan's view, was the project he had intimated to Baade a year earlier: the identification of ionized HII regions, like the California Nebula close to Xi Persei, the so-called Barnard Loop in Orion, and the Rosette Nebula in Monoceros. They were, he recognized, completely analogous to the HII tracers of the spiral arms which Baade in 1944 had identified in the Andromeda Nebula. Inspired by Baade's photographs, Morgan combined his plots of OB associations and the HII regions of the Milky Way in a newly-energized and more focused attempt to trace the spiral arms.

At Yerkes there was at the time a wide-angle camera with a field of 140 degrees (see Figure 12). It had originally been developed during World War II by staff astronomers Jesse L. Greenstein and Louis G. Henyey for use as a projection system to train aerial gunners. However, it could equally well be used the other way around, as a camera, and under Morgan's direction two graduate students, Donald Osterbrock and Stewart Sharpless, began using it to photograph the Milky Way with narrow-band Hα filters (which had become available only after the War) in search of HII regions (Figure 13). Many of the HII regions were already well known; however, some were new, and because of the extraordinarily wide field of the photographs they were strikingly represented as the important extended objects they are (Morgan et al., 1952).

Sharpless had been one of Morgan's students, but Osterbrock's thesis adviser was Subrahmanyan Chandrasekhar, the master-theoretician whose approach was in many ways diametrically opposite to Morgan's. The theoretical astrophysicist Dimitri Mihalas, who later had an office across from Morgan and was befriended by him, has noted (pers. comm., November 2002) that Morgan and Chandra were like two mountain peaks —one was an observer and a pure morphologist, the other a mathematician and a master of theoretical deduction. Everyone else fell somewhere in the chasm between them. Young Don Osterbrock (Figure 14), through remarkable interpersonal tact and the astonishing versatility he later exhibited during a long and distinguished career as a research astronomer, administrator and historian of astronomy, was one of the few who managed to bridge that chasm.

The spiral structure of our Galaxy, if it existed, had proved to be much more difficult to recognize than anyone had ever imagined. It is in the nature of such things that it all seems perfectly clear in retrospect. In order to grasp just what was involved in making this discovery one must try to take oneself back in time. Morgan (1978) later recounted to David DeVorkin:

> One was looking at how these [OB stars] were and so on. Remember, there was nothing whatever known about the arms before. You have to remember this, because one goes back and thinks, well, you knew there was a tilt there … there were certain things at certain distances.

Although "Chance favors the prepared mind", as Louis Pasteur used to say, Morgan was hardly a 'sleepwalker', in Arthur Koestler's sense; he had been engaged in purposeful, goal-directed activity, following his hunch that plots of these highly luminous stars and the HII regions would finally led to the identification of the spiral arms. He had immersed himself in the problem for many years. But the discovery, when it came, came not as the result of the logico-deductive process; instead, he always insisted that it came in a flash—in a sudden dramatic moment of pattern-recognition.

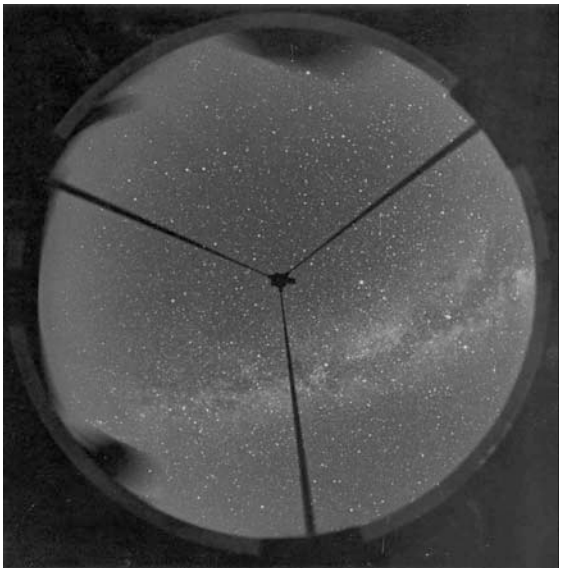


Figure 13: Representative image taken with the Greenstein-Henyey Camera (courtesy: Yerkes Observatory Archives).

Figure 14: Don Osterbrock, during the War years, before entering graduate school (courtesy: Irene Osterbrock).

More than most astronomers, Morgan was receptive to the idea that the unconscious mind plays an important role in the discovery process. His personal notebooks are filled with reflections on psychoanalysis, and a number of passages allude specifically to the way he experienced the discovery of the spiral-arm structure in the fall of 1951 during what he described as his "… most creatively productive period … the two years centered on my 1952 breakdown." (Morgan, 1956: 9 December). Later, in this same notebook he wrote:

> December 29, 1956.
> Dear Book, what a strange thing the unbridled mind is. A sequence of thoughts can develop – move rapidly from stage to stage, and end in a conclusion (a definite, unique conclusion) in a few eye-closings. And what is the "unique conclusion" worth? Perhaps absolutely nothing. Conclusion may not result from premise; there may be spaces – infinities wide – between successive steps.

Morgan's most complete account of what happened that fall evening is given in an August 1978 American Institute for Physics interview with David DeVorkin, which is a singularly-valuable document (along with Morgan's various personal notebook entries on this subject) about the mysterious workings of a creative mind:

> This was in the fall of 1951 [he says elsewhere in the same interview that it was in October], and I was walking between the observatory and home, which is only 100 yards away [see Figure 15]. I was looking up in the sky … just looking up in the region of the Double Cluster [in Perseus], and I realized I had been getting distance moduli corrected the best way I could with the colors that were available, for numbers of stars in the general region … Anyway, I was walking. I was looking up at the sky, and it suddenly occurred to me that the double cluster in Perseus, and then a number of stars in Cassiopeia, these are not the bright stars but the distant stars, and even Cepheus, that along there I was getting distance moduli, of between 11 and 12, corrected distance moduli. Well, 11.5 is two kiloparsecs … and so, I couldn't wait to get over here and really plot them up. It looked like they were at the same distance … It looked like a concentration … And so, as soon as I began plotting this out, the first thing that showed up was that there was a concentration, a long narrow concentration of young stars … There are HII regions along there too … And that was the thing that broke [the problem] down. (Morgan, 1978).

This first spiral arm—the Perseus Arm—was traced between galactic longitudes 70 and 140 degrees (according to the system of galactic coordinates in use at the time).[16] As he plotted the OB stars, Morgan found out that in addition to this arm there was another, the Orion Arm, extending from Cygnus through Cepheus and Cassiopeia's chair past Perseus and Orion to Monoceros, i.e., between galactic longitudes 20° and 180 or 190°. The so-called Great Rift of the Milky Way marked a part of the inner dark lane of this arm; the Sun lay not quite at the inner edge but 100 or 200 light years inside it. It was the Sun's proximity to—indeed near-immersion in—this arm that had made its existence so difficult to identify.

There is no reason to doubt Morgan's account of that autumn night at Yerkes. As he walked from the Observatory to his home (and apparently right back again in order to do his plot), he experienced a 'revelation-flash', a moment of sudden pattern-recognition. As so often happens with those who have experienced a 'Eureka!' or 'aha!' experience (an insight-based solution to a seemingly unbeatable vexing problem), Morgan (1978) saw it as something ineffable, something that was impossible to define in words; it seemed to him to be an inspiration breaking through from the subconscious mind:

> The main thing that's of interest to me about this is that there was no syllogistic operation – given this, then this, and then this, and all that sort of thing. Nothing whatever. It was a flash. And this is the way things come, in flashes – everything I've ever been concerned with in discovery, has been a question of flashes. That doesn't mean one develops them. One had better get them down somewhere, if it's the middle of the night, or they're dead the next morning. You don't know that you have them.

Figure 15: The Morgans' house on the grounds of the Yerkes Observatory (courtesy: Yerkes Observatory Archives).

Morgan, of course, had long demonstrated an unusual aptitude for pattern-recognition tasks, and had even based his MKK atlas of spectra on the human brain's marked ability to distinguish patterns. He himself had been born left-handed, but forced to learn to write with his right-hand (Mihalas, pers. comm., November 2002). He was, among astronomers, exceptionally artistic and highly creative. Neuropsychologically, he seems to have been either mixed-dominant or right-hemisphere dominant. Recent research on the psychology of insight by psychologists Edward Bowden of Northwestern University, Mark Jung-Beeman of Drexel University and their colleagues suggests that, although all thinking involves complementary right and left hemisphere processes, "… right hemisphere processing plays an important role in creative thinking generally and in insight specifically." (Bowden et al., 2005: 325).

It is certainly remarkable but entirely consistent with the literature on psychology of insight that even though Morgan had worked for years on the problem of the spiral-arm structure of our Galaxy (pursuing it through a systematic investigation involving a clear plan, implemented with meticulous attention to detail that began with identifying in low-dispersion spectra the distant O and B stars, continued through the correction of the effects of interstellar reddening, and culminated in his working out the luminosities, plotting the associations, and reinforcing the outlines of they defined with his map of the HII regions), that in the end the solution came to him in a flash, in a virtual eye-blink as the long-elusive embedded figure emerged, "… the flash inspiration of the spiral arms … a creative intuitional burst." (Morgan, 1956). As in the case of others who have experienced such insight-based solutions, Morgan "… experienced the solution as sudden and obviously correct (the Aha!) … [and] could not report the processing that had enabled him to reach the solution." (cited in Bowden et al., 2005: 323). As an artist, he was gratified that the resolution of his perplexity emerged from an inscrutible subconscious source.

Morgan's discovery was incarnated in a model in which old sponge rubber was used to depict the OB groups that he had identified (Figure 16). Later he added some concentrations of early B stars from the southern hemisphere (stars classified by Annie Jump Cannon as BO stars, those with hydrogen lines weak in the spectra that turned out to be a close approximation to Morgan's OB stars).

This more detailed scale model, constructed using balls of cotton, he presented in a slide at the American Astronomical Society in Cleveland, the day after Christmas 1951 (see Figures 17 and 18)—the meeting at which he received the ovation. The seats in the auditorium are located in banked rows that ascend from the stage, and the audience not only clapped their hands but they rose to their feet and started stomping on the wooden floor—in that acoustical space the effect was thunderous (David DeVorkin, pers. comm., July 2007). Since Oort, after introducing Morgan, had taken his seat, Morgan had nowhere to sit.

Morgan had finally received what he had always craved, the recognition of his peers. But within months he suffered a mental collapse; it was a "… complete personal crisis." (Morgan, 1978). During the spring after he discovered the spiral arms, he became depressed and unable to work, and his condition deteriorated to the point where he had to be hospitalized that summer. By the time he could return to work, the radio astronomers were rushing in and claiming enthusiastically that they had taken the mapping of the Galaxy so much further.

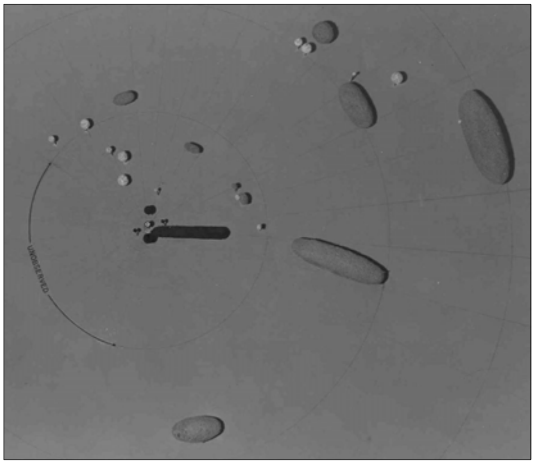


Figure 16: Morgan's sponge-rubber model (courtesy: Yerkes Observatory Archives).

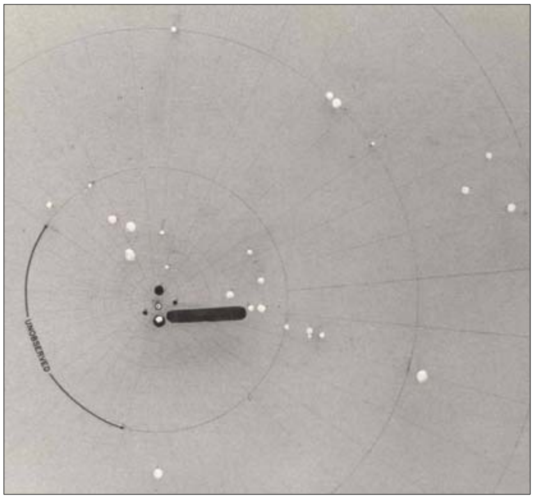


Figure 17: Morgan's later cotton-ball model and annotated diagram (courtesy: Yerkes Observatory Archives).

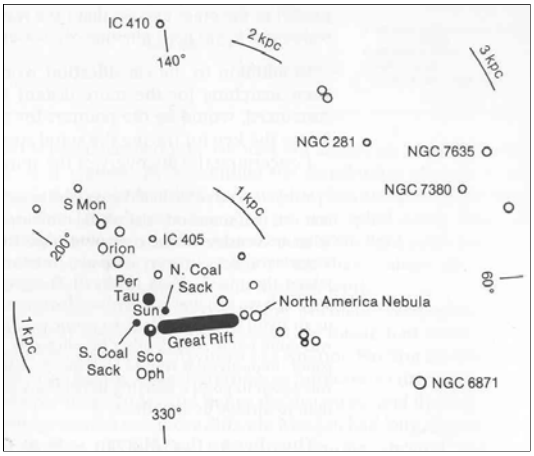


Figure 18: Legend to the model shown in Figure 17.

As he worked his way back to health—in part by means of those writing-exercises he committed to his notebooks—Morgan came to identify with Freud's self-analysis undertaken at the time Freud was making his most important discoveries in psychoanalysis. While reading the Freud-Fliess letters, he quoted Freud, who had said "… you can imagine the state of mind I am in—the increase of normal depression after the elation." To this, Morgan (1957b) added: "How true – how true! How often I have experienced this same phenomenon! Intrinsically, temperamentally, how similar I am to Freud!" In the same notebook, he later wrote: "Always there was melancholy in spring for me." (ibid.).

10 CONCLUDING REMARKS

One can speculate that if the radio astronomers had not 'stolen his thunder' then Morgan might even have won the Nobel Prize for his discovery. If he had, would it have resolved his struggle for self-esteem? Instead he continued, through a long and accomplished career, to grapple with the classification of galaxies and with the alternating creative phases of elation and let-down. His was the condition of many of those with creative temperaments, "… the greatness and misery of man …" as Pascal put it. Near the end of his life, Morgan (1983) wrote:

> A crucial conceptual breakthrough conversation with Osterbrock this morning. He would like to write my life … In the following conversation, I said I was not a genius; he said he was not sure I was right—that I had made "Conceptual Breakthroughs." The implication seemed to be that I might be … I told him that he had just given me the highest honor of my entire life.

In the discovery of the spiral-arm structure of our Galaxy, Morgan had achieved one of the most important scientific breakthroughs of twentieth-century astronomy and also caught an inspiration worthy of the great artists he so admired—those of the Trecento, the period from Cimabue to Giotto, who were the visual artists Morgan always thought had gone furthest in probing 'deepest reality'. Morgan (1956), like them—like Plato in the eternal realm of his universals—had secured his achievement in "… the hours of stillness – with supple brain – deep in the vistas of space, time, and form – that Heavenly World of Form."

11 NOTES

1. The only time this had happened previously was when V.M. Slipher announced the discovery of the large velocity-shifts of the nebulae at the A.A.S. meeting in 1913. Otto Struve (1953: 277) described the response to Morgan's paper as "… an ovation such as I have never before witnessed. Clearly, he had in the course of a 15-minute paper presented so convincing an array of arguments that the audience for once threw caution to the wind and gave Morgan the recognition which he so richly deserved."
2. When George Willis Ritchey finished working with the Mt. Wilson 60-inch Reflector in 1908, George Ellery Hale wrote in his *Annual Report of the Director* for 1909 that the observing program for the telescope was "… not yet definitely arranged." But Hale's plans would be decisively influenced by Kapteyn, who had sought the cooperation of major observatories in an observing program he called 'The Plan of Selected Areas', which aimed at nothing less than to determine the large-scale dynamics and structure of the Universe—a Universe which was then still thought by most astronomers to be a sidereal system bounded by the Milky Way. Kapteyn's plan called for the statistical analysis of results obtained from detailed surveys to be conducted in 206 'selected areas'—representative swatches evenly distributed around the sky. Hale was convinced, and argued that Kapteyn's work—especially his putative discovery that the stars moved in one of two opposing streams—bore directly on the problem which interested him most, that of stellar evolution. Although neither Kapteyn's 'star streams' nor his model of the Universe survived the test of time, the programs begun with the 60-inch telescope in the selected areas proved far-reaching, and much of the telescope's working life would be bound up in the great interwoven quests for the answers to stellar evolution and galactic structure. Kapteyn himself spent most summers as a Research Associate at Mt. Wilson from 1909 until 1914, advising Hale on the scientific course for the big telescope. Kapteyn was accompanied by his wife, and since women were not permitted to stay in the 'Monastery' (the main residence built for astronomers working on the mountain), Kapteyn lived in another cottage on Mt. Wilson; it is still known today as the 'Kapteyn Cottage'.
3. S Andromeda almost reached naked-eye visibility, and at the time it was assumed that it was like the ordinary galactic novae, which placed the Andromeda Nebula close by (see Jones, 1976). It was not until ordinary novae were finally identified in the Andromeda Nebula that astronomers realized the difference between novae and supernovae.
4. For biographical information on Morgan see Garrison (1995) and Osterbrock (1997).
5. In later years, Morgan sometimes tried to put a more positive 'spin' on his father's personality and approach to life. "My father, in a sense, was a very great man …" he told David DeVorkin (Morgan, 1987). "He told me once it took two generations to make a gentleman and he was the first. His father was the same kind of person he was. But it was very, very rough." (ibid.). He even dedicated his essay, "The MK System and the MK Process", in the proceedings of a workshop held in his honor at the University of Toronto in June 1983 "To my father, William Thomas Morgan (1877-??). You will never know what I owe you." (see Garrison, 1984: 18n). The use of ?? for the unknown date of his father's death strikes me as particularly poignant.
6. Morgan's first notebook was started in 1955, and the last (No. 247) was completed in 1990, by which time his thoughts were becoming scattered and somewhat random as he was suffering from Alzheimer's Disease. These notebooks are a unique resource, and document his mental life in almost Proustian detail. Of the 247 notebooks, 244 are at Yerkes Observatory. Jean Morgan, Morgan's second wife, after consulting with his closest and most trusted friends, Donald Osterbrock, Robert Garrison, and Dimitri Mihalas, wanted them to remain there and to be available to scholars. This was as she and they judged that Morgan himself would have wished. There are also a few earlier notebooks, which were

not kept as part of this series, but which contain fascinating insights into his active interest in art as well as stellar classification in the early 1940s. Of those running continuously and in seriatim, the first begins on 20 April 1955, and the last, begun on 11 September 1990, peters out into rambling free-associations. The two earliest were removed; one was sent by Jean Morgan to a friend for consultation as to whether they might contain sensitive and highly personal materials. Another Morgan himself lost, and yet another one was given to extended family members and has not been available for study. The author has begun a close study of these notebooks as a step toward the goal of eventually producing a full-length biographical study of Morgan.

7. Frost's comment calls to mind Walter Baade's comments about Hubble's Ph.D. thesis, which, according to Osterbrock (pers. comm., March 2002), he called "… the most miserable thesis you ever saw."
8. The Balmer lines of hydrogen are, of course, very susceptible to Stark broadening, so the wings are a direct measure of the electron pressure in the stellar atmosphere. These lines are very strong in the B stars, and so the broadening can be detected at relatively low resolution. As the pressure is the weight of the overlying gas and the hydrogen is mostly ionized in the B stars, the stars with large radii (and hence luminosities) have the lowest pressures and the narrowest lines.
9. Note there is a difference between 'resolution' and 'dispersion'. The Mt. Wilson astronomers, with much larger telescopes, could afford to throw away most of the light on the spectrograph slit, thereby obtaining much better resolution at the same dispersion compared to those obtained with objective-prisms like the early Harvard spectra.
10. Although the reason that all the great spectral classifiers before Morgan were women was a result of Edward C. Pickering's scheme of having otherwise unemployed ladies do the routine work at the Harvard College Observatory—often for no pay at all—their aptitude may also have been, in part, a result of the general superiority of women over men for tasks such as facial-recognition (e.g. see Baron-Cohen, 2003). In this respect, the following notebook entry by Morgan (1957a), dated 1 January 1957, may be relevant: "My artistic sensitivity … may well have some personal feminine characteristics at its base."
11. But Andrew T. Young (pers. comm., 7 March 2007) glosses over it. He says: "I might add that I had a try at learning spectral classification myself. It is not at all an easy skill to pick up … So it's hardly true that 'anyone' can classify spectra—though the *Atlas* is certainly a big help … [and] the difficulty of spectral classification has turned out to be so severe that only a handful of people have become adept at it. For mass-produced luminosities, multicolor photometry has turned out to be the preferred way to go—along with the recognition that colors and MKK classifications don't match up, even for supposedly 'normal' stars."
12. It turns out that while the much fainter giants in globular clusters can be seen only a degree or so away from the Galactic Center in 'Baade's Window'—a real 'hole' in the dust, almost in the Herschelian sense—the O stars remain invisible at a fraction of that distance.
13. Many of the required photometric measurements had already been obtained by Joel Stebbins, C. Morse Huffer, and Albert Whitford at the University of Wisconsin. Stebbins and Whiford had devised a six-color spectrum, but Morgan, in collaboration with Harold L. Johnson, later invented the UBV system as a simpler version intended as an essential partner to the two-dimensional classification system. Their paper (Morgan and Johnson, 1953) is one of the most widely-cited papers in the general astronomical literature.

   Though it had long been known that interstellar extinction, like atmospheric extinction, is 'selective'—that is, greater at shorter wavelengths—so that it produces reddening, there is not as strict a connection between spectral types and colors as everyone at first believed. There is a pretty tight correlation, but it is not perfect. The hope was that stars with identical spectral features would have identical colors, but this did not turn out to be true; the small but significant discrepancies are both puzzling and a considerable hindrance to getting accurate 'spectroscopic parallaxes' or photometric distances of individual stars. According to Andrew T. Young (pers. comm., 7 March 2007), the reason seems to be that "… colors depend mostly on sources of continuous opacity in the stellar atmospheres, but spectral types depend more on the line extinction coefficients, and these aren't as tightly coupled as one might hope, even for "normal" (Pop. I) stars; it's even worse for Pop. II, because the continuum opacity in cool stars is mainly the H-minus ion, where the electrons come from ionization of the metals; so less metals means less continuum opacity, so you see deeper into the star, and see more of the few metal atoms there. As a result, the spectra don't change nearly as drastically as the metal content does."
14. According to Andrew T. Young (pers. comm., 3 March 2007), "It is important to bear in mind that a star traveling at a speed of a kilometer per second will travel a parsec in a million years (very nearly). These young stars typically have speeds of only 10 or 20 km/sec. And, as their ages are generally on the order of 10 million years, they can travel only 100 or 200 pc before vanishing. Another essential fact is that even 20 million years is small compared to the timescale for the epicyclic motion about the local mean galactic rotation. The epicyclic period must be around 150–200 million years locally; divide by 2π to get the characteristic timescale, and you still have 30 million years or so—longer than all but the oldest B stars live."
15. The Distance Modulus is related to the distance of a star (d) in parsecs, and is given by the following formula, where m is the apparent magnitude, and M the absolute magnitude:

$$m - M = 5 \log d - 5$$

16. The old system, which measured the galactic longitude from one of the points where the Galactic Equator intersects the Celestial Equator—a choice that is purely arbitrary and without physical significance—has now been replaced by a new system using a slightly different Pole and measuring galactic longitude with respect to the Galactic Center (see Mihalas and Routly, 1968).

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The late Professor Osterbrock—one of Morgan's collaborators in the discovery of the spiral arms—was particularly helpful, discussing Morgan's life and work with me on a number of occasions over many years. He also kindly made valuable unpublished material available to me. An early version of this paper was presented on the occasion of his Eightieth Birthday Celebration at the University of California, Santa Cruz. Unfortunately, he did not live to see the completion of this final version, or the full-length biography of Morgan which he hoped I would one day complete.

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THE PLEASURES OF THE VAULT

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Abstract: Lick Observatory's photographic plate archive is among the world's most extensive. Its value lies both in its scientific potential and in the history it preserves. Its direct and spectrographic plates constitute a hundred years of data, valuable for a variety of investigations, especially those concerned with time-varying phenomena. Its historical importance lies in a wealth of material illuminating the life and work of Lick astronomers. Don Osterbrock, more than any other Lick astronomer, recognized and exploited the Plate Vault's historical potential, however many more treasures await discovery.

Keywords: Lick Observatory, photographic plate archives; history of astronomy; time-varying phenomena

1 INTRODUCTION

Behind an inconspicuous door off the rear courtyard of the Main Building of the University of California's Lick Observatory, on Mt. Hamilton, lie four rooms in which are housed the labors of a century. The Plate Vault is the Observatory's mass storage for the terabytes of data collected in its first century of operation. But instead of ones and zeroes on disks and tapes, these data are locked in uncountable grains of silver-on-glass plates, and in tens of thousands of pages of logbook entries.

2 THE LICK OBSERVATORY PLATE VAULT

2.1 Contents of the Vault

Lick Observatory has one of the world's major stores of astronomical plates, and on a conservative estimate contains ~150,000 individual pieces of glass. They range from a vast collection of low-resolution spectra on plates no larger than a stick of gum—compiled by the Mills spectrograph on the 36-inch refractor and by the Crocker Southern Expedition in Chile, for the purpose of a massive survey of radial velocities (Figure 1)—to the high-resolution coudé plates made with the Shane 3-meter reflector, where the dispersion could be so high that the spectrum had to be recorded on two 10-inch-wide plates butted end to end. They include drawer after drawer and shelf after shelf of direct images taken with the 12-inch and 36-inch refractors, the famous Willard lens, the 36-inch Crossley Reflector, the 20-inch dual astrograph, and the 3-meter prime focus (Figure 2).[1]

Figure 1: One of dozens of drawers in which are stored the low-resolution stellar spectra taken during the first few decades of the 20th century on Mt. Hamilton and at the Mills Southern Station in Chile, as part of Lick's massive radial velocity survey.

Figure 2: Cabinets in the Plate Vault's north room contain direct images taken with the 36-inch refractor, the 20-inch twin astrograph, and a variety of other instruments. Here are found E.E. Barnard's epoch-making images of the Milky Way, an extensive collection of photographs of the Moon, planets, comets, and asteroids, and the many plates produced by 40 years of Lick Observatory solar eclipse expeditions.

One room is devoted to the written observing records, put down in hundreds of logbooks, in the hands of some of the finest astronomers of the twentieth century (Figure 3). There you will find the handwritten observing notes of Barnard, Keeler, Trumpler, Curtis, Campbell, Wright, Herbig, Stebbins, Whitford, Kron, and others.

2.2 Research Potential

The Plate Vault has indisputable scientific value, and over the last twenty years it has done a slow but steady business fulfilling requests for archival data. Two recent examples illustrate the nature and diversity of the requests for historical data. One was from an astronomer at Cambridge who, in improving the orbital elements of the spectroscopic binary Beta Scuti, found that several of the velocities for this star published by Lick Observatory in the 1920s were wildly out of keeping with his orbital solution. The original plates, combined with the written observing records, revealed that at least one of the published velocities was derived from an observation of the wrong star!

The second request originated with a researcher who was trying to provide a date for a young supernova remnant in a nearby galaxy, discovered on a Lick plate that had been published in the 1960s but without a recorded date for the observation. The date of the discovery plate, along with four others in the collection—one dating back to 1917—allowed the researcher to place a minimum age on the SNR.

Such requests are typical, and underscore the particular importance of historical data to time-varying phenomena. The eventual aim is to make these data available to today's—and tomorrow's—researchers: a first volume of observations, seamless and continuous with modern digital data. Digitization and distribution on the worldwide web make this an attainable goal. We must begin with cataloging the most fruitful plates in order to make their existence known to the wider community, and then reduce them to a format that is indistinguishable from modern, digital observations.

But the Plate Vault's scientific usefulness represents only part of its value. The archive is a place where science and history of astronomy intersect. Everywhere along its shelves, in its drawers and cabinets, one encounters a past intimately tied to the fascinating, sometimes turbulent history of the Observatory, and to the extraordinary scientists who advanced it (e.g. see Osterbrock et al., 1987). The remainder of this note will focus on those historical treasures and the aesthetic delights waiting behind the door of the Plate Vault.

3 HISTORICAL TREASURES OF THE PLATE VAULT

I was hired by Lick in 1987 in anticipation of the retirement of a mountain legend and one of its most memorable characters, Gene Harlan. Gene, whose art was direct and spectrographic photography, and who knew the workings of the Great Refractor, the Crossley, and the 3-meter coudé like no other, patiently, if sometimes grumpily, initiated me into the arcana of cutting, sensitizing, and preflashing plates, of finding guide stars and estimating exposure times, of safely loading and unloading the sensitive glass, and the mysteries of the dark room. Both of us knew that he was handing down a skill that was already disappearing from astronomy, already nearly obsolete, but neither of us ever spoke of that. Just before Gene left the mountain for the last time, he unceremoniously handed me the key to the Plate Vault, informing me that those four rooms—and the task of fulfilling requests for plates—were now my responsibility

I was given that key not because of any special qualification but because no one else wanted it! The Plate Vault at Lick Observatory—and I suspect at other observatories, too—occupies a peculiar place: it is simultaneously venerated and neglected. The key to its door is perhaps the most closely guarded on the mountain (except for the one that unlocks the freezer in the diner where the cookies are kept!), but for all the reverence it is accorded, almost everybody is, quite understandably, too busy with the work of the present and with plans for the future to have much time for the past. One person, however, most notably did not neglect the Plate Vault. That person was Don Osterbrock.

When I would pick up my telephone to find Don at the other end odds were better than even that he was going to ask me to look for something in the Plate Vault. Whether his request was for the spectra that brought Fath so close to announcing the true nature of the spiral nebulae ten years before Hubble, for the observing book in which Barnard had noted his discovery of the fifth moon of Jupiter, or to locate the huge glass eclipse plates with their displaced stars corroborating the proof of General Relativity, it was always a delight and an honor to be part of the treasure hunt.

Thanks to Gene, Don and my own ramblings through the archive (born of nosiness and a librarian's heart), I have had the good fortune to unearth and study some of the Plate Vault's treasures.

On the eve of the 2004 transit of Venus, the Vault yielded 140 plates made at the last transit, in 1882. With the collaboration of William Sheehan, the plates ignited an exciting investigation into the fascinating story of the eccentric astronomer David Todd, and his expedition to Mt. Hamilton to photograph the transit (see Misch and Sheehan, 2005).[2] As a prelude to the 2004 event, the rediscovered plates were assembled into a time-lapse movie showing Venus 120 years ago, in her stately crossing of the photosphere (Misch and Sheehan, 2004). The movie stands as one of the earliest events to be reanimated as a motion picture.[3]

Figure 3: The Plate Vault's south room is devoted to written records that provide invaluable documentation supporting the plate collection. These shelves hold some of the observing logs kept by individual staff members.

For several years in the 1990s, the plate vault was the catalyst for a happy and fruitful association with internationally-known photographer Linda Connor, who spent many days on Mt. Hamilton, using sunlight to print Lick plates, which she then paired with her own photographs to great critical acclaim. Linda's work with the Lick images was widely exhibited, culminating in a limited edition fine press book published by the Whitney Museum of American Art (Connor and Simac, 1996).

Edward Emerson Barnard's extraordinary drawings made at the eyepiece of the 36-inch Refractor during the 1894 opposition of Mars, stored in the Plate Vault, inspired a collaborative exploration with Sheehan and others of the roles of hand, eye, and brain as they interact under the conditions unique to the astronomer at the eyepiece. The investigation culminated in a memorable two-week project of drawing Mars during the 2003 opposition, using the 36-inch Refractor under conditions similar to those encountered by Barnard (see Misch, Sheehan, Stone and Hatch, 2003).

Rediscovered in the Plate Vault were more than 200 plates, made between 1889 and 1934, documenting 17 expeditions by Lick astronomers to observe total eclipses of the Sun on five continents—often in remote places and under difficult conditions (see Osterbrock, 1980). These extraordinary images have inspired new historical research (e.g. Pearson and Orchiston, 2008) and provided the author with material for presentations from the Griffith Observatory to the British Museum.

4 CONCLUDING REMARKS

The Plate Vault amply demonstrates its depth and importance—assets that Don Osterbrock never lost sight of. Recently, action has begun to improve the environmental conditions in which the plates are stored, ensuring their lasting preservation. There is every reason to hope that the Lick Observatory Plate Vault will continue to provide pleasures to curious investigators and yield undiscovered surprises, both scientific and historical.

5 NOTES

1. The 3-meter prime focus collection is among the smallest of the individual collections, the days of the photographic plate's supremacy being already numbered by the time the 3-meter telescope went into service.
2. It was during this expedition, while his wife remained in Amherst, that Todd was famously cuckolded by Emily Dickinson's brother, Austin (see Sheehan and Misch, 2004).
3. To see the movie go to the following web site: http://www.skyandtelescope.com/observing/objects/daylightphenomena/3308756.html

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Anthony Misch was Support Astronomer at Lick Observatory for twenty years, where, among other duties, he served as Curator of the Plate Vault. He now works for Lick part time, and continues his connection with the archive and pursues his interest in the Observatory's history.

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THE 40-FOOT SOLAR ECLIPSE CAMERA OF THE LICK OBSERVATORY

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Abstract: The primary goal of the Lick Observatory's direct solar eclipse photography program was to secure high-resolution images of inner coronal structure and images in which coronal brightness could be studied. Between 1889 and 1932 the Observatory sent out seventeen eclipse expeditions worldwide. During these expeditions, direct coronal photography was a significant part of the program for the first couple of decades. By the end of the expedition series, spectrographic observations became of primary importance, yet direct coronal imaging continued.

Lick Observatory astronomer, John M. Schaeberle, conceived and constructed a large portable camera of 5-inch aperture with a focal length of 40-feet, and from 1893 the so-called 'Schaeberle Camera' became a hallmark of the Observatory's eclipse expeditions. In this paper we provide details of the Schaeberle Camera's design, setup and operation, and we briefly discuss some of the ways in which Lick Observatory staff and other astronomers used the plates obtained during the various eclipse expeditions in their investigations of the solar corona.

Keywords: Lick Observatory, John M. Schaeberle, Edward S. Holden, William W. Campbell, solar corona, solar eclipse expeditions, Schaeberle Camera

1 INTRODUCTION

During the nineteenth century knowledge of the solar corona, which could only be seen during a total eclipse of the Sun, developed rather slowly due to the rarity of viewable eclipses. According to Lick Observatory's W.W. Cambell (1923), a typical astronomer would only be able to observe a little over one hour of totality in a lifetime! Until the latter part of the nineteenth century drawing was the dominate method to make permanent records of a solar eclipse. During this period, astronomers began to use photography to generate permanent records that could be subjected to latter analysis. The first successful coronal images were obtained by Father Secchi and Warren De La Rue in 1860, from two different observing sites (Clerke, 1908; Pang, 2002; Proctor, 1871; Ranyard, 1879). While coronal imaging slowly improved as photography evolved from wet plates to the more sensitive dry-plate process, it was the Lick Observatory's first eclipse expedition, in January 1889, that set a new standard for producing high-resolution coronal images.

In 1873 the ailing entrepreneur James Lick decided to fund an observatory that would "… rank first in the world." (Wright, 2003: 13), and as a personal monument to himself Lick commissioned what was to be the world's largest refracting telescope, with a 36-inch objective. The giant telescope saw first light in 1888, and the fledging Lick Observatory (henceforth LO) was turned over to the University of California (henceforth UC). Edward S. Holden was appointed inaugural Director of the Observatory, and his special talent lay in raising funds from private sources (Osterbrock et al., 1988). He quickly convinced San Francisco banker and UC Regent, Charles F. Crocker, to provide funding for solar research and the Observatory's solar eclipse expeditions were named in his honour.

The Observatory's very first expedition was to Bartlett Springs (California) for the eclipse of 1 January 1889 (see Figure 1), and fine images of the inner corona were obtained by Staff Astronomer Edward E. Barnard with the Clark & Sons 'water reservoir' telescope (Barnard, 1889).[1] It is noteworthy that Barnard's best images gave more coronal detail than the images brought home by the well-equipped Harvard College party under W.H. Pickering (see Holden, 1889a, 1889b; Holden et al., 1889).

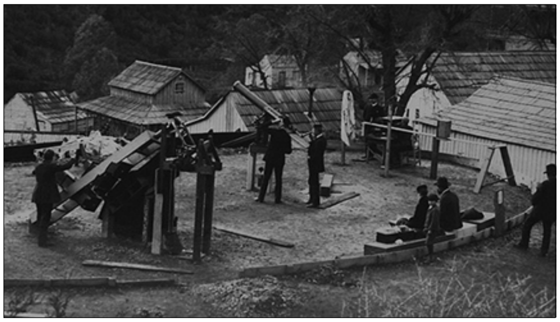

Figure 1: The Lick party at Bartlett Springs (Mary Lea Shane Archives).

2 JOHN M. SCHAEBERLE

One of those who was involved in preparing for Lick Observatory's second solar eclipse expedition was Staff Astronomer, John M. Schaeberle. Schaeberle emigrated from Germany in 1853, and his early life was spent in Ann Arbor (Michigan) where he broadened his background in a number of ways that would later make him an accomplished astronomer. After spending three years as a machine shop apprentice he studied astronomy and mathematics at the University of Michigan. In 1876 he was appointed an Assistant at the University's observatory, later becoming an Instructor of Astronomy and an Acting Professor. He was an avid telescope-maker and constructed a number of reflecting telescopes. In 1888, Schaeberle joined the staff of the Lick Observatory (Hussey, 1924).

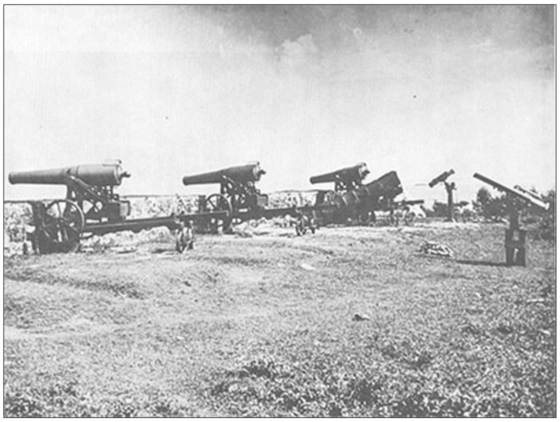

Figure 2: The battery site with Schaeberle's 18-inch reflector astride one of the cannon carriages, center-back of image (Mary Lea Shane Archives).

Schaeberle became Acting Director of the Observatory after Holden was forced to resign, his appointment becoming effective on 1 January 1898 (Osterbrock et al., 1988: 105-107), but he only served in this new capacity for four months, before being replaced by J.E. Keeler. As he was in charge of the Observatory during the January 1898 eclipse, Schaeberle asked W.W. Campbell to lead the expedition and conduct the research program designed by Holden (1897), who wished to determine whether the corona rotated with the Sun.

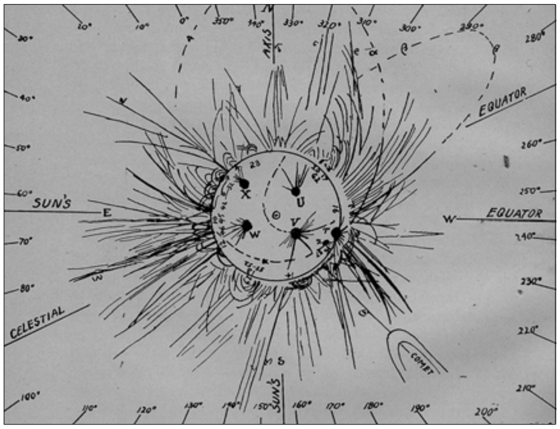

Figure 3: Skeleton drawing of a model showing the paths of ejected matter from the Sun's surface (after Schaberle, 1895: Plate 9).

After Keeler took over the Directorship of the Observatory Schaeberle was able to work closely with Barnard preparing a fully photographic program for the 21-22 December 1889 eclipse expedition to Cayenne, French Guiana (see Figure 2). Staff Astronomer S.W. Burnham joined Schaeberle on the expedition (Osterbrock *et al.*, 1988), and they used an 18-inch Newtonian telescope made from barrel hoops and packing crate wood with a mirror figured by Schaeberle to obtain eleven large-scale eclipse images (Holden, 1891a). When Holden reviewed the badly-overexposed plates and heard of the lack of success from the other expeditions, he proclaimed:

> The Lick Observatory expedition succeeded while NO other expedition (as I know) has succeeded at all. These twelve photographs will be the data on which *all the world* will have to depend. It is a great *credit* to America, to the state, and to the Lick Observatory. Burnham and Schaeberle have no superiors … *The English astronomers, I see, are doubting the reality of the extensions of the corona first photographed.* There is no doubt, *really*, for I found it on photographs taken from different places and our eye drawings. (Holden, 1890a; his underlining and italics).

The overexposure of the plates was a direct result of Holden's inflexible expedition directives (Holden, 1889c; 1890b). Making working conditions even more difficult, long argument was to ensue over the unstable Carcel lamp that was used to standardize the plates for photometry (Holden, 1890c).

Homeward bound from the December 1889 eclipse, Schaeberle laid the groundwork for a new theory to explain the intricate coronal features that he recorded in drawings and on photographs (e.g. see Figure 3). However, this was to be a momentary 'diversion', for he grew increasingly disillusioned at being passed over for the Directorship, and he eventually decided to leave the Observatory. He then began traveling internationally, with no immediate plans to return to astronomy. Nonetheless, he was again considered for the Lick Directorship in 1900, following Keeler's sudden death, but W.W. Campbell was appointed to the post (Osterbrock et al., 1988).

3 SCHAEBERLE'S MECHANICAL THEORY OF THE SOLAR CORONA AND THE DEMAND FOR A NEW TYPE OF ECLIPSE CAMERA

Schaeberle (1890: 68) first outlined his theory in a brief paper that appeared in the *Publications of the Astronomical Society of the Pacific*, where he stated that

> … his investigations seemed to prove conclusively that the solar corona is caused by light emitted and reflected from streams of matter ejected from the sun, by forces which, in general, act along lines normal to the surface of the sun; these forces are most active near the centre of each sun-spot zone ...
>
> The variations in the type of the corona [from eclipse to eclipse] admit of an exceedingly simple explanation, being due to nothing more than the change in the position of the observer with reference to the plane of the sun's equator …
>
> Mr. SCHAEBERLE … stated that he had thus far been unable to find a single observed phenomenon which could not be accounted for by his mechanical theory.

Further details appear in a second paper published the following year (Schaeberle, 1891).

The passage of time would show Schaeberle's mechanical theory to be flawed (especially when Hale was able to demonstrate the critical role of the Sun's magnetic field), but in the interim it inspired the design and construction of a new type of camera capable of providing the Lick astronomers with large solar images that would reveal fine coronal structure.

4 THE 40-FOOT SCHAEBERLE ECLIPSE CAMERA

Schaeberle (1895) designed a direct-imaging camera in place of the horizontal heliograph favored by other solar researchers, reasoning that the additional optical surfaces of a horizontal heliograph would degrade the quality of the image due to heat expansion issues. Furthermore, his design would eliminate the image-rotation issues and pronounced driving clock errors of the horizontal heliograph. As it turned out, his reasoning stood the test of time.

In 1908 Campbell (1908a; 1908c) published his thoughts on the advantages of Schaeberle's design. A lens, with its tube assembly mounted well above the ground, is easily ventilated and will be subjected to far less image-degrading ground heat-radiation. Schaeberle's Camera's components could be rigidly fixed in place and be independent of each other, thereby ensuring that any vibrations from the tube section would not transmit to the lens or plate holder. Schaeberle (1895) realized that "Any advantage due to the large scale given by a telescope 40-feet long will, in a great measure, be lost unless great stability of the image on the photographic plate is secured."

To test the feasibility of his concept, Schaeberle placed the Clark & Sons lens from the Observatory's horizontal photoheliograph in the slit of the meridian circle room. From star trail tests, the best focus was found to be at 40 feet and 1.2 inches (Schaeberle, 1895).

The original version of the new 'Schaeberle Camera' was assembled on Mt. Hamilton in the autumn of 1892. The Camera's length was kept near the sloping ground with its lens supported on an inclined plank-tripod. The moving plate carriage system was mounted on its own pier. The Camera's tube was made of black painted canvas which was attached to a wooden framework with cord via iron rings. The support for the tube frame consisted of wooden posts that were placed vertically in pairs at intervals up the sloping hillside. The rigid wooden tube frame was secured to the posts. A canvas tent covered the plate area. The Camera survived several stormy days and produced good test exposures of star fields and the Moon. The ability to change plates quickly was tested and found to be satisfactory (ibid.).

4.1 The Camera's Unique Moving Plate-Holder System

Schaeberle (ibid.) designed a moving plate-holder (Figure 4) that would follow the diurnal motion of the Sun and keep the Sun's image centered on the photographic plate. He assembled a quadrilateral iron-frame track guide to accept a wheeled triangular-shaped plate holder. The plate holder traveled on three carefully-made wheels on tracks whose surfaces were machined as smooth as possible. The lower wheels had knife-edges that followed a V groove cut into the face of the iron frame work.

Schaeberle then devised a procedure for setting the correct plate velocity for diurnal motion. The linear motion was obtained by using the Sun's hourly motion from the Ephemeris on the date and time of the eclipse. This hourly motion, along with the focal length of the Camera, was used to compute the distance that the Sun's image would travel on a stationary photographic plate. The clock mechanism—linked to one of the chronometers—governed the rate of motion. Schaeberle (1895) described the adjustment of the clock:

> The lateral motion (diurnal) was given to the plate by the unwinding of a strong, flexible wire wound around a drum mounted on the clock's winding axis. The size of this drum was determined by computation, and the final adjustment of the velocity was then made by shifting the balls of the centrifugal governor.

Final adjustments were made by observing stellar images at the negative's plane for movement. Then long exposure plates of stellar sources were made and inspected for any residual motion in the stars' positions.

By the time of the 1896 and 1898 eclipses, Schaeberle and Campbell had revised and refined the components, alignment and focusing procedures of the Camera. Campbell provided a description of the revised Camera's parts and the method used for optical alignment in a letter written to Ormond Stone in 1899.

Figure 4: The original plate-holder and drive clock as it appeared at the December 1889 eclipse site (Mary Lea Shane Archives).

The objective lens and the plate-holder were mounted on the tube and aligned with the optics of the 36-inch refractor. The plate-holder could be tilted 45º. The lens was carefully collimated using a candle flame at night. The plate-holder was then tilted until the candle flame and its reflections from the lens elements were all in one straight line. On a warm night, the telescope was directed towards some bright stars at the same altitude and azimuth as the upcoming eclipse, and the stars were allowed to trail across a photographic plate twice while the declination of the plate holder was varied slightly on each occasion. The plate-holder was then set for best focus. On future eclipses, the pictures were found to be beautifully sharp (Campbell, 1899).

The Camera's moving plate-holder was constructed by a machinist under daily supervision. According to Campbell, no shop drawings were made nor computations kept of the plate-holder's construction, but in his letter to Stone, Campbell (ibid.) created detailed drawings and descriptions of parts of the photographic plate system (see Figures 5 and 6).

In his letter to Stone, Campbell (ibid.) also describes how the plate-holder worked:

> As the Sun moves up and southward during the eclipse, in a curved diurnal path, the plate-holder must move downwards and to the north, in a slightly curved path which is concave to the southward. The plate carriage travels on five wheels that are about 1.5 inches in diameter. The one wheel on the northern edge and two wheels on the southern edge simply bear the carriage up from the supporting track. The other two wheels on the

> southern side guide the carriage along the curved side of the track.

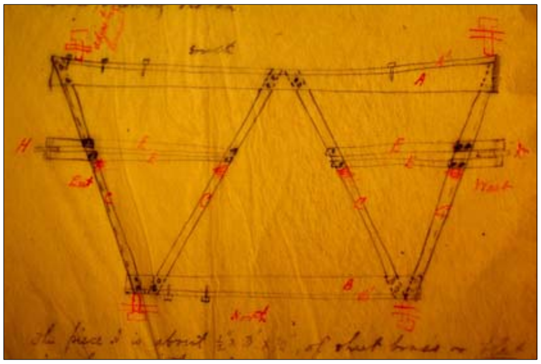

Figure 5: Plate carriage track drawing showing the arrangement of the curved wheel guides (Campbell Copy Book – Mary Lea Shane Archives).

The plate carriage consisted of a skeletal frame that was firmly attached to a top plate of sheet to avoid flexure in the assembly. Two holes were cut in the plate for the camera-operator's hands to handle the plate boxes. The metal track assembly was attached to a strong wooden framework. Adjusting screws allowed fine calibration of the diurnal motion. To finally bring the plate into delicate focus, the lens could be moved in or out and then recollimated.

The original cardboard photographic plate-holder boxes were subsequently replaced with thin wooden boxes with removable lids. Each plate box lid would serve as the Camera's shutter. A plate-holder would be secured in place on the plate carriage by metal stops, forming a three-point support system to float the plate-holder above the top of the plate carriage.

4.2 The Revised Tube and Support Structure

The original wood tube frame was replaced with a 0.5-inch iron pipe frame. The new tube was made of an exterior white duck cloth cover and lined on the inside with two thicknesses of black muskin. Campbell (ibid.) noted that "Black outside absorbs the heat which is extremely objectionable." This cover was fitted with iron rings along its length in order to secure it to the pipe frame.

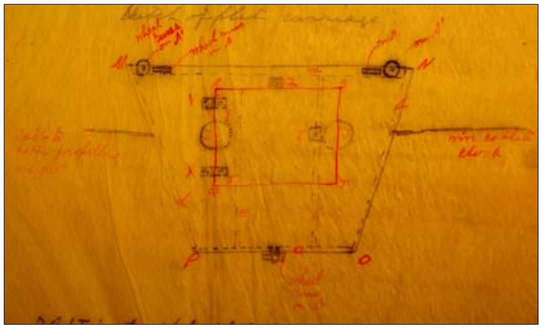

Figure 6: Plate carriage drawing showing the top plate overlay on the skeletal frame; the position of the plate-holder is marked in red (Campbell Copy Book – Mary Lea Shane Archives).

At the 1898 eclipse in India, Campbell erected an independent two-tower system to support the objective lens and the tube. The towers were isolated from each other so that any tube motion due to wind would not affect the objective lens. Tower materials were obtained on site until 1905 and then became part of the cargo manifest at subsequent eclipse expeditions. According to Campbell (ibid.), an excavated pit for the plate system was needed, "… if there should be a wind storm, of some violence within a few days before the eclipse, the tent on level ground might be blown down and smash the final adjustments."

4.3 The Objective Lenses

The primary objective lens used for expeditions was the Alvan Clark & Sons 5-inch aperture lens from the Observatory's horizontal photoheliograph that was installed on Mt. Hamilton by D.P. Todd for the 1882 transit of Venus. This lens had been especially figured for solar photography, and Holden (1892; 1895) wished to retain it at the LO for ongoing projects.

In 1895, Holden commissioned J.A. Brashear to make a 6-inch aperture lens of the same focal length, and this was delivered to the Observatory in March of 1896 (Brashear, 1895). Unbeknownst to the Lick astronomers, problems with this lens were to persist for many years, and they were never fully resolved. The ensuing interaction between the astronomers and Brashear would consume countless hours of time and energy that could have been used more profitably for research. Upon testing the lens the astronomers found its focal length was too short, and it was promptly returned to Brashear for correction. He responded (Brashear, 1896):

> According to our measures the objective was a very little short but we had no idea that you demanded such an accuracy in focal length … as 1/20,000″ in the versed sign of any of the curves will make as great a difference as you indicate in your letter [of 17 April].

As it turned out, Brashear's tape measure was defective (ibid.). The lens was star-tested on the evening of 5 August 1897 and found to have very bright triangular ghost images. Campbell (1897b) declared that "The lens is not right, I cannot waste any more time with it, and cannot wait to have it returned to Brashear." For his part, Brashear (1897) thought that the problems were due to unequal separation of the lens elements which could produce the 'triangle' ghost images. Brashear elaborated: "We feel so certain that the lenses were worked correctly and that the glass is all right ... I beg you to understand we are making no excuse for the lens in any way, shape or form." Schaeberle (1897) had his own ideas about the problem with the lens and informed Brashear that

> The trouble seems to be due to the very fact that the two surfaces (inner) have the same radius of curvature, so that by double reflection from their practically parallel surfaces the reflected rays being parallel to the direct rays (or nearly so) come to a focus in the same plane in the the (sic) principle image. [His underlining].

Campbell finally decided not to use the lens, and it would remain in storage until the eclipse of 1918.

A 4-inch, 40-foot focal length lens was also made by Brashear at Holden's request, and was taken as a back-up lens on the 1893 eclipse expedition. Brashear (1891) forwarded Holden his and Hastings' comments regarding this lens:

> Neither Dr. Hastings nor I can see how you will use it, or what use it will be after it is made, as it will in practically be identical with pin hole photography and of no value in your work.

Perhaps the idea of this lens emerged when Holden learnt of Harvard College Observatory's futile attempts to photograph the 1886 eclipse with a 4-inch horizontal heliograph of 38.5-feet focal length (Baily, 1931).

4.4 Image Sizes of the Observatory's Photographic Lenses

Table 1 lists the range of cameras that traveled with the LO on its various eclipse expeditions. The lunar disk image scale can be seen to increase by a factor of 9 from the half inch image produced by Barnard's water reservoir telescope in 1889 to the 4.5 inch images produced by the Schaeberle Camera from 1893.

5 A BRIEF ACCOUNT OF THE SCHAEBERLE CAMERA ON THE DIFFERENT SOLAR ECLIPSE EXPEDITIONS

Table 2 lists the eclipse years, site location, duration of totality, altitude of the eclipsed Sun, plate types and sizes for the Schaeberle Camera, and exposures used.

5.1 Mina Bronces, Chile: 16 April 1893

This eclipse presented the first opportunity to use the Camera under actual field conditions (see Figure 7). Schaeberle, alone, represented the Observatory, and he secured volunteers en route and at the mining camp reached by rail and rough wagon road (Schaeberle, 1895).

Precise positional coordinates were obtained from repeated sextant readings at the eclipse site along the eclipse path, and these were used to align the supports for the Camera (ibid.). The chronometer was calibrated at the port and transported to the eclipse site. In positioning the Camera, Schaeberle (ibid.) admitted, "I confess to having asked myself several times, Will the sun's image fall centrally upon the photographic plate at the critical moment?"

Assembly and stabilization of the Camera were accomplished with the utmost care by Schaeberle (Eddy, 1971). The upper end of the slope was excavated two feet deep into broken rock for the lens pier. A three foot pit was excavated into broken rock for the plate holder. The track and plate carriage framework were securely fastened to the ground with a liberal supply of mortar. Guy wires were rigged to the top of all supporting frame posts and anchored firmly to the ground with iron pins. A curtain was attached to the front end of the Camera for wind protection. The ground within the plate area tent was covered in a plaster 'barro' to prevent dust. The lens was positioned very close to the tube material to minimize any stray light leakage into the interior of the tube. Another light trap was arranged by sewing a piece of black fabric on the front of the tube immediately before the lens mount, with a hole left for the lens. A cardboard partition, with a hole, was placed one foot in front of the lens to block off-axis light (Schaeberle, 1895).

In order to collimate the objective lens with the plate-holder, a plane mirror was placed at the slide-holder. The slide plane was collimated by reflecting lantern light from an observer at the top end back to the observer looking down the optical axis and adjusting the plate-holder as necessary. The lens was then collimated in the same manner as for the plate-holder by reversing the positions of the plane mirror and the observer respectively. The final alignment was accomplished by using an eyepiece at the focal plane to view stellar images and after that by exposing a plate at night to record star images (ibid.).

At the time of the eclipse, Schaeberle (ibid.) alone operated the Camera. He commanded the start of all the eclipse instruments whilst viewing the large image present on the plate-holder. Volunteer J.J. Aubertin exclaimed, "God's picture … one grand, overwhelming figure is the symmetrical corona, of a deep, circular margin extending all around into valance or festoons of lovely texture."

The excellent plates, which were developed at the site, revealed prominences and fine detail in the solar corona. Schaeberle (ibid.) declared that the results were a further verification of his coronal theory.

5.2 Akkenshi, Japan: 8 August 1896

Schaeberle traveled to Japan with the Camera for this eclipse, and the program was to be fully photographic in nature with a range of cameras with different apertures and focal lengths. However, the sky was completely covered by clouds and the eclipse was not observed (Campbell, 1894; Holden, 1896).

5.3 Jeur, India: 22 January 1898

For the 1898 Crocker Eclipse Expedition to Jeur, India, a program of coronal photography and coronal spectral studies was planned. A change to another site nearby became necessary due to an outbreak of bubonic plague. The new location lacked a suitable hillside to support the Camera, so W.W. Campbell set forth and constructed two towers to raise the lens and tube of the Camera to an altitude of nearly 51° (see Figure 8). Campbell did most of the work himself after he fired the local lead worker. Mrs Campbell (1898) noted in her diary:

Table 1: Cameras used in the LO direct coronal photography program, eclipse year first used, and their basic optical specifications.

Photographic Instrument	Year First Used	Clear Aperture (inches)	Focal Length (inches)	Focal Ratio (f/)	Image Size On Plate (inches)
Dallmeyer Portrait Lens Camera	1889	6.0	33	5.5	0.3
Clark Equatorial Refractor	1889	6.5	76	11.7	0.7
Barnard's Water Reservoir telescope	1889	2.0	49	24.5	0.5
Schaeberle's Newtonian	1889	18.0	150	8.3	1.4
Schaeberle 40-foot Camera	1893	5.0	482	96.4	4.5
Schaeberle 40-foot Camera	1893	4.0	482	120.5	4.4
Regular "instantaneous" Camera	1898	1.4	11	7.8	0.1
Floyd Camera-Willard Lens	1898	5.0	68	13.6	0.6
Schaeberle 40-foot Camera	1918	6.0	482	80.3	4.5

Table 2: Eclipse dates and LO eclipse site locations, Schaeberle Camera plate emulsion type, plate sizes, and exposure times.

Year	Site	2nd-3rd Contact (m. s.)	Solar Altitude (°)	Plate Type	Plate Size (inches)	Exposures (seconds)
1893	Mina Bronces, Chile	2 51		Seed 26	18x22	½,2,4,8,16,32,24,1/4
1896	Akkeshi, Japan				18x22	Clouds
1898	Jeur, India	1 59.5	51		14x17	
1900	Thomaston, Georgia, USA	1 30				
1901	Padang, Sumatra	6 09		Seed 27	18x22, 14x17 8x10	½,1,2,4,16,40,150,4, 25,8,1,1/2
1905	Alhama, Spain	3 45	55	Seed 27	18x 22, 14x17	½,1,4,8,64,32,24,¼
1905	Aswan, Egypt	2 26		Seed 27	18x22, 14x17	½,1,4,8,64,32,24,¼
1905	Cartwright, Labrador	2 30		Seed 27	18x22, 14x17	Clouds
1908	Flint Island, Pacific Ocean	4 06	74	Seed 27		4,2,32,16,64,32
1914	Brovarỳ, Russia				14x17	Clouds
1918	Goldendale, Washington, USA	1 57.4	45	Seed 30 Process		1/4,4,8,32,¼, + 5 not listed
1922	Wallal, Australia	5 15.5	58			Range from ¼ to 64
1923	Ensenada, Baja California, Mexico	3 34		Seed 30 Process	14x17 8x10	Clouds

Figure 7: The Schaeberle Camera mounted on the hillside in Chile. Schaeberle is standing centre right with his outstretched hand on the framework (Mary Lea Shane Archives).

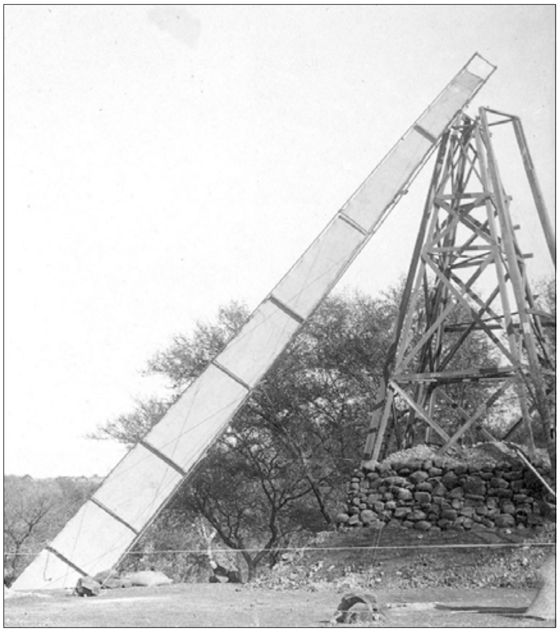

Figure 8: The two towers of the Schaeberle Camera and the rock wall at Jeur (India), with overall height lowered by use of a pit for the plate-holder (Mary Lea Shane Archives).

> He is working from before dawn till after the sun has left the sky. Stones that four men cannot move he lifts with ease. And he is never tired!

The tube end of the Camera was held in place by iron pins driven into the ground, and the tube was then anchored with a system of duplicate wire cables. A nine foot rock wall anchored the bottom of the tower. The day before the eclipse, Campbell (1898) discovered that the guiding tracks and clock had been bumped or tampered with by an unknown person or animal, so he made sure that the camp was well guarded that evening.

On eclipse day the plates made with the Camera were considered excellent and "… as expected …" by Campbell (ibid.). A unique feature on the plates was the presence of coronal streamers, with streamer hoods inclosing the prominences (ibid.). Campbell (ibid.) also remarked that "It is plain that no astronomer was ever more assisted by volunteer observers."

5.4 Thomaston, Georgia, USA: 28 May 1900

On short notice, the LO assembled a Crocker Eclipse Expedition for the May 1900 eclipse, which was visible from the USA; this was attended by W.W. Campbell and C.D. Perrine. The program consisted of cameras for direct coronal photography and a range of spectrographs for chromospheric and coronal studies. For the first time, the LO arranged for time signals to be sent directly by telegraph wires from the United States Naval Observatory (USNO) to the chronograph at the eclipse site. This enabled the astronomers to obtain a precise set of location coordinates for the site (Campbell and Perrine, 1900).

The astronomers were popular with the locals, with the notable exception of the landlord of the eclipse site location whom Campbell (1900a) referred to as a 'terror'. However, the quality of the local food was poor and Perrine became seriously ill, although he did manage to perform his duties on the day of the eclipse.

The plates obtained with the Schaeberle Camera were of good quality, which could not be said for plates taken by other expeditions sited along the eclipse path. In fact, the results from the other parties were so poor that Campbell commented that there seemed to have been a 'hoodo' on this eclipse.

Accordingly, the local people and the Lick Observatory party wasted no time in promoting their success. Campbell also came to the rescue of O. Stone from South Carolina who did not know how to process his plates (Campbell, 1900a; 1900b; 1900c; 1900d; Campbell and Perrine, 1900).

5.5 Padang, Indonesia: 17-18 May 1901

The 1901 Crocker Eclipse Expedition to Padang, in Sumatra, came upon the heels of the death of the late Director of the Observatory, J.E. Keeler. C.F. Crocker had also passed on, but his brother, W.H. Crocker, agreed to fund this expedition and future LO eclipse expeditions. The long duration of totality and the high altitude of the Sun provided ideal conditions for coronal observations. LO staff had one month to make preparations prior to departure, and the voyage out to Sumatra then took seven weeks (Perrine, 1901a; 1901b). In addition to the regular program of direct coronal photography and the making of spectrograms of the solar surface and corona, a search for intra-Mercurial planets was planned (Perrine, 1901b).

The expedition was led by C.D. Perrine, who selected fifteen volunteers to assemble the camp and to man the instruments on the day of the eclipse. Perrine soon faced his first substantial problem at the site when local religious leaders prophesied that the expedition had caused an epidemic in the nearby town of Kampong and threatened to attack the camp. Luckily this did not eventuate (Perrine, 1901b).

The Camera's towers had to be thirty six feet high, (Figure 9), as a pit area could not be dug. The inner and outer towers were constructed of bamboo and covered with thatch. On eclipse day the viewers saw a great comet at totality, while the exposed solar plates revealed valuable coronal detail:

> … clouds of coronal matter were piled up as if by an explosion of the Sun's Surface … The disturbed area appeared to have its origin … near a compact prominence, and masses of matter are shown radiating from it in almost all directions … The whole area resembles the condensations seen in photographs of the *Orion* and other irregular nebulae (Perrine, 1901b).

Perrine was convinced that the observed events demonstrated that the corona is directly linked with other solar phenomena, all needing a concise explanation. Perrine (1901a) summarized the observations with these comments: "The greatest enthusiasm was manifested by all in the preliminary rehearsals as well as in the observations on eclipse day".

5.6 Labrador, Spain and Egypt: 30 August 1905

The Lick Observatory sent expeditions to Labrador, Spain and Egypt to observe the August 1905 eclipse. Parties equipped with Schaeberle type tower cameras were sent to locations separated 2.5 hours apart on the eclipse path in the hope that plates from different sites would yield answers to questions concerning changes in the fine detail within the corona over time.

At Cartwright, a Hudson Bay Company post in Labrador, the expedition under the direction of H.D. Curtis established camp. Only direct coronal photography and an intra-Mercurial planet search would be conducted. The expedition's personnel were subjected to vicious biting flies and hoards of mosquitoes, and a fierce gale arrived and threatened the towers of the 40-foot camera. On the vital day no results were obtained owing to the dense clouds (Curtis, 1905; Campbell, 1904; 1905).

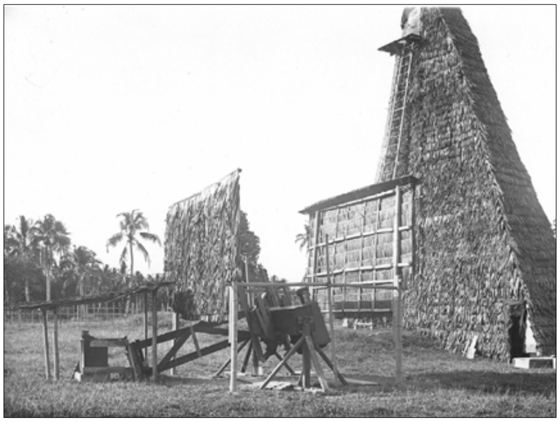

Figure 9: The thatch-covered bamboo towers of the Camera in Sumatra. A flip-top cover protected the other cameras and the spectrograph in the foreground (Mary Lea Shane Archives).

W.J. Hussey led the Egyptian contingent, and they set up camp on the bank of the Nile River at Aswan (Figure 10). Hussey, who received a great deal of assistance from the Egyptian Government, was joined by H.H. Turner from Oxford University. Direct coronal photography, an intra-Mercurial planet search and a single spectrogram of the general spectrum of the corona made up the program. The 40-foot camera was equipped with a 5-inch lens obtained from the USNO (Hussey, 1906; Campbell, 1904). After the eclipse, the plates were safely secured for shipment back to Mt. Hamilton.

The third expedition, to Alhama in Spain, was led by Campbell, who used a series of maps provided by the Madrid Observatory to settle on the location of the observing site. According to Campbell and Perrine (1906), their volunteers consisted of a group of academic professionals who successfully undertook the exceedingly strenuous task of setting up the eclipse instruments, often working in the rain. The original Schaeberle Camera was raised to an elevation of 55° (Figure 11). On eclipse day, the spectrographs were started late as totality began eighteen seconds earlier than anticipated, but excellent plates revealed coronal streamers out to one solar diameter. Upon subsequent examination, the prominences and coronal features were found to be highly structured (ibid.).

Figure 10: The 40-foot Schaeberle Camera on the bank of the Nile in Egypt (Mary Lea Shane Archives).

Figure 11: The Schaeberle Camera at the Alhama eclipse camp in Spain (Mary Lea Shane Archives).

5.7 Flint Island, Pacific Ocean: 3 January 1908

Flint Island, a member of the Line Islands, is a narrow almost inaccessible Pacific atoll, and was selected as the only suitable site for the January 1908 Crocker Eclipse Expedition. It was a logistical challenge getting there and then landing the thirty-five tons of equipment though the rough surf. Campbell's party was joined at the last minute by the Smithsonian Institution, a USNO representative and E.P. Lewis of University of California at Berkeley (Campbell, et al. 1908). Another eclipse party, from Sydney (Australia) and Auckland (New Zealand) also used Flint Island as their observing base. All of the visitors were greeted by biting flies, mosquitoes and giant turtles (e.g. see Figure 12).

A rather ambitious science program included direct coronal photography, a search for intra-Mercurial planets, coronal photometric and polarization studies and a range of spectrographic studies (Campbell, 1908b; 1908c; Campbell et al. 1908; Perrine, 1908; 1909). This was to be the first time that heat radiation from corona studies would be measured on a LO expedition, by guest astronomer C.G. Abbot (1909).

Figure 12: Mrs Campbell posed for her portrait on a giant turtle during the Flint Island expedition (Mary Lea Shane Archives).

Lumber for the Camera's towers was shipped from San Francisco and erected on site, and a 15-inch pit was dug for the plate-holder (Figure 13). Although it rained right up to the moment of totality, a hole then appeared in the clouds and the tarp over the objective lens was quickly uncovered and the eclipse was photographed (Campbell and Albrecht, 1908). The resultant plates were considered excellent, and coronal streamers were recorded out to two solar diameters (ibid.). Campbell (1908b) also observed a particular coronal feature:

> There was a conspicuous conical pencil of radiating streamers [see Figure 14] … whose vortex, if on the sun's surface, would be within the largest sunspot group visible on June 3.

Figure 13: The Schaeberle Camera among the palm trees on Flint Island (May Lea Shane Archives).

5.8 Brovarỳ, Imperial Russia: 20 August 1914

The 1914 Crocker Eclipse Expedition to Brovarỳ in Imperial Russia would become an adventure for the unsuspecting LO group. P.A. Hearst joined Crocker in funding a "… powerful equipped expedition …" which was led by Campbell and H.D. Curtis. The expedition would conduct the same range of coronal studies that was carried out at previous eclipses and would focus on direct photography of star fields in the region of the Sun in order to investigate Einstein's Theory of Relativity. Cameras used for the previous intra-Mercurial planet search were refined for this purpose. Camp life (Figure 15) was described by Campbell and Curtis (1914) as pleasant and delightful.

Unfortunately the eclipse was clouded out, and the LO party then found itself isolated in a Russia that was by now caught up in a national revolution *and* World

War I. Expedition members were forced to flee the war zone and to leave all of their instruments behind. Eventually the equipment found its way to the National Observatory at Pulkowa, and it would remain there for the next four years (ibid.).

5.9 Goldendale, Washington, USA: 8 June 1918

When it was realized that the equipment left in Russia would not arrive back in the USA in time for the June 1918 Crocker Eclipse Expedition to Goldendale (Washington state), instruments were hastily assembled from spare and borrowed components. Its defects forgotten, the old 6-inch Brashear lens was even used in a 40-foot camera (Campbell, 1918a; 1918b). The expedition was under the command of W.W. Campbell, and established itself by invitation on the grounds of the Morgan Estate. Local lumber was acquired to build the towers for the 40-foot camera.

It was cloudy on eclipse day, except for the moment of totality when a hole in the clouds miraculously opened up. Campbell was at the Camera and was so surprised by the number of brilliant points of light caused by surface variations on the Moon that he almost delayed giving the 'Go' command to the expedition members. In the event, the defective lens produced ghosts, which were visible on the processed plates, but Campbell (1918b) noted that "The scientific values of the plates were not reduced in any way." in that they revealed remarkable sheaths of streamers and large prominences covered by hoods of curved streamers. According to Campbell, (ibid.) during totality the atmosphere was tranquil and seeing conditions were magnificent.

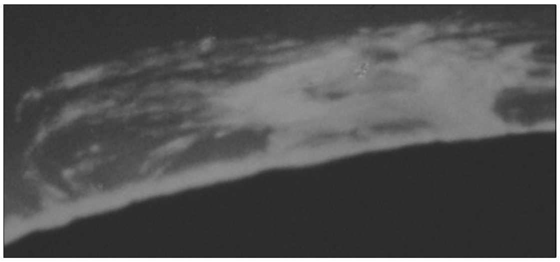

Figure 14: A large quiescent prominence captured on a Schaeberle Camera plate (Mary Lea Shane Archives).

This would be the LO's second chance to secure plates for the verification of the Einstein effect, and the same plates would be used for a continued intra-Mercurial planet search that was all but abandoned by Campbell after the 1908 eclipse (Campbell, 1908a; Curtis, 1919). While the resultant plates were of good quality, it was questionable whether they provided the accuracy needed to validate the Einstein effect (Campbell, 1922b; 1923).

5.10 Wallal, Australia: 21 September 1922

With a high level of support from the Australian Government, Campbell mounted the September 1922 Crocker Eclipse Expedition to Wallal in Western, Australia. Wallal was a sheep and telegraph station situated along Ninety Mile Beach (Figure 16), on the northwestern shores of the Australia continent (Campbell, 1923). The nearby Perth Observatory party, under their Director, Curlewis, would accurately determine the coordinates of the LO position for timekeeping purposes (ibid.). A full complement of instruments would make the trip, in order to continue the coronal studies that had been conducted at previous eclipses. Again, emphasis was placed on the Einstein effect as there seemed to be some continuing doubt about the 1919 eclipse results obtained by Eddington's party (e.g. see Jeffery et al., 1989).

Figure 15: The eclipse instruments at Brovarỳ in Russia (Mary Lea Shane Archives).

The goal of the coronal program for the Schaeberle Camera, now home from Russia, was to secure images for photometric studies of the brightness of the corona. In addition, a search for coronal structure motion would be made by comparing the Wallal plates with those taken with a borrowed LO 40-foot camera by an Adelaide party located at Cordillo Downs, thirty-five minutes away. The plates of the partial phases would be used in the determination of the relative positions of the Sun and Moon (Campbell, 1922a; 1923). The cloth-covered towers of the camera (Figure 17) also provided additional shade for the sensitive Einstein cameras. As daytime temperatures soared, local Aborigines placed branches around the instruments to hold down ground-heat radiation and poured coarse sand to hold the fine dust down. On eclipse day, the Aborigines sprinkled water continuously to cool the surrounding ground (Campbell, 1923). C.E. Adams, the Government Astronomer of New Zealand, took the exposures with the Camera, and obtained excellent results (see Burman and Jeffery, 1990).

Figure 16: Unloading the eclipse freight on Ninety Mile Beach, Western Australia (Mary Lea Shane Archives).

5.11 Ensenada, Mexico: 10 September 1923

The September 1923 Crocker Eclipse Expedition to Ensenada, Mexico, was to be the last time that the LO would set up the Schaeberle Camera (Figure 18) in order to continue the "… systematic accumulation of observation material relating to eclipses of the Sun." (Wright, 1923). As luck would have it, this eclipse was clouded out.

Figure 17: At Wallal, protective cloth covered the Schaeberle Camera and the Einstein camera (Mary Lea Shane Archives).

5.12 Camptonville, California, USA: 28 April 1930

The April 1930 Crocker Eclipse Expedition to Camptonville, California, proved unusual in that totality would be last a mere 1.5 seconds, so the Schaeberle Camera could not be used. LO Director, R.G. Aitken, decided to set up three stations across the predicted width of the umbral shadow just in case the site position calculations were incorrect, but the computations turned out to be very accurate (Aitken, 1930; Moore, 1930).

5.13 Fryeburg, Maine, USA: 31 August 1932

For the August 1932 Crocker Eclipse Expedition to Fryeburg in Maine, Aitken selected J.H. Moore to conduct a program that would continue the systematic accumulation of observations of the solar corona and chromosphere. A refined group of moving-plate and jumping-film spectrographs was meant to record chromospheric and coronal spectra.

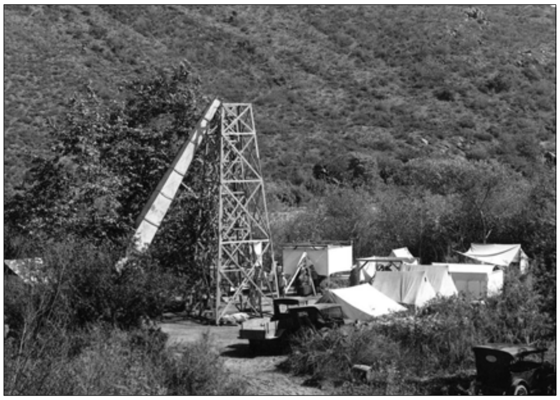

Figure 18: The eclipse camp in Ensenada, Mexico (Mary Lea Shane Archives).

Direct coronal photography would continue, however, without the Schaeberle Camera. W.H. Wright and Aitken determined that the corona images obtained with the 5-foot and 15-foot Einstein cameras were of sufficient quality to make use of the 40-foot Camera unnecessary. Photographic emulsions were now fine grained permitting large scale enlargements to be made (Wright, 1932).

The Schaeberle Camera was eventually transferred to the University of Michigan and was used under the direction of H.D. Curtis, a former employee of the Lick Observatory (see Eddy, 1971).

6 DISCUSSION

6.1 Fate of the Mechanical Theory of the Solar Corona

The Schaeberle 40-ft Camera was the mainstay of the Lick Observatory solar eclipse program for thirty years and provided a succession of excellent photographs of the solar corona, but what of the theory that inspired it?

As soon as the 40-ft Camera was operational Schaeberle used photographs obtained with it to evaluate his theory. Upon examining images of the 1893 eclipse, he discovered that his theory needed to be developed further as his initial work only pertained to the ideal case of streamers uniformly distributed in sunspot zones (Schaeberle, 1893). Unfortunately, these ideal occurrences were found to be the exception. Schaeberle (ibid.) described structure in the equatorial regions which had the appearance of two opposite magnetic poles on the Sun's equator, but still defined by gravitational forces.

After the appearance of the Lick Observatory's report on the 1893 eclipse (Schaeberle, 1895), no further publications by Schaeberle about his mechanical theory appeared in print, with one minor exception. In an article submitted to the *San Francisco Examiner* newspaper on 19 April 1898, Schaeberle confirmed his 1893 claims:

> All the evidence given by the prominences leads to the conclusion that this matter is in rapid motion and that instead of rising from the sun's surface in irregular masses, the structure is just as definite as is found to be the case in the coronal streamers. In other words, every prominence and protuberance visible during this eclipse was made of individual streams of matter apparently moving in elliptical orbit with the sun's center as their foci. The almost certain conclusion appears to be that all prominences are of the same general structure. (Schaeberle, 1898).

At Campbell's invitation, J.A. Miller visited the Lick Observatory during the summer of 1909, and accessed the plates obtained during the 1893 through 1905 eclipses, in order to evaluate Schaeberle's theory. Whilst agreeing in principle with many of the points made by Schaeberle, he begged to differ on at least a couple of points. For instance, he suggested that radiant pressure generated by disturbances may play a part in explaining Schaeberle's observations (Miller, 1911).

A little later, W.W. Campbell (1918c) wrote that coronal matter could be transported by volcanic force (as predicted by Schaeberle), radiation pressure, or a combination of these and other unknown forces. However, the arrangement of coronal matter in well-defined streamers may result from the Sun's magnetic properties, as predicted by Bigelow and others who felt that local magnetic fields were in control.

Surprisingly, Campbell does not mention George Ellery Hale's work at Mt. Wilson Observatory, and specifically his 1908 discovery of the Zeeman splitting of spectral lines associated with sunspots. It was primarily the research conducted by Hale that sounded the death-knell for Schaeberle's mechanical theory of the solar corona, and it is relevant to point out that after the detection of Zeeman splitting the Schaeberle Camera was only used successfully for two further eclipses (in 1918 and 1922).

6.2 The Scientific Contribution of the 40-ft Camera: An Introductory Note

Excellent photographs of coronal form and structure were obtained with the Schaeberle Camera on many of the Lick Observatory expeditions (e.g. see Figures 19 and 20), and sometimes these were combined with spectral and polarization data obtained with other LO equipment.

The specific role that the Schaeberle Camera played in the overall Lick Observatory solar research program will form part of another paper, so we will not discuss it here other than to highlight three particularly noteworthy accomplishments:

1. A contour map of the solar corona was generated from photographs taken during the 1893 eclipse, and this was then compared with maps made during the two 1889 eclipses (Schaeberle, 1895).
2. Studies of precise coronal brightness were highly suspect up to and including the 1905 eclipses, because of problems with the stability of the Carcel standard lamp that was used to calibrate the photographic plates (Osterbrock, et al., 1988). This problem was then solved and during the 1905 and 1908 eclipses standardized plates were used with the Schaeberle Camera to determine the levels of intrinsic actinic light in different regions of the solar corona (Perrine, 1908). Later these measurements were compared from eclipse to eclipse.
3. Plates obtained during the 1922 Wallal eclipse were used in the search for coronal motion. These same plates were later examined by J.A. Eddy and J. Goff (1971) when preparing their atlas of the white light corona, and they were again used when Eddy (1973) was researching evidence for a neutral sheet within the corona.

Finally, it is interesting to note that at no time did any of the LO staff use coronal features displayed on the Schaeberle Camera plates to investigate magnetic models of the solar corona.

7 CONCLUDING REMARKS

It was the merging of the highly regarded talents of three men that successfully launched the acclaimed direct photography program of the LO eclipse expeditions. E.E. Barnard came to the Observatory as a skilled photographer, S.W. Burnham was a photographic emulsion and processing expert and J.M. Schaeberle was a skilled telescope-maker with a background in optical theory. The three of them made for a powerful team.

From its inception, the Schaeberle Camera, with its novel moving plate-holder, produced fine eclipse images of large size and continued to produce outstanding plates until taken out of service at the LO after eleven expeditions. Other institutions would build similar cameras modeled after the Schaeberle Camera and achieve equally good results.

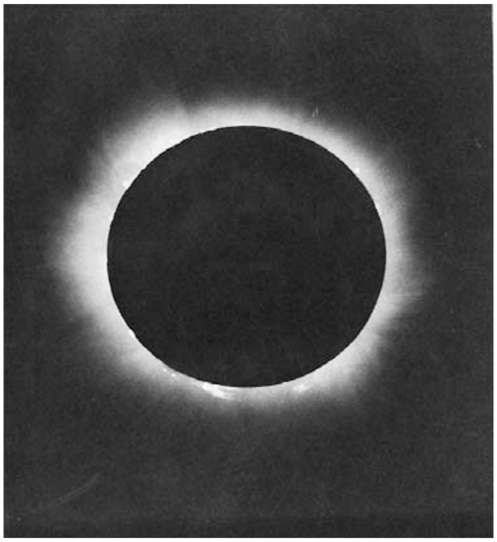

Figure 19: Schaeberle Camera photograph of the 1893 solar eclipse (Mary Lea Shane Archives).

The LO solar eclipse expeditions can be considered a bold adventurous project for a young cash-strapped institution. An eclipse expedition to a distant country, in the late nineteenth century, was a non-trivial challenge. The Schaeberle Camera and other instruments had to be readied and tested at home, with some indication that the observations would yield the intended research results. Permissions were required in advance for the transport of equipment and personnel through foreign lands. The transportation of fragile instruments by ship, rail and wagon—often under rough conditions—was always charged with at a high-level of risk. The establishment of an eclipse station, where staff and volunteers would live for some time, needed careful thought and planning. Then, during those brief moments of totality, the weather needed to cooperate, observers needed to successfully perform their assigned tasks on time, and the Schaeberle Camera and other instruments needed to function as designed. There were also other unforeseen issues that one could not prepare for but which had to be resolved.

Figure 20: Schaeberle Camera photograph of the 1898 solar eclipse (Mary Lea Shane Archives).

Although the Lick Observatory's ambitious 40-year solar eclipse program was a resounding success in that the Crocker Expeditions provided invaluable new information on prominences and the corona, Moore was moved to point out in 1933 that in spite of these achievements it still was not possible to adequately explain all observed coronal phenomena. Clearly a means of successfully viewing the solar corona outside of eclipse were called for, and with the advent of the coronagraph this became a reality.

8 NOTES

1. This January 1889 solar eclipse launched the Lick Observatory's coronal science research program. J.E. Keeler began by repeating the observations of C.S. Hastings who, in 1883, had theorized that the light of the corona was a diffraction effect caused by the Moon (Holden, 1889c). Keeler's observations were considered as further proof that Hastings was wrong. At the same time Holden et al (1889) suggested that branching coronal forms were due largely to the presence of streams of meteorites drawn in towards the Sun.

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SA = Mary Lea Shane Archives, University of California at Santa Cruz

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ANCIENT GREEK HELIOCENTRIC VIEWS HIDDEN FROM PREVAILING BELIEFS?

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Abstract: We put forward the working hypothesis that the heliocentric, rather than the geocentric view, of the Solar System was the essential belief of the early Greek philosophers and astronomers. Although most of them referred to the geocentric view, it is plausible that the prevalent religious beliefs about the sacred character of the Earth as well as the fear of prosecution for impiety (*asebeia*) prevented them from expressing the heliocentric view, even though they were fully aware of it. Moreover, putting the geocentric view forward, instead, would have facilitated the reception of the surrounding world and the understanding of everyday celestial phenomena, much like the modern presentation of the celestial sphere and the zodiac, where the Earth is at the centre and the Sun makes an apparent orbit on the ecliptic. Such an ingenious stance would have set these early astronomers in harmony with the dominant religious beliefs and, at the same time, would have helped them to 'save the appearances', without sacrificing the essence of their ideas.

In Hellenistic and Roman times, the prevailing view was still the geocentric one. The brilliant heliocentric theory advanced by Aristarchos in the early third century B.C. was never established, because it met with hostility in Athens—Aristarchos was accused of impiety and faced the death penalty.

The textual evidence suggests that the tight connection which existed between religion and the city-state (*polis*) in ancient Greece, and which led to a series of impiety trials against philosophers in Athens during the fifth and fourth centuries B.C., would have made any contrary opinion expressed by the astronomers seem almost a high treason against the state.

Keywords: heliocentric Solar System, geocentric Solar System, spherical Universe, impiety, 'save the appearances'

1 INTRODUCTION: THE HELIOCENTRIC VERSUS THE GEOCENTRIC VIEW IN ANCIENT GREEK ASTRONOMY

In this paper we propose the working hypothesis that the actual belief of the early Greek philosophers and astronomers was the heliocentric, rather than the geocentric, view of the Solar System.[1] As an indication of the heliocentric view of the world we take the assumption of a spherical Universe, which is considered as "… the most fundamental assumption of Greek astronomy." (Evans, 1998: 75, cf. 216-219). It is possible that the idea of a spherical cosmos existed already among the Ionian philosophers, at least from Anaximander onwards (Kahn, 1960: 92-94; Vernant, 1983: 180, 183, 187, 190-211),[2] but perhaps even much earlier, and that it was not expressed clearly, because it ran counter to the conventional religious views and/or because it aided the perception of everyday celestial phenomena.

The earliest evidence about the astronomical knowledge of the ancient Greeks dates from the eighth century B.C. It is found in the epic poems of Homer and Hesiod (Aveni and Ammerman, 2001; Dicks, 1970: 27-38; Evans, 1998: 3-5; Papathanassiou, 2007), while some archaeological correlates to this written evidence have been pointed out recently (Coucouzeli, 2006; Dimitrakoudis et al., 2006). Astronomical knowledge appears to have been used in eighth century B.C. Greece for the purposes of cultivation, navigation, calendar regulation, worship and even politics.

However, astronomical interest in Greece seems to go much further back in time, to the second millennium B.C. An important source of information in this respect is the Orphic texts (*Orphica*). Although these texts were recorded and translated at the time of Peisistratos (sixth century B.C.) or, mostly, in later times (Kern, 1922; West, 1983), they seem to have existed for many centuries. According to Chassapis (1987), the Orphic Hymns were formulated in the period between 1841 and 1366 B.C. (i.e. during the Minoan and Mycenaean times), since they seem to refer to the vernal equinox and the summer solstice, when these took place in the Taurus and Leo constellations, respectively, up to 1841 B.C., as well as to the phenomenon of the equality of the summer and winter seasons, which occurred around 1366 B.C. In addition, the Orphics appear to have known about the sphericity of the heavens as well as the two basic postulates of the heliocentric theory, according to which: a) the Earth is spherical and rotates around its own axis; and b) the movement of the Earth around the Sun causes the occurrence of the four seasons. In fact, the Orphics were teaching about the equal duration of the Earth's rotation and of the apparent motion of the celestial sphere around the same axis (cf. Orpheus saying to his son Musaeus: "…as this (the Earth), which is round, rotates in equal time round its own axis."—Aristobulus' fragment '*Diatheke*' or 'Testament' from his *Explanation of the Mosaic Law* recorded in Eusebius, *Praeparatio Evangelica*, 13, 12), and they accepted the Sun explicitly as the centre of attraction, around which the Earth describes an ecliptic orbit (*dromos*, i.e. 'way'). They also appear to have introduced the notion of the zodiac circle, the names of the constellations, etc. (see also Ovenden, 1966; Papathanassiou, 1991; 2007).

Turning to archaeology, there is now increasing evidence concerning the astronomical interest of the Minoans and Mycenaeans, thanks to numerous archaeoastronomical studies, which were conducted during the last decade in peak sanctuaries, palaces and tombs on Crete (Blomberg and Henriksson, 1996; 2000; 2003; 2005). The study of orientations of buildings has shown that the sunrise and sunset positions at the four solar stands, the full Moon and the heliacal setting of Arcturus, were all taken into account by the ancient inhabitants of Crete since the Early Minoan Age in an effort to establish a physical relationship between themselves and the sky for the sake of keeping a calendar, for navigation, and perhaps also for religious and political purposes (Henriksson and Blomberg, 1996: 113). Apart from orientations, a number of ceramic figurines representing animals, humans or parts of the human body from two peak sanctuaries on Crete have been interpreted, on the basis of ancient written accounts (e.g. Aratos), as having had an astronomical significance related to the recognition of the zodiac (Blomberg, 2000). Finally, a number of Linear B tablets from Pylos and Knossos, dating from later Mycenaean times, record calendar months (Papathanassiou, 2007).

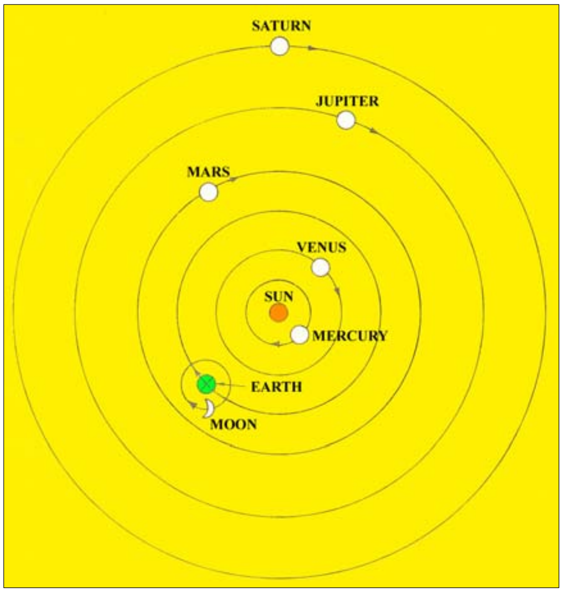

Figure 1: The heliocentric system of Aristarchos.

It is therefore possible that Greek astronomy slowly built upon this earlier knowledge going back to the Minoan and Mycenaean civilizations, which were as developed as those of Egypt and Babylon. This knowledge would have been transmitted from generation to generation into the so-called 'Dark Ages' (ca. 1100-700 B.C.) and into Archaic and Classical Greek times (e.g. Liritzis and Vassiliou, 2003). Observations may have been carried out by means of various types of sighting aids and measuring devices, such as the *gnomon*, the *klepsydra*, the *polos* or the *parapegma*, perhaps also including simple forms of armillary spheres (Dimitrakoudis et al., 2006) or even wooden tubes (containing lenses?)[3] in the manner of a primitive *dioptra* (cf. Evans and Berggren, 2006: 27-42).

To return to our basic assumption of a spherical Universe, it is worth pointing out that the symbolism of the circle was pre-eminent in traditional Greek cosmological thought. The two-dimensional circular shape was considered as the most perfect and sacred, and it must reflect some concept of the wider Universe as a sphere, the most beautiful and divine three-dimensional shape (Edmunds, 2006; cf. also Geminos' *sphairopoieia*, i.e. the spherical construction of the cosmos according to nature, in his *Introduction to the Phenomena*—see Evans and Berggren, 2006: 51-53). The Greek philosophers' consideration of the sphere as the shape of the divine substance is attested as early as Xenophanes (sixth century B.C.); it is further elaborated by Plato (*Timaeus*, 33b) and it is also encountered in Aristotle (*On the Heavens*, II. 286a10: "But such is the heaven, viz. a divine body, and for that reason it possesses the circular body which by nature always moves in a circle."; Leggatt, 1995: 227; cf. Vernant, 1983: 183). However, the symbolism of the circle and the sphere may be a lot older. As we have seen, the accounts attributed to the Orphics refer to a spherical Universe with revolving celestial bodies and a solar centre. As far as the archaeological evidence is concerned, it is worth mentioning that a circle representing the two celestial hemispheres connected with the Dioskouroi, Castor and Pollux, seems to appear on a cryptographic seal dating from ca. 750-700 B.C. (Coucouzeli, 2006), while a series of votive artefacts, dating from ca. 750-480 B.C., may well represent celestial spheres with meridians and sometimes also an equator (Dimitrakoudis, et al., 2006).

The astronomical views and discoveries of the ancient Greek philosophers and astronomers, in particular those regarding the relative positions of the Earth and the Sun, are well-known and they date from the earlier historical era of Thales (ca. 624-547 B.C.) to the later times of Ptolemy (A.D. 87-150) (see, for instance, *Aristarchos the Samian*, 2003; Dicks, 1966, 1970; Heath, 1913; 1932; Kahn, 1960; Kirk, Raven and Schofield, 1983; Lloyd, 1970; 1973; 1991; Noack, 1992). All of them held the picture of a spherical Universe (see Dicks, 1966: 30) and, as we will show below, most of them seemed to artificially consider the Earth at the centre of the Universe. Indeed, throughout Greek cosmological thought, as a general rule, man's position in the Universe is considered as a privileged one. Nevertheless, the doctrine that the Earth we inhabit occupies the centre of the Universe *was* contested. Some philosophers and astronomers even went as far as setting the Sun (or a fiery substance reminiscent of the Sun) in a central position, at the risk of being subjected to public anathema. A small number of relevant views is discussed below.

Thales (ca. 624-547 B.C.) conceived the Earth as a disc at the centre of the Universe, floating on water (an implication of the celestial equator?). He seems to have predicted the total solar eclipse on 28 May 585 B.C.—a major achievement, which cannot be explained on the basis of the existing evidence about his knowledge—and to have produced a model of the celestial globe. Thales may well have known about the actual movements of the celestial bodies, but could not express his views openly in opposition to existing religious beliefs (the story about Thales' prediction of the solar eclipse has been widely discussed, e.g. by O'Grady, 2002). Anaximander (ca. 610-546 B.C.) envisaged the Earth suspended at the centre of a spherical Universe, he distinguished between fixed stars and planets, and he made the first attempt at a 'mechanical model' of the Universe, which appeared as a revolving sphere (Lloyd, 1970: 17). Anaximenes (ca. 585-525 B.C.) gave a privileged status to the Sun against the other celestial bodies in the spherical cosmos, arguing that it gave light to the Moon. A central role was also given to the Sun by Heracleitos (ca. 540-480 B.C.), who postulated that the celestial orbit had characteristics related to a constant law of cosmic fire (this is vaguely reminiscent of Newton's Law of global attraction). Pythagoras (ca. 572-495 B.C.) pictured a spherical Earth kept at the centre of the world by its equilibrium and containing a fiery core, the central 'hearth' ('Hestia'); he also advanced the idea of the revolution of the cosmic sphere on an axis passing through the centre of the Earth and he identified the five zones of the Earth (which were also adopted slightly later by Parmenides, ca. 504-450 B.C.). Oinopides of Chios (ca. 490-420 B.C.) identified the ecliptic as the oblique orbit of the Sun with respect to the celestial equator, which led to the definition of the four solar stands and the four seasons. Anaxagoras maintained that the Sun and all the stars in the spherical Universe are fiery stones, while the Moon is made of earth and receives its light from the Sun, thus providing the clearest explanation of the solar and lunar eclipses. The intriguing theory of Empedocles (ca. 484-424 B.C.), according to which there are two suns, a real or archetypal one (the fire of the Earth in the centre) and an apparent one (the visible Sun), which is a reflection of the archetype on a crystal bowl, a theory that stresses the Sun's extrapolated projection opposite the Earth, probably implies knowledge of the obliquity of the ecliptic, but also a representation of the Sun revolving around the Earth, which would have served pedagogical purposes.

As for the Pythagoreans, the evidence is somewhat confused, but they essentially denied that the Earth is at the centre of the Solar System. Some inklings of a heliocentric view of the world appear in the theory of the Pythagorean Philolaos (ca. 480-405 B.C.), who posited the existence of a central fire ('Hestia' or 'Tower of Zeus'), around which revolve the celestial bodies, including the Earth (see Gavroglou, et al., 2003; Huffman, 1993).

Plato (ca. 427-347 B.C.) adopted the Pythagorean theory of the circular motion of the Earth ('winding round' – *eillomenen*) up and down on the axis of the Universe. He assimilated the latter with the spindle of Necessity, which in his view consisted of eight nested whorls representing successively the circle of the fixed stars ('circle of the Same') and the circles of the Sun, the Moon and the five planets ('circles of the Other'). In his mystic vision of the Universe, Plato also distinguished between two kinds of motion, the motion along the equator (or 'circle of the Same') and the motion along the ecliptic (or 'circle of the Other'). Plato uses an obscure language probably in order to avoid expressing the heliocentric view in a straight manner. Nevertheless, it is clear that he considers a spherical Earth revolving around itself and around the Sun, and that he describes a very complex cosmological model, which combines the shared characteristics of an articulated sphere, a planar astrolabe and the forerunner of an orrery.

In an interesting passage given to us by Aristotle (*On the Heavens*, II. 293a17-293b1) it is stated that, besides the Pythagoreans, "… many others …"—whom some assume to have been Plato himself and/or a group associated with Plato's Academy (see Leggatt, 1995: 253-254)—held the view that fire occupies the centre of the Universe and the reason they gave is that fire, rather than earth, is the most honourable thing and therefore deserves the most honourable place. Up until the time of Aristotle, therefore, there were astronomers who had no qualms about abandoning the traditional view, which gave the Earth the central position. Their motivation may have been a purely religious or symbolic one (Lloyd, 1970: 27; 1991: 157). As for Aristotle himself, he states that the centre is the reference point of all motions and that the (spherical) Earth happens to be at the centre (*On the Heavens*, II. 296a24-298a15).

On the other hand, there were those who clearly adopted the geocentric view of the cosmos and even introduced additional mechanical models regarding the spherical Universe in order to explain the planetary movements, to save the appearances and to offer a theory on the real nature of the celestial bodies. These are, for instance, Eudoxos of Cnidos (ca. 408-355 B.C.), who, while being influenced by the cosmological speculation of Pythagoras and Plato (Goldstein and Bowen, 1983) suggested that the celestial bodies revolve around the Earth upon a series of interconnected concentric spheres turning on their own axes; Aristotle (384-322 B.C.) with his theory of crystalline interconnected spheres in a uniform circular motion around the Earth; Heraclides of Pontos (ca. 387-312 B.C.), who argued that the alternation of day and night is caused by the eastward rotation of the Earth on its axis once a day, rather than by the rotation of the heavenly bodies around the Earth, and who probably also put forward a circumsolar theory of the planets Venus and Mercury (but see Eastwood, 1992); and, later on, Apollonios of Perge (ca. 262-190 B.C.), who introduced the geocentric model of the epicycles and eccentric circles, which was enriched and expanded by Hipparchos (ca. 190-120 B.C.) and Ptolemy (A.D. 87-150).

However, the current of thought denying geocentricity had not died out. Thus, Aristarchos of Samos (310-230 B.C.) was the first astronomer to put forward a heliocentric astronomical theory in an explicit and unquestionable manner (Heath, 1932; Noack, 1992). Archimedes wrote about him:

> His hypotheses are that the fixed stars and the sun remain unmoved, that the earth revolves about the sun in the circumference of a circle, the sun lying in the middle of the orbit … (*Psammites*, I. 4-7; see Figure 1).

Nevertheless, the striking hypothesis advanced by Aristarchus met with great hostility in Athens and—with the sole exception of Seleucos of Seleucia, who espoused it vividly more than a century later—does not seem to have created any solid following (Heath, 1913: 305-307; Lloyd, 1973: 57-58; 1984, 276; 1991: 367 n.40; Noack, 1992: 4).

It seems, therefore, that throughout the history of ancient Greek astronomy theories supporting a geocentric or a heliocentric (or at least a 'fire-centred') world co-existed in opposition to each other at any one time (Thales vs. Pythagoras, Oinopides of Chios vs. Philolaos, Apollonios of Perge vs. Aristarchos of Samos, Hipparchos vs. Seleucos). Nevertheless, the geocentric view of the Universe prevailed throughout Hellenistic and Roman times (Figure 2), whereas the heliocentric view was abandoned, only to be rediscovered by Copernicus in the sixteenth century.

The reasons why geocentrism prevailed are complex, but in the following two sections we will explore two of what might have been among the main reasons:

a) heliocentrism, as the true system of the world, was conceived from a purely philosophical point of view; it co-existed with geocentrism, but it was obscured and carefully hidden from the predominant religious beliefs from fear of persecution for impiety; and
b) geocentricism, cleverly conceived as the Earth-centred celestial sphere, explained in a convincing manner the motions of the celestial bodies, and aided in the determination of calendric time (especially prior to Eudoxos, who is largely responsible for turning astronomy into a mathematical science—see Goldstein and Bowen, 1983), as well as in the prediction of weather phenomena by means of the risings and settings of the fixed stars or constellations, something that heliocentricity could not help with.

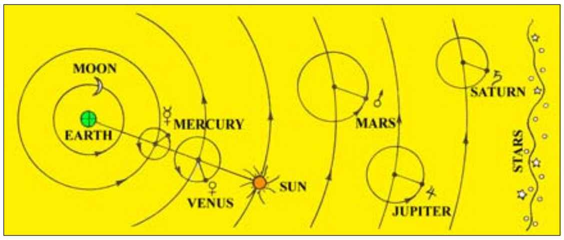

Figure 2: The geocentric system of Ptolemy.

2 IMPIETY (*ASEBEIA*) AND ANCIENT GREEK ASTRONOMY

In the previous section, it was mentioned that the heliocentric theory of Aristarchos of Samos met with strong opposition in Athens. Indeed, Athens does not seem to have been as tolerant and open-minded with regard to deviant religious actions and opinions, as some romantic views about Athenian democracy want us to believe (Cohen, 1991: 211, 215; Dodds, 1951: 189-190, 201 n.63; Garnsey, 1984; Price, 1999: 67 f.). As the most important city of the Greek world, Athens became a great intellectual centre in the second half of the fifth century B.C. It attracted many philosophers and sophists, who formed part of the so-called 'Enlightenment' movement, which drew its origins in mid-sixth century B.C. Ionia (Xenophanes, Heracleitos). Especially, Pericles attracted around him a circle of intellectuals, the most eminent among whom was Anaxagoras, who introduced Ionian philosophy into Athens. The free, rational thinking of all these intellectuals about the gods and the world was a great challenge to traditional Athenian religion. It brought about atheism and it was checked by a series of trials (Derenne, 1930; Dodds, 1951: 179-206; Garland, 1994: 97-102). Between 432 B.C. and the end of the fourth century B.C., a series of philosophers, including astronomers, were prosecuted for impiety because of their 'blasphemous' beliefs (Table 1), besides other people (intellectuals or not). As Dodds (1966: 189) commented: "The Great Age of Enlightenment was also, like our own time, an Age of Persecution." Impiety (*asebeia*) referred both to sacrilegious actions and to the expression of scandalous beliefs concerning the gods (Cohen, 1991: 203-217; Derenne, 1930: 9-12, 217-245; Price, 1999: 82). It was considered a major crime and it was punished by death or perpetual exile.

All these impiety trials started with the introduction of the famous law or decree of Diopeithes (432 B.C.), which stated that "… public accusation should be laid against persons who did not believe in gods or who taught doctrines regarding the heavens." (Plutarch, *Pericles*, 32. 1). This decree was especially designed by the seer Diopeithes to eliminate his main rival, the philosopher and astronomer Anaxagoras. As a professional seer, Diopeithes was fighting for the preservation of the traditional religious beliefs, since his own craft assumed that the sky was replete with divine omens (Derenne, 1930: 19-24; Garland, 1992: 139-141, 205f.; 1996: 94; MacDowell, 1978: 200-201). Anaxagoras was prosecuted "… for saying that the sun is a stone and the moon is made of earth." (Plato, *Apology*, 26d). This was a very shocking idea indeed, given that ancient Greek religion (like any ancient religion) regarded the heavenly bodies and the heavens themselves as gods (Vegetti, 1995: 277f.; Vernant, 1974: 104-112; 1983: 197). Thus, Plato, arguing against the atheists about the existence of gods, says that

> … all the Greeks and barbarians, under all conditions of adversity and prosperity, directed [their prayers] to [the sun and the moon], not as though they were not gods, but as though they most certainly were gods beyond the shadow of any doubt. (Plato, *Laws*, 887e).

And Aristotle writes on this subject:

> Our forefathers in the most remote ages have handed down to their posterity a tradition, in the form of a myth, that [the heavenly] bodies are gods, and that the divine encloses the whole of nature. (Aristotle, *Metaphysics*, 12, 8).

Anaxagoras appears therefore to have been prosecuted, because he dared reduce the celestial divinities into stones and earth (Derenne, 1930: 23-25), although some of his contemporaries saw the case as a direct consequence of his friendship with Pericles (Derenne, 1930: 23-25; Plutarch, *Pericles*, 32). Eventually, Anaxagoras was not executed, but fled Athens with the assistance of Pericles (Derenne, 1930: 39-41; Plutarch, *Pericles*, 32).

Around 416 B.C., Protagoras, the sophist, was brought to trial for impiety, accused for his impious book *On the Gods*, as well as most probably for his astronomical theories. Protagoras escaped death, either because he was banished or because he fled before his trial. After his exile or escape, all the copies of his book were burnt in the public square, the *agora* (Derenne, 1930: 46-55)—this was the first public burning of a book in history!

In a significant passage, Plutarch talks about Anaxagoras and Protagoras, as proof of the Athenians' aversion towards natural philosophers and astronomers:

> The first man to put in writing the clearest and boldest of all doctrines about the changing phases of the moon was Anaxagoras. But he was no ancient authority, nor

> was his doctrine well-known, but it was still under seal of secrecy and circulated among a few people only, who received it with a certain caution, rather than with implicit confidence. For there was widespread intolerance of natural scientists and "star-gazers", as they were called at the time, on the grounds that they reduced the divine to irrational causes, blind forces and necessary incidents. Hence it was that Protagoras was banished and Anaxagoras cast in prison and rescued with difficulty by Pericles, and Socrates, though he had nothing whatever to do with such matters, nevertheless lost his life, because of philosophy. (Plutarch, *Nicias*, 23, 2-3).

The memory of Anaxagoras' trial must have been still very vivid in Athens in 399 B.C., the time of the most famous impiety trial, that of Socrates (Cohen 1991: 213-215; Derenne, 1930; Stone, 1989). Prosecuted "… for not believing in the gods of the city-state, but in other new divinities ..." (Plato, *Apology*, 24b; cf. Diogenes Laertius 2. 40), the philosopher, in his defense, refuses to be associated with the astronomers "… because those who hear them think that men who investigate these matters do not even believe in gods." (Plato, *Apology*, 18c), thereby disclaiming any knowledge of astronomy attributed to him by Aristophanes in the *Clouds* (423 B.C.); a little later in his apology, Socrates denies that he is a complete atheist and affirms that he does "… believe that the sun and the moon are gods, like all the other people do ...", unlike Anaxagoras, implying that the astronomers' beliefs *do* support the accusation of impiety (Plato, *Apology*, 26d).

Xenophon expresses even more clearly the opinion of Socrates about Anaxagoras and the astronomers, in general, as reckless atheists (talking about *hybris*), when he declares that

> With regard to the phenomena of the heavens, [Socrates] disapproved strongly of attempts to work out the machinery by which the god operates them; he believed that their secrets could not be discovered by man, and that any attempt to search out what the gods had not chosen to reveal must be displeasing to them. He said that he who meddles with these matters runs the risk of losing his sanity as completely as Anaxagoras, who took an insane pride in his explanation of the divine machinery … When [Anaxagoras] pronounced the sun to be a red-hot stone, he ignored the fact that a stone in fire neither glows nor lasts long, whereas the sun-god shines with unequalled brilliance for ever. (Xenophon, *Memorabilia*, IV, 7, 6f.; see also Liritzis, 2003).

Anaxagoras and his astronomical doctrine was also attacked by Plato, who alludes to him in his *Laws*, when he makes the Athenian say:

> But as to our younger generation and their wisdom, I cannot let them off when they do mischief. For do but mark the effect of their words: when you and I argue for the existence of the Gods, and produce the sun, moon, stars, and earth, claiming for them a divine being, if we would listen to the aforesaid philosophers we should say that they are earth and stones only, which can have no care at all of human affairs, and that all religion is a cooking up of words and a make–believe. (*Laws*, 886d-e).

Immediately afterwards, Plato declares such philosophers to be "… unholy men … impiously disposed …" (*Laws,* 887a), and therefore people who would be liable to "… be punished with death …" by the impiety law of his ideal State (*Laws*, 910c-d; Cohen, 1991: 216-217; Derenne, 1930: 248-252).[5]

The disapproval and distrust of the astronomers (and their supporters) on religious grounds was widespread in Classical Athens (Dodds, 1951: 201 n64). In the *Laws* (967a), Plato writes that people "… imagine that those who study [the heavenly bodies] in astronomy … become atheists through observing … that all things come into being by necessary forces …" And he continues:

> … all that moves in the heavens appeared to them to be full of stones, earth and many other soulless bodies … These were the views which … caused them many charges of atheism and much antipathy, and which also incited the poets to abuse them by likening philosophers to 'dogs howling at the moon. (*Laws*, 967c).

The famous orator Gorgias, in his display speech on the power of rhetoric, said:

> To understand that persuasion, when added to speech, is wont also to impress the soul as it wishes, one must study: first, the words of Astronomers who, substituting opinion for opinion, taking away one but creating another, make what is incredible and unclear seem true to the eyes of opinion … (*Gorgias Encomium of Helen*, 13; Sprague, 1972: 50-54).

Table 1: Impiety trials against philosophers in ancient Athens.*

Date (BC)	Name	Accusation	Verdict
432	Anaxagoras	For saying that the Sun is a stone and the Moon is made of earth.	Death
ca. 416	Protagoras	For his impious book *On the Gods* and for his astronomical theories.	Death or exile
399	Socrates	(Amongst others) 'For not believing in the gods of the city-state, but in other new divinities.'	Death
323	Aristotle	For giving divine status to his father-in-law Hermias, thereby introducing new gods.[4]	(Fled before trial.)
317-307	Stilpon	For claiming that Athena of Phidias is not a god.	Exile
317-307	Theodoros	For claiming that a high-priest of the Eleusinian Mysteries was impious.	Exile
316	Theophrastos	Unknown	Acquitted for lack of evidence

* Sources: Bruyn (1995) and Derenne (1930), where all the references to the ancient texts are cited.

Even more vehemently, the tragedian Euripides declared:

> Has not the man … who apprehends god cast far away the crooked deceits of those who observe the heavens? Their poisonous tongue, although it possesses no way of

> knowing, talks at random of invisible things. (Derenne, 1930: 24; Nauck, 1964: Frag. 913).

And Eupolis, in his comedy *Kolakes* (421 B.C.), made fun of Protagoras in these words: "… that man, who boasts like a criminal about celestial phenomena, while eating the things that come from the earth." (Diels 1907, Volume II: 530, 14-16).

About one hundred and fifty years after the trial of Anaxagoras, ca. 286 B.C., the astronomer Aristarchos of Samos, the proponent of the revolutionary heliocentric theory, living in Athens, appears to have been accused of impiety by the head of the Stoic school at Athens, Cleanthes (Derenne, 1930: 215; Heath, 1913: 304; Lloyd, 1973: 58; 1991: 157 n.46; Noack, 1992: 4), who even wrote a book entitled *Against Aristarchos* (Diogenes Laertios, 7, 174). In the words of Plutarch:

> Cleanthes thought that the Greeks ought to lay an action for impiety against Aristarchos the Samian on the ground that he was disturbing the Hearth of the Universe [i.e. the Earth], because he sought to save the appearances by assuming that the heaven is at rest while the earth is revolving along the ecliptic and at the same time is rotating about its own axis. (Plutarch, *On the Face in the Orb of the Moon*, 6, 923a).

By moving the Hearth of the Cosmos from its central location Aristarchos dared upset the tranquility of the Olympian gods. He claimed that the Earth was not the great goddess of the hearth, Hestia, the sister of Zeus (the master and king of the Universe, the incarnation of justice and order; see Vernant, 1974: 104-114), she who, according to the general belief, is enthroned immobile at the centre of the world and of the 'House of the Gods' (Plato, *Phaedrus*, 247a; Dicks, 1970: 114-115; Heath, 1913: 304; Lloyd, 1973: 58; Vernant, 1983: 128, 159-161, 188-189, 195-196), but it was a mass, which, like the other planets, turned around the Sun. Aristarchos ventured to explain in a mechanical manner phenomena that were regarded by everybody as the work of divinities. It is to him that the Platonist philosopher Dercyllides (first century A.D.) alluded, when he announced that

> … we must suppose the Earth, the Hearth of the House of the Gods, according to Plato, to remain fixed, and the planets with the whole embracing heaven to move, and reject with abhorrence the view of those who have brought to rest the things which move and set in motion the things which by their nature and position are unmoved, such a supposition being contrary to the hypotheses of mathematics. (Theon of Smyrna, *Mathematical Knowledge Useful for the Reading of Plato*, iii 34; see also Heath, 1913: 304; Hiller, 1878: 200, lines 7-12).

It is worth noting here that although Dercyllides reacts with horror to an attempt to put the Earth in motion, he also says that it is contrary to the hypotheses of the mathematicians; so it seems that not only religious feeling, but also the mathematicians themselves were opposed to the idea. This, however, does not alter our basic hypothesis. Indeed, by placing the Earth at the centre of the cosmos, the mathematicians and astronomers could explain the seasons, the movements of the planets Venus and Mars, weather phenomena and other celestial parameters made by observations. And this could have been independent of their potential philosophical view that the Sun is at the centre (see Section 3 below).

Having, no doubt, scandalized the public opinion and facing the death penalty, Aristarchos had no option but to flee Athens and never come back (see also, Christianidis et al., 2002).

Impiety trials were the violent reaction of the community of the Greek city-state or *polis*, which felt its integrity to be under threat. In the absence of any dogma or any organized priesthood in ancient Greek religion, it was the city-state itself, i.e. the citizen body or *demos*, that undertook to prosecute and punish those who were 'impiously disposed'. In addition, in Athens, like in all Greek cities, the cult community was identified with the citizen body and the cult guaranteed the unity of the citizens, of the state. As Louis Gernet put it, the city-state considered itself to be "… a concrete and living entity under the sure protection of the gods, who would not abandon it, as long as it did not abandon them." (Gernet and Boulanger, 1932: 295).

Religion and the state being inextricably linked in ancient Greece, any crime against religion was considered as an attack against the whole of the citizen body, against the security of the state, i.e. as a crime of high treason. This is why attacks by the philosophers and natural scientists on traditional religious beliefs and on the sacred 'ancestral customs' (*ta patria*), were seen by the community as seriously undermining the social order, the stability of the *polis*, and were severely punished (Bruit Zaidman and Schmitt Pantel, 1992: 11-15; Derenne, 1930: 247-267; Dover, 1974: 246-254; Garland, 1994: 25-26, 88-89, 97-102; 1996; Vegetti, 1995; Sakellariou, 1999: 15, 274-276; Price, 1999: 67-88; Mikalson, 2005: 181-184).

Given the nature of the evidence at our disposal, we have concentrated so far on the religious values of Classical Athens, the best-documented city of Greek antiquity. The evidence regarding impiety laws in the rest of the Greek world is much more scattered both in time and space, and we are not in a position to know whether the fear of prosecution for impiety actually applied to, say Rhodes, at the time of Hipparchos, around 120 B.C., or Alexandria, at the time of Ptolemy, around A.D. 150.

As far as pre-classical times are concerned, textual evidence going back to the sixth century B.C., to the time of the first Presocratic philosophers (Thales, Anaximander and Anaximenes), suggests that there was already religious intolerance and talk about impiety not only in Athens, where, at the time of Solon (594 B.C.), it was considered that "… piety resided … in the absolute respect of the customs handed down by the ancestors …", as mentioned by Isocrates (*Areopageticus*, 29-30; Derenne, 1930: 235-236; Mikalson, 2005: 183), but also elsewhere in Greece: the first appearance of the word 'impiety' (*asebeia*) is in a poem by Theognis of Megara (ca. 580 or 545 B.C.): "Respect and fear the gods. This keeps a man from doing or saying anything that is impious." (Theognis, lines 1179-80, to his friend Cyrnos; Garland, 1992: 138; Mikalson, 2005: 188), while a fable by Aesop (ca. 620-560 B.C.), from Samos, describes how a sorceress was condemned to death "… for making innovations in religion." (Aesop, *The Sorceress*; Derenne, 1930: 232-233; Temple 1998: 72, no. 91)—even though it is a fable, it might reflect the existence of a law prohibiting innovative religious practices already at that period. It is even likely that obedience to the ‘ancestral customs’ was imposed by law in Athens as early as the seventh century B.C.: the first Athenian lawgiver, Draco (621 B.C.), whose harsh legal code punished almost every offence with death, apparently was the first to introduce a law instructing the Athenians “… as a group to honour the gods and local heroes in accordance with the ancestral practices.” (Porphyry, *On Abstinence from Animal Food*, 4.22; Garland, 1992: 138; 1996: 94; Mikalson, 2005: 183).

In view of the tight connection between state and religion in ancient Greece, religious conservatism and intolerance may, in fact, be as old as the eighth century B.C., the time of the formation of the city-state or *polis* (Garnsey, 1984; Sakellariou, 1999: 275).

3 THE GEOCENTRIC VERSUS THE HELIOCENTRIC MODEL FOR ‘SAVING THE PHENOMENA’?

In the previous two Sections we examined the possibility that the heliocentric theory was well known to the ancient Greek philosophers and astronomers, but it was not presented to common people as such due to the generally-accepted religious values. In this Section, we will advance one additional reason why this might have happened, and this is the possibility that it may have satisfied one of the astronomers’ theses, that of ‘saving the phenomena’, which implies on their part an apparent indifference to the real nature of the things that they continuously searched for.

It is likely that putting the geocentric view forward would have also facilitated the reception of the surrounding world in a didactic way, much like in the modern representation of the celestial sphere, where the Earth is at the centre and the Sun makes an apparent orbit on the ‘celestial sphere’ along the zodiac, defining the ecliptic (Figure 3). Thus, in antiquity, the philosophers and astronomers might have used geocentrism for the observation of astronomical phenomena, because it is more easily visualized and provides a much clearer understanding of most every-day phenomena (see below), without necessarily having to sacrifice their heliocentric views. Such an endeavor would not have prevented them from seeking a successful scientific theory, which contained at least some elements of the true nature of the world (Evans and Berggren, 2006: 50; Lloyd, 1978; 1991: 248-277).

The geocentric theory with the irregular movements of the Sun, Moon and planets, is apparent, and the contrast is between those movements and the true circular, orderly, and regular motions in terms of which those irregularities are to be explained (as suggested by Plato). Simplicios (*On Aristotle’s* On the Heavens, 422.3ff., 427.10ff.) points out critically that the astronomers have not demonstrated their hypotheses, and that he is aware of the fact that the same phenomena were sometimes explained by different hypotheses.

A geocentric representation of the Universe could have been justified primarily by the main concern of the Presocratic philosophers and astronomers, from Parmenides onwards, to defend the view of the common people that the world of the senses, of the visible phenomena, has a real dimension, in other words, by their concern ‘to save the appearances’. The Greek expression ‘to save the appearances’ or ‘to save the phenomena’ (*sōzein ta phenomena*) occurs for the first time in Plutarch (*On the Face in the Orb of the Moon* 6, 923a), but Simplicios attributed it to Plato (*On Aristotle’s* On the Heavens, comments in II.12) (see Evans and Berggren, 2006: 49-50; Goldstein, 1997: 7). ‘Saving the appearances’ patently meant engaging in precise observation, recording and prediction of the apparent movements of the celestial bodies, and striving to explain them, at the same time as attempting to know the real nature and composition of the heavenly bodies. Thus, the importance of any astronomical theory lay in its precise physical interpretation, but also in its prognostic and explanatory ability and its simplicity.

The major achievement of the Presocratic philosophers and astronomers is precisely the effort to interpret the cosmos on the basis of logic, in a rational and abstract way, by rejecting all the supernatural interpretations, which were based on religious or magical beliefs and which prevailed hitherto among the Greeks. Later on, after Plato’s sensible recommendation to apply mathematical methods to the explanation of natural phenomena, this attitude provided the ancient Greek philosophers and astronomers with the power to put forward and use ideal, geometrical models of the Universe, without necessarily believing in their physiccal existence (Plato, *Republic*, VII, 529-530; Dicks, 1970: 107-108; Farrington, 1963; Lloyd, 1970: 66-79).

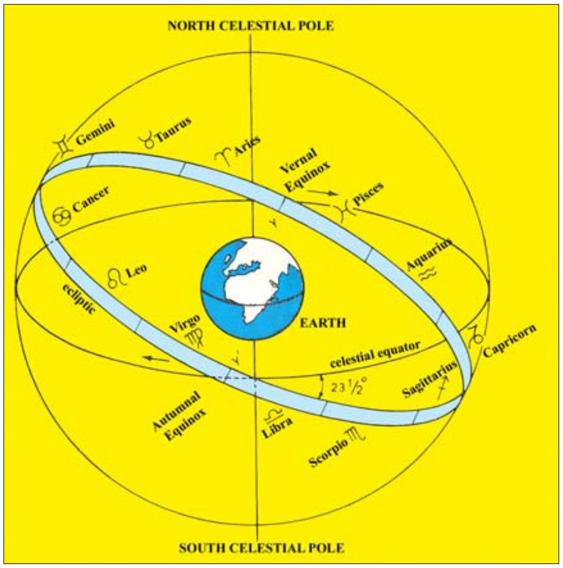

Figure 3: The apparent movement of the Sun round the zodiac circle in one year.

The ancient Greek astronomers’ ideas, in fact, began to blossom when they were applied to the available observational data and a very clear view emerged soon, at least regarding the solar orbit with the solstices and equinoxes defining the four seasons of the solar year. An essential part of this picture was the discovery of the Sun’s apparent circle around the ‘celestial sphere’ each year, denoted by the Sun’s passage through the zodiacal band of constellations, on a tilted plane with respect to the plane of the ‘celestial equator’ (Figures 3 and 4). This discovery is variously attributed by the doxographers to Oinopides of Chios, to the Pythagorean Philolaos or to Pythagoras himself (Diels-Kranz 1951, Volume I: 393-394, no. 41.7). However, it may well go back to Anaximander, since this remarkably original thinker, who was the first to put forward the hypothesis that the Earth is suspended freely in the Universe, is also the first to be credited with the discovery not only of the solstices, but also of the equinoxes: indeed, if the recognition of the solstices implies no astronomical theory whatsoever, since they can be determined by simple observation alone, it is otherwise for the equinoxes, which presuppose a comparatively advanced level in astronomical thought, i.e. knowledge of a celestial sphere, with the Earth at the centre and with the equator, tropics, and the ecliptic as the Sun's path round the Earth, inclined at an angle to the celestial equator (Dicks, 1966: 31-32).

The concept of the celestial sphere and all that it implied undoubtedly offered an adequate framework, but a lot more theoretical work was needed to correlate the existing observational material with it. This is the reason why Plato urged the astronomers to focus on the theoretical side of their subject and develop a mathematically-based system, which would explain the movements of the visible celestial bodies.

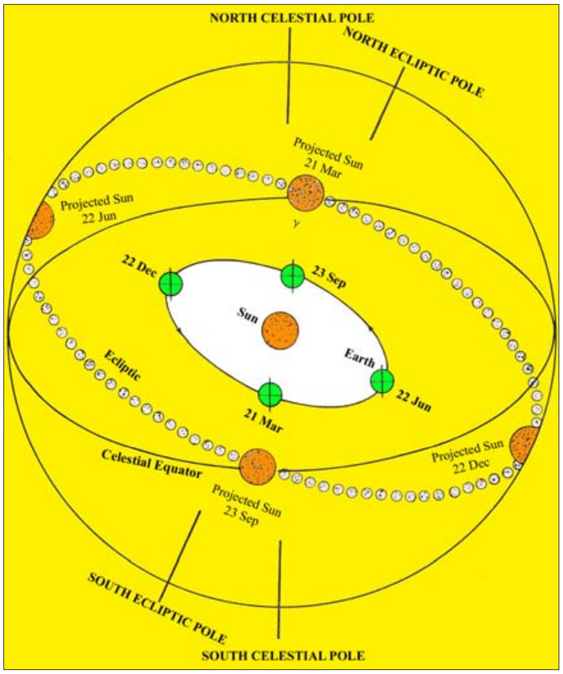

Figure 4: The Sun is at the centre and the Earth moves round the Sun making an internal ellipse in one year. On March 21, the Sun is projected onto the sky at point (γ), the opposite part of the Earth's position. As the Earth moves towards June 21, September 23 etc., the Sun seems to move in the opposite part and in the same direction.

The reason for the return, after Aristarchos of Samos, from the heliocentric to the geocentric system —apart from the still existing prejudices and religious beliefs, which set the Earth-Hestia at the centre of the Universe—was the failure of the heliocentric model to 'save the appearances'. More especially, the heliocentric theory failed to account for a number of physical and astronomical considerations. First, it was inconsistent with ordinary experience of motion: if indeed the Earth was subject to daily axial rotation around the Sun, this would have had a serious effect on the movement of heavy objects (since they naturally travel towards the centre of the Earth) or of objects moving through the air, of winds and clouds (since the Earth would be spinning at incredible rates of speed), whereas no such effects were observed. Second, this theory did not help to explain the apparent absence of stellar parallax (i.e. of any change in the relative positions of the stars as observed from different points of the Earth's orbit), nor did it account for the inequality of the seasons as defined by the solstices and the equinoxes or for the anomalies in the orbits of the celestial bodies, which became obvious as observations improved. Concerning the objection regarding parallax, it is worth mentioning here in particular the Pythagorean attempt to accommodate the phenomenon of lunar parallax, as reported by Aristotle:

> For, since the earth is not the centre, but is distant from it by a whole hemisphere of the earth, nothing … prevents the apparent facts (*τα φαινόμενα*) occurring in the same way when we do not live at the centre as they would were the earth to be at the centre. For even as it is, nothing makes it obvious that we are at a distance of half a diameter from the centre [i.e. on the Earth's surface]. (*On the Heavens*, II. 293b25-30; see also Leggatt, 1995: 255-256).

On the other hand, the model of epicycles and eccentrics, first propounded by Apollonios of Perge and expanded later by Hipparchos and Ptolemy, which assumed a geocentric system, could satisfy with enough accuracy the reconstruction of the celestial phenomena and could compromise with a stationary Earth. Indeed, it was not judged necessary for any mathematical constructions used by astronomical models, such as the model of epicycles and eccentrics or, before it, Eudoxos' model of concentric spheres, to have a physical basis, but rather to be suitable in predicting the planetary positions, (cf. Plato's and Ptolemy's '*hypotheseis planōmenōn*'). Astronomy was a mathematical exercise designed to 'save the appearances', to account for the motions of the heavenly bodies by making use of mathematical hypotheses.[6] The astronomical models aimed at a better estimation of the phenomena connecting the model with the observation. Thus, what counted as phenomena to be saved did not change with time, as Greek astronomy matured. Because of this Ptolemy's model certainly is not matured astronomy, but rather a culmination of astronomy in terms of complex mathematical models.

It is known that the general frame or model adopted finally was that of the celestial sphere with the spherical Earth immobile at the centre. This conception of the Universe proved valuable and long lasting. Even today the model of the celestial sphere is used as a necessary basis for the drawing of sky maps, or in planetaria and with orreries, which by their very nature require the observer to occupy a central position. To ordinary observers on Earth, the stars appear to be attached to the inside of a vast hollow globe, which spins round the Earth from east to west once a day. Although this view is not true—given that the Earth is not at the centre of the Universe, but is only a small-sized planet spinning on its axis in its orbital motion around a brighter than average star in a larger than average galaxy—it could have been useful (and often still is) for astronomers to pretend that this globe, or celestial sphere in the sky, really does exist. Moreover, the cyclic orbit in the heliocentric model does not explain the planetary positions with any accuracy as the geocentric model does.

Indeed, an understanding of most everyday phenomena is made easier if one constructs an image of the 'celestial sphere' having the Earth at the centre, whereby the Sun is projected opposite the Earth's orbit (Figures 3 and 4). Such phenomena include the following:

1) The Earth's revolution around an axis passing through its centre, and turning from east to west, determining day and night.
2) The appearance of the Earth as suspended in cosmic space.
3) The Earth's movement around itself and around the Sun.
4) The ecliptic and the celestial equator.
5) The four seasons of the year.
6) Lunar and solar eclipses.
7) The ecliptic circle and the zodiacal band of constellations.
8) The apparent movement of the Sun through the stars.
9) The four solar stands (two equinoxes and two solstices) during a year.
10) The precession of the equinoxes.
11) The obliquity of the ecliptic, i.e. the 23.5° angle between the plane of the ecliptic and the plane of the celestial equator.
12) The determination of the planetary positions at a particular time.

Geocentrism could have also assisted in the fixing of calendric time, as well as in the prediction of weather phenomena by means of the risings and settings of the fixed stars or constellations (Taub, 2003).

Moreover, the brilliant idea of considering a celestial sphere having the Earth at its centre, with the Earth's poles, as well as lines of latitude and longitude projected on it, would have proved extremely useful to the ancient Greek astronomers themselves, as it is often helpful to astronomers nowadays, because it makes it easier to observe far away celestial bodies by placing them on the surface of the 'celestial sphere' and by assuming them to be at an infinite distance.

For all the above reasons geocentrism could have apparently won out vis-à-vis heliocentrism. Moreover, the adoption of a geocentric view would have allowed the ancient Greek philosophers and astronomers not only to harmonize their theories with the religious beliefs of their time, but also to facilitate the reception of the surrounding world by the ordinary people so as to 'save the appearances', without sacrificing the essence of their ideas. Furthermore, it could have assisted their own observations of the celestial bodies.

4 CONCLUSION

The spherical Universe, apparently implying a belief in the heliocentric system, may have been the prevalent view of the ancient Greek philosophers and astronomers. However, the opposition of such a view to the established religious beliefs, which assumed that the Sun-god circled the Earth-Hearth of the world, and the fear of prosecution for impiety, at least in Athens during the Classical period, may have prevented the promotion of the heliocentric model. Moreover, a model placing the Earth at the centre of the Universe with the Sun revolving around the Earth—much like the modern representation of the celestial sphere—would have 'saved the appearances': it would have explained day and night, the four seasons of the year, the solstices and the equinoxes, the apparent movement of the Sun through the stars and constellations, as well as lunar and solar eclipses, and planetary motions, without exposing the inherent beliefs of the philosophers and astronomers.

5 NOTES

1. This is an expanded version of a paper delivered at the International Conference on "Decoding the Antikythera Mechanism. Science and Technology in Ancient Greece", which was held in Athens on 30 November-1 December 2006.
2. As opposed to the views attributing the concept of the spherical Universe to Pythagoras or the Pythagorean Philolaos, or even to Oinopides of Chios, at the earliest (e.g. Dicks, 1966: 30; Evans, 1998: 75; see, also, Diels and Kranz, 1951: 393-394, no. 41.7). That Anaximander seems to have already espoused the view of the sphericity of the heavens is supported by his being credited with the determination of the equinoxes, which in itself presupposes the knowledge that the Earth is the central point of a celestial sphere (Dicks, 1966: 32 and *passim*; however, Dicks refuses to ascribe the idea of a spherical cosmos to Anaximander and dismisses it as anachronistic, as opposed to Kahn, 1960: 92-94). For an earlier discussion of the subject, see Heath, 1913: 28-39.
3. A whole series of lenses has been found in archaeological sites in the Aegean world, Troy, Cyprus and the Middle East, dating from the third millennium B.C. to Roman times. The most frequent type of lens (viz. with a plano-convex shape), made of rock crystal, has a nominal magnification ranging from 2× to as much as 20× and may well have served as a magnifying or burning glass, an identification which is also supported by ancient literature (Plantzos, 1997: 454; Sines and Sakellarakis, 1987: 191, 193 Figure 3). A case for such lenses having been used in observing the heavens has been made by Temple (2000). On the subject of the observational tools used by ancient Greek astronomers, the prevailing view is that, until the time of Hipparchos (who is known to have used the *dioptra* and the equinoctial armillary), the only sighting aids used were very primitive instruments, such as the *gnomon* or vertical rod (see Lloyd, 1970: 97; 1991: 309). For a more extreme view, in favour of non-instrumental observations, based on the use of body parts, such as the hands, see Rihll, 1999: 69. However, an apparatus as elaborate as the astronomical clock or computer, known as the 'Antikythera Mechanism', which appears to have been made at the time of Hipparchus (ca. 120 B.C.) or thereabouts (Edmunds et al., 2006), presupposes a long period of experimentation with astronomical equipment.
4. It is possible that 'impiety' was used in the case of Aristotle as a convenient way to persecute him for his connection to Alexander the Great, since the charge was made shortly after Alexander's death (see e.g. Meyhew, 1996).
5. In fact, according to Momigliano (1978: 188), "… the Athenian prosecutions of the philosophers are the historical precedent – and the paradoxical justification – for the penalization of religious opinions advocated by Plato in the *Laws*. Plato contributed to the notion of heresy in so far as he contributed to the

idea of intolerence and inquisition." Similarly, Price (1999: 133-134), writes: "Plato built on the Athenian impiety law … to formulate his own far more extensive impiety law, which makes him the first political thinker to argue that matters of belief can be criminal offences."

6. Lloyd (1978; 1991: 248-277) has refuted Duhem's (1908) instrumentalist view, according to which the models of the Greek astronomers were not intended to represent the true system of the world, by showing that this scholar misunderstood his sources and that all the ancient Greek astronomers of whom we know enough to be able to say anything with confidence were realists. According to our working hypothesis, the seeming realism of the ancient Greek astronomers emerged when they were involved with mathematical models, but while still retaining their philosophical views.

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MILUTIN MILANKOVIĆ AND THE REFORM OF THE JULIAN CALENDAR IN 1923

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Abstract: At the Orthodox Church Ecumenical Congress of 1923 in Constantinople one of the important questions discussed was the Julian Calendar reform. In the delegation of the Serbian Orthodox Church was the accomplished Serbian geophysicist and astronomer Milutin Milanković (1879–1958), who played a critical role in the proceedings, and whose proposition for calendar reform was adopted. The issues relating to that proposal are discussed here, along with a short history of Milutin Milanković and his work.

Key words: History of astronomy, Milutin Milanković, calendar reform, Julian Calendar, Orthodox Church

1 INTRODUCTION

Patriarch Meletios IV (1922–1923), head of the Orthodox Churches, convened an Ecumenical Congress in Constantinople in May 1923, where one of the principal topics of discussion was the reform of the Julian Calendar. In the Serbian delegation were Gavrilo Dožić and Milutin Milanković. At the time, Dožić was the Metropolitan of Crna Gora and Primorje (Montenegro and the Littoral), and later became Patriarch of the Serbian Orthodox Church. Milutin Milanković (Figure 1) had been a very successful civil engineer before accepting the Chair of Applied Mathematics at the University in Belgrade in 1909. From this point on, Milanković applied himself to the study of climatic change due to thermal heating by solar radiation. He developed an astronomical theory for the evolution of planetary climates and explained the phenomenon of the Earth's Ice Ages and polar motion. One of his contributions was his analysis of the Earth's period of rotation, which resulted in his proposal at the Congress in Constantinople to reform the Julian Calendar.

We will first present Milutin Milanković's principal scientific results, before discussing the reform of the Julian Calendar at the Congress in Constantinople of 1923 and his contribution to it.

2 MILUTIN MILANKOVIĆ

Milutin Milanković, who was born in Dalj on 28 May 1879 and died in Belgrade on 12 December 1958, is best known for his ground-breaking work on the causal relationship of solar heating to the phenomena of the Ice Ages. He graduated from the Vienna University of Technology with a degree in civil engineering (1902) and a Ph.D. in technical sciences (1904), and remained there for five years designing dams, bridges and viaducts. In 1909, he was offered the Chair in Applied Mathematics at Belgrade University, and he relocated to Serbia where he taught mechanical and theoretical physics and celestial mechanics.

Milanković began occupying himself with the astronomical origins of planetary climate changes and the mathematical theory of climate. In 1912, he published *A Contribution to the Mathematical Theory of Climate*; in 1913, *On the Application of the Mathematical Theory of Warmth Transmission to the Problems of Cosmic Physics*; and in 1916, *Investigation on the Climate of Mars.*

Figure 1: Milutin Milanković, 1879–1958 (after Pantić, 2001: 171).

In his *Mathematical Theory of the Thermal Phenomena Caused by the Solar Radiation* Milanković (1920) developed a theory based on the principles of celestial mechanics and theoretical physics which explained the distribution of solar radiation throughout interplanetary space and over the planetary surfaces. He indicated also the connection between the insolation (i.e. incoming solar radiation) and the temperature of the planetary layers, and he determined daily, annual and secular changes in the insolation. In 1926 he published the research paper titled "Investigation in the thermic constitution of the planetary atmospheres." In all of these works he devoted particular attention to the climate of Mars, establishing beyond doubt the mean annual temperature on the planet's surface to be about –17° C.

In his foremost work, *Kanon der Erdbestrahlung und seine Anwendung auf das Eiszeitenproblem* (*The Canon of the Earth's Insolation and its Application to the Ice Ages Problem*) which was published in 1941, Milanković collected the results of his 28 previously-published researches and assembled them in one monograph. He added new analyses and supplements, including numerous applications of his theory, demonstrating that long-period cyclical changes in the Earth's climate and the occurrence of Ice Ages were associated with the following causes:

(1) Changes in the Earth's axis of inclination between 22° and 24.5° with a 41,000-year period, as a result of which the insolation at any particular point on the Earth's surface also undergoes change.
(2) Changes in the eccentricity of the Earth's orbit around the Sun, with a 100,000-year period, bringing about changes in the Earth's distance from the Sun, which in turn give rise to changes in the duration of the seasons.
(3) Polar precession, causing the point of the winter solstice to be shifted along the Sun's annual apparent path, affecting the duration of the seasons with a period of 22,000 years.

In order to solve the problem of the occurrence of the Ice Ages in Europe during the Quaternary Period, in 1932 Milanković arrived at his famous differential equation of the Earth's polar motion (Milanković, 1933). He found that some 300 million years ago, the Earth's North Pole was in the Pacific Ocean at +20° latitude and 168° E longitude. At present, the North Pole is moving towards its equilibrium point in Siberia, near the location where the Pechora River flows into the Arctic Ocean. Today we know that this is a consequence of the movement of the continental plates.

Milanković paid considerable attention to the history of science. In his *Memories, Experiences, Insights* (Milanković, 1997) he points out that: "Any science may be comprehended in its fullness only after one gets acquainted with its origins and its gradual development." He then describes how for him the history of science became the most magnificent part of the entire history of humanity. In his book *Techniques during the Remote Centuries*, Milanković (1955) states with regret that "While the works on the world history might fill a large library, the most important works on the history of Mathematics, Astronomy and Physics might be well stored in any personal library."

Milutin Milanković was the Vice-president of the Serbian Academy of Sciences and Arts and from 1948 to 1951 Director of the Belgrade Astronomical Observatory. To honor his scientific achievements in astronomy, a crater on the far side of the Moon (coordinates +170°, +77°) was given his name at the 14th I.A.U. General Assembly in Brighton in 1970. His name was also given to a crater on Mars (coordinates +147°, +55°) at the 15th I.A.U. General Assembly in Sydney in 1973. In 1982, a minor planet discovered in 1930 by Milorad Protić and Pero Djurković and provisionally designated 1936 GA, received its permanent name, 1605 Milanković (Dimitrijević, 2002).

3 CALENDAR REFORM AND THE PANORTHODOX CONGRESS IN CONSTANTINOPLE IN 1923

At the First Council of Nicea (A.D. 325), the Christian Church adopted the Julian calendar, introduced by Julius Caesar in 47 B.C. In this calendar, leap years occur every fourth year, provided the numerals of that year are divisible by four. Although this system was a very good approximation to the natural cycle, its year was over eleven minutes longer than the tropical year. By the sixteenth century, the accumulated time difference reached ten days.

On 24 February 1582, Pope Gregorius XIII commanded the introduction of the following reforms: (i) the accumulated discrepancy would be eliminated by making the day after 4 October the 15th of October 1582; (ii) the only secular leap years would be those where the number of the centuries is divisible by four.

The Eastern Orthodox Churches, not wanting to follow the dictates of the Catholic Church, chose to retain the Julian calendar. By the twentieth century, the discrepancy between the two calendars had grown to thirteen days.

At the Ecumenical Congress of Orthodox Churches of 1923 in Constantinople, one of the important questions was the reform of the Julian calendar, and representatives of the Serbian and Romanian Orthodox Churches submitted two elaborate propositions (a detailed description of the calendar reform and of the Pan Orthodox Congress in Constantinople is given in Milanković, 1923; 1995; 1997; see, also, Dimitrijević, 2002 and Dimitrijević and Theodossiou, 2002). The Serbian delegation came to the Congress with a proposition for calendar reform authored by Maksim Trpković. He proposed the intercalation rule that the secular years in centuries which when divided by 9 have remainders of 0 or 4 will be leap years. In such a way seven days will be omitted from nine centuries, so that the calendar will be closer to the tropical year than the Gregorian calendar, and the vernal equinox will always fall on 21 March or very close to it.

The Romanian delegation consisted of Archimandrite Julius Scriban and Senator Dragici. They came with the following proposal for calendar reform: each year is to have 364 days (exactly 52 weeks) so that every date has a fixed day in the week. March, June, September and December have 31 days, and the other months 30 days. An additional week is added every five years between 31 June and 1 July, whose number of days corrects the difference with the tropical year. The first day of Easter is fixed at 29 April, and all other holidays become fixed. Senator Dragici presented the unsigned proposition to the Congress as his, but he told Milanković that the author was actually Baron Bedeus from Sibiu. The Baron was not an Orthodox Christian, so it was inappropriate that his name should appear on the proposal.

A scientific commission comprising Milutin Milankovic, Senator Dragici and Archimandrite Scriban was formed to examine the two proposals, but both were ultimately rejected by the Congress. What they found objectionable in the proposition of the Serbian delegation was that the year 2000 would not be a leap year, as in the Gregorian calendar, and only after 77 years would a difference of one day appear between the Gregorian and the New Rectified Julian calendars. The general opinion of the participants was that the better solution was to retain the Julian calendar as it was and only delete thirteen days, in order to bring it into line with the Gregorian calendar. In this way, a one-day difference would appear after 177 years, in the year 2100.

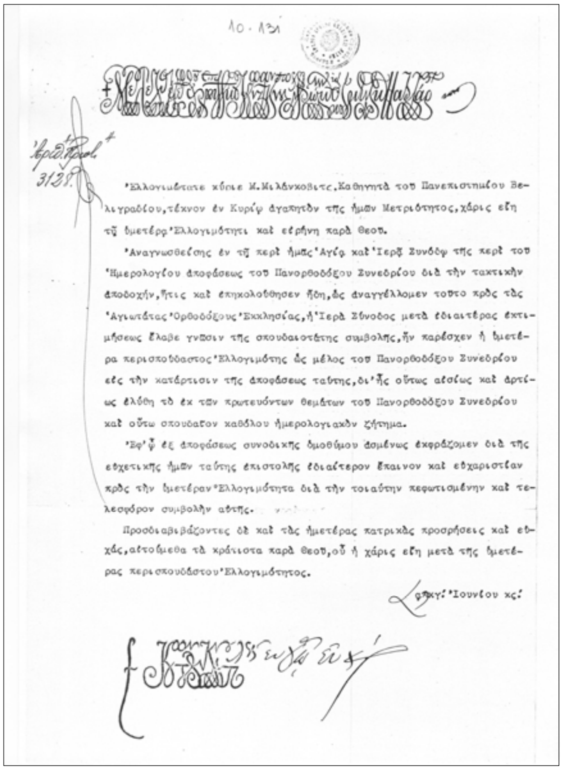
Milutin Milanković was then given the task of developing a new proposal for calendar reform. He concluded that the wish of the majority of participants was that the calendar of the Eastern Orthodox Church should not be identical to the Gregorian calendar, but that the two should parallel one another as far as possible. Consequently, instead of trying to fix the date of the vernal equinox at 21 March, as in Trpković's proposal**,** he tried to obtain the longest possible consonance of the two calendars. Finally, he developed a new intercalation rule: that secular years are leap years only provided that the number of centuries they belong to when divided by 9 yields the remainder 2 or 6. In this way he obtained a calendar that was more precise than the Gregorian one but consistent with it up to 2800 (i.e. for 877 years from the time of the Ecumenical Congress in Constantinople). The result was that the years 2100, 2200, 2300, 2500, 2600 and 2700 are ordinary years according to both calendars. The years 2000 and 2400 are leap years according to the Gregorian calendar since 2000 and 2400 can be evenly divided by four, and according to Milanković's New Rectified Julian calendar as well because when 2000 is divided by 9 the remainder is 2 and for 2400 the remainder is 6. The year 2800 is a leap year according to the Gregorian calendar since 28 can be evenly divided by 4, but according to the New Rectified Julian calendar it is an ordinary year since when 28 is divided by 9 the remainder is 1. One should take into account the fact that the New Rectified Julian calendar of the Orthodox Church will be in better agreement with nature than with the Gregorian calendar: a disagreement of just one day between the New Rectified Julian calendar and the tropical year will only accumulate after almost 30,000 years!

Milanković presented his new proposal to the Congress at its 23 May 1923 session. This new proposition by the Serbian Orthodox Church was signed by him and by Gavrilo Dožić. In his historic speech to the Congress, Milanković told the delegates that if they only decided to delete thirteen days from the Julian calendar, the Orthodox Church would be in an inferior position in any future discussion on the calendar question. On the other hand, with the proposition of the Serbian delegation, the Orthodox Church would have the most precise and most scientific calendar in the Christian world, so it could confidently enter into any negotiations on the calendar question with Western Churches. Milanković underlined also that with such a decision, the Orthodox Church would not be accepting the calendar of the Roman Catholic Church, but would be adopting a better one.

Also attending the Congress was Anthimos Metropolitan from Viziys, who proposed to determine the exact date of Easter by astronomical methods, with help from observatories and universities in Athens, Belgrade, Bucharest and Pulkovo.

Figure 2: Conclusion or Concentrasion of the all Orthodox Congress in Constantinople in 1923. In the centre is the head of all Orthodox Churches, Patriarch Meletios IV. Milutin Milanković is sitting on the extreme right, and beside him is the Metropolitan of Montenegro and Coast Gavrilo Dožić. The signature across the photo is of Patriarch Meletios IV (after Milanković, 1995).

The date of Christian Easter had originally been linked to that of the Jewish Passover because it was generally thought that the Last Supper was a Passover meal. The synod of Nicea, however, decided to separate these holidays and determined that Easter would take place on the first Sunday after the full Moon that follows the spring equinox (which occurred on 21 March at that time). Calculations using whole numbers and different calendars resulted in differences between the two holidays of up to four weeks. The proposed calendar reform would also result in different dates, in spite of the fact that the calendars paralleled one another.



10.131

Ἀρχ. Πρωτ. 3128

Ἐλλογιμώτατε κύριε Μ. Μιλάνκοβιτς, Καθηγητὰ τοῦ Πανεπιστημίου Βελιγραδίου, τέκνον ἐν Κυρίῳ ἀγαπητὸν τῆς ἡμῶν Μετριότητος, χάρις εἴη τῇ ὑμετέρᾳ Ἐλλογιμότητι καὶ εἰρήνη παρὰ Θεοῦ.

Ἀναγνωσθείσης ἐν τῇ περὶ ἡμᾶς Ἁγίᾳ καὶ Ἱερᾷ Συνόδῳ τῆς περὶ τοῦ Ἡμερολογίου ἀποφάσεως τοῦ Πανορθοδόξου Συνεδρίου διὰ τὴν τακτικὴν ἀποδοχήν, ἥτις καὶ ἐπηκολούθησεν ἤδη, ὡς ἀναγγέλλομεν τοῦτο πρὸς τὰς Ἁγιωτάτας Ὀρθοδόξους Ἐκκλησίας, ἡ Ἱερὰ Σύνοδος μετὰ ἰδιαιτέρας ἐκτιμήσεως ἔλαβε γνῶσιν τῆς σπουδαιοτάτης συμβολῆς, ἣν παρέσχεν ἡ ὑμετέρα περισπούδαστος Ἐλλογιμότης ὡς μέλος τοῦ Πανορθοδόξου Συνεδρίου εἰς τὴν κατάρτισιν τῆς ἀποφάσεως ταύτης, δι' ἧς οὕτως αἰσίως καὶ ἀρτίως ἐλύθη τὸ ἐκ τῶν πρωτευόντων θεμάτων τοῦ Πανορθοδόξου Συνεδρίου καὶ οὕτω σπουδαῖον καθόλου ἡμερολογιακὸν ζήτημα.

Ἐφ' ᾧ ἐξ ἀποφάσεως συνοδικῆς ὁμοθύμου ἀσμένως ἐκφράζομεν διὰ τῆς εὐχετικῆς ἡμῶν ταύτης ἐπιστολῆς ἰδιαίτερον ἔπαινον καὶ εὐχαριστίαν πρὸς τὴν ὑμετέραν Ἐλλογιμότητα διὰ τὴν τοιαύτην πεφωτισμένην καὶ τελεσφόρον συμβολὴν αὐτῆς.

Προσδιαβιβάζοντες δὲ καὶ τὰς ἡμετέρας πατρικὰς προσρήσεις καὶ εὐχάς, αἰτούμεθα τὰ κράτιστα παρὰ Θεοῦ, οὗ ἡ χάρις εἴη μετὰ τῆς ὑμετέρας περισπουδάστου Ἐλλογιμότητος.

ϡκγ΄ Ἰουνίου κϛ΄

Figure 3: The letter from his beatitude Ecumenical Partriarch Meletios IV to Milutin Milanković (courtesy: Archive of the Serbian Academy of Sciences and Arts, 10.131/III – 101).

Milutin Milanković completed the final version of the calendar reform, which was then adopted by the Congress. The relevant document was signed on 8 June 1923, just prior to the conclusion of the Congress, by Patriarch Meletios IV, Kalinikos (Metropolitan of Kyzikos), Alexander (Archbishop of North America), Gavrilo Dožić (Metropolitan of Montenegro and Littoral), Vasilios (Metropolitan of Nicaea), Jakub (Metropolitan of Durachion), Archimandrite Julius Scriban, and Professors E. Antoniadis and Milutin Milanković. The Congress was especially grateful to Milanković for his valued and very substantial input, and on 26 June 1923 Patriarch Meletios IV sent him a heartfelt letter of thanks. This is reproduced here in Figure 3, and an English translation is provided in Appendix 1.

The date of the official inception of the New Julian calendar was originally scheduled for 1 October 1923, but it was subsequently changed to 14 October. This was the date when the calendar reform would be introduced in the Ecumenical Patriarchate and in the Greek Churches, but without the part concerning the Easter determination, where the old Julian calculation was retained. Today, Patriarchates of Constantinople, Alexandria and Antioch, Churches of Greece, Cyprus, Romania, Poland, Finland and most recently, Bulgaria (in 1968) and the Orthodox Church in America (on 1 September 1983; see e.g. http://www.holy-trinity.org/modern/calen2.html) use the 'New', 'Revised' or 'Rectified' Julian calendar. On the other hand, the Patriarchate of Jerusalem, and the Churches of Russia and Serbia, along with the monasteries on Mt. Athos, all continue to adhere to the old Julian calendar (see http://www.yalchicago.org/paschacalculation.html).

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APPENDIX 1: TRANSLATION OF THE LETTER OF THANKS TO PROFESSOR MILANKOVIĆ (REPRODUCED IN FIGURE 3)

The most learned gentleman M. Milanković, professor of the Belgrade University dear in Lord, child of humbleness, let boon be with your Eruditeness and peace from God.

Since the decision of the Pan-Orthodox Conference on the calendar question is proclaimed in our holy and sacerdotal Synode in order to be correctly adopted, as we communicate to the most serenes Orthodox Churches, honorable Synode with particular respect noting your very precious advice, with which your high Eruditeness contributed, as a member of the Pan-Orthodox Conference, to the formulation of the decision with which it so luckily and favourably solved one of the leading subjects of the Pan-Othodox Conference and the important calendar question.

In that name, with this our synodal decision, we cordially express by this our prayering-letter exceptional laudation and thanksgiving to your high Eruditeness for your enlightened and useful advice.

Addressing to you our paternal laudations and blessings we pray that God's boon always be with your extraordinary Eruditeness.

26 June 1923
By Mercy of God
Archbishop of Constantinople-New Rome and Patriarch
Meletios IV

It is important to note that this letter was written in the old ceremonial Greek language, Katarevusa, and in translating it we tried to preserve the ceremonial and archaic spirit of the original terminology. Consequently, several unusual words which are not widely used are included. For example, the word "boon" is a wish usually granted by a god to a person or group of people (thus "… a spanking breeze is a boon to sailors."). Meanwhile, the term "your Eruditeness" is analogous to "your Highness", in that the Patriarch wanted to express his admiration to Milanković for his knowledge and his erudition.

Dr Milan S. Dimitrijević is an astronomer at the Belgrade Astronomical Observatory. His scientific interests include spectroscopy of astrophysical and laboratory plasma, stellar astrophysics, collisions and their influence on spectral lines, and history and philosophy of astronomy. He has published several books, around 200 papers in international journals and several hundred contributions in conference proceedings and newspapers.

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Petros Z. Mantarakis received a BS in astronomy from the California Institute of Technology, and an MS in astronomy from the University of Arizona. He worked in industry for thirty years, where he attained the level of President of several companies. He has 20 patents, and has published two books and numerous articles. He lives in Los Angeles (California), where he continues to write and do consulting work.

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THE (ALMOST) UNSEEN TOTAL ECLIPSE OF 1831

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Abstract: The total eclipse of August 1831 began at sunrise in Australia, swept across the western South Pacific Ocean, and ended at sunset in the central South Pacific. As a result of the eclipse's path over mostly uninhabited ocean, the region's sparse European (British) population, and near-useless local predictions of the event at Hobart and Sydney in almanacs sold to the general public, almost no one witnessed its passage. In an attempt to document the eclipse, journals of naïve observers—those having no access to a prediction—were examined. Thus far, the sole record is in the *Pitcairn Island Register Book*. Considering the Pitcairners' extreme isolation and the rather modest partial eclipse that occurred there, the entry is a surprising one; however, it can be explained in terms of events associated with their initial removal to Tahiti in March 1831 followed by their return home in June. Further, an authoritative means to identify any issues associated with eclipse predictions compiled for private-sector almanacs came in 1833 when sweeping changes in the British *Nautical Almanac*'s section on eclipses were instituted..

Key Words: *solar eclipse, almanac, ephemeris, Australia, South Pacific*

1 INTRODUCTION

Two solar eclipses took place during 1831. The first, in February, was an annular eclipse that passed over the United States. The event was predicted and also highly publicized in the country's newspapers. Thanks to clear weather in many places, it was seen by a multitude of specialists and non-specialists alike.

The second eclipse came in August. This one, whose duration of darkness was 3 min 20 sec at maximum and whose path of totality was almost 160 km wide, began at sunrise in Australia, swept across a wide expanse of the western South Pacific Ocean, and ended at sunset in the central South Pacific. It, too, was predicted. However, in sharp contrast to the February eclipse, it passed over a very sparsely inhabited part of the globe. But other reasons diminished the chance of sightings, and they are discussed here.

2 ECLIPSE PREDICTIONS

2.1 Great Britain's *Nautical Almanac*

During 1828 the Commissioners of Longitude ordered the printing of *The Nautical Almanac and Astronomical Ephemeris for the year 1831*, this volume continuing a series begun in 1766 with the printing of an ephemeris for 1767. Among the sections in the publication was a list of solar and lunar eclipses. The solar eclipses, one predicted for 12 February and the other for 7 August, would be invisible at Greenwich.

For each solar eclipse only a few lines of information were printed: data regarding the moment of conjunction, and the apparent time at Greenwich when the eclipsed sun was centered on the local meridian.[1] No diagram for either eclipse was included. For the total eclipse germane to this study the following was given

> Aug. 7. The SUN eclipsed, invisible at Greenwich.
> ⊙ will be centrally eclipsed on the Meridian at 10^{h}.24¼^{m}, in Long. 156°.2′ West, and in Lat. 26°.35¾′ South.

2.2 An American Almanac

Late in 1830 the privately-issued *American Almanac and Repository of Useful Knowledge, for the year 1831* appeared. A several-hundred-page compendium, it included sections on the year's solar and lunar eclipses, including a discussion of data sources. The compiler was the astronomer-computer Robert T. Paine of Boston (1803-1885).

For the annular eclipse of 12 February, Paine gave lengthy descriptions of the eclipse's progress, from its start at sunrise in the North Pacific Ocean to its conclusion in the Atlantic Ocean southwest of Iceland. He provided details for twenty-five cities—from New Orleans to Canada's Halifax—along the eclipse's path, as well as two sketches showing the appearance of the sun at conjunction of the sun and moon and at greatest obscuration. The *American Almanac*'s publishers added a map showing the annular eclipse's central path across the United States.

Paine also provided information about the total eclipse of 7 August, an event invisible in the United States. He detailed its progress and extent as well as listing specific parameters for the partial eclipse at the Parramatta Observatory (Paine, 1830: 27):

> At the Astronomical Observatory in Par[r]amatta, in New Holland, in latitude 33° 48′ 49.8″ S., Longitude 151° 1′ 34″ E., the Sun will rise eclipsed.
> The greatest obscuration (10½ digits) will take place at 19h. 7½m. Mean Time at Par[r]amatta.
> The end of the Eclipse [will occur at] 20h. 16½m. Mean Time at Par[r]amatta.

2.3 Two Australian Almanacs

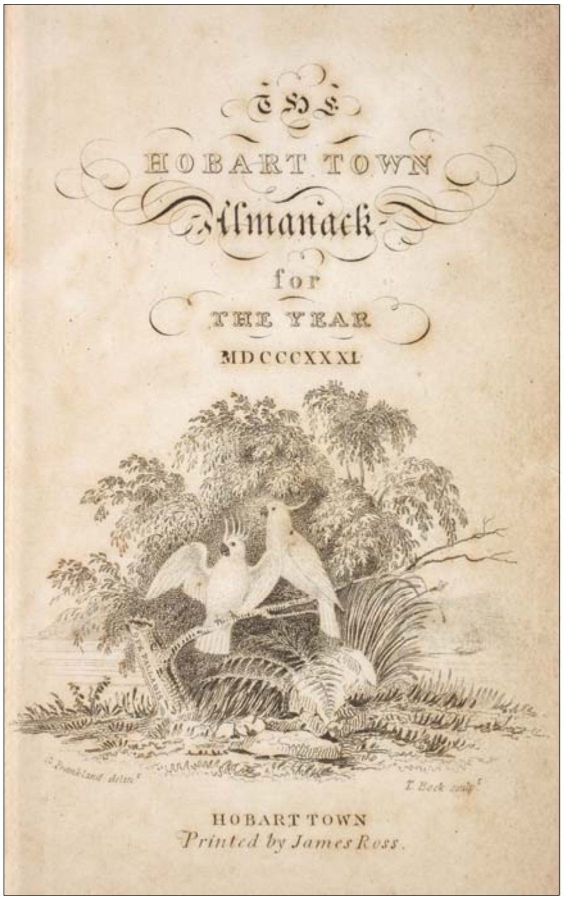
Early in 1831 two almanacs intended for the general public became available, one printed in Hobart (Figure 1), the other in Sydney (Figure 2). The sections listing eclipses for the year were quite similar to the one given in the British *Nautical Almanac,* but with alterations germane to the Australian locales. The prediction printed in the Hobart almanac for the August solar eclipse read as follows:

> … an eclipse of the Sun, centrally eclipsed in longitude 156 degrees 2 minutes west, and latitude 26 degrees 35 minutes South, at 14 minutes past 8 am the morning of the 8th of August at Hobart Town.

The prediction given in the Sydney almanac was more detailed:

> Aug. 7. The Sun eclipsed, visible at Port Jackson.
> Centrally eclipsed on the Meridian at 24 minutes 15 seconds past 10 at night, Greenwich time, in latitude 26° 35′ 45″ south, and longitude 156° 2′ west.

> On the meridian of Port Jackson, the Eclipse will begin at 36 minutes past 7 in the morning.
> Middle of the Eclipse, 25 minutes 36 seconds past 8.
> End of the Eclipse, 45 minutes 36 seconds past 9.
> The Total obscuration will be in the South Pacific Ocean, 10° south by west of Taheite.
> Digits eclipsed 9° 30′.


HOBART TOWN
Almanack
for
THE YEAR
MDCCCXXXI

G. Frankland delint T. Beck sculpt

HOBART TOWN
Printed by James Ross.

Figure 1: Second title page of *The Van Diemen's Land Anniversary and Hobart Town Almanack for the Year 1831*, one of two separate almanacs printed in Hobart during the year and the one discussed in the text (courtesy: Heritage Collections, State Library of Tasmania).

On 6 August, a Saturday, the *Sydney Gazette* summarized the almanac's prediction (Editor, 1831a):

> ECLIPSE OF THE SUN.---Our readers are reminded, that tomorrow morning there will be that rare celestial phenomenon--a visible eclipse of the sun. It will begin at 36 minutes past 7; its middle will be a little sooner than half past 8; and will end at 45 minutes and 36 seconds past 9: the total continuance being 2 hours and 40 minutes. The total obscuration will occur in the South Pacific Ocean, 10° South by west of Taheite.---

As we shall see, this summary led not only to the questioning of the almanacs' predictions, but also much finger pointing. In order to appreciate the issues, some authoritative information is needed.

3 THE AUGUST 1831 ECLIPSE

EmapWin, freeware developed by Shinobu Takesako of Japan, is an extremely convenient way to examine solar eclipse parameters; an accompanying file gives details of the program's data sources. The path of the total eclipse given on the Mercator map (Figure 3) is based on EmapWin results. The various values calculated via this program will represent the actual eclipse's progress.

3.1 British and American Predictions vs. EmapWin Results

The prediction presented in the *Nautical Almanac* is in very good agreement with the EmapWin result, the difference in location between the two being 14 km. Paine's predictions for the Parramatta Observatory location are also in good agreement with EmapWin (Table 1).

3.2 Australian Predictions vs. EmapWin Results

For Hobart the apparent time given for when the sun is centrally eclipsed on the meridian is simply a shift of the *Nautical Almanac*'s value to reflect the fact that Hobart's local time is $9^h49^m16^s$ ahead of Greenwich time. Note that the *Hobart Town Almanack* shows the event date as 8 August, which is correct; however, the time listed is apparent time, not the more-useful mean time. Further, an unsophisticated reader might conclude that the time given reflects local circumstances, which it does not. The time is for an event taking place over 5300 km away (Table 2).

Table 1: Almanac predictions (converted to civil time) compared with EmapWin results.

Source	Event	Predicted Value	EmapWin
Nautical Almanac	Centrally eclipsed on the meridian	August 7, 22:24:25 Greenwich apparent time [22:30:04 GMT] at 26.60°S, 156.03°W	August 8, 12:00 local apparent time [22:30:30.6 GMT] at 26.66°S, 156.15°W
American Almanac	Sunrise Greatest obscuration End of eclipse	Rises eclipsed 7:07:30 mean time 8:16:30 mean time at Parramatta Obs'y	6:47:08 local mean time 7:07:51 local mean time 8:16:58 local mean time at 33.81°S, 151.03°E

Table 2: Predictions from Australian almanacs compared with EmapWin results. EmapWin co-ordinates are 26.66°S, 156.15°W.

Source	Event	Predicted Value	EmapWin
Hobart Town Almanac	Centrally eclipsed on the meridian	August 8 (26.60°S, 156.03°W) 8:14 Hobart apparent time	----
Australan Almanac	Start of eclipse	August 7, 7:36 Sydney apparent time	August 8, 6:54 apparent time, 6:59:39 mean time (first contact)
	Middle of eclipse	8:25:36 Sydney apparent time	8:28 apparent time, 8:33:59 mean time (event mid-point)
	End of eclipse	9:45:36 Sydney apparent time	10:03 apparent time, 10:08:19 mean time (last contact)

Table 3: EmapWin results for selected locations (times rounded to nearest minute).

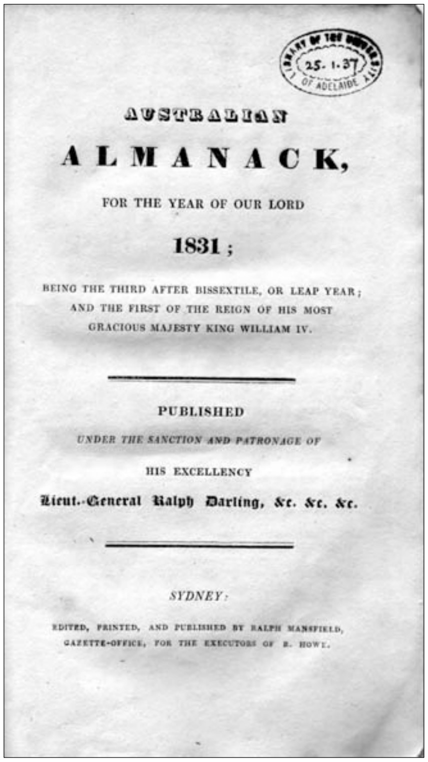
Location	Latitude	Longitude	Maximum Eclipse	Local Mean Time
Hobart	42.88°S	147.32°E	0.713	7:05 sun rises eclipsed at sunrise 8:09 last penumbral contact
Parramatta Observatory	33.81°S	151.03°E	0.878	6:47 sun rises eclipsed 7:04 mid-eclipse 8:17 last penumbral contact
Sydney	33.87°S	151.22°E.	0.876	6:47 sun rises eclipsed 7:09 mid-eclipse 8:18 last penumbral contact
Maitland	32.73°S	151.55°E	0.899	6:45 sun rises eclipsed 7:09 mid-eclipse 8:18 last penumbral contact
Brisbane	27.47°S	153.03°E	Total eclipse 1m 29.3s duration	6:37 sun rises eclipsed 7:08:34 first umbral contact 7:09:18 mid-eclipse 7:10:03 last umbral contact 8:20 last penumbral contact
Norfolk Island	29.05°S	167.95°E	0.892	7:14 sun rises eclipsed 8:24 mid-eclipse 9:46 last penumbral contact
Tonga	20.00°S	175.00°E	0.961	8:29 sun rises eclipsed 9:54 mid-eclipse 11:30 last penumbral contact
Cook Islands	20.00°S	158.00°E	0.875	10:11 sun rises eclipsed 11:48 mid-eclipse 13:23 last penumbral contact
Pitcairn Island	25.07°S	130.08°E	0.642	13:25 sun rises eclipsed 14:45 mid-eclipse 15:56 last penumbral contact

The predictions in the *Australian Almanack* are an example of a badly thought-out listing. The tabulation begins correctly with a 7 August date for Greenwich, but the compiler apparently assumed that any reader would understand that the next line's "in the morning" was for 8 August, the following day—or simply forgot to advance the date to conform with Sydney time, which is $10^h4^m52^s$ ahead of Greenwich. Further, the value given for the so-called beginning of the centrally-eclipsed sun seems to be in error. The words "total obscuration" would certainly cause a reader to think that totality was limited to a small area on the globe, not along a fairly narrow path thousands of kilometers long. Finally, the values in the table given are in poor agreement with EmapWin results.

Throughout this tabulation, it is left to the reader to grasp that the values are not for local circumstances at Sydney, but for locations from 660 to more than 5,000 km away: at first contact of the umbra, and when centrally eclipsed on the meridian. Small wonder that the *Sydney Gazette's* editor summarized the predicted event … on the wrong day.

3.3 Other EmapWin Results

Table 3 lists local circumstances for places of interest. Not surprisingly, for Hobart and Sydney the times shown are different from those given in the respective almanacs. In the case of Hobart, if an unsophisticated reader assumed—incorrectly—that the almanac value of 8:14 in the morning was linked to local circumstances, by that time the actual eclipse was over. Even if a hopeful observer had turned up earlier to make sure he would see the event, the hilly terrain across the River Derwent would have blocked the first minutes of the eclipsed Sun's rising, reducing his window of opportunity.



AUSTRALIAN

ALMANACK,

FOR THE YEAR OF OUR LORD

1831;

BEING THE THIRD AFTER BISSEXTILE, OR LEAP YEAR;
AND THE FIRST OF THE REIGN OF HIS MOST
GRACIOUS MAJESTY KING WILLIAM IV.

PUBLISHED
UNDER THE SANCTION AND PATRONAGE OF
HIS EXCELLENCY
Lieut.-General Ralph Darling, &c. &c. &c.

SYDNEY:
EDITED, PRINTED, AND PUBLISHED BY RALPH MANSFIELD,
GAZETTE-OFFICE, FOR THE EXECUTORS OF R. HOWE.

Figure 2: Title page of the *Australian Almanack, for the Year of our Lord 1831* (courtesy: Special Collections, Barr Smith Library, University of Adelaide, South Australia).

The 8 August sunrise at Sydney was a different matter; there are no heights to the east-northeast to block the view. Ironically, if an unsophisticated reader in Sydney concluded (incorrectly) that the almanac values were linked to local circumstances and used the beginning value given there, he actually would have seen a partial eclipse—but *only* if the correct date had been printed with it.

4 AFTER THE EVENT

4.1 Hobart Town

The first hint that something was amiss came in an August 9 letter sent to the editor of Hobart's *Colonial Times*. Its author had not seen the eclipse and scolded the almanac compilers (G., 1831):

> The Hobart Town Almanacks publish an account of an eclipse of the sun, which was to have taken place yesterday, and let me assure your readers was not visible, although the publications say the contrary. With whom the negligence arises, is not for me to determine, but allow me to recommend their compilers to be more careful in future.

Apparently no immediate attention was paid to the letter.

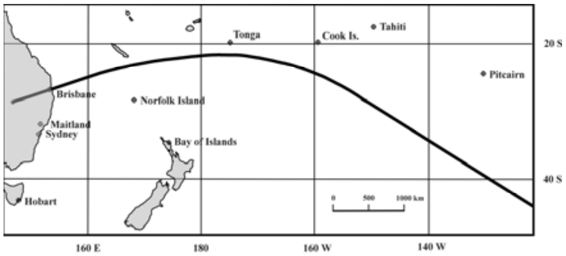


Figure 3: The path of totality for the total eclipse of 1831 (the entire path is not depicted). The data used for drawing the smooth curve are from EmapWin; the Mercator-projection base map is taken from "Oceania," CIA map 802480 (R02111) 1-97.

4.2 Sydney

On 11 August the *Sydney Gazette* printed a long and detailed letter received by its editor (Correspondent, 1831). The writer, whose equipment was a 4¼ in reflecting telescope having an achromat eyepiece of 25 power and a deep red filter placed in the focus, recounted his failure to observe the eclipse. He stated that he had used the *Australian Almanack*'s predictions to guide him, and that he had examined the sun continuously since shortly after sunrise until at least fifteen minutes after the eclipse was supposed to begin: that is, until 7:50. Further, he had continued observing the sun throughout the day, "… but there was no obscuration from the time of rising, to setting, not even the most partial." To emphasize that an overcast sky was not involved, the letter writer added, "Sunday [7 August] was a remarkably fine day."

The writer, undoubtedly a qualified observer and likely with tongue-in-cheek, noted:

> This [lack of an eclipse] is a most remarkable fact; should notice of it reach the eye of any of our Astronomers in England, especially those gentlemen who calculate the elements for the Board of Longitude, I have no doubt this statement will lead them to ascertain if there had been an error in the calculation, or whether the moon has been attracted from her orbit by means of a comet.

The *Gazette*'s editor, who was also responsible for the *Australian Almanack*'s content, answered with (Editor, 1831b)

> In reference to the calculations in the Australian Almanack, we are at liberty to say, they were corrected by one who had devoted the greater part of a long life to mathematical and astronomical studies, and who was considered to possess superior talent as well as ample experience.

In spite of this somewhat effusive appeal to the skills of a deceased calculator, the issue would not go away. On the 22nd of the same month the *Gazette*'s editor received another long and detailed letter, this time from Australia's Reverend Henry Fulton (1761–1840), well-versed in mathematics thanks to his initial training at Trinity College, Dublin. He began his exposition—also tongue-in-cheek?—with

> Observing in the newspapers extraordinary conjectures about the approach of a comet to our regions, so near as to attract the moon out of her usual orbit, and yet not perceivable by human eyes; I had the curiosity to calculate from Ferguson's and Maskelyne's tables, the correct times of full and change of the moon in this month, and also her distance from the node at these times. I found that these times agreed with the times in the *Australian Almanack*, and the distances from the nodes were such as to cause great eclipses … (Fulton, 1831).

Having calculated approximate values for the two lunar and two solar eclipses occurring in 1831, Reverend Fulton then stated that for the third one, its middle time was on "… the 8th of August, at 8 minutes past 8 in the morning—without an error of more than 20 minutes." (The EmapWin result for the eclipse's mid-point is 8:21.) His final words were damning: all the *Australian Almanack*'s eclipse predictions were seriously—hours—in error, "… nearly 24 hours in the third …", the eclipse under discussion here.

The editor put the best face on the situation by noting that Reverend Fulton had shown that a portion of the work in the *Australian Almanack* was satisfactory (the phases of the Moon). However, the editor wrote, "… the eclipses, which he [the almanac's compiler] entrusted to one whom he considered more competent than himself, are wrong by several hours." Then he deliberately(?) confused the issue by adding:

> … with respect to the solar eclipse, the middle of which Mr. F. fixes at 8 minutes past 8 of the morning of the 8th of August, we are as much in the dark as ever, for the sun was carefully watched in Sydney on the 8th as well as on the 7th, but there was no indication of shadow visible on his disk. (Editor, 1831c).

Toward the end of September the editor tried once again to soften the blow aimed at the *Australian Almanack*'s reliability. After seeing a column in *Colonial Times* discussing the failure to observe the eclipse there, he prefaced his publication of it with:

> Is it not a little strange, that it now appears that the Hobart Town Almanacks were as much out in their reckoning concerning the late solar eclipse, as least as far from being confirmed by the fact, as was that of Sydney. (Editor, 1831d).

He quoted in full the 9 August letter to the *Colonial Times* given above as well as the Hobart newspaper editor's 7 September response to the complaint:

> On receipt of this letter, we very naturally concluded that the Hobart Town savants were wrong, and we were unwilling to expose their error, but on skimming over the *Sydney Gazette*, we find our neighbors have also been deceived as well as ourselves.

(The *Colonial Times* editor followed this reader's statement by reprinting the critical remarks given in the *Sydney Gazette*'s 11 August issue.)

What to make of all this? Most likely a general reader could figure out that something had gone awry. But he might not conclude correctly on whose doorstep the error should be placed. For the printed exchange tends to indicate that both editors were not yet willing to accept the fact that an 'error' actually existed in the almanacs—that the compilers had provided information essentially of no use at all to their readers. Further, if they had admitted it, then their own ignorance with regard to what they were placing in the newspaper columns would have been exposed.

5 WHO SAW THE ECLIPSE?

5.1 The 'Naïve Observer'

In the early years of the nineteenth century, European communities in Oceania were few and far between. An idea of Australia's small population can be gleaned from official statistics. In 1830 the population of Tasmania (Van Diemen's Land) was 24,279 persons; that of New South Wales in 1833 was 60,794, of whom 16,232 lived in Sydney; and in 1836 an estimated 5,000 persons were living in Parramatta. In the year of the eclipse, the Moreton Bay Penal Settlement—today's Brisbane—numbered 1,066 convicts and 175 soldiers. Elsewhere in Oceania a tiny handful of Christian missionaries was busy proselytizing on a few of the South Seas islands, with many of them keeping a daily journal of events.[2]

This vast region's population was almost entirely composed of non-specialists having no access to the prediction of an approaching event. A total eclipse's sudden darkness would have taken any one of them by surprise; in general, a partial eclipse would have escaped notice. 'Naïve observer' is an apt characterization.

At some stage during an eclipse's progression toward totality and darkness, even a naïve observer would become aware of the decrease in the sky's brightness. A number of eclipse descriptions intended for the general public include the following:

> A total solar eclipse is not noticeable until the sun is more than ninety percent covered by the moon. At ninety-nine percent coverage, daytime lighting resembles local twilight.

While the second statement can be traced backed to numerous experimental verifications including photometric studies undertaken in the 1960s under the aegis of the Air Force Cambridge Research Laboratories in Massachusetts (Sharp, et al., 1970; and Silverman and Mullen, 1975), no comparable reference for the first one has been found. Nevertheless, this psychophysical statement allowed us to drop from consideration journals of missionaries on Tahiti and at the Bay of Islands, New Zealand, where the magnitude of the maximum eclipse was 0.717 and 0.740, respectively, and almost certainly would not have been noticed.

5.2 Candidate Locations

For the places listed in Table 3, only a very few written records were found. In this section the specific source, if any, is identified along with a summary of the entry for the day of the eclipse.

5.2.1 Australian sites

5.2.1.1. Hobart and Sydney. Already discussed in detail are the almanac predictions and the subsequent exchanges in Hobart's *Colonial Times* and in the *Sydney Gazette*.

5.2.1.2. Parramatta Observatory, maximum eclipse 0.878. No astronomer was at the Observatory from the beginning of 1829 until late 1831; James Dunlop's observing books begin in January 1832.

5.2.1.3. Maitland, maximum eclipse 0.899. On 8 August 1831 surveyor Felton Mathew was working for the New South Wales Survey Department in or near Maitland. His journal entry for that day does not mention the partial eclipse (Mathew, 1831).

5.2.1.4. Brisbane, total eclipse. No mention of the eclipse was found in the official correspondence from the Moreton Bay Penal Settlement. The archived Register for the Brisbane General Hospital, 1825-1844, which includes the state of the weather, are missing the daily records for 1831.

5.2.1.5. Norfolk Island, maximum eclipse 0.892. The surviving records for 1831 of the second penal settlement at Norfolk Island, Lieutenant-Colonel James Morisset commandant, have no eclipse-related information.

5.2.2 Tonga (maximum eclipse 0.961)

The daily journal of John Thomas, a member of the Wesleyan Methodist Missionary Society (London) assigned to Tonga, does not mention the partial eclipse. However, his entry for the following day (9 August) includes "Yesterday we had bad weather, but was able to get on with the translations." Extracts of the journals of James Watkin and Peter Turner for the same period do not mention the event.

Considering how close these missionaries were to the path of totality and the resulting great obscuration of the Sun, the comment regarding bad weather provides a possible explanation for them not noticing a decrease in the sky's brightness: throughout the morning the sky was overcast (heavy rains?).

5.2.3 Cook Islands (maximum eclipse 0.875)

The manuscript journal of Charles Pitman, one of three members of the London Missionary Society on Rarotonga during 1831, was examined. No mention of the partial eclipse was found.

5.2.4 Pitcairn Island (maximum eclipse 0.642)

This magnitude is far below the level at which a naïve observer would become aware of the event. However, a completely unexpected entry appears in the records being kept on the island, carrying a 7 August 1831 date: "Sun Eclypsed." A detailed explanation follows.

The *Pitcairn Island Register Book*, a record of births, deaths, and marriages, was initiated by John Buffett, who came to the island in 1823 (Figure 4). The original, whose entries are quite sparse between 1790 and 1823 and which ends in 1854, has been microfilmed (S.P.C.K., 1977). Its contents were transcribed and published, along with an introduction by Sir Charles Lucas who added appendices dealing with the fascinating history of the *HMS Bounty* mutineers and their descendents (Lucas, 1929). The entry, "Sun Eclypsed," with its 7 August date, is in the "Remarkable Family Events" section of the *Register Book*.

The date of 7 August stems from the fact that in 1831 the Calendar or Date Line passed east of Tahiti and the Tuamotu Archipelago and west of Pitcairn Island; that is, the Pitcairners were keeping Western or American dating (Bartky, 2007). That the event was actually recorded is a consequence of the turbulence that was altering the lives of these isolated descendents of the mutineers.

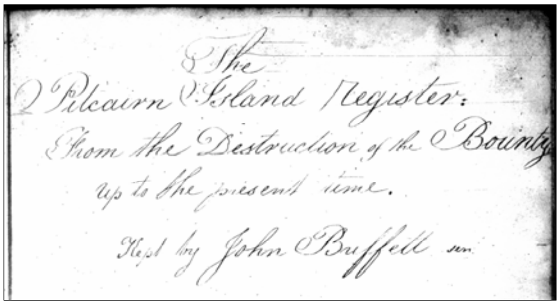

The
Pitcairn Island Register:
From the Destruction of the Bounty
up to the present time.
Kept by John Buffett sen

Figure 4: Half-title of the *Pitcairn Island Register Book*, 1790-1854 (courtesy: Society for the Promotion of Christian Knowledge, London).

In the late 1820s, fearing that the expanding population on Pitcairn Island would exceed available resources, the British Government decided to remove them to Tahiti. On 6 March 1831 all eighty-six inhabitants embarked on a transport ship, arriving at Papeete, Tahiti's major settlement, on the 21st. In this new environment many became ill; over the next two months a dozen Pitcairners would die. Further, this most devout group of Christians was profoundly shocked at what was to them the licentious behavior of the Tahitians. They pleaded with the London Missionary Society's authorities on the island to be allowed to return to their homeland, and arrangements to have them go back to Pitcairn were initiated.

Not anxious to linger on Tahiti for some unknown period of time hoping for a solution to their plight, on 24 April twelve Pitcairners under the leadership of John Buffett sailed back to their home island in a small schooner. According to an entry in the *Register Book*, adverse winds stranded them on Lord Hood's Island in the Tuamotu Archipelago, and the schooner that was to have taken them home returned to Tahiti.[3] The Pitcairners waited there for further transport, during which time they suffered the death of one of their number. Finally on 21 June they embarked on the "French Brig Bordeaux packet" (*Courier de Bordeaux*?) and were taken to Pitcairn Island. The group arrived on the 27th, noting their return in the *Register Book*. It had taken them two months to come home (Lucas, 1929: 36, and Moerenhout, 1837: 442-444).

The printed edition of the *Register Book* includes a section, "Arrivals." Transcribed from the original document, also kept by John Buffett, it is an incomplete but chronological listing of the vessels stopping by Pitcairn Island. An entry dated 3 July 1831 lists "[Whaleship] *Origon* of Fairhaven [Mass.], 307 tons, Jabez Delane [*Delano*, master]."

The next chronological entry of interest appears in the "Remarkable Family Events" section of the *Register Book*. It is dated 2 September, and notes the return from Tahiti of the rest of the Pitcairn Islanders on the American brig *Charles Doggett* via a sea voyage lasting only seventeen days.

In summary, these entries demonstrate that eleven Pitcairners—the first wave of returnees—were on Pitcairn Island the day of the eclipse, which was a partial one at the island's location. Thus far, the *Register Book* is the sole document linking the solar event to an observer.

5.2.5 Other Locations

Surviving logs of eight whaling ships, selected from the lists of vessels known to be in the South Pacific Fishing Grounds during 1831, were examined to determine if any August eclipse entries, partial or otherwise, were recorded. Unfortunately, no entries were found.

6 CONCLUDING REMARKS

First of all, it is understatement to term the Pitcairn Islanders 'naïve'. Prior to their disastrous move to Tahiti, the vast majority of them had never been off the island. Just as with such observers elsewhere, the Pitcairners should have sensed nothing different in the sky's brightness during the solar event; the evidence in the *Pitcairn Island Register Book* indicates otherwise. A plausible explanation for this apparent contradiction is at least one of the eleven Pitcairners on the island was informed of the event prior to its occurrence. Who informed them? Since the logs of the French packet ship and the American whaler have never been located, one can only speculate: the navigator of one of the two vessels was the source of the Islanders' information.

Second, as is shown in Table 1, the *Nautical Almanac*'s total-eclipse prediction, the partial-eclipse prediction from the *American Almanac*, and Reverend Fulton's calculations demonstrate convincingly that various aspects of the solar event could be calculated quite accurately. On the other hand, the several almanac compilers in Australia lacked comparable skills, and this fundamental problem was exacerbated by the editors' lack of understanding of what was being placed before them. Fortunately for all, soon after the 1831 eclipse a major transformation of the *Nautical Almanac* was initiated, one part of which gave editors a means for judging their compilers' products.

In a report to the Lords Commissioners of the Admiralty dated 19 November 1830, a distinguished committee of the Astronomical Society of London (today's Royal Astronomical Society) recommended sweeping changes in the annual issuance. Among them was Recommendation 23, focused on the section devoted to solar and lunar eclipses:

> The Committee recommend [sic.] that in the account of the solar eclipses, there should be given the elements employed in the computation, the line of the moon's umbra across the earth, together with a diagram of the same; and generally more particulars relative to the phenomena, as in the Berlin Ephemeris.

The Admiralty ordered the committee's recommendations to be carried into effect (Astronomical Society, 1830: xix).

The changes took effect in the *Nautical Almanac and Astronomical Ephemeris for 1834*, printed in 1833. An eclipse diagram of the kind recommended by the committee astronomers was included (Figure 5). Together with accompanying data, it allowed anyone with no more than modest cartographic skills a means for judging the quality of a compiler's products. For example, a user or a local editor could glance at a diagram in the style of Figure 5 and see that the eclipse predicted for, say, Cape Town would be a partial one, and that the path of totality would begin at sunrise in the Atlantic Ocean, cross Africa, and end at sunset in the Indian Ocean. With additional mathematical skills and knowledge of astronomical conventions, the time of the eclipse in a particular locale could be estimated.

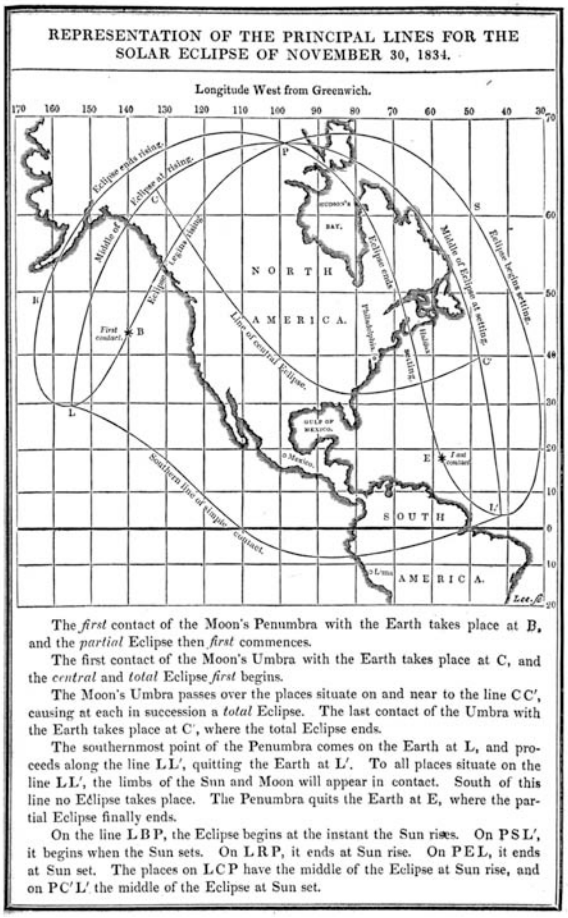
The impact of this transformed, authoritative source on locally-produced almanacs is not a subject of this study. However, being printed well before the event—the Admiralty also adopted the recommendation that the *Nautical Almanac* be made available four years in advance—doubtless it had an impact on the quality of these later, derived publications.

7 ACKNOWLEDGEMENTS

Numerous individuals on several continents and islands contributed to the completion of this near-decade-long effort. Former career diplomat for New Zealand, Rhys Richards, now a maritime historian specializing in Pacific Ocean history prior to 1850, located and transcribed the Hobart Town almanac discussed here, forwarded an overlooked item in the *Sydney Gazette*, and re-checked the daily journals of several South Seas missionaries; he also selected and examined the whaling ship logs. Professor (ret.) Niel Gunson, Australian National University, found in his research notes the 'solution' to the lack of an eclipse sighting by the missionaries on Tonga. Nigel Erskine, Curator for Exploration, Australian National Maritime Museum, reviewed the records for the penal settlement on Norfolk Island; Nick Lomb, Curator of Astronomy, Sydney Observatory/Powerhouse Museum, provided the details on the Parramatta Observatory; and Tony Marshall, Senior Librarian (Heritage Collections), State Library of Tasmania, guided me through the thicket of almanacs printed in Hobart during the 1830s.

During my sojourn in the United Kingdom, the Council for World Mission gave me permission to examine the archives of the London Missionary Society, as did the Methodist Church with regard to the archives of the Wesleyan Methodist Missionary; both collections are held at the School of Oriental and African Studies, University of London.

On this continent Wendy Schnur, Collection Research Center, Mystic Seaport, Connecticut, corrected one of my ship identifications; Librarian Gregory Shelton located a large number of sources in the Naval Observatory Library's superb astronomy collections; and USNO Public Affairs Officer Geoff Chester, who has seen more than a half-dozen total eclipses, shared his experiences of the changes in the sky's brightness as totality approached, as did David DeVorkin, National Air and Space Museum, Smithsonian Institution. At the Library of Congress, Science Reading Room librarians Constance Carter and Margaret Clifton tracked down a large number of early nineteenth-century works dealing with Australia. Retired Naval Observatory astronomer Alan Fiala, who spent most of his career at the Nautical Almanac Office, including serving as its chief, commented in detail on an earlier draft, thereby strengthening my subsequent one. I am also indebted to Professor of Astronomy Jay Pasachoff, Williams College, Mass., whose comments and suggestions regarding my earlier draft were invaluable.



REPRESENTATION OF THE PRINCIPAL LINES FOR THE SOLAR ECLIPSE OF NOVEMBER 30, 1834.

The *first* contact of the Moon's Penumbra with the Earth takes place at B, and the *partial* Eclipse then *first* commences.

The first contact of the Moon's Umbra with the Earth takes place at C, and the *central* and *total* Eclipse *first* begins.

The Moon's Umbra passes over the places situate on and near to the line CC′, causing at each in succession a *total* Eclipse. The last contact of the Umbra with the Earth takes place at C′, where the total Eclipse ends.

The southernmost point of the Penumbra comes on the Earth at L, and proceeds along the line LL′, quitting the Earth at L′. To all places situate on the line LL′, the limbs of the Sun and Moon will appear in contact. South of this line no Eclipse takes place. The Penumbra quits the Earth at E, where the partial Eclipse finally ends.

On the line LBP, the Eclipse begins at the instant the Sun rises. On PSL′, it begins when the Sun sets. On LRP, it ends at Sun rise. On PEL, it ends at Sun set. The places on LCP have the middle of the Eclipse at Sun rise, and on PC′L′ the middle of the Eclipse at Sun set.

Figure 5: Representation of the principal lines for the solar eclipse of 30 November 1834. Diagram from *The Nautical Almanac and Astronomical Ephemeris for the Year 1834*, 471.

Two additional individuals helped make this undertaking a success. Shinobu Takesako's eclipse program made it possible for me to analyze the various almanacs at a useful level of detail. And without the encouragement and support of Elizabeth Bartky, this study never would have seen the light of day.

8 NOTES

1. The dates and time are in terms of the then-current Astronomical Day, which began at noon. After the

year 1828 responsibility for the almanac series was transferred to the Board of Admiralty. Starting with the volume for 1834, Greenwich mean time replaced apparent time in the various predicative tables.

2. Native populations are not included in these data. The Australian statistics are given in Lang (1837) and Steele (1975). For the South Seas missions, see Gunson (1978) and Lovett (1899).
3. Lucas (1929: 17) errs when he identifies this location as Hood Island, which lies far to the northeast of Tahiti in the Marquesas. Lord Hood's Island, now called Marueta Atoll, about 1,537 km to the southeast of Tahiti, is somewhat to the east of a direct line from Papeete to Pitcairn Island, which lies 681 km beyond the atoll. Today Marueta is one of several locales where black pearls are cultivated. See also Moerenhout (1837: 312).

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The late Ian Bartky died from lung cancer on 18 December 2007, after this paper was accepted for publication. Ian was born in Chicago and received a Ph.D. in physical chemistry from the University of California at Berkeley in 1962. He worked at the Bureau of Standards and after retiring in 1992 turned his attention increasingly towards the history of time-keeping. With support from the National Science Foundation, the Dudley Observatory and a Caird Research Fellowship at the National Maritime Museum (London) he was able to work on a succession of research papers and books. *Selling the True: Nineteenth-Century Timekeeping in America*, the first comprehensive history of time-keeping in the USA, was published in 2000, and his most recent book, *One Time Fits All: The Campaigns for Global Uniformity*, appeared shortly before his death (and is reviewed in this issue of *JAH*^{2}). Ian was a member of the American Association for the Advancement of Science, the Society for the History of Technology and the Historical Astronomy Division of the American Astronomical Society. He is survived by his wife of 47 years, Elizabeth Hodgins Bartky and his two children, David J. Bartky and Anne B. Goldberg.

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MOLONGLO OBSERVATORY: BUILDING THE CROSS AND MOST

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Abstract: When Bernard Mills left the CSIRO in 1960 to establish a radio astronomy group in the School of Physics, University of Sydney, he had not only invented the principle of cross-type radio telescopes but proved their great efficiency at surveying the positions, intensity and structure of radio sources. He had ambitious plans for a second generation Cross—a radio telescope with arms one mile long.

This paper describes the circumstances of Mills' appointment as Professor of Astrophysics and the recruitment of an international Department that achieved his vision with the Molonglo Cross. The construction involved interaction with many colleagues—engineers in other university departments and government agencies, and with the contracting firms. Formal links were set up with the Electrical Engineering Department through The Radio Astronomy Centre in the University of Sydney and then with Arecibo Observatory through the Cornell-Sydney University Astronomy Center.

When the Molonglo Cross completed its main survey in 1978 after eleven years, it was switched off and the EW arm was then converted to the Molonglo Observatory Synthesis Telescope. Many of the staff involved with the MOST are now challenged by SKAMP, testing systems for the Square Kilometre Array with cylindrical geometry in the Molonglo Prototype. These two later developments out of the original Cross telescope are described briefly.

Keywords: radio astronomy, Molonglo Cross, MOST, SKAMP, B.Y. Mills.

1 THE CONCEPT—THE FIRST CROSS-TYPE RADIO TELESCOPE

Much of the earliest radio astronomy in Australia was carried out by staff from the CSIR's Division of Radiophysics at a number of field stations in or near Sydney using simple aerials, some of which were based on radar technology (Orchiston et al., 2006; Orchiston and Slee, 2005; Sullivan, 2005). In the case of the famous Dover Heights cliff interferometer, sources were observed at low elevation angles and a second 'antenna' was formed vertically below the cliff by an image in the ocean (see Bolton and Slee, 1953). In 1949, the Head of the Division's radio astronomy group, Dr Joe Pawsey, suggested to one of the young research scientists, B.Y. Mills (Figure 1), that he should begin a research program on discrete radio sources. However, "… Pawsey knew that the future lay with the use of horizontal baselines and Bolton was still making effective use of the interferometer that had proved so successful for him previously." (Mills, 2006: 3).

In the early 1950s, Mills developed large EW baselines with a 3-element interferometer at Badgery's Creek near Sydney (see Figure 2, inset) and observed near the zenith to obtain accurate positions in a sky survey of 77 discrete sources (Mills 1952). He realised that the next deeper survey would require more sensitivity and greater NS and EW spacings. His solution in 1953 was a new cross-type telescope in which the correlation of two intersecting fan beams yielded a high resolution pencil beam. The design could also separate the sensitivity of the telescope (dependent on its area) from the resolution (dependent on the maximum arm length and hence the extent of the intercepted wavefront). In the face of official skepticism, Mills built a 120-ft prototype cross at Potts Hill field station near Sydney (Figure 2, inset) to prove his concept, and he was joined in this venture by a Technical Officer, Alec Little, at the start of what would turn out to be a 32-year partnership (Mills and Little, 1953).

Figure 1: Rudolph Minkowski (left) and Bernard Mills at the Radiophysics Division's Fleurs field station during the 1950s (courtesy: Woody Sullivan).

CSIRO allocated the necessary resources and the 85.5 MHz 'Mills Cross' was built at Fleurs field station (Figure 2, inset) in 1953-1954, largely by Little while Mills was on a 6-month study tour in the USA. The subsequent MSH survey was an outstanding success, giving Mills data on source counts and structure for the Galactic Plane and the extragalactic sky with 49 arcminute resolution (Mills et al, 1958).[1] At about the same time, W.N. Christiansen developed a cross array of small dishes at 1,415 MHz for synthesis mapping of the solar disk (Christiansen et al., 1961) and C.A. Shain built a third cross at Fleurs for low frequency observations (Shain, 1958).

Despite the versatility and performance of these cross-type radio telescopes, CSIRO abandoned their 15-year tradition of astronomy using ingenious small aerial systems and did no further development at Fleurs. The era of large parabolic dishes had already begun in Netherlands, Germany and England, especially with the 250-ft Jodrell Bank Radio Telescope. In 1953, the CSIRO's Division of Radiophysics embarked on the planning of a 'Giant Radio Telescope' (GRT). Chief of the Division, Dr E.G. ('Taffy') Bowen, succeeded in obtaining funding from both the Carnegie Corporation and the Rockefeller Foundation in USA for a GRT in Australia and the design contract for it was placed in 1956 (Robertson, 1992). When Mills and Christiansen drove in the peg to mark its position on a farm near Parkes, both knew that there was no future within CSIRO for their types of cross and array radio telescopes. All remaining resources available to the Radiophysics Division were reserved for Paul Wild's Culgoora Radioheliograph, and there was nothing left for a large cross (Bowen, 1981).

The planning and commitment to the GRT at Parkes triggered major career changes for Mills and many other Radiophysics staff who left for research positions elsewhere.[2] Mills investigated chairs of Electrical Engineering at Adelaide, Melbourne and Sydney but these did not offer the financial support to build his large cross-type telescope. He found this in 1960, not in Engineering, but in Physics, at Sydney University.

2 THE SCHOOL OF PHYSICS AT SYDNEY UNIVERSITY

Harry Messel was appointed as Professor of Physics at Sydney University on 1 September 1952 and established The Nuclear Research Foundation (the first in the British Commonwealth) to "… promote, foster, develop and assist …" research with grants from "… fees, donations and the like." Between November 1959 and November 1961 Messel recruited new Physics Professors in Theoretical, Thermo-nuclear (plasma), High Energy Nuclear (cosmic rays) and Electronic Computing (The Nuclear Research Foundation, 1962). When Robert Hanbury Brown in Manchester sought funds and a site for his optical intensity interferometer, Messel began an astronomy group with Richard Twiss, Cyril Hazard and John Davis to build the interferometer near Narrabri in northern NSW. Messel also had funds for a complementary photometric telescope and sent Colin Gum to Europe to examine optical designs. Unfortunately, in April 1960 Gum died in a skiing accident in Switzerland and the telescope project never went ahead. Messel contacted Mills, approved the concept of a large cross-type radio telescope and offered him seed money sufficient to build a 408 MHz Cross with arms about 400m long. Mills joined the School of Physics in June 1960 with his initial plans dependent on funding. He comments:

> From the beginning there seemed to be few problems in constructing a Cross within the available budget of $200,000 which would be able to survey the sky at metre wavelengths with a sensitivity and resolution at least equal to that anticipated for the Parkes radio telescope operating at its optimum wavelength. But why stop there? (Mills, 1991: 720).

Any further funds would mean longer arms replicating a flexible modular design. The challenge was to find the additional financial support for a large cross.

Through some of his many overseas contacts (probably Thomas Gold at Cornell) Messel learned that the newly-established National Science Foundation was willing to make grants outside of the USA. Mills quickly wrote a proposal for his ambitious 1-mile cross-type radio telescope. In support of this he provided results from his 85.5 MHz Fleurs Mills Cross survey, and made precise predictions of possible observing programs, the number of fainter sources expected, their confusion levels and the sensitivity required of the telescope.

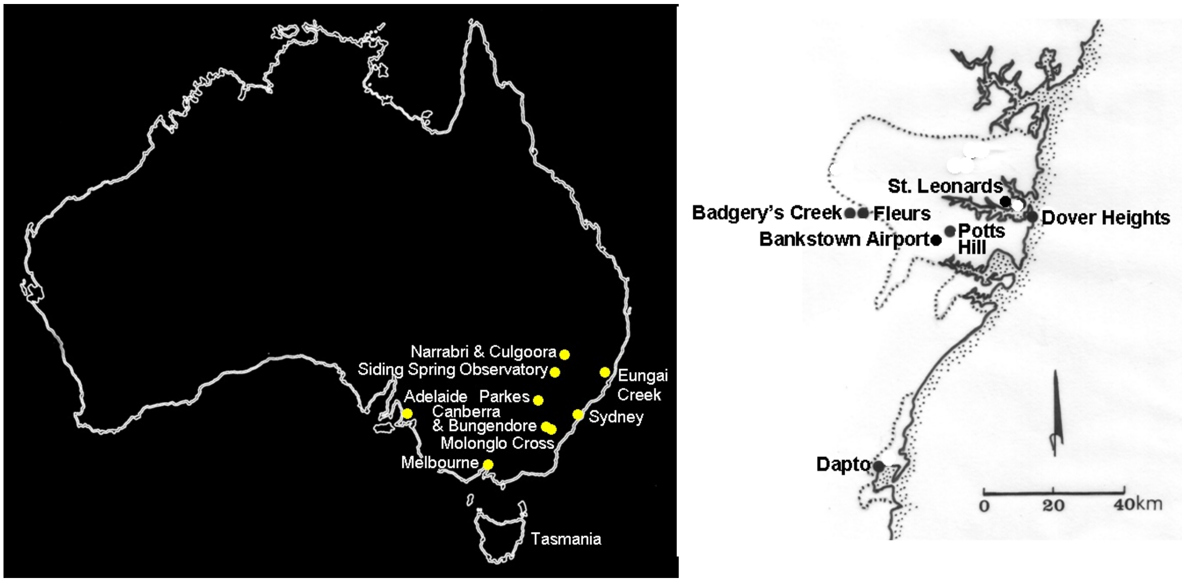

Figure 2: Australian localities mentioned in the text. The map on the right shows sites in or near Sydney (the dotted lines show the approximate present-day boundaries of suburban Sydney and Wollongong).

The application was received sympathetically, but then met opposition from Dr Bowen who advised against any grant, stating that a small university group could never manage such a large project. When the NSF sent a staff member, Geoffrey Keller, to Australia to investigate, Messel advised him to go to Canberra and talk to Bart Bok. The visit reassured the NSF, and in 1962 they approved the first (and perhaps the only) large foreign grant that the NSF ever made. The initial funding of US$746,000 was followed by US$107,500 in 1964 and allowed the Molonglo Cross project to go ahead with its planned mile-long arms. In his first published description of the project, Mills (1962) wrote: "This is a greatly enlarged version of the original 'Mills Cross' put into operation by the CSIRO in 1954 ... the beamwidth would be about 2.8 arcmin and the sensitivity adequate to detect more than a million radio sources."

Meanwhile Messel negotiated the purchase of a site for the new Cross in a wide flat valley near Canberra. This was one of the sites that had been investigated for the GRT but which had been rejected in favour of Parkes. The height of the GRT would have put it in line of sight over hills to the transmitters on Black Mountain in Canberra but the cylindrical reflectors of the Cross were lower and remained shielded. Thus in the Parish of Molonglo on a branch of the Molonglo River, construction of the Molonglo Radio Observatory was commenced in 1961.

2.1 The Astrophysics Department and Colleagues

Mills recruited staff for his new Department from industry and radio astronomy centres around the world. Arthur Watkinson joined first from CSIRO in 1960 and Terry Butcher, tool maker, recently returned from radio and TV work in Canada. Alec Little, who had been Mills' assistant at CSIRO since 1948 and was completing an M.Sc. at Stanford University with Ron Bracewell, was persuaded to return to Sydney early in 1961, but only after two meetings with Harry Messel in San Francisco. The appointment of Pat O'Brien (Cambridge) was a disappointing bungle—the letter of offer never reached him, and he took a Chair of Physics at Khartoum instead. Bruce McAdam (Cambridge) came from New Zealand in Easter 1961 to visit Dapto, Parkes and Sydney University before deciding to join Mills in June. Two more technical staff members, Jack Howes (from AWA) and Grant Calhoun, were appointed in 1962. The initial academic team was completed when Michael Large (Jodrell Bank) and Tony Turtle (Cambridge) arrived from the UK in February 1963. Table 1 gives dates of appointments and resignations of relevant staff at the University of Sydney in the order of their association with the Molonglo story, up to 1978.

This small university group built the Cross over the next six years, but did so in cooperation with many university and industrial colleagues. Many years later, Mills (2006: 10) was to reminisce: "I found myself manager of a big engineering project. It was not an enjoyable job but there was no one else to do it and I was much helped by my engineering contacts, stretching back in some cases to student days."

From the start there was a major partnership with W.N. ('Chris') Christiansen in the Department of Electrical Engineering who took responsibility for the receiving system. The cooperation was made formal with the formation of the Radio Astronomy Centre in the University of Sydney (Messel, 1960). Professor Aitchison enticed Bob Frater to leave industry (AWA, OTC and then DUCON) and join the Electrical Engineering Department in 1961 specifically to work on the electronic design of the Molonglo Cross using the (then new) transistor technology. Frater (2005) later commented: "I jumped at the opportunity. Bernie had in mind an instrument where the technical demands stretched significantly beyond the technology of the time."

Table 1: University of Sydney staff associated with Molonglo Observatory, 1960 to 1978.

Name	First University Appointment	Resigned or Retired	Comments
School of Physics			
Richard Twiss	1 July 1957	14 May 1963	Returned to UK
Colin Gum	1959	Killed 28 April 1960	skiing accident
Bernard Mills	11 July 1960	31 December 1985	Retired in Sydney
Arthur Watkinson	10 October 1960	22 December 1961	To Dwingeloo
reappointed	16 April 1964	16 August 1964	To Fleurs Observatory
reappointed	January 1967	?	Died 1997 May 12
Terry Butcher	6 February 1961	29 January 1965	Retired in Tasmania
Alec Little	13 April 1961	Died 20 March 1985	
Bruce McAdam	21 June 1961	21 September 1993	Research Associate
Alan Le Marne	5 February 1962	30 April 1972	Retired in Sydney
Jack Howes	1 February 1962	6 March 1976	Died
Grant Calhoun	2 May 1962	6 July 1979	Retired in Eungai Creek
Tony Turtle	6 February 1963	30 November 1998	Research Associate
Michael Large	12 February 1963	Died 4 March 2001	
Michael White	5 January 1964	31 August 2005	Retired in Bungendore
John Horne	22 February 1965	6 August 1993	Retired in Sydney
Hugh Murdoch	January 1951	August 1986	Retired in Sydney
David Crawford	January 1969	2004	Back from Cornell
John Durdin	1 July 1975	10 January 1986	Now in Tasmania
Electrical Engineering			
Wilbur Christiansen	21 April 1960	21 December 1978	Died 26 April 2007
Ron Aitchison	?	?	Died
Ian Lockhart	?	?	Died 2 May 1976
Bob Frater	5 June 1961	31 October 1980	Now in Sydney

Dudley Rannard, one of Mills' student colleagues, took leave from the NSW Government Public Works Department and lived at Molonglo as site engineer during the concrete and steel construction stages. Specialist surveying help came from Phil Jones, Lecturer in Civil Engineering, who did the local survey to define the cardinal directions of the Cross and later, in October 1963, checked the aperture of the EW steel frames. In December 1965, B.P. Cook of the Division of National Mapping fixed the location of the intersection of the arms and alignment on the Australian Geodetic Datum (Cook, 1965). The arms of the Cross have become a land mark for local pilots. They are true north and east within 4 arcseconds and are known to 0.3 arcseconds.

Figure 3: The beginning of the Cross. Terry Butcher and Alec Little with the prototype EW module at the old St. Leonards brick pit in Sydney. The prominent building in the background is part of the Royal North Shore Hospital.

2.2 The Cornell-Sydney University Astronomy Centre

Through the close friendship of Messel and Tommy Gold, the Radio Astronomy Centre was soon expanded to the Cornell-Sydney University Astronomy Center which shared expertise between Molonglo and the newly established Arecibo Observatory. Many Sydney students spent some years at Ithaca or Arecibo, including Ron Wand, Don Campbell, John Sutton, Tony Bray, Juris Ulrichs, Dave Jauncey, Michael Yerbury, Ian Johnston and David Crawford. Other staff made short visits on study leave: Raphael Littauer came to Sydney in 1963 and Hugh Murdoch visited Cornell in 1969.

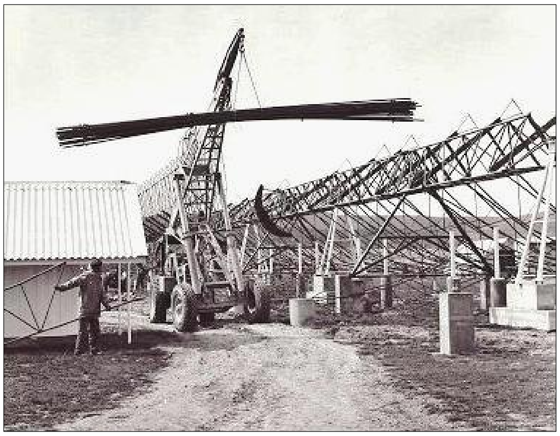

Figure 4: Erection of the East arm reaches the centre. The insulated phasing hut is on the left.

3 CONSTRUCTION OF THE MOLONGLO CROSS

The Cross was formed by two intersecting parabolic cylinders that were built with 29 foot modules. The east and west arms each had 88 modules and were separated by the continuous north-south arm with 177 modules. The foundations and steel parabolic frames were designed under the supervision of consulting engineers Macdonald Wagner and Priddle, with advice from Dudley Rannard and De Havilland engineers. Prototype modules with mesh and feeds were erected in a disused brickpit at St. Leonards for mechanical and RF testing (see Figure 3).

Contracts with Samsons (foundations and buildings) and Tubewrights (Australia) Ltd (steelwork) were signed in May 1962 and construction began at Molonglo at the end of 1962. The telescope foundations, control buildings and quarters were built while the steel frames were prefabricated in Sydney. Assembly at Molonglo was rapid (see Figure 4): the EW arms were completed first, and then the whole mile of fixed NS frames were erected in just three weeks, by mid 1963.

The design of the radio antenna feed systems for the two arms took much longer. The RF dipoles were supported along the line focus by aluminium frames. For the E and W arms, the frame for a module formed two waveguide cavities, each excited by eight dipoles. The two arms formed a meridian transit telescope with 2,816 in-phase dipoles exciting 352 waveguides. The contracts for these waveguides and dipoles were placed in March 1963 with De Havilland at Bankstown Airport where Mills knew a colleague from student days. Assembly and testing was relatively simple for the waveguide feeds along the EW arms and they began operating in May 1965 as a fan beam with a resolution of 4 degrees × 85 arcseconds. Astronomy had commenced at last.

The Molonglo Radio Observatory was formally opened by Prime Minister, Robert Menzies, on 19 November 1965 in the presence of the US Ambassador, NSF and Cornell representatives and many astronomers (Figure 5).

The RF design of the NS arm was much more complex. Technically the major problem was phasing the 4,248 dipoles along the mile arm. Each module had 24 dipole elements at the focus that required a different phase gradient for every declination observed. After some experimenting, the phase for each dipole was set by rotating a helical directional coupler within a parallel transmission line (Figure 6). The RF phase changed as the coupler was set to its appropriate angle by a chain of gears, different for each dipole in a module, and driven by a common 1 mile shaft (Mills et al, 1963). It was an ingenious but complicated scheme that worked reliably for eleven years.

A prototype NS feed arrived from De Havillands on 29 December 1964 and was hoisted to the roof of the Physics building where testing and final RF design took place over several months before factory production could begin. The first batch of feeds was delivered to Molonglo in August 1965. The dipoles, couplers and gears were then fitted and each feed was checked for mechanical and RF performance until it met Mills' stringent requirement that all dipoles had RF phases within 3 degrees (6 mm path) and gains equal within 0.6 dB. The assembly, testing and adjustment was a slow and meticulous task. After a batch of 10-15 feeds was ready all staff would join for a day to lift them into position along the NS arm. With experience, the production rate increased from one module in the first week to eighteen per week during the final month and the last of the 177 feeds was completed on 16 November 1966.

During this time the receivers, IF phasing, delay lines, display and recording systems were designed and built. All demanded innovative design using silicon transistors and the first simple integrated circuits that were only just coming available. Signals from the 177 NS modules were phased into a comb of eleven fan beams across a 15 arcmin zone of declination. When correlated with the EW transit fan beam they produced eleven overlapping pencil beams. In December 1970, two more EW early and late transit beams were added to give 33 simultaneous pencil beams.

Mills realised that the detailed analysis of the observations made by this complex system required a computer and hence a digital output system: “The principal output of the instrument will be punched paper tape for processing by an electronic computer.” (Mills, 1962). The School of Physics had built and operated the SILLIAC computer since 1956. With a memory of 1,024 words, this was far too small for the Molonglo data but a larger KDF9 computer was about to be commissioned. KDF9 had adequate processing and memory, but its input was also only by punched card or paper tape. Astrophysics collaborated with Dr Sam Luxton in the Mechanical Engineering Department to design and build a system that recorded data at a field station on digital magnetic tapes. The slow data rate from the Molonglo Cross observing over 18 hours each day used the same protocols as the fast data rate from Luxton’s low-turbulence wind tunnel with short experiments on heat exchange. The magnetic tapes were later transferred for replay to a KDF9 input buffer. This data transfer could take several days and Mills insisted that there must be an analogue display for monitoring the telescope during an observation. A facsimile chart recorder was designed that displayed the output of the fan beams and eleven pencil beams together with a contour plot of all sources within the declination zone while the transit observation was made (Large and McAdam, 1966).

‘First light’ for five pencil beams occurred in March 1967 and the full comb of eleven pencil beams was ready soon after. In August 1967 the Cross began routine observing with 2.8 arcminute resolution. The sensitivity was improved in 1969 by installing preamplifiers on the EW arms. There was a similar upgrade in 1976 in collaboration with CSIRO. For the second pulsar survey of the Galactic Plane, Andrew Lyne, on leave from Jodrell Bank, built low-noise RF amplifiers for each of the 352 waveguides along the EW arms. Table 2 shows the final performance specifications for both the Cross and its successor, the MOST.

4 OBSERVATIONS 1965 TO 1978

Up to the start of observations using the EW fan beam in May 1965, only four papers had been published by the group. The first paper using Molonglo data was a survey of nearby spiral galaxies (Mills and Glanfield, 1965). In the following three years twenty papers were written, mostly by the research students who had done much of the repetitive construction and were given priority in observing. Their targets were spiral galaxies, planetary nebulae and the Galactic Centre. Once the Cross was observing, its major task for eleven years was a 408 MHz survey of the southern sky from the South Celestial Pole to declination +18 degrees. The resulting catalogue of 12,141 sources was published by Large et al in 1981.

Figure 5: Alec Little at the control Desk on Opening Day, 19 November 1965.

Other papers gave source positions better than 2 arcsec and consequent optical identifications, describeed Galactic sources, mapped the Large and Small Magellanic Clouds, and reported many pulsar discoveries. The scientific output was an average of sixteen papers each year and the total reached 194 by 1978. Among these, Mills (1991, 2006) mentions two that had particular significance: David Wyllie used a standard dipole with the East and West arms to determine an absolute flux density scale at 408 MHz which was eventually adopted worldwide, while Peter Shaver (from Canada) and Miller Goss (from USA) did much to quell the rivalry between the Molonglo and CSIRO radio astronomers as they jointly observed radio emission from Galactic HII regions using data with comparable resolution from both Cross (408 MHz) and Parkes (5006 MHz). Brian Robinson (2002) comments on the origin and resolution of this rivalry:

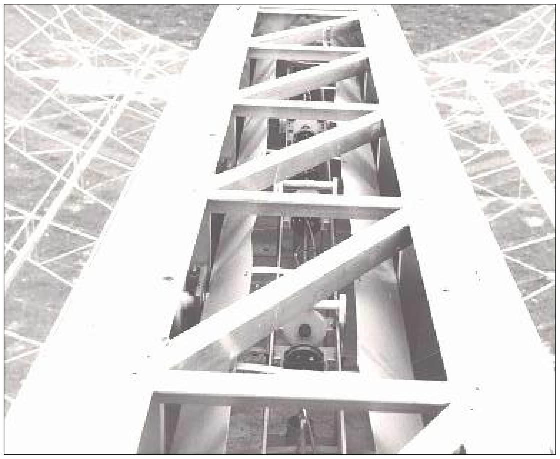

Figure 6: The top view of N88 feed showing worm gears and directional couplers.

Table 2: Specifications and Final Performance of the Cross and MOST

Feature	Cross	MOST
Centre Frequency (MHz)	408	843
Effective Bandwidth (MHz)	2.5	3
Time resolution (microsec)	0.5	0.5
Declination Coverage (°)	+18.5 to –90	+18.5 to –90
for full HA Synthesis	----	–30 to –90
Meridian Angle Swing (°)	0 (transit)	60
Field size for full synthesis	----	23' × 23' cosec dec
Time shared	----	160' × 160' cosec dec
Synthesized Beam	2.8' x 2.8'sec Z	43" × 43"cosec dec
System Temperature (K)	120	110
System noise (12 hr, mJy)	16	0.2

Table 3: Molonglo Research Students and their Topics, 1960 to 1978

Student Name	Submitted	Thesis Title
John Sutton	1962*	Aerial Performance and Information Theory in Radio Astronomy.
Ross Glanfield	1962*	Observational Properties of Relativistic World Models at Radio Wavelengths.
John Rome May	1964*	Galactic structure deduced from radio measurements.
Donald Campbell	September 1964*	Data Reduction in Radio Astronomy.
Kent Price	January 1966*	Strip Scanning of Seven Radio Sources with a 1.6'arc Interferometer.
John Sutton	November 1966	The determination of the positions of radio sources.
Michael Kesteven	January 1968	Radio Observations of Some Supernova Remnants.
David Wyllie	September 1968	An Absolute Flux Density Scale at 408 MHz.
Alan le Marne	1969 #	
Philip Harris	January 1969*	Number-Flux Density Relationship for Radio Sources.
Peter Shaver	1970	Radio Emission from Galactic HII Regions.
Malcolm Cameron	September 1970	Radio Observations of Bright Galaxies.
Trevor Clarke	August 1971	The Measurement of the Angular Sizes of Radio Sources by Model Fitting.
Robert Munro	August 1971	Identifications of Radio Sources from the Fourth Cambridge Catalogue.
Richard Hunstead	May 1972	Studies of Selected Radio Sources.
Anne Green	September 1972	Spiral Structure of the Galaxy from a Radio Continuum Survey.
Richard Schilizzi	November 1972	Structures of Extragalactic Radio Sources.
Ian Davies	1973 #	
Alan Vaughan	May 1974	Pulsar Observations at Molonglo.
James Clarke	May 1974	A High Resolution Survey of the Magellanic Clouds at 408 MHz.
Robert Milne	January 1976	Interplanetary Scintillation at 408 MHz.
Michael Batty	February 1976	Low Frequency Recombination Lines.
Gordon Robertson	December 1976	Radio Source Surveys at 408 MHz.
Graeme White	February 1977*	Optical and Radio Studies of a Molonglo Deep Survey.
David Hoskins	1977 #	
Andrew G Wilson	April 1977*	Absolute Flux Density Measurements at 111 MHz.

Key: * = M.Sc. thesis
= Did not complete

> In 1958-1960 there was much strife at Radiophysics, arising from Bowen's support of the Culgoora Solar Observatory as a second project after the Parkes dish. Chris Christiansen resigned and went to the Chair of Electrical Engineering at Sydney University, then Bernie Mills went to the Chair of Astrophysics set up by Harry Messel. A right royal battle then raged between Bowen and Messel. Animosity existed until the combined Parkes-Molonglo observations of Peter Shaver and Miller Goss. Then later came the Molonglo-Parkes observations of pulsars by Michael Large and Dick Manchester. I was Leader of the Astrophysics Group at Radiophysics over that period, and worked very hard to convince Bernie Mills that he could trust us to work with Molonglo on those two projects.

From the start of the Astrophysics Department in 1960 until 1978, the range of projects observed by the fan beam and the Cross are shown in Table 3 which lists the research students and their thesis topics completed in this period. The cut-off is chosen because on 24 August 1978 at 10am the Cross was switched off and we prepared to lift feed modules down for conversion from 408 to 843 MHz (Figure 7).

5 AFTER THE CROSS

5.1 The Molonglo Observatory Synthesis Telescope (MOST)

By the early 1970s, digital computers had both the speed and reliability to take real-time control of a radio telescope. Mills realised that if a fan beam tracked a field for twelve hours, the rotation of the Earth would move the beam through 180° on the sky and allow the synthesis of a pencil beam (Mills et al, 1976). He designed a synthesis telescope for 1,420 MHz, and Alec Little had developed a prototype feed for the EW modules when they learned that CSIRO was planning the Australian (later The Australia) Telescope for this and higher frequencies. Mills then chose a new frequency of 843 MHz which is not a protected radio astronomy band but, with cooperation from the Australian Post Office, has been kept reasonably free of interference by nearby radio phone links.

The conversion of the Cross to the MOST reused the East and West arms and gave a powerful new telescope at very low cost. The NS arm was abandoned. All the concrete, steel and mesh of the EW arm had been designed with possible use at 1,420 MHz in mind and needed no change except for the addition of a slow tilt drive. A new mile of RF focus at 843 MHz reused the waveguides from the Cross, but replaced dipoles with 7,744 ring elements that were phased by computer to track a field for twelve hours. The conversion of the feeds and construction of new receivers, digital delays and correlators to produce 128 contiguous fan beams took three years to complete (Robertson, 1991).

The first synthesis map of source 1836-631 was made on 15 July 1981 with 43 arcsec resolution over a 23 arcmin field. Switching beams across three adjacent centres increased the field to 70 arcmin and detailed images of known sources up to one degree in size were observed for a decade. In 1991, development of precise digital phase units (Amy and Large, 1992) gave computer control of phase for all 176 modules, thus removing grating lobes and giving a great improvement in dynamic range over the 70 arcmin field. A further installation of phase control to the separate waveguides within each module in 1996 increased MOST's field of view to 2.7° (and the current observing parameters for MOST are given in Table 2). With the large field of view, it became feasible to undertake an 843 MHz survey of all of the southern sky (from declination –30° to –90°). The Sydney University Molonglo Sky Survey (SUMSS) was began in 1998 and is now (August 2007) effectively finished. A second project has mapped the southern sweep of the Galactic Plane through the Galactic Centre.

5.2 The Square Kilometer Array and SKAMP

Evolution of the Molonglo Telescope is continuing. Like many of the world's radio observatories, MOST staff are testing concepts and systems for the future Square Kilometer Array in a project called the SKA Molonglo Prototype (Square Kilometer Array). They have developed wide-band feeds for the cylindrical reflector as well as new correlators and agile control elements. First fringes from modules of the new system were produced on 5 May 2004 but the full SKAMP has to wait for the MOST surveys to finish. MOST has a 10 hectare collecting area, wide field of view and complete uv-coverage out to its maximum 1 mile spacing. We expect its successor, the third Molonglo telescope, covering frequencies 300 to 1,420 MHz will explore hydrogen in the high red-shifted Universe.

6 NOTES

1. This extensive source survey "… had profound cosmological implications in terms of the competing 'Big Bang' and 'Steady State' theories which were prevalent at the time and led to the notorious 'Fleurs-2C Controversy …" (Orchiston and Slee, 2005: 152). For details of the 'Controversy', which soured relations between Australian and Cambridge radio astronomers for many years, see Mills (1984) and Sullivan (1990).
2. R.N. Bracewell went to Stanford and Frank Kerr to Maryland. In 1961 J.L. Pawsey was offered the first Directorship of the National Radio Astronomy Observatory in USA, but tragically, died on 30 November 1962 before he could take up the position. J.H. Piddington moved to the CSIRO Division of Physics. K. Westfold went to the University of Sydney and later became the Dean of Science at Monash University in Melbourne. J.V. Hindman went to the ANU's Siding Spring Observatory. W.N. Christiansen went to the University of Sydney on 21 April 1960, succeeding Professor D. Myers in the Peter Nicol Russell Chair of Electrical Engineering (*Nature*, 188, 784, 1960). He was soon joined by R.F. Mullaly and A. Watkinson.

Figure 7: Closing the Cross. The East 1 feed is lifted off the telescope near midday on 24 August 1978.

7 ACKNOWLEDGEMENTS

I thank many colleagues for discussions about Molonglo history, especially Terry Butcher, Bob Frater, Richard Hunstead and Bernard Mills for details of the 1960s decade and Wayne Orchiston for references to early CSIRO research. The late Brian Robinson provided reminiscences of Cambridge and CSIRO. I thank Nyree Morrison, Reference Archivist, for details of University staff appointments, and Woody Sullivan (University of Washington) for providing Figure 1. I appreciate my access to the Physics archives, now held at Molonglo, as well as log sheets and notebooks used by Alec Little and Michael Large. Above all, I am grateful for my experience of stimulating research over 45 years with Molonglo colleagues.

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THE AUSTRALIAN SOLAR ECLIPSE EXPEDITIONS OF 1947 AND 1949

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Abstract: On 1 November 1948 the Radio Astronomy Group within the Commonwealth Scientific and Industrial Research Organisation's Division of Radiophysics observed a partial solar eclipse on a range of radio frequencies at three different sites within Australia. These observations helped establish Australia's reputation as a leader in solar radio astronomy. A second partial eclipse occurred on 22 October 1949, and the Division again mounted a major expedition, this time with very different results.

This paper examines the contribution of the eclipse observations and contrasts the very different results achieved. While scientific progress is generally well documented, stumbling in the path of progress is often overlooked. In looking to future research it is important to realise that progress is often only made in the face of adversity.

Keywords: radio astronomy, solar radio emission, eclipses, Division of Radiophysics

1 INTRODUCTION

The history of early radio astronomy in Australia has been well documented by Orchiston and Slee (2005), Robertson (1992) and Sullivan (2005), amongst others, and its success largely revolved around the Council for Scientific and Industrial Research's Division of Radiophysics team that was associated with the development of radar during WWII. At the end of the war, in 1945, the CSIR decided to retain this Division, refocus on peacetime research, and appoint a number of bright new staff members. Under the inspired leadership of J.L. Pawsey this strategy paid off, and Australia was soon at the forefront of the emerging field that would become known as 'radio astronomy'.

One of the key events that helped establish Australia's reputation in solar radio astronomy was the partial solar eclipse of 1 November 1948. Another partial eclipse was visible from Australia on 22 October 1949, and, keen to back up their earlier successes, the Division also mounted a major expedition to observe this event. However, no results were ever published, and were it not for two very brief references by Orchiston and Slee (2005: 135) and Orchiston et al. (2006: 48) this expedition would have escaped notice. Prior to 1948 the Division also considered sending an expedition to Brazil in order to observe a total solar eclipse, and this, too, has only received the briefest of mentions in the historical literature (e.g. see Bolton, 1982: 350).

The purpose of this paper is to recognise that both success and failure contributed to the building of Australia's radio astronomical reputation. As such, this paper provides an historical account of the 1949 eclipse observations, as well as backgrounding the aborted 1947 expedition to Brazil.

2 SOLAR ECLIPSES AND RADIO ASTRONOMY

A detailed history of the genesis of solar radio astronomy in Australia has been published (Orchiston et al., 2006). One of the key challenges for the early researchers was the low resolution of the aerials being used, as this inhibited the ability to determine the precise positions of the sources of solar radio emission. Some early progress was made by McCready et al. (1947) using interferometric techniques, but it was soon realised that solar eclipses offered a more sophisticated method of establishing the locations of the different radio-emitting regions in the solar corona (see Hey, 1955). In 1946, the Canadian radio astronomer, Arthur Covington, used the opportunity presented by the partial solar eclipse of 23 November to accurately measure the time—hence position projected onto the solar disk—when radio emission at 2,800 MHz was masked by the passage of the Moon's disk (Covington, 1947). Sander (1947) also used this same eclipse to examine the distribution of radiation at the higher frequency of 9,428 MHz. Although Dickie (1946) was the first to observe a solar eclipse at radio frequencies, it was Covington who first showed that strong emission was associated with a sunspot group that was occulted during an eclipse.

3 THE 1947 ECLIPSE EXPEDITION

The Radiophysics group in Australia had initially planned to conduct its first eclipse observations during an expedition to Brazil to observe the total solar eclipse of 20 May 1947. In a proposal from the Chief of the Radiophysics Division, Dr E.G. ('Taffy') Bowen (1946) to the Chief Executive of the C.S.I.R., Dr F.W.G. White, the rationale for conducting the eclipse observations was outlined. By this time the Radiophysics researchers had determined that there were three quite distinct components of 'solar noise': (1) steady radiation, which was believed to be of thermal origin, (2) enhanced levels of solar noise believed to be associated with sunspots and of non-thermal origin (the so-called 'slowly-varying component'), and (3) sudden bursts of short duration, which also were believed to be non-thermal in origin.

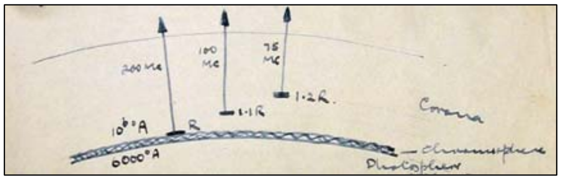


Figure 1: Illustration from the original Brazilian eclipse expedition proposal showing the different levels in the solar corona where the steady component of solar noise could be expected to originate; R = solar radii (courtesy of National Archives of Australia).

The evidence they had accumulated suggested that the steady component of the radiation came from different levels in the Sun's atmosphere. Calculations suggested that observations of the noise conducted at a number of different frequencies would provide an opportunity to investigate the properties of the corona at a series of different levels. Included with the proposal was a hand-drawn diagram illustrating the levels at which thermal noise at 200, 100 and 75 MHz could be expected to originate, and these extended from the top of the chromosphere to a point at 1.2 solar radii in the corona (see Figure 1).

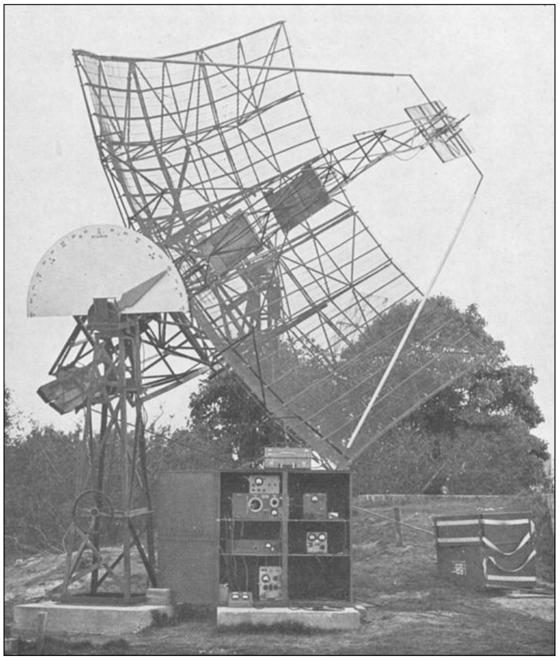


Figure 2: The 16 × 18-ft paraboloid at Georges Heights field station. It was proposed to ship this aerial to Brazil for the 1947 total solar eclipse (courtesy ATNF Historical Photographic Archive: B1164).

The proposal (Bowen, 1946) suggested that a solar eclipse offered the best opportunity to measure the apparent diameter of the Sun's disk at different frequencies and hence provide experimental confirmation of the ideas illustrated in Figure 1. In a later update of the proposal, Pawsey (1946c) expanded on the scientific objectives of the observations:

> A quantitative theory concerning the steady component of radiation has now been advanced by D.F. Martyn. This assigns a distribution of intensity over the disc of the sun which changes radically with the frequency of observation. An interesting prediction is that, in the region of 600 Mc/s, the radiation should be intense near the edge of the sun and weak in the centre, so that the sun should appear as a bright ring and not a disc. Such a distribution gives an intensity variation during the eclipse markedly different from that from a disc. The part of the theory dealing with intensity distribution over the surface is as yet unsupported by experiment and it appears that eclipse observations provide a sound method of verification. This quantitative verification is an extension of Bowen's suggested measurement of the apparent diameter of the sun's disc.

In order to observe the eclipse at different frequencies, receivers were constructed to operate at 100, 200, 600 and 1,200 MHz. It was proposed to ship the 16 × 18-ft paraboloid (Figure 2) that was in operation at the Georges Heights field station to Brazil, and it was to be fitted out to operate simultaneously at 200, 600 and 1,200 MHz. In addition, two separate single Yagi antennas fitted to operate at 100 and 200 MHz were also to be shipped (Pawsey, 1946a). Besides simple intensity measurements, the Yagis could be used to measure right-hand and left-hand polarisation by switching between feed elements oriented at 90° with respect to each other. In total, over 3 tons of equipment was estimated to be needed for the expedition.

The proposed members of the expedition were Pawsey, L.L. McCready and D.E. Yabsley. The Cambridge University radio astronomy group also considered sending an expedition to Brazil, but in a letter written in September 1946 J.A. Ratcliffe told Bowen that if Radiophysics was definitely to proceed with its expedition then Cambridge would withdraw and focus its efforts on making solar observations in the U.K. For Radiophysics, an eclipse expedition to Brazil was a major undertaking, and the high level of funding involved (~£6,000) required Ministerial approval. Although there were some concerns that the expedition might not be funded, approval was granted on 13 November 1946 and the Cambridge group therefore elected to withdraw its expedition.

Despite having obtained Ministerial support for the expedition, it soon transpired that the Radiophysics radio astronomers had badly underestimated the logistical difficulty of transferring the equipment from Australia to Brazil. Shipping could only be made via London, and the transit time, plus delays in customs, meant that the equipment would only arrive in Brazil after the eclipse! In December 1946, Pawsey (1946b) reluctantly wrote to Ratcliffe and informed him of the decision to abandon the expedition. He also expressed his regret for disrupting the Cambridge plans.

The cancellation of the Radiophysics expedition, however, provided a new set of opportunities: Bolton and Stanley were granted permission to use the 100 and 200 MHz equipment for their research programs at

Dover Heights (Bolton, 1982: 350), and the Georges Heights 16 × 18-ft antenna and its receivers were relocated to the newly-established Potts Hill field station in time for Australia's next partial solar eclipse, which was scheduled for 1 November 1948.[1]

The 1947 total eclipse was ultimately successfully observed at 200 MHz by a Soviet expedition that used the steamship *Griboedov* as an observing platform (Dagkesamanshii, 2007: 395). Their observations confirmed that a significant proportion of the radiation at this frequency originated in the corona, something that was independently predicated by L. Ginzberg in 1946, but was unknown to Radiophysics staff at that time.

4 THE 1948 ECLIPSE OBSERVATIONS

The Division of Radiophysics' assault on the 1948 eclipse has already been discussed (Orchiston, 2004, Orchiston et al., 2006). Observations were made at 600, 3,000 and 9,428 MHz with a variety of instruments located at three different sites in Australia, and the results were published in *Nature* and in the *Australian Journal of Scientific Research* (Christiansen et al., 1949a; Christiansen et al., 1949b; Minnett and Labrum, 1950; Piddington and Hindman, 1949).

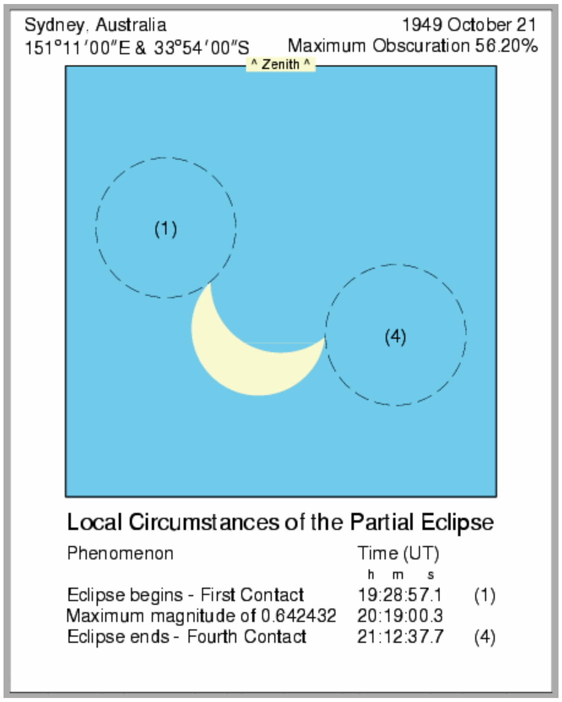
The eclipse observations provided key data relating to the quiet Sun and the slowly-varying component. While optical emission is strongest in the lowest layer of the solar atmosphere, the photosphere, the radio observations clearly showed that the radio-quiet component of the radiation had its origin in the upper chromosphere and in the corona. Much higher temperatures than the 5,800 K typical of the photosphere were observed, ranging from 10^4 K in the chromosphere to 10^6 K in the corona. At the time the emission was thought to be thermal in nature, although a non-thermal origin was not ruled out. From the observations it was also clear that at 600 MHz the emission extended well beyond the visible disk of the Sun, confirming an origin in the corona, but the limb-brightening predicted by D.F. Martyn (1946) was not definitively observed. At the higher frequencies of 3,000 and 9,428 MHz the emission appeared to originate from a region that more closely approximated the optical disk, and at all three frequencies there was a definite correlation between the slowly-varying component and sunspot area. At 600 MHz, the positions of the radio-emitting regions in most instances were found to coincide with existing sunspot groups or sites where sunspots were noted during the previous solar rotation. Circular polarisation was also detected at 600 MHz, and although the existence of a general solar magnetic field had been proposed many years earlier (Hale et al., 1918) no evidence of it was found during the eclipse. Later Smerd (1950: 265) used the 1948 eclipse to establish "… an upper limit of 11 gauss for the surface field-strength at the solar poles at the time of observation."

Part of the 1948 eclipse record that has escaped notice until now is the fact that John Bolton and Gordon Stanley also joined the expedition to Strahan in Tasmania (Bowen 1948). They had just returned from a very successful expedition to New Zealand where they observed Centaurus-A, Cygnus-A, Taurus-A and Virgo-A at 100 MHz (see Orchiston, 1993; 1994), and they were keen to use the same equipment to observe the eclipse and to make further observations of Taurus-A. No results of Bolton and Stanley's 1948 eclipse observations were ever published, and in fact the entire Strahan expedition was almost a disaster. In the first instance, the petrol power generator could not be started. After various efforts it was realised that the generator had been drained of all fluids for transport, but the team did not know the generator's air filter was an oil-bath type that had also been drained. It was some time before this was diagnosed and the generator was able to be made functional (Murray, 2007). On completion of the observations all the equipment was loaded onto a borrowed Army truck. Unbeknown to the team the truck had a large hole in its muffler and the wooden frame and its canvas cover caught fire. The truck was extensively damaged, but apart from John Bolton's briefcase, the eclipse records survived unscathed (Bolton and Stanley, 1948).

5 THE 1949 ECLIPSE OBSERVATIONS

A second partial solar eclipse visible from eastern Australia occurred on 22 October 1949. On this occasion the eclipse occurred in the early morning Australian Eastern Standard Time. Figure 3 shows the local circumstances of this eclipse. The maximum obscuration from Sydney was 56% compared to 55% during the 1948 Eclipse.

In a memo from Bowen to the Secretary of the C.S.I.R.O. the success of the 1948 observations at 600 MHz are noted. Bowen (1949b) stresses that as eclipses are rare events the Division should seize this opportunity to mount another major expedition. For this 1949 eclipse the intention was to observe at the higher frequency of 1,200 MHz. Besides repeating the previous year's observations at the higher frequency, the intent was also to conduct polarisation measurements in order to obtain experimental evidence of the existence of a general magnetic field of the Sun.



Local Circumstances of the Partial Eclipse

Phenomenon	Time (UT) h m s	
Eclipse begins - First Contact	19:28:57.1	(1)
Maximum magnitude of 0.642432	20:19:00.3	
Eclipse ends - Fourth Contact	21:12:37.7	(4)

Figure 3: Local circumstances of the 22 October 1949 partial solar eclipse (© HM Nautical Almanac Office, CCLRC Rutherford Appleton Laboratory 2005).

Figure 4: An AN/TPS-3 radar in operation in 1944 (courtesy *CE LCMS Historical Office Department of the Army, USA*).

Two new temporary field stations were established for the eclipse observations. This was necessary as the partial eclipse occurred early in the morning and therefore a clear easterly aspect was required (unlike the sunset eclipse that had occurred in 1948). One of the new sites was at Bairnsdale aerodrome in south-eastern Victoria (147º 35′ E; 37º 53′ S); the other was at Eaglehawk Neck near Hobart in Tasmania (147º 56′ E; 48º 01′ S).[2]

Figure 5: The 10-ft parabola being set up at Eaglehawk Neck for the 1949 eclipse observations. From left to right are Harragon, Murray and Yabsley (courtesy *The Mercury*).

Jim Hindman was placed in charge of the Bairnsdale observations with Howarth and Trensky supporting him. The Tasmania team led by Don Yabsley comprised John Murray and Jack Harragon, and they were assisted by G. Ellis and N. Gerrard, research students in physics and electrical engineering respectively from the University of Tasmania ("Ready for today's eclipse", 1949). The same ex-Army surplus AN/TPS-3 portable 10-ft parabolic aerials that had been used in 1948 eclipse observations were used for the 1949 eclipse, except that a polar mount and a motor drive had been added, in order to automatically track the Sun ("Experts arrive …", 1949; "Ready for today's eclipse", 1949).

The AN/TPS-3 radar aerial (Figure 4) had been developed during WWII by the U.S. Army Signal Corps as a light weight portable 600 MHz early warning radar (Orr, 1946). These aerials were also known as the 'British Type-63 Radar'. The aerial was made up of eight 45º aluminium frame sections covered with wire-mesh that could be packed in a very compact bundle and quickly reassembled through a series of speed-clips; according to John Murray (2007), two people could assemble an aerial in about five minutes.

Figure 5 shows the team setting up the antenna at Eaglehawk Neck, and Figure 6 shows them preparing to observe the eclipse. The equipment featured in the latter photograph included an Esterline-Angus chart recorder, and was housed in "… an unimpressive-looking caravan …" (Ready for today's eclipse, 1949).

In addition to the observers at the two remote sites, observations were also carried out at the Division's Dover Heights and Potts Hill field stations in Sydney, and at the 'Eagle's Nest', on the roof top of the Division's Headquarters building in the grounds of the University of Sydney. Collectively, these observations spanned the frequencies of 9,400, 3,000, 1,200, 600, 200, 100 and 60 MHz (Bowen 1949c). As the eclipse occurred early in the morning, the measurements were complicated by ground reflection effects, and in order to allow for these, observations were made in the week leading up to the eclipse and for up to three days afterwards in order to obtain a base set of measurements.

Not to be left out of the action, Cla Allen from the Commonwealth Observatory at Mount Stromlo also observed the eclipse. He made use of the 4-Yagi array and receiver that was used for regular solar monitoring at 200 MHz (e.g. see Allen, 1947); this equipment was installed by Radiophysics staff back in 1946.

The preliminary results of the eclipse observations appeared in the minutes of the Radio Astronomy Committee meeting of 17 November 1949 (Christiansen, 1949; our italics):

> Report on Eclipse Records:
> 3-cm. Satisfactory record obtained at Sydney. Record shows a smooth change in intensity with time.
> 10-cm. Unsatisfactory record.
> 25-cm. Good results at Bairnsdale and Sydney. Eaglehawk Neck record slightly doubtful. Records show effects of "active" areas on solar disk.
> 50-cm. Good record at Sydney. Effects of active areas can be seen.
> 150-cm. Sydney record shows diminution of activity after eclipsing of an active area, but otherwise appears to be of little value. Stromlo record unsatisfactory.
> 300-cm/500-cm. Eclipse record at Dover, Hornsby and Potts Hill shows diminution of activity when a certain area is eclipsed. The calculated position of the active area appears to agree with interferometer measurements at Potts Hill on day of eclipse, which suggest that the source was off the limb of the solar disk. *No decision was made regarding publication of eclipse results*.

At a later meeting of the Committee it was noted that

> The eclipse records have been partly analysed, but final conclusions have been formed. The records show a marked asymmetry about the maximum phase of the eclipse. Small changes in slope are not as clearly marked as those in the 1948 eclipse records. An interesting feature of most eclipse records, to date, is that the west-limb of the sun appears to have great radio brightness. (Christiansen, 1950).

The only other recorded detail of the eclipse observations outside the internal RP files is in the 1949/1950 Annual Report of the Division, which states:

> The intensity of the radio waves from the sun has been observed systematically throughout most of the year on a number of wavelengths, namely 3, 10, 25, 50, 300 and 500 centimetres. During the partial solar eclipse of October 1949, these observations were extended to include simultaneous observations on 25 centimetres at two sites, one in Victoria and one in Tasmania. During the previous year the techniques had been successfully developed of using spaced receivers during an eclipse to determine the position on the sun of highly-emitting areas. The results this year may be less interesting from this point of view (*because the sun was rather free from such areas on the day of the eclipse*) but, in consequence, more accurate measurements of the distribution of radio "brightness" across the "quiet" sun were obtained. (Bowen, 1950a; our italics).

Figure 6: The team examining a test recording immediately prior to the 1949 eclipse. From left to right are Murray, Ellis (standing) and Gerrard (courtesy *The Mercury*).

This report suggests that the Sun was free of sunspots at the time of the eclipse, but in fact there were eight sunspot groups visible on the day of the eclipse, which is comparable to the level of sunspot activity during the 1948 partial eclipse. Furthermore, Allen had supplied all three RP observing sites with a table listing the estimated times of covering and uncovering of the different sunspot groups during the eclipse. Four of the sunspot groups were considered major, two were old and two were new. Figure 7 shows the positions of the different sunspots, as supplied by Allen. Previously, Mount Stromlo had agreed to supply optical observations during the eclipse, but on the vital day the sky was completely overcast which prevented any observing.

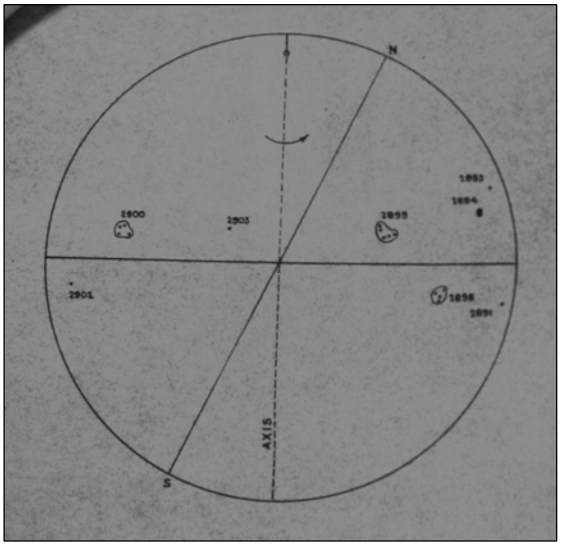


Figure 7: Mount Stromlo prediction of probable positions of sunspots during the eclipse of 22 October 1949 (courtesy of the National Archives of Australia: 972423 - C3830 - C6/2/4).

The presence of sunspot groups on the day of the eclipse is independently supported by sunspot observations made at the National Astronomical Observatory of Japan in Tokyo, as shown in Figure 8.

No optical observations were obtained at Mount Stromlo, but the Carter Observatory in Wellington (New Zealand) sent Bowen (1949d) thirteen photographs that were taken during the eclipse. These clearly showed the presence of sunspot groups. Unfortunately no spectroheliograph observations were made at the Carter Observatory on the day of the eclipse.

Although it appears that the eclipse was successfully observed at many of the Radiophysics field stations and remote sites, there is very little information on record as to what results were obtained.[3]

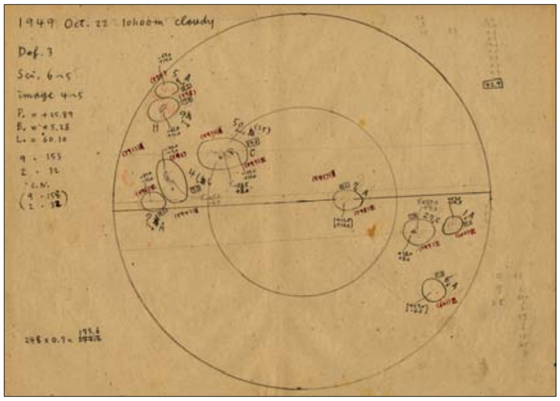


Figure 8: Sunspot observations on 22 October 1949. Note that North is at the top and East is to the right (courtesy of the National Astronomical Observatory of Japan).

One of the few references in the Radiophysics files is a letter from Allen to Pawsey after the 200 MHz Mount Stromlo observations had been sent to Radiophysics for analysis. Allen (1949) noted:

> At about 0600 attempted to take reading on the micro-ammeter, but found a kick every time the aerial passed a certain hour angle. This made it impossible to turn the aerial off the sun without introducing ambiguity as

> the movement was spoiling the record. Although poor, thought you may be able to recognise the true bursts by comparison with the Sydney recordings.

This letter was also circulated to Yabsley, Christiansen and Payne-Scott. Pawsey's reply (1949) simply read:

> I have passed it [the 200 MHz records] round among those who had similar recordings but we do not seem able to get much from it. This holds also for all our own long wavelength observations. The shorter wavelength ones are promising but the work of reduction is tedious and has not yet reached the interesting stage.

This is the last mention on file of the results of the observations. During the 1948 eclipse the most useful results had come from the 600 MHz measurements, while the higher frequencies gave less definitive information as the Moon's disk covered and uncovered different sunspot groups. However, the higher frequencies did indicate the possible presence of limb brightening.

Meanwhile, a letter from Bowen to Commonwealth Observatory Director, Richard Woolley, dated 6 September (Bowen 1949a) noted that the 1949 observations made at Sydney in the early morning could be complicated by ground effects, but since the low position of the Sun above the horizon was not dissimilar to that of the 1948 eclipse this could not be considered a major reason for the lack of published results.

It is interesting to note that the Cambridge group also produced a non-result when they observed a partial eclipse on 28 April 1949. In a letter to Pawsey dated 28 September 1949, Ryle (1949) noted:

> I do not know if you have done any more experiments on the distribution of intensity across the solar disc. We were hoping to get some results during the partial eclipse in April and were recording on four frequencies. Unfortunately the sun did not co-operate and produced a largish "outburst" half-way through the eclipse which rather spoilt the experiments.

In response to a subsequent question from Ryle on plans for future eclipse observations, Joe Pawsey replied:

> … we have no plans for observations though we regard the technique as very useful. You know of our 1948 eclipse observations here. A second one in 1949 has not proved so fruitful *owing to lack of solar activity at the time*. The results are not fully reduced yet ... I think the longer wavelength observations suggested well worth while if feasible – so far we have had little joy from these at eclipses but the Russians did well. There is a large element of chance here owing to varying solar activity – I wish you luck." (Pawsey, 1950; our italics).

Note that Pawsey cites a lack of solar activity at the time of the eclipse, despite clear evidence of sunspots being present. This may have indicated a minimum level of the slowly-varying component, even though sunspots were present.

Although the Radiophysics Division did not mount further eclipse expeditions after their unhappy 1949 experience, they did provide support for the U.S. Naval Research Laboratory when it observed the total solar eclipse of 12 September 1950. The eclipse was only partial in Australia, but observations were taken at Potts Hill and Dover Heights at 600, 3,000 and 9,400 MHz (Bowen, 1950b), and these provided an independent set of baselines for the U.S. measurements. A similar service was also provided in support of J.F. Denisse's French expedition to observe the 1 September 1951 solar eclipse.[4]

The last mention of the results of the 22 October 1949 eclipse observations found in the archives is a letter from Pawsey to M. Servajean at Meudon Observatory (in Paris) dated 6 April 1951. In this, Pawsey apologies for the delay in writing, explaining that he has been awaiting completion of the reduction of the observations, but these were still not finished. He notes:

> [Enclosed] A sketch of the Sun at the time of the eclipse of 21st October 1949, showing sunspots and areas of excess brightness observed on 25 centimetres wavelength. At this eclipse the "bright" areas are much less well defined. Christiansen thinks the area well off the Sun (A) is real ... We tried but failed to do similar observations on metre wavelengths and so locate corresponding places of high emission at these longer wavelengths. The emission was too variable to apply this method. (Pawsey 1951).

This reference is perhaps the best indication of the results of the 1949 eclipse observations. Unlike the earlier suggestions, it is clear that enhanced radio emission was observed and correlated with sunspot areas in much the same way as during the 1948 eclipse. The clearest results from the 1948 eclipse had come from the 600 MHz observations, with the higher frequencies showing less definitive results. It seems that at the higher frequency of 1,200 MHz there was also a correlation in 1949, but it was much less well defined. Pawsey's letter suggests that, if anything, the Sun may have been too active at the time of the eclipse as he notes the longer wavelength measurements were "… too variable". This also tallies with a reference by Allen to observing bursts at 200 MHz during the eclipse. It is not clear why some of the earlier reports suggested a lack of solar activity on the day of the eclipse.

6 DISCUSSION

As no results of the 1949 eclipse observations were ever published a definitive statement of the results of the 1949 eclipse program cannot be presented. In our opinion, the most likely outcome was that after the very successful observations of 1948, the 1949 observations provided no 'new' information of sufficient importance to warrant publication. The Division of Radiophysics had a particularly stringent internal refereeing system for new research papers (see Sullivan, 2005), and the absence of any cancelled or rejected papers about this eclipse in the Radiophysics Archives at Epping (M. Goss, pers. comm., 2008) would strongly suggest that no manuscript was ever prepared for publication.

Perhaps it was this conspicuous non-result—after so much sustained effort—that finally inspired Christiansen to develop his Potts Hill solar grating array (Christiansen and Warburton, 1953), so that he no longer had to wait for suitable solar eclipses in order to investigate the distribution of radio-emitting regions in the solar corona (see Christiansen, 1984: 117).

7 CONCLUDING REMARKS

This paper documents the CSIRO Radiophysics Division's successful attempts to observe the 22 October

1949 partial solar eclipse and it also provides background information on the aborted Brazilian expedition of 1947.

In this context, it is important to record not just the scientific 'successes' that occurred during the formative years of radio astronomy in Australia—and there were many—but also the overall progress of scientific research, including the setbacks.

8 NOTES

1. The history of this pioneering radio telescope is recounted in Orchiston and Wendt (n.d.).
2. In Orchiston and Slee (2005: 135) the remote observation sites were incorrectly identified as Strahan in Tasmania and a site near Sale in Victoria (while the actual sites were Eaglehawk Neck, near Hobart, in Tasmania and Bairnsdale aerodrome—which is near Sale). However, the correct sites were listed in a subsequent paper (Orchiston, Slee and Burman, 2006: 48).
3. John Murray (2007) has confirmed that successful observations were indeed made at Eaglehawk Neck in Tasmania.
4. For details of the French observations of this eclipse see Orchiston and Steinberg (2007: 13-15).

9 ACKNOWLEDGEMENTS

We are grateful to John Murray for sharing his recollections of the expeditions to Strahan and Eaglehawk Neck in Tasmania. We would also like to acknowledge the assistance of Kathryn Brennan from the National Archives of Australia for her help in accessing archive material and Takashi Sakurai from the National Astronomical Observatory of Japan for permission to reproduce Figure 8. We are also grateful to the LCMS Historical Office, Department of the Army (USA) and *The Mercury* newspaper (Hobart, Tasmania) for permission to publish Figures 4, 5 and 6. Finally, we wish to thank Miller Goss (NRAO), Jean-Louis Steinberg (Paris Observatory), Richard Strom (ASTRON) and Richard Wielebinski (MPIfR) for reading and commenting on the manuscript.

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Journal of Astronomical History and Heritage, 11(1), 79-84 (2008).

BOOK REVIEWS

Jesse Ramsden (1735-1800) London's Leading Scientific Instrument Maker, by Anita McConnell (Ashgate Publishing House, Aldershot, 2007), pp. 340, ISBN-13: 978 0 7546 6136 8 (hardback), GB£60.00, 232 x 158 mm.

Before reading this book (Figure 1), my only knowledge about Jesse Ramsden was that he was the inventor of the Ramsden eyepiece. Dr McConnell's excellent biography of the man shows that there was much more. She quotes extensively from documents written by his contemporaries to show that they considered him the finest instrument-maker of his time. It was recognized that not only were the instruments constructed extremely well, but that they often showed innovation of design which in one case would lead the Royal Society to award him the Copley Medal.

Ramsden was also famous for his tardiness, with instruments delivered often years after being ordered. Dunsink Observatory had its transit circle delivered 23 years late. An acquaintance, Richard Edgeworth remarked that not only was Ramsden a mechanical genius, but he was also a genius at the invention of another sort, the invention of excuses. McConnell relates many such anecdotes, and one of the more amusing ones has Ramsden showing up unannounced at the residence of King George III declaring that the King wished to see him. The pages and attendants were rather dubious, but checked with the King just in case, and much to their amazement the King insisted that Ramsden be brought to his presence at once. Ramsden had with him an item that the King had ordered. After examining it and finding that it met expectations, he said to Ramsden that he "… brought home the instrument on the very day that was appointed. You have only mistaken the year."

We have little to go on with respects to Ramsden the man, because few of his personal documents have survived. For example, we know that he was born in Yorkshire, but after he moved to London we have no idea whether he maintained contact with friends or relatives. Ramsden did marry Sarah Dollond, the daughter of the optician John Dollond, who held the patent for the achromatic lens, but nothing is known on why the marriage broke up after eighteen years, which was after the death of the elder Dollond. Very possibly it had something to do with an argument that Ramsden had with Sarah's brother Peter, on who really invented the achromatic lens, an argument that would lead to Peter Dollond denouncing Ramsden at a meeting of the Royal Society.

The book also gives us some idea of what it was like to have an instrument-making business. Ramsden had upwards of fifty people working for him who put in 72 hours of work per week. Orders for expensive items like transit telescopes, transit circles, large quadrants and astronomical telescopes were rare, so Ramsden also sold devices such as compasses and sextants, for which there was a big demand from the various naval and commercial ships, while his theodolites and other surveying and cartographic instruments found a ready market.

Dr McConnell has written a book of great scholarship that is also fascinating reading. The book is well illustrated and makes extensive use of the available documentation. And the price of 60 pounds is reasonable for a book of this kind.

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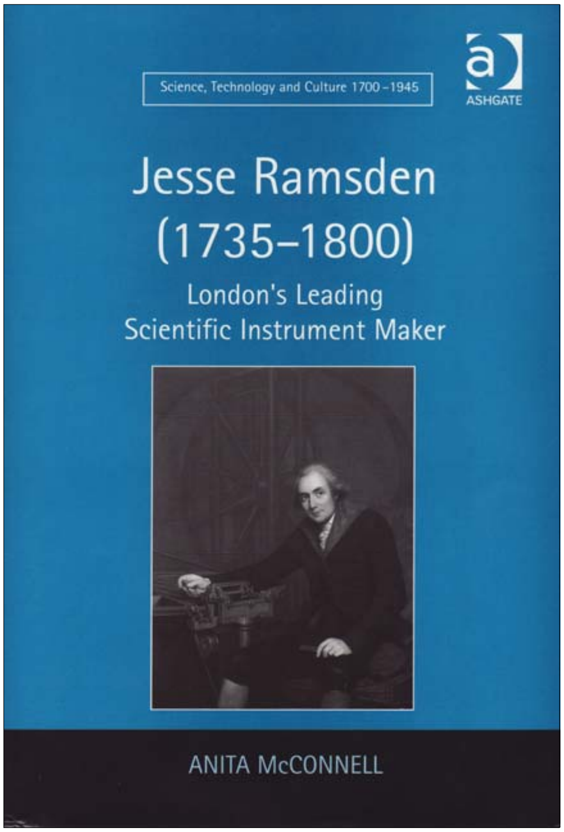

Figure 1: Dust jacket of the Jesse Ramsden book.

Mission to Jupiter: A History of the Galileo Project, by Michael Meltzer (NASA SP 2007-4231, Washington, DC, 2007), pp. 318 (hardback), US$25.00, 250 x 165 mm.

Michael Meltzer's *Mission to Jupiter: A History of the Galileo Project* (NASA SP 2007-4231) describes the first program to investigate Jupiter from orbit and by entry into the planet's atmosphere. Not counting perhaps twenty years of preliminary activity, the project lasted from 1977, when the United States Congress gave authorization, until 2003, when the orbiter was intentionally destroyed. Of these twenty-six years, twelve were spent in a long struggle to build and launch the spacecraft, while six more passed with it *en route* to Jupiter. Galileo spent eight productive years at Jupiter.

Meltzer has written a well-documented history. His access to persons who were closely involved with the program adds authority to his presentation. He gives credit to the people who operated Galileo through its many challenges to make the program a long-term success story. The book summarizes many technical facts. It seems likely that only the most specialized readers will want more detail. Less specialized readers could better appreciate the spectacles of the planet's atmosphere and the four main satellites if more color images had been used. As it is, their use is only representative.

Who is the audience for this book? A senior manager in the Galileo program wrote in the preface: "I think that people who are interested in the space program, its science achievements, and its contributions to technology in general will really appreciate this history. It's comprehensive, it's complete, and it seems to me to be pretty even-handed." The most likely audience is persons who participated in the Galileo Project, but historians will find useful material, and space exploration enthusiasts may be interested. What about the general public? Meltzer has summarized much in straightforward language, yet despite his hopeful inclusivity that Galileo was "... the eyes, ears, and fingertips of humankind ... it is we who were exploring uncharted frontiers …" (p. 299), his presentation is unlikely to be popular. Its thoroughly technical nature will appeal to technical people but may be perceived as dry by general readers. *Mission to Jupiter: A History of the Galileo Project* is a useful and worthwhile record of the first long-term visit to a gas giant planet.

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"... eine ausnehmende Zierde und Vortheil" – Geschichte der Kieler Universitätssternwarte und ihrer Vorgängerinnen 1770-1950, by Felix Lühning, (Wachholtz Verlag, Neumünster, 2007), pp. 752, ISBN-13: 978 529 02497 9 (hardback), €35,00, 236 x 160 mm.

This book, which tells the detailed history of Kiel University Observatory and its predecessors, comprises the habilitation thesis submitted by F. Lühning to the Faculty of Mathematics at Hamburg University in 2004. With financial support from various organizations, it was issued in a very attractive form as a special publication of the Society for the history of the city of Kiel. The nice layout, the graphical sketches of buildings, instruments, and astronomical connections—often designed by the author—the scientifically-precise text, written with a sense of humor, make a pleasant reading, in spite of sometimes quite extensive descriptions of architectural details or 'operating instructions' for meridan circles etc. I have rarely read such an appealing text on astronomical history.

The single chapters deal with the beginnings of astronomy in Kiel (1770-1820), Schrader's giant telescope from the late eighteenth century, Altona Observatory (1823-1850), the first years of the journal *Astronomische Nachrichten*, the last years of Altona Observatory (1850-1872), the private Bothkamp Observatory (1870-1914), the genesis of Kiel Observatory (1874-1880), the era of its Director, Krueger (1880-1896), the Kiel Chronometer Observatory (1893-1913), the era of Harzer (1897-1925), the era of Rosenberg (1927-1934), the decline of Kiel Observatory (1935-1950) and the evolution of *Astronomische Nachrichten* under its editor Kobold (1907-1938). The book concludes with a glossary of technical terms, biographical sketches of known and unknown persons, as well as a list of references.

The author presents lively sketches of people who were astronomically active in Altona, Kiel and its surroundings for a time interval of about two hundred years. To achieve this, he studied many files from the Secret State Archive Preussischer Kulturbesitz (Berlin), the Schleswig-Holsteinisches State Archive (Schleswig) and the Hamburg State Archive, from which he quotes extensively. He also has evaluated private documents, and has interviewed surviving witnesses of the 1930s and 1940s. Nevertheless, such sources may turn out to be unreliable: the custodian said that the spouse of the last official Observatory Director, Hans Oswald Rosenberg, was "… Verena Borchardt, a Jewess from St. Petersburg." (p. 583). Her family, however, lived for some years in Moscow, where her father was the representative of the *Königsberger Thee-Compagnie*. In 1880, his daughter Helene was born there, and she later married the astronomer Wirtz. In 1882, the Borchardt family moved to Berlin, where Verena was born. The family was "… of reformed confession, of Jewish origin." (Rudolf Borchardt)—only in Nazi ideology was she a *Jewess*.

On page 583, too, Wirtz' capricious political views are quoted: "The day when the French troops entered Strasbourg was the happiest one in my life."—taken from a 1999 paper by Theiss, where the author states that the source is not given. In fact, Theiss uses a study by Duerbeck and Seitter (1990), where the precise reference in the *Kiel Acta* (kept in the Prussian State Archive) is given, and an explanation of this statement is offered. Another overlooked—although not very informative—source is the voluminous edition of the collected letters of Rudolf Borchardt, the poetical brother of 'Vera' Rosenberg and 'Lene' Wirtz.

Another series of peculiar statements refers to the *Astronomische Nachrichten* (p. 666): neither did they publish, after 1945, "… sometimes only Russian articles …", nor "… only articles in English …" after 1993; some later German astrometric articles will presumably stand the test of time better than the plethora of English articles on cosmology of that time. Totally fabricated is the author's statement that the journal is now published by "… the Astronomical Computing Centre [sic] in Heidelberg."

In spite of my critical notes on some irrelevant details, I can wholeheartedly recommend this book: it is an indispensable source of information for anyone who is interested in the history of astronomy in German-speaking lands in the nineteenth and the first half of the twentieth centuries.

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Un Astronome des Lumières: Jérôme Lalande, by Simone Dumont (coédition Vuibert, Paris/Observatoire de Paris, 2007), pp. 359, ISBN-13 Vuibert: 978 2 7117 4028 4, ISBN-13 Obs. de Paris: 978 2 901057 54 3, €35,00, 240 x 158mm.

The infant baptised on 12 July 1732 as Joseph Hyérosme Lefrançois was born at Bourg-en-Bresse and educated there by the Jesuits. Arriving in Paris as a young law student, he expanded his surname to Lefrançois de La Lande, and by happy chance took lodgings at the hotel de Cluny (now the Musée de Cluny) where the astronomer Joseph Nicolas Delisle had an observatory. This proximity allowed him to develop his boyhood interest in the heavens into serious observation; with his law studies completed, he became totally dedicated to astronomy and its mathematical underpinning. When La Caille departed to make observations at the Cape, Lalande was sent in 1751 to Berlin (at a similar longitude in the northern hemisphere) to make similar observations. Here his youthfulness and competence surely boosted his reputation. Returning the following year, he moved easily into the Parisian astronomical circle, becoming one of its brightest planets, and soon became known to the wider astronomical world as Jérôme Lalande. Given the limited membership of the Académie Royale des Sciences, only the wait for dead men's shoes slowed his way up the various rungs of that gilded ladder.

Undoubtedly Lalande benefited from favourable circumstances in his early life, which nurtured his driving ambition, his self-esteem, and his desire to control the society in which he moved (in which nepotism played a large, and indeed acceptable part). Thus he was able to recommend for promotion the competent young men he encountered in the various public and private observatories in Paris and the provinces, and those he met on his travels in Germany, Italy and England. Although Lalande seems to have put eye to telescope at most, if not all, of the military, college and private observatories in Paris, given his long absences it must have devolved to his students and employees to maintain the observations and calculations while he was away. Their results contributed to his fame as it expanded with the publication between 1764 and 1792 of the three editions of his *Astronomie* and the *Connaissance des Temps* for 1760-1772. This happy era ended with the French Revolution.

Lalande's influence on the upper echelons of government derived from his Directorship of the Paris Observatory, and that of the Bureau des Longitudes, set up in 1795. His initial respect for Napoleon changed as the political aftermath of the Revolution imposed new unsympathetic masters, and far worse—indeed impossible for an astronomer concerned with the provision of a nautical almanac (the *Connaissance des Temps*) for 1795-1807. From 1793 to 1806 he battled against the imperial decree to adopt the new metric system, with its total revision of dimensions (where 400 grads replaced the ancient circle of 360 degrees), and the calendar (where hours, days, months and years were reformulated to a decimal system).

The private lives of such men are often hidden from posterity by the 'delicacy' of friends and the lack of personal letters, but this was not so with Lalande, as 'official', personal and family letters flew from his pen. His friendship with two non-Parisian astronomers in particular, Honoré Flaugergues and Franz Xaver von Zach, generating many letters and exchanges of data, is examined here. Simone Dumont has delved into the wealth of correspondence, now dispersed on both sides of the Atlantic, in order to uncover aspects of his 'other' lives. We learn about Lalande's participation in the Académie des Sciences, his role as a freemason, his atheism, his pleasure in the company of educated ladies and the way in which he dealt with the resulting offspring of these passions. In death, as in life, Lalande continued on the move; his expressed wish to make his body available for dissection then to be interred under the instruments at the Ecole Militaire was overruled. His heart was given to his family but his bones were shifted from place to place until they were rescued from possible transfer into the Paris Catacombs, reunited with his heart and, forty-five years after his demise, laid to rest in Père Lachaise Cemetery.

Such a busy life inevitably poses structural problems for the biographer—resolved here by breaking the story into five chronological chapters: (1) Jesuit college to the Académie and the Collège Royale, 1732-1770; (2) Lalande, encyclopedist and freemason, 1770-1789; (3) From the start of the Revolution to the Directoire, 1789-1795; (4) From the Directoire to the Empire, 1795-1804; and (5) Last years, 1804-1807. Each chapter is subdivided into the various aspects of his professional life, namely his astronomical and mathematical achievements, his publications, students, correspondents, travels and so forth, also his family life at Bourg-en-Bresse, freemasonry and personal life; these subdivisions are clearly set out in the Table of Contents. Useful appendixes cover the 'Character and opinions of Lalande', the afterlife of his works, the dispersal of his papers, and generally wind up the story. A Bibliography and Index to names are provided.

I was left undecided as to whether I should admire or dislike Jérôme Lalande. A man obsessed, he generated an immense amount of numbers and writings. Yet this productive work is tainted by his domineering character, perhaps compensating for his small size and unshapely appearance; he was a man who gave orders to all and sundry, from his students to the Emperor Napoleon, and expected his will to be obeyed. On the credit side, he was a man who having dallied with certain ladies then arranged their marriages and ensured that his children (who fortunately seem to have inherited their parents' mathematical abilities) were brought into his own astronomical world as his 'nieces' and 'nephews'. He was a man with a vast number of correspondents, some close friends and a few enemies; a man whose comprehensive books on astronomy the modern historian turns to with gratitude, just as we shall turn to Simone Dumont's biography, knowing that here we surely have The Complete Lalande, Astronomer of the Enlightenment.

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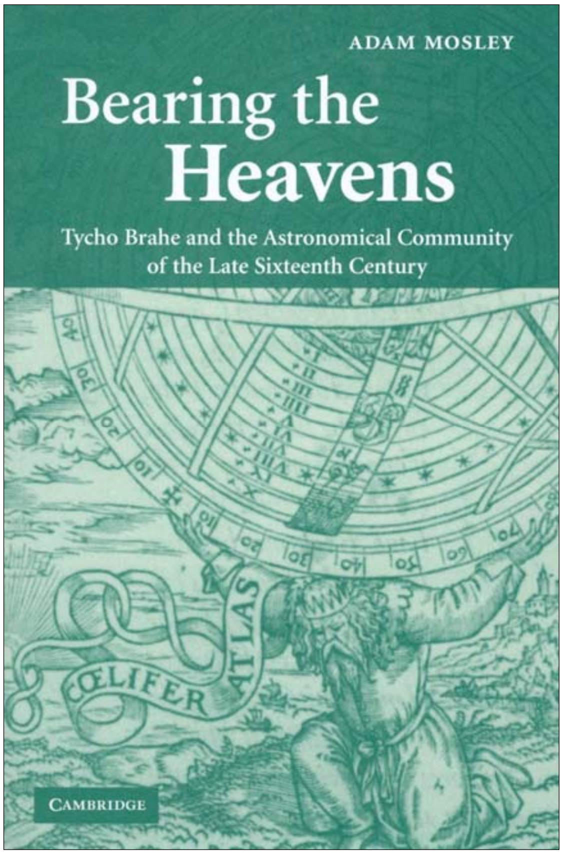

Figure 2: Front cover of Adam Mosley's book.

***Bearing the Heavens: Tycho Brahe and the Astronomical Community of the Late Sixteenth Century*, by Adam Mosley (Cambridge, Cambridge University Press, 2007), pp. 354, ISBN 978 0 521 83866 5 (hardback), GB£55.00, 228 x 158mm.**

> "We need therefore to press on with studying the astronomy of the early modern period via a history of communication; a history in which we consider both who was communicating with whom, and how, as well as what it was what they said. For the history of communication must be a part, and an important one, of the history of science as a practice. Only a history that encompasses the transmission and evolution of techniques and technologies, as well as the sharing and evaluation of data and ideas, can claim to represent the culture of science, and hence account for what is taken to be its product, knowledge of the world." (p. 297).

This is the conclusion and quintessence of Mosley's book (Figure 2), which was submitted in 2000 as a PhD thesis at Cambridge University ("Bearing the Heavens: Astronomers, Instruments and the Communication of Astronomy in Early Modern Europe"). Consequently the author sets out to examine the ways in which members of the nascent international astronomical community shared information, attracted patronage and respect for their work, and conducted their disputes. It highlights the significance of instruments, letters and books for the development of astronomy in the sixteenth century.

The practice of astronomy in the early modern period consisted not only in reading and writing books, but also in reading and writing letters. Mosley utilizes correspondence as a key resource and examines the 'epistolary culture' of the 16th century. His study is centered on Tycho Brahe, who published a selection of his correspondence in 1596.

Tycho's *Epistolae Astronomicae*, consisting mainly of letters he had exchanged with Landgrave Wilhelm IV of Hesse-Kassel and his court mathematician Christoph Rothmann, was a means of communicating valuable information about the practice of astronomy and its theoretical development in Hven and Kassel, but—as Mosley argues—these letters

> … are not incidentally instructive about the astronomical activities and cosmological beliefs of Rothmann and Tycho, but are actually constitutive of a form of astronomical practice. Communication by letter was one way for astronomers to overcome the contingent obstacles that prevented observation of phenomena at one particular location. (p. 113 seq.).

Mosley points out that Tycho's *Epistolae* is not a mere edition of letters. Their publication served manifold purposes: this book was intended as a serious scholarly text and vehicle for propagating the Tychonic reform of astronomy, but it was also seen as a memorial and gift for an audience of nobles. In order to correct the astronomy of the ancients, comparisons of observations were necessary at different sites, and the exchange of information between Hven and Kassel was partly collaborative and partly adversarial. The questions of instrument-construction and alignment, observing methods, techniques of recording and retrieving data and the corrections applied for atmospheric refraction, were all subjects that were discussed by Tycho and Rothmann. Tycho's collection of astronomical data and modelling of his world system were elaborated within a community of scholars bound together by letters. The correspondence network established precedence and was a forum for the public resolution of academic disputes.

Alongside the letters and books, a third realm is treated: instruments (such as globes, armillary spheres and models of planetary motion) conveyed astronomical knowledge and concepts in a visual way. Astronomical instruments were not only technical devices suitable for angular measurements, but their decoration also carried symbolic meanings concerning status, expertise, patronage and wealth. By distributing engravings and descriptions of his apparatus Tycho established the credentials of his observational programme, and by dedicating his *Astronomiae Instauratae Mechanica* (1598) to Rudolf II he presented his work symbolically to the Emperor.

In this book, Mosley displays a painstaking handling of original sources. This very valuable study will be of interest not only to historians of astronomy in the narrower sense, but also to historians of early modern culture in general.

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One Time Fits All: The Campaign for Global Uniformity, **by Ian J. Bartky (Stanford, Stanford University Press, 2007), pp. xiv + 292, ISBN-13: 978-0804756426, US$49.95, 231 x 152 mm.**

As a former member of the staff of the Royal Greenwich Observatory and a co-author of a paper on the astronomical background to the International Meridian Conference of 1884, I was shocked to read in the preface to this book that the author, Ian Bartky, considered that it was a failure. After all, the Greenwich meridian became the initial meridian for the measurement of longitude and Greenwich midnight came into use for the beginning of the universal day for mapping and timekeeping throughout the world. This account of the campaigns for the adoption of international time systems shows, however, that opposition to the recommendations and lobbying for alternative proposals continued for many years. This book contains detailed accounts of these campaigns, and also of those for the introduction of standard time zones and later of daylight saving (or summer) time, up to the 1920s, by which period these ideas had become generally accepted. The book starts, however, with the events and arguments that led to the evolution of a date line in the Pacific area on either side of which the date and day of the week are different. Otherwise, there is no consideration of calendarial matters and the change from the use of apparent time to mean time is not discussed.

There is no doubt that Bartky has gone to enormous lengths to find and document the fine detail of this complex story. This is shown first of all in the long list of acknowledgements to persons who helped him in his searches, then in the text itself, in the many pages of notes and in the long bibliography. A great deal of effort has been devoted to the index, which includes helpful references to individual notes that often give information about aspects of the story that might not be expected from the text that prompted the note. The whole volume has been very carefully edited and checked. It is, however, ironic that one typographical error is in the spelling of my name in the bibliography and that Bartky did not appreciate the subtle distinction between the Royal Greenwich Observatory and the Royal Observatory at Greenwich.

Eight of the eleven chapters of the book are concerned with the campaigns for a uniform time system for the world, starting with the review by Otto Struve of Russia in 1870 of the multiplicity of initial meridians used for land maps, marine charts, atlases and other purposes. He found that the most common initial meridian for scientific and practical purposes was that of Greenwich, and after reviewing the advantages and disadvantages of other options, he concluded that this would be best choice for general use. His views were, however, strongly contested and, for example, others argued for neutral meridians not related to a particular observatory. There was an even greater diversity in the local mean time systems that were in use for particular areas and by railway companies as they were usually based on that for an important town. The differences between 'railway times' and the local times at the stations on long routes, such as those in the United States, led to suggestions for the adoption of a single timescale for all purposes. These were, however, soon replaced by proposals for the use of timescales that differed by an integral number of hours from that of a standard longitude, but there was at first no agreement on a single standard for all countries.

A large measure of agreement at the scientific level was reached at the meeting of the International Geodetic Association at Rome in 1883 and the U.S. Government was persuaded to call the conference in 1884 at which it was hoped that representatives of the governments would agree to adopt the proposals for the use of the Greenwich meridian for the unification of longitude and time. An amended set of proposals was eventually adopted, but with some objections and abstentions. One major change was that the 'universal day' is to begin at mean midnight of the initial meridian rather than at noon as was then the case with the astronomical and nautical days. The hope was expressed that these days would be arranged to begin at midnight 'as soon as may be practicable', but even the British *Nautical Almanac* did not change until 1925. The national governments also failed to adopt the other resolutions. A table shows that in 1898 none of the 16 principal countries (other than Great Britain) used the Greenwich meridian to define longitude on their topographic maps. It was later found (in 1957) that the British Ordnance Survey continued to use longitudes measured from the Bradley meridian, rather than from the Airy meridian that was implied in 1884.

The resolutions of the 1884 conference did not include any direct reference to the introduction of hourly time zones, but during the following years the benefits of using such zones gradually overcame local objections. The boundaries between the zones were, however, usually chosen to match the frontiers between countries or other civil administrative areas. France continued to use time based on the meridian of the Paris Observatory until 1911 after it had started in 1910 to broadcast from the Eiffel Tower high-power time signals based on Paris mean time, which conflicted with the signals broadcast by Germany and the U.S.A. The French then, however, took the lead and the Bureau International de l'Heure was established in Paris later in 1911. France adopted the international meridian for its hydrographic charts in 1914, by which date all the other principal countries had at last adopted the 1884 resolution for such charts, but not for topographic maps.

The last two chapters of the book deal with the 'employment of clock time as a social instrument' and, especially, with the proposals and counter arguments concerning the introduction of daylight-saving time during extended summer periods. The system was first introduced in Germany in 1916 during World War I in order to save fuel used for generating electricity for lighting, and this lead was soon followed by Great Britain and other countries. The change was not introduced in the U.S.A. until 1918, but even then some states changed their standard times or the boundaries of their time zones so as to nullify the effect. Further changes took place during the following years, and local options destroyed the attempts to introduce uniformity across the country.

This book provides a detailed and fascinating account of the campaigns to promote the unification of the time systems in use throughout the world. The differences and similarities in approach in the activities in North America and Europe are made apparent and attention is drawn to the important contributions of many individuals who are not mentioned in popular accounts of these matters. The negative attitudes of some scientists now seem surprising, while the reluctance of legislators to reduce the confusion that must have been caused by the multiplicity of timescales seems to have been common to all societies.

Apart from an epilogue that is mainly concerned with recent developments, the account closes in the 1920s. Unfortunately, the author died in December 2007 and so we must hope that someone else will write a similar comprehensive account of the subsequent changes in the use and basis of the timescales for both civil and scientific use. None of the persons mentioned by Bartky could have imagined the high precision with which time is now determined and made readily available throughout the world. The unit of time is no longer based on the rotation of the Earth, but the distribution of accurate time still depends on the monitoring of the changes in rate and direction its rotation. Astronomy still has a vital role in global positioning and timekeeping!

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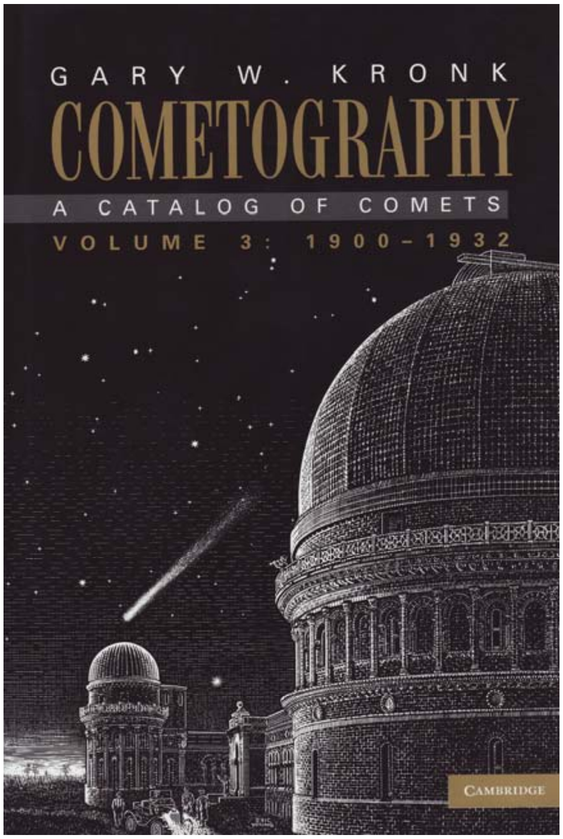

Figure 3: The attractive dust jacket of Kronk's third book in the Cometography series.

Cometography. A Catalog of Comets. Volume 3: 1900–1932, **by Gary W. Kronk (Cambridge, Cambridge University Press, 2007), pp. xvi + 650, ISBN 978-0-521-58506-4, £150, 259 x 185 mm.**

For those of us with a research interest in the history of cometary astronomy, the *Cometography* series by Gary Kronk is an absolutely indispensable resource, and each new volume is looked forward to with great anticipation.

The first volume in this series focused on comets from ancient times to 1799 and was published in 1999. Volume 2 covered the period 1800-1899 and was published in 2004, and now we have the third in the series, which discusses comets observed between 1900 and 1932 (see Figure 3). The progress of each comet, from discovery until disappearance, is discussed in detail, and each entry is accompanied by a full suite of references, so if perchance there is inadequate information in Kronk's weighty tome to satisfy all of your research needs then you know precisely where to look.

Although they were not nearly as abundant as during the glorious thirty years from 1860 to 1889, a number of majestic comets did make an appearance in the first three decades of the twentieth century, beginning with the

Great Comet of 1901, which was conspicuous in southern skies during April and May. Halley made its long-awaited appearance in 1910, but this same year is also remembered for the Great January Comet which, from all accounts, was equally impressive. Another prominent naked eye comet was C/1927 X1 (Skjellerup-Maristany). In Kronk's book you will find 3.5, 9, 24 and 5 pages respectively assigned to these four comets. Two other comets that were widely photographed because of their impressive and changing tails were C/1907 L2 (Daniel) and C/1908 R1 (Morehouse), and Krong devotes 9 and 10 pages to them.

From my own personal perspective, one of the curious features of cometary astronomy during the period 1900-1932 is the comparatively large number of officially credited discoveries and recoveries made from Australia (e.g. by Dodwell, Gale, Ross, Skjellerup), New Zealand (Grigg, Geddes) and South Africa (Blathwayt, Ensor, Forbes, Houghton, Reid, Skjellerup, Taylor, Woodgate). When independent discoveries are added, the list becomes even longer.

Apart from the dossier of information on each comet, Kronk provides a 24-page Appendix with material on "Uncertain Objects", some of which were undoubtedly comets but simply lacked the requisite number of reliable reported observations.

This is a beautifully-prepared and beautifully-presented book, and at 666 pages is no lightweight effort! Gary Kronk is to be commended for his scholarship and for once again providing us with an invaluable repository of information. Although the third volume of *Cometography* belongs on the bookshelf of all those with an interest in the history of cometary astronomy, I worry that the relatively high purchase price may deter some astronomers.

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