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COVER PHOTOGRAPH

The 4.9-m University of Texas Millimeter Wave Observatory that is described by Paul A. Vanden Bout, John H. Davis and Robert B. Loren in the paper starting on page 232.

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IS SPACE FLAT? NINETEENTH-CENTURY ASTRONOMY AND NON-EUCLIDEAN GEOMETRY

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Abstract: The geometrical structure of space entered astronomy in the second half of the nineteenth century, but slowly and hesitantly. Although in this period non-Euclidean geometry became a very important branch of mathematics, it aroused little interest among the astronomers. Nonetheless, there were more contributors to 'non-Euclidean astronomy' than usually supposed, and their attempts to forge links between the new geometries and the astronomical sciences merit attention. While some astronomers, such as R.S. Ball and K. Schwarzschild, discussed the observational evidence for curved space, in one case the hypothesis was used to solve a cosmological problem, namely, Olbers' Paradox. This paper reviews developments from N.I. Lobachevsky in 1829 to P. Harzer in 1908.

Keywords: space, non-Euclidean geometry, N.I. Lobachevsky, C.S. Peirce, K. Schwarzschild.

1 INTRODUCTION

In the early part of the nineteenth century it was recognized that Euclid's parallel postulate is not true by necessity and that there exist other geometries than the Euclidean system. A small group of mathematicians argued that the geometrical structure of physical space can be determined only by empirical means such as astronomical measurements. One might believe that astronomers eagerly took up the challenge, but this is not what happened. By and large, curved space was a non-subject in nineteenth-century astronomy. Only with Einstein's General Theory of Relativity did the curvature of space (or space-time) enter significantly into to the physical and astronomical sciences.

Although non-Euclidean geometry only played a very limited role in astronomy before Einstein, it was not completely ignored. A handful of astronomers investigated the possibility that space might be curved, a hypothesis that in the first decade of the twentieth century was well known and had permeated even into the more popular literature. For example, the recognized and widely-read *Newcomb-Engelsmanns Populäre Astronomie* included a brief account of finite, positively-curved space (Kempf, 1911: 664). A review of the development from about 1830 to 1910 reveals a history that is richer and more interesting than what can be found in most histories of either astronomy or mathematics.

2 FROM GAUSS TO LOBACHEVSKY

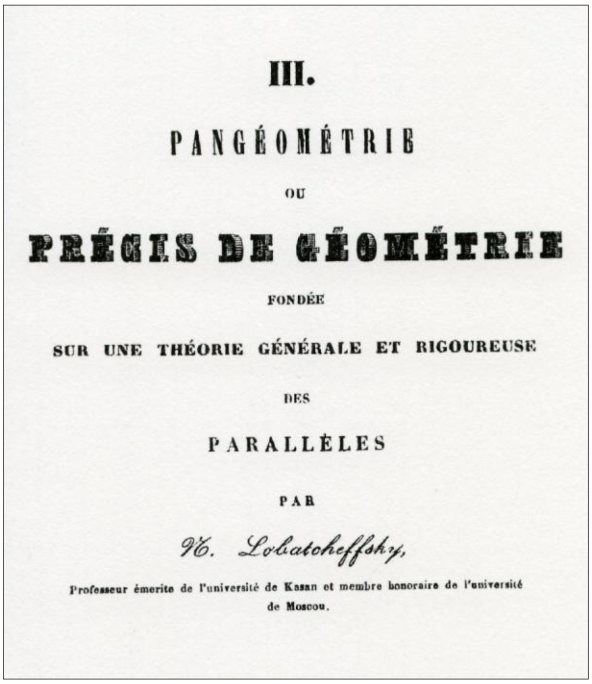
"Maybe in another life we shall attain insights into the essence of space which are now beyond our reach. Until then we should class geometry not with arithmetic, which stands purely a priori, but, say, with mechanics …" (Gauss, 1900: 177; my English translation). Thus wrote Karl Friedrich Gauss (1777–1855) in a letter of 28 April 1817 to the Bremen astronomer Heinrich Wilhelm Olbers (1758–1840), thereby indicating that ordinary Euclidean geometry was not true by necessity. The following year, while serving as Director of the Göttingen Observatory, Gauss was requested to undertake a major cartographic survey project with the purpose of mapping the state of Hanover (to which Göttingen belonged) by means of triangulation. As part of this project he made geodetic measurements of unprecedented precision of a triangle extending between three mountain peaks. The sides of the Brocken-Hohenhagen-Inselsberg triangle were approximately 69, 85 and 107 km. For a long time it was generally believed that the theoretical purpose of these measurements was to test the assumption of Euclidean geometry, namely, to establish whether or not the sum of the angles in the triangle deviated from 180°. This is a myth that can still be found in the mathematical and astronomical literature. However, historians of science agree that Gauss' work had nothing to do with the possibility of physical space being non-Euclidean (Breitenberger, 1984; Miller, 1972).

Figure 1: The Russian stamp of 1956 commemorating the centenary of Lobachevsky's death.

The non-Euclidean geometry anticipated by Gauss was discovered independently 1829–1831 by János Bolyai (1802–1860) in Hungary and Nikolai Ivanovich Lobachevsky (1792–1856) in Russia. While both of the two mathematicians reached the insight that the truth of Euclidean geometry was a question to be determined empirically, it was only the ten years older Lobachevsky who contemplated the problem within an astronomical perspective and further developed it (Figure 1). He suspected that the truth of geometry "… can only be verified, like all other laws of nature, by experiment, such as astronomical observations …" (Engel, 1898: 67; my English translation; cf. Vucinich, 1962). As a young student at Kasan University, Lobachevsky had studied astronomy under the Austrian Johann Joseph Littrow (1781–1840), who in 1810 had established an Astronomy Department at the University and later became Director of the Vienna Observatory. Recognizing the outstanding mathematical abilities of his student, Littrow made some astronomical observations with him. For example, in the summer of 1811 they observed a large comet. From 1819, Lobachevsky served as the Director of the Kasan University Observatory. Although a mathematician, he was thus well acquainted with astronomical theory and practice.

Already in his 1829 memoir in the *Kasan Messenger* "On the Principles of Geometry" Lobachevsky suggested an astronomically testable consequence of his 'imaginary' (or hyperbolic) geometry. It can be shown that for any triangle the difference of the angle sum ($\alpha + \beta + \gamma$) from 180° is given by the product of the space curvature K and the area δ of the triangle:

$$\alpha + \beta + \gamma - \pi = K\delta \qquad (1)$$



III.

PANGÉOMÉTRIE

OU

PRÉCIS DE GÉOMÉTRIE

FONDÉE

SUR UNE THÉORIE GÉNÉRALE ET RIGOUREUSE

DES

PARALLÈLES

PAR

N. Lobatcheffsky,

Professeur émerite de l'université de Kasan et membre honoraire de l'université de Moscou.

Figure 2: The title page of the French edition of Lobachevsky's *Pangeometry*.

In the case of hyperbolic or Lobachevskian space, the curvature is negative. As Lobachevsky pointed out, this implies that the angle sum of a triangle is always less than 180° and the more so the bigger the triangle becomes. He reasoned that this prediction might be checked by considering the parallax of stars such as 29 Eridani, Rigel and Sirius. For the last-mentioned star he quoted a parallax value of 1.24″ recently published by the French amateur astronomer François-Clément D'Assa-Montardier (1828; 1769–1840).

Lobachevsky concluded that the angle sum of the triangle spanning the Sun, the Earth and Sirius deviated from the Euclidean value of 180° by at most 0.000372″. As was only recognized much later, due to some mistake or misprint, the value he gave in 1829 was too large, as it should have been only 0.00000372″ (Brylevskaya, 2008: 132). At any rate, the tiny deviation strongly suggested that space was Euclidean, and yet Lobachevsky refrained from drawing this conclusion in firm terms (Bonola, 1955: 94-96; Daniels, 1975). Realizing that while it could in principle be proved that astronomical space is non-Euclidean, it could never be proved to be Euclidean, so he tended to see his calculations as inconclusive. In any case, at the time no reliable determination of a stellar parallax had been made. Only in 1838 did Friedrich Wilhelm Bessel (1784–1846) succeed in finding an annual parallax of 0.3136″ for the star 61 Cygni, corresponding to a distance from the Earth of 657,000 AU. The modern value of the parallax of Sirius is 0.37″, less than a third of the value adopted by Lobachevsky. In another line of reasoning, Lobachevsky showed that, if the world geometry is hyperbolic, the radius of curvature must be greater than 3×10^5 AU.

Lobachevsky also discussed the relevance of his new geometry to astronomical space in later publications, such as his *Pangeometry*, which was published in Russian in 1855 and translated into French in 1856, the year of his death (Figure 2). He wrote: "The distances between the celestial bodies provide us with a means for observing the angles of triangles whose edges are very large …" (Lobachevsky, 2010: 76). Consider a triangle spanned by a star and the two positions of the Earth half a year apart in its orbit around the Sun. Let the angle at the star be denoted α and the two angles at the positions of the Earth be β and γ. Then the parallax angle, p, can be expressed as

$$p = \pi - (\beta + \gamma) = \alpha - K\beta \qquad (2)$$

While in Euclidean space ($K = 0$) the parallax tends toward zero as the distance increases toward infinity ($\alpha = 0$), Lobachevsky realized that there must be a minimum parallax for all stars irrespective of their distances from the Earth. His general conclusion was that since the deviation from flat space was smaller than the errors of observation, Euclidean geometry was a perfect approximation for all practical purposes.

3 RIEMANNIAN SPACE

The ideas of non-Euclidean geometry circulated slowly in the mathematical community and only became generally known about 1870, chiefly through the works of Eugenio Beltrami (1835–1899), Hermann von Helmholtz (1821–1894) and Felix Klein (1849–1925). By that time it was realized that there are three possible geometries of constant curvature K, a quantity that has the dimension of an inverse area. It relates to the radius of curvature R by

$$R^2 = \frac{k}{K}. \qquad (3)$$

The curvature constant, k, distinguishes between flat or Euclidean space ($k = 0$), spherical space ($k = +1$) and hyperbolic space ($k = -1$). The possibility of a positively-curved space was recognized by the German mathematician and physicist Bernhard Riemann (1826–1866) in a famous address of 1854 in which he put the concept of curvature as an intrinsic property of space on a firmer basis and effectively founded differential geometry. Of relevance here is that Riemann (1873: 36) was the first to point out that, in the case of constant positive curvature, the traditional identification of a finite three-dimensional space with a bounded space is unwarranted. Infinity does not follow from space being unbounded, he said, for

> … if we assume independence of bodies from position, and therefore ascribe to space constant curvature, it must necessarily be finite provided this curvature has ever so small a positive value. (ibid.).

It is only in retrospect that Riemann's address, which remained unpublished until 1867, has become a classic of non-Euclidean geometry. In fact, although he may have known of Lobachevsky's work, he did not refer to it and also did not mention the contributions of Bolyai. He only alluded in passing to astronomy:

> If we suppose that bodies exist independently of position, the curvature is everywhere constant, and it then results from astronomical measurements that it cannot be different from zero; or at any rate its reciprocal must be an area in comparison with which the range of our telescopes may be neglected. (Riemann, 1873: 36).

According to Riemann, the metrical structure of space was likely to be of relevance to microphysics, at the atomic or molecular level, but he did not take an interest in the space of the astronomers. Questions about the global properties of space he cut short as "… idle questions." (Riemann, 1873: 37).

Riemann's emphasis on the possibility of an unbounded yet finite space failed to attract the attention of astronomers. Only in 1872 did the Leipzig astrophysicist Johann Carl Friedrich Zöllner (1834–1882) make astronomical—or rather cosmological—use of Riemann's insight. Primarily known for his pioneering contributions to astrophotometry, Zöllner also carried out important work in spectroscopy, solar physics, stellar evolution and the theory of comets. After 1877 he focused on what he called 'transcendental physics', the study of spiritualist phenomena based on the postulate of a fourth space dimension. As one might expect, this line of work created so much public attention that it damaged his scientific reputation (see Kragh, 2012).

Acquainted with the mathematical literature on non-Euclidean geometry, in his book *Über die Natur der Cometen* Zöllner (1872: 308-314) argued that cosmic space might well be positively curved (Figure 3). He considered Riemann's idea the key that would unravel the secrets of the Universe and dissolve the problems of a materially-finite Universe, for "… it opens up for the deepest and most fruitful speculations concerning the comprehensibility of the world." (Zöllner, 1872: 312; my English translation). According to Zöllner,

> The assumption of a positive value of the spatial curvature measure involves us in no way in contradictions with the phenomena of the experienced world if only its value is taken to be sufficiently small. (Zöllner, 1872: 308; my English translation).

Based on the assumption of a Riemannian Universe with only a finite number of stars, he could explain Olbers' Paradox without having to assume interstellar absorption of starlight or taking recourse to a limitation of either cosmic time or space (see Jaki, 1969: 158-164). Zöllner's aim was not only to demonstrate how an astronomical problem could be solved on the basis of Riemann's hypothesis, but more generally to argue for a closed cosmic space. He suggested that the laws of nature might be derived from the dynamical properties of curved space.

Zöllner's book, *Über die Natur der Cometen*, attracted much attention in Germany and was reprinted in 1883 and 1886. Nonetheless, Zöllner's pioneering contribution to cosmology is not well known, and it was even less well known in the nineteenth century. While it attracted some interest among German philosophers, it was either unknown or ignored by his colleagues in physics and astronomy. For this reason, and also because I have recently described Zöllner's Universe in detail (Kragh, 2012), I shall pass on to other attempts to apply ideas of non-Euclidean geometry in astronomical contexts.

4 ROBERT STAWELL BALL

During the last quarter of the nineteenth century, non-Euclidean geometry became a 'hot topic' in mathematics and philosophy, and was discussed in hundreds of books and scientific papers. On the other hand, the number of astronomers who expressed interest in the topic can be counted on the fingers of one hand. Moreover, the interest rarely went beyond uncommitted comments. One of those who did express a more substantial interest was the Irish astronomer Robert Stawell Ball (1840–1913), who in 1874 was appointed Royal Astronomer of Ireland and Professor of Astronomy at the University of Dublin, a position that included the Directorship of the Dunsink Observatory. Then from 1892 until his death in 1913 he served as Lowndean Professor of Astronomy and Geometry at Cambridge University, succeeding John Couch Adams (1819–1892) of Neptune fame (Ball, 1915; MacPherson, 1914). Sharing his scientific work between mathematical physics and astronomical observations, he

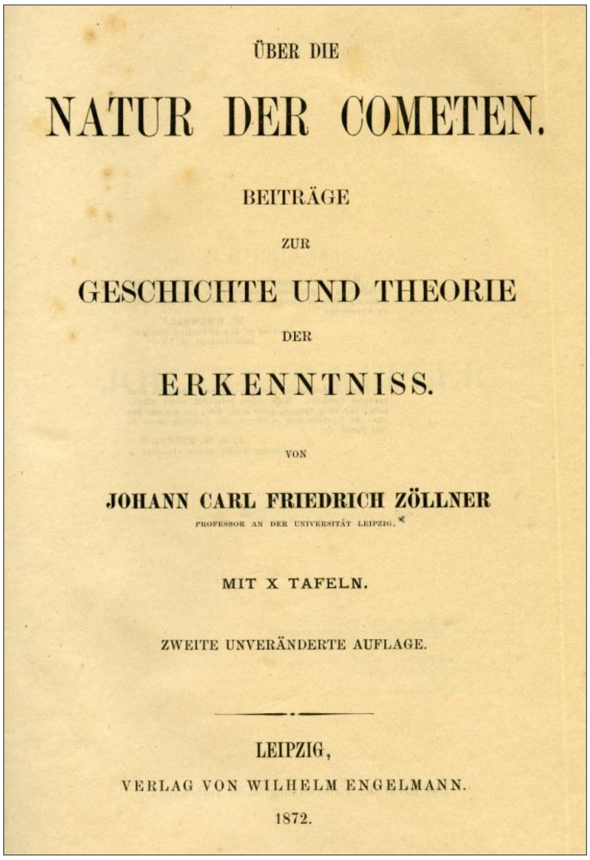

ÜBER DIE
NATUR DER COMETEN.
BEITRÄGE
ZUR
GESCHICHTE UND THEORIE
DER
ERKENNTNISS.
VON
JOHANN CARL FRIEDRICH ZÖLLNER
PROFESSOR AN DER UNIVERSITÄT LEIPZIG.
MIT X TAFELN.
ZWEITE UNVERÄNDERTE AUFLAGE.
LEIPZIG,
VERLAG VON WILHELM ENGELMANN.
1872.

Figure 3: Zöllner's 1872 treatise on the theory of comets, which included a chapter advocating a closed Riemannian Universe.

was also a well-known and much-esteemed author of popular astronomy in the Victorian tradition.

While at Dunsink, Ball directed a large-scale observational research programme in determining stellar parallaxes. Among the problems that faced astronomers in this area was the choice of comparison stars for parallax measurements, by taking into account the proper motions of the stars. Ball and his collaborators paid particular attention to the star 61 Cygni that Bessel had originally used in his discovery of the annual parallax. In a lecture given to the Royal Institution in London on 11 February 1881, he discussed the complex questions of comparison stars and proper motions in relation to parallax measurements. At the end of the lecture, he briefly alluded to the nature of space:

> If space be hyperbolic the observed parallax is smaller than the true parallax, while the converse must be the case if space be elliptic. The largest triangle accessible to our measurements has for base a diameter of the earth's orbit, and for vertex a star. If the defect of the sum of the three angles of such a triangle from two right angles be in any case a measurable quantity, it would seem that it can only be elicited by observations of the same kind as those which are made use of in parallax investigations. (Ball, 1881: 92).

What Ball called the "… true parallax …" is the angle under which the radius of the orbiting Earth appears for an observer located at a star; the "… observed parallax …", on the other hand, is half the annual change of the angular distance between the star and some comparison star close to it.

Ball was well acquainted with non-Euclidean geometry, but his remarks in the 1881 address had the character of an afterthought rather than a serious proposal for investigating the geometry of space by astronomical means. He did not return to the subject in his later scientific work, but, characteristically, chose to mention it only in his popular books. One of these was *In the High Heavens*, a book published in 1893. Ball discussed in a general way whether space is finite or infinite, a question which

> … is rather of a metaphysical complexion … [and] depends more on the facts of consciousness than upon those of astronomical observation … (Ball, 1893: 247; cf. Whiting, 2011: 143-158).

Having argued that the number of matter particles in the Universe must be finite, he proceeded to space itself and the possibility of "… a space which is finite in dimensions." (Ball, 1893: 251). With this he did not mean a finite-dimensional space, but rather a three-dimensional spherical space. Although Ball did not explicitly endorse a positively-curved space, he stressed that it was consistent and intuitively acceptable. Indeed, he expressed sympathy for the hypothesis, which

> … provides the needed loophole for escape from illogicalities and contradictions into which our attempted conceptions of [infinite] space otherwise land us. (Ball, 1893: 252).

In this context may be mentioned also the American mathematician James Edward Oliver (1829–1895), Professor at Cornell University, who according to George Halsted (1853–1922) was "… a pronounced believer in the non-Euclidean geometry." (Halsted, 1895: 545). Halsted recalled how Oliver tried to convince him that astronomical evidence pointed to space being closed. On one occasion, Oliver

> … explained a plan for combining stellar spectroscopy with ordinary parallax determinations, and expressed his disbelief that C.S. Pierce [sic] had proved our space to be of Lobachévsky's kind, and his conviction that our universal space is really finite, therein agreeing with Sir Robert Ball. (Halsted, 1895: 545).

It remains unknown what Oliver's ideas were, more precisely, since he never published on the subject.

5 NEWCOMB'S ELLIPTIC SPACE

The distinguished American astronomer Simon Newcomb (1835–1909) took an interest in non-Euclidean geometry, both from a mathematical and an astronomical point of view. As early as 1877, at a time when he had just become Superintendent of the Nautical Almanac Office, he published a mathematical paper on the geometry of space with positive curvature, but without relating his investigation to astronomy (Newcomb, 1877). Newcomb's space was not quite the same as Riemann's, but described by what soon became known as 'elliptic geometry' (and to which Ball referred in the quotation above). While in spherical or Riemannian space all geodesics from a given point intersect again at a distance πR, in elliptic space two geodesics can have only one point in common. In the latter case the largest possible distance between two points is $\frac{1}{2}\pi R$, whereas it is πR in the spherical case. Both spaces are finite, but for the same radius of curvature the volumes differ. Today spherical space is often seen as a special case of the elliptic space.

Newcomb (1877: 299) pointed out that

> … there is nothing within our experience which will justify a denial of the possibility that the space in which we find ourselves may be curved in the manner here described.

On the other hand, he seems to have been reluctant to part with the infinite Euclidean space. On some occasions he mentioned the possibility of curved physical space, but in popular contexts only and without taking it too seriously. In the widely-read *Popular Astronomy*, a book first published in 1878 that over the next twenty years went through many editions (and was translated into German, Russian and Norwegian), he discussed what would happen with the heat of the Sun. Would it forever be lost? Or would it, if space were curved, eventually return to the Sun? He wrote:

> Although this idea of the finitude of space transcends our fundamental conceptions, it does not contradict them and the most that experience can tell us in the matter is that, though space be finite, the whole extent of the visible universe can be but a very small fraction of the sum total of space … (Newcomb, 1878: 505).

But Newcomb did not believe in the possibility of a positively-curved space in which the solar heat would return to its source. On the contrary, he dismissed the hypothesis as "… too purely speculative to admit of discussion." (Newcomb, 1878: 504).

Many years later, in an address given to the American Mathematical Society on 29 December 1897, Newcomb dealt in a general way with what he called the philosophy of 'hyperspace', a concept that included non-Euclidean spaces as well as spaces with more than three dimensions. As he pointed out, the hypothesis of curved space was testable, if more in principle than in practice:

> Unfortunately, we cannot triangulate from star to star; our limits are the two extremes of the earth's orbit. All we can say is that, within those narrow limits, the measures of stellar parallax give no indication that the sum of the angles of a triangle in stellar space differs from two right angles. (Newcomb, 1898: 7).

He continued with an argument that effectively ruled out elliptic space as more than a speculation, at least as seen from the astronomer's perspective:

> If our space is elliptical, then, for every point in it – the position of our sun, for example – there would be, in every direction, an opposite or polar point whose locus is a surface at the greatest possible distance from us. A star in this point would seem to have no parallax. Measures of stellar parallax, photometric determinations and other considerations show conclusively that if there is any such surface it lies far beyond the bounds of our stellar system. (Newcomb, 1898: 7).

6 PEIRCE, A COMMITTED NON-EUCLIDEAN

Newcomb's cautious ideas about non-Euclidean space form an instructive contrast to those of his compatriot and friend, Charles Sanders Peirce (1839–1914; Figure 4). Although today mostly known as a philosopher, as a young man Peirce was primarily recognized as a promising astronomer and chemist. While at Harvard College Observatory he did important work in photometry and spectroscopy, and he was among the first to study the spectrum of an aurora, which he did as early as April 1869. Elected a member of the U.S. National Academy of Sciences in 1877, he spent most of his professional career as a practicing scientist associated with the United States Coast and Geodetic Survey. Contrary to the four years older Newcomb, Peirce was convinced that space is non-Euclidean—indeed *must* be non-Euclidean—a claim he supported with both philosophical and observational arguments (Dipert, 1977).

In letters and manuscripts written between the years 1891 and 1902 Peirce investigated various aspects of the structure of space, which led him to conclude that it was either of the Lobachevskian or the Riemannian kind. In a paper published in *The Monist* of 1891 he discussed the question in terms of stellar parallaxes, although at the time without suggesting a definite answer to the sign of space curvature:

> I think we may feel confident that the parallax of the furthest star lies somewhere between –0.″05 and +0.″15, and within another century our grandchildren will surely know whether the three angles of a triangle are greater or less than 180°, – that they are *exactly* that amount is what nobody ever can be justified in concluding … (Peirce, 1891: 174).

Peirce had a predilection for hyperbolic space, as is evidenced from his manuscripts and correspondence with Newcomb in the early 1890s.

Thus, in one of his manuscripts of 1891 he listed no fewer than fifteen "… methods of investigating the constant of space …" (Peirce, 1891: 229) that included parallax measurements, ideas of stellar evolution, the proper motions of stars, and Doppler shifts in stellar spectra. In addition, Peirce (2010: 230) concluded that "… the relative numbers of stars of different magnitudes depend on the constant of space." In a lengthy letter to Newcomb he convinced himself—and in vain tried to convince Newcomb—that astronomical data provided support for his "… attempt to make out a negative curvature of space." Although realizing the hypothetical nature of his conclusion, he had no doubt of its significance:

> The discovery that space has a curvature would be more than a striking one; it would be epoch-making. It would do more than anything to break up the belief in the immutable character of mechanical law, and would thus lead to a conception of the universe in which mechanical law should not be the head and centre of the whole. It would contribute to the improving respect paid to American science, were this made out here … In my mind, this is part of a general theory of the universe, of which I have traced many consequences, – some true and others undiscovered, – and of which many more can be deduced; and with one striking success, I trust there would be little difficulty in getting other deductions tested. It is certain that the theory if true is of great moment. (Eisele, 1957: 421-422).

Figure 4: Charles S. Peirce, 1839–1914 (after: http://psychology.wikia.com/wiki/Charles_Peirce).

Peirce's optimism was short-lived, as indicated in a letter he wrote to Newcomb on 21 December 1891:

> I have for the present given up the idea that anything can be concluded with considerable probability concerning the curvature of space. (Eisele, 1957: 423).

Newcomb welcomed Peirce's more agnostic attitude, which he mistakenly took to be support of his own view, namely, "… that all philosophical and logical discussion is useless." (Eisele, 1957: 424). This was definitely not a view shared by Peirce, who never did quite abandon the matter. Thus, in a manuscript note of 1894 he wrote:

> I made the necessary computations for a selection of stars. The result was markedly in favor of the hyperbolic geometry. (Dipert, 1977: 411).

Peirce's attempt to conceive celestial space as non-Euclidean was the most elaborate and serious one of the few such attempts in the nineteenth century. However, he made no impact at all, primarily because he did not publish his arguments in journals read by most astronomers and mathematicians. Although his ideas were known to some American scientists, they were not convinced. As Newcomb wrote him in March 1892,

> … the task of getting the scientific world to accept any proof that space is not homoloidal [flat], is hopeless, and you could have no other satisfaction than that of doing a work for posterity … (Eisele, 1957: 424).

When Newcomb died in 1909, and when Peirce died just five years later, observational proof of curved space was still lacking.

7 FRENCH DISCUSSIONS

References to the possible astronomical consequences of non-Euclidean space appeared not only in the contexts of astronomy, but sometimes also in the mathematical and philosophical literature. According to the conventionalist view of Henri Poincaré (1854–1912), one of the most eminent and influential scientists at the turn of the century, the geometry of space could not be determined objectively. According to him, it made no sense to say that one geometry was more true than another, only that it was more convenient. For example, if the sum of angles in a celestial triangle were found by astronomical measurements to be, say, 185° ± 1°, one might assume the physics of light propagation to be correct and change to a spherical geometry; but one might also choose to maintain Euclidean geometry by changing the theory of how light propagates through space. Because Poincaré (1892) found Euclidean geometry to be the most simple and convenient system, he saw no reason to consider other candidates for the structure of space.

Although many French scientists were influenced by Poincaré's conventionalism, not all agreed that Euclidean geometry was always to be preferred because of its simplicity. Auguste Calinon (1850–1900), a mathematician and philosopher, argued that the different geometrical systems were not physically equivalent. It was, he maintained, legitimate to ask about the particular geometry that is realized in the physical world. And yet, although he spoke of astronomical measurements of celestial triangles as a "… mode of verification …" of Euclidean geometry, he may not have believed that a non-Euclidean structure of space might ever be revealed observationally. Calinon said (1889: 595; my English translation):

> All that can legitimately be concluded, is that the differences which might exist between Euclidean geometry and that realized by the universe are due to experimental error.

In a later paper, Calinon (1893) argued in agreement with Poincaré that astronomical problems might be approached with the kind of geometry most suited to produce a simple solution. The choice of geometry might vary from one problem to another, he suggested, and even from one area of the Universe to another.

A contemporary of Calinon, the mathematician Paul Barbarin (1855–1931), was a prolific writer on non-Euclidean geometry. Contrary to Poincaré, he was an empiricist in the sense that he believed that the geometry of space was a question that could, and could only, be determined observationally. This is what he argued in his book of 1902, *La Géometrie Non-Euclidienne*, which included a chapter on what he called geometrical physics (Barbarin, 1902: 81-86). According to the French geometer, measurements of very small stellar parallaxes indicated that the radius of curvature exceeded 400,000 AU, which made him conclude that our part of the Universe might possibly be curved. On the other hand, it might just as well be Euclidean, and from a practical point of view there was not as yet any means of distinguishing between the two possibilities. Barbarin derived formulae for celestial triangles that could in principle distinguish between the three geometries associated with the names of Euclid, Lobachevsky and Riemann. However, he had to admit that his formulae were of no practical value as they relied on angle measurements much more precise than 0.01″. Yet he optimistically expressed his belief that the problem would be solved in the near future, thanks to the rapid progress in astronomical observational technology.

The works of French mathematicians such as Poincaré, Calinon and Barbarin were basically geometrical exercises rather than contributions to astronomy. Tellingly, they did not refer to values of stellar parallaxes or other astronomical data. From an astronomical point of view they were barren, doing nothing to change the general opinion of *fin-de-siècle* scientists, such as the mathematician-philosopher Bertrand Russell (1872–1970) summarized it in a dissertation of 1897:

> Though a small space-constant is regarded as empirically possible, it is not usually regarded as probable; and the finite space-constants with which Metageometry is equally conversant, are not usually thought even possible, as explanations of empirical fact. (Russell, 1897: 53).

This was indeed the consensus view at the turn of the century, shared by the majority of astronomers and physicists. In his lecture course in Vienna on natural philosophy in 1903-1906 Ludwig Boltzmann (1844–1906) referred several times to the possibility of a positively-curved stellar Universe. He found it fascinating that in principle an answer might be obtained by measurements of heavenly triangles with stars at their vertices:

> The spherical non-Euclidean space is completely closed in itself; it is not infinite, but has some finite size. If we know how large the triangles must be to correspond to a certain deviation from the sum of angles 180°, then we could also construct the size of the entire universe. We would then have a space which ends nowhere and as a whole returns into itself. (Fasol-Boltzmann, 1990: 215; my English translation).

He thought this was a perspective that offered "… enormous logical advantages." (ibid.). But logic is one thing; empirical reality is another. While in one of his lecture notes Boltzmann considered a closed Universe to be not only possible, but even probable, in a later note he held it to be "… not likely, yet it is a possibility that measurements of the stars will prove space to be non-Euclidean." (Fasol-Boltzmann, 1900: 255; my English translation).

8 TWO GERMAN ASTRONOMERS

Contrary to his French contemporaries and most other scientists at the turn of the century, young Karl Schwarzschild (1873–1916) considered curved-space astronomy a possibility that deserved serious attention.

He was a student of the distinguished Munich astronomer Hugo von Seeliger (1849–1924), according to whom non-Euclidean geometry could not possibly be useful in elucidating questions relating to physics, astronomy or cosmology. Space, Seeliger claimed, was nothing but an abstract reference system and devoid of properties of any kind. He consequently warned against

> … the common and therefore very fatal misapprehension that one … [is] able to decide by measurement which geometry is the 'true' one, or even, which space is the one we live in. (Seeliger, 1913: 200; my English translation).

Schwarzschild (Figure 5) disagreed with his former professor.

In an important lecture given on 9 August 1900 to the Astronomical Society in Heidelberg, Schwarzschild discussed from a modern perspective what Lobachevsky had done much earlier, namely, how to determine the geometry of space from observations. As one among other possible observational tests, he mentioned star counts relating the number of stars to their magnitudes:

> I have found that the number grows with magnitude more slowly in pseudospherical [hyperbolic] space, and more quickly in elliptic space, than under the same assumptions in Euclidean space. (Schwarzschild, 1900: 345; my English translation).

However, he focused on the classical case of parallax measurements.

While in Euclidean space the parallax, p, of a star infinitely far away is zero, in hyperbolic space there will be a minimal non-zero parallax that decreases with the curvature radius, R, such as shown by Equation (2). Let the radius of the orbit of the Earth be r, then $p \geq r/R$, as shown already by Lobachevsky in 1829. Thus, a measurement of the smallest known parallax imposes a lower limit on R. Schwarzschild estimated $p_{min} = 0.005''$, from which he concluded that $R > 4 \times 10^6$ AU. The bound, corresponding to about 20 parsecs or 60 light years, was an order of magnitude higher than the one estimated by Lobachevsky. Schwarzschild commented:

> Thus the curvature of the hyperbolic space is so insignificant that it cannot be observed by measurements in the planetary system, and because hyperbolic space is infinite, like Euclidean space, no unusual appearances will be observed on looking at the system of fixed stars. (Schwarzschild, 1900: 342; cf. Schemmel, 2005).

With regard to positively-curved space, Schwarzschild argued that the spherical case would lead to physically-unacceptable consequences, and for this reason he discussed only the elliptic possibility. In this case there are no infinite distances, and every parallax, including $p = 0$, corresponds to a finite distance. The relevant formula replacing $p \geq r/R$ is

$$\cot \frac{d}{R} = p \frac{R}{r} \tag{4}$$

where R is real and d is the distance from the object (star) to the observer along a geodesic. Contrary to the hyperbolic case, "… it is a mistake to believe that a limit for R can be found simply from measurements of the parallax of fixed stars." (Schwarzschild, 1900: 342; my English translation). Therefore, physical considerations were needed to determine the minimal value of R. Based upon star catalogues, he argued that all stars having a parallax smaller than $0.1''$ were located within a finite volume, and from this, and by assuming a uniform distribution of the stars, he reached the conclusion that $R = 1.6 \times 10^8$ AU = 2500 light years.

Schwarzschild further pointed out that in elliptic space a ray of light will return to its starting point after having traversed the world. We should therefore expect to see an antipodal image of the Sun, a 'counter-Sun', identical to our ordinary image of it but in the opposite direction. Of course, no such second image of the Sun is observed, a problem that Schwarzschild solved, or explained away, by assuming a suitable absorption of light in interstellar space. He summarized his results as follows:

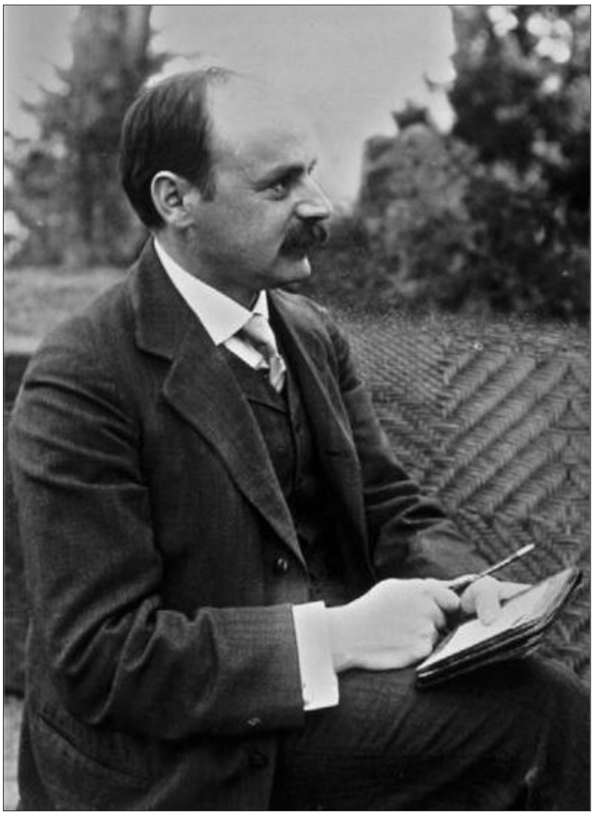


Figure 5: Karl Schwarzschild, 1873–1916 (after Runge, 1916: 545).

> One may, without coming into contradiction with experience, conceive the world to be contained in a hyperbolic (pseudo-spherical) space with a radius of curvature greater than 4 000 000 earth radii, or in a finite elliptic space with a radius of curvature greater than 100 000 000 earth radii, where, in the last case, one assumes an absorption of light circumnavigating the world corresponding to 40 magnitudes. (Schwarzschild, 1900: 345; my English translation).

He saw no way to go further than this rather indefinite conclusion and decide observationally whether space really has a negative or positive curvature, or whether it really is finite or infinite. Nonetheless, from a philosophical point of view he preferred a closed Universe. It would, he said, be "… satisfying to reason …" if we could conceive of

> … space itself as being closed and finite, and filled, more or less completely, by this stellar system. If this were the case, then a time will come when space will have been investigated like the surface of the earth, where macroscopic investigations are complete and only

> the microscopic ones need continue. A major part of the interest for me in the hypothesis of an elliptic space derives from this far reaching view. (Schwarzschild, 1900: 342; my English translation).

In his systematic discussion of a curved cosmic space there was one assumption that he, contrary to Zöllner nearly thirty years earlier, failed to mention, namely, that the Universe had existed for an eternity of time. But this was an assumption rarely questioned or even mentioned at the time, and one that also went unquestioned in the early relativistic models of the Universe.

While Schwarzschild's paper of 1900 is well known, an interesting paper by Paul Harzer (1857–1932) eight years later has rarely if ever received mention in the literature on history of astronomy. The reason may be that it was published in a mathematical and not an astronomical journal. It deserves to be better known, for Harzer, a Professor of Astronomy at the University of Kiel, went further than Schwarzschild's investigation by extending it to the distribution of stars. Starting in 1898, Seeliger had developed a model of our Galaxy by means of an elaborate mathematical analysis of star counts and stellar magnitudes (Paul, 1993). While Seeliger based his 'statistical cosmology' on the unstated assumption of Euclidean space, in a lecture of 1908 Harzer transformed the calculations to a space of constant positive curvature. In this way he arrived at a modified picture of our Galaxy.

Harzer's stellar Universe was enclosed in a finite cosmic space with a volume about seventeen times that of the stellar system. As to this stellar system, it contained the same number of stars but was compressed to a size approximately one half of what it had in Seeliger's infinite Euclidean space. The size of the entire Universe was given by the time it took for a ray of light to circumnavigate it, which Harzer estimated to be 8,700 years. During its travel round the world the light would became dimmer because of absorption, and by taking into account the motion of the Solar System he arrived at a loss in light intensity corresponding to thirteen magnitudes. This was a more realistic value than Schwarschild's forty magnitudes, yet it was sufficient to make the problem of the counter-Sun go away.

Harzer took the model of a closed stellar Universe no less seriously than Schwarzschild, but of course he realized that it was hypothetical and lacked the support of solid observational evidence. Consequently, his conclusion was cautious:

> This picture includes no features that can be characterized as improbable … But the picture speaks of the *possibility* of the finite space only, not of its *reality*, and as yet we have no evidence for this reality. (Harzer, 1908: 266; his italics; my English translation).

The Schwarzschild-Harzer suggestion of a closed space filled with stars had the conceptual advantage that it did away with the infinite empty space, but it made almost no impact on mainstream astronomy. The cosmological problem that moved to the forefront of astronomy in the 1910s was concerned with the size of our Galaxy and the question of whether the spiral nebulae were external objects or belonged to our Galaxy. This was a problem in which the geometry of space was considered irrelevant. When it was finally solved in the mid-1920s it was by observational means, namely, Edwin Hubble's (1889–1953) famous discovery of Cepheid variables in the Andromeda Nebula (Hubble, 1925; cf. Berendzen, Hart, and Seeley, 1984).

9 CONCLUSION

Whereas non-Euclidean geometry flourished as a mathematical research field in the last half of the nineteenth century, its connection to the real space inhabited by physical objects was much less cultivated. The large majority of mathematicians did not care whether real space was Euclidean or not; and those who did care only dealt with the subject in a general and often casual way, and avoided dealing seriously with the possibility of determining a space curvature different from zero. After all, that was supposed to be the business of the astronomers. While some mathematicians, following Poincaré, declared the problem meaningless, others admitted that in principle space might be curved—but in principle only—and left it at that.

Most astronomers were well aware of the possibility of space being non-Euclidean, but it was considered a remote possibility and not one that would keep them awake at night. Astronomy and cosmology books in the early twentieth century usually presented the material world as consisting of a huge conglomerate of stars, essentially our Galaxy, floating in the infinite Euclidean space. What might be beyond the stellar system was left to speculation. It might be empty space or some ethereal medium, in any case it was regarded as irrelevant from an astronomical point of view. As the historian and astronomy author Agnes Mary Clerke (1842–1907) expressed it, "With the possibilities beyond, science has no concern… " (Clerke, 1890: 368).

Astronomers had their own reasons, different from those of the mathematicians, to ignore non-Euclidean geometry. Lack of awareness of the new forms of geometry or lack of mathematical competence were not generally among the reasons as many astronomers had strong backgrounds in mathematics and were conversant with the technicalities of non-Euclidean geometry. But while the motion and properties of celestial bodies were definitely the business of the astronomers, the space in which the bodies move was not seen as belonging to the domain of astronomy. It was a kind of 'nothingness' that philosophers could speak of, and did speak of. Newcomb (1898: 5) probably spoke for the majority of his colleagues when he warned against "… the tendency among both geometers and psychoogists to talk of space as an entity in itself." To arouse interest in the astronomical community, theories of non-Euclidean space would have to be observationally testable or offer opportunities for solving problems of astronomical relevance. They scored badly on both counts.

Even though non-Euclidean geometry was thought to have little or no explanatory force, there was the possibility that it could be verified by measurements. While it could never be proved that space was Euclidean, it could conceivably be proved that it was not. As we have seen, a few astronomers and other scientists—such as Ball, Newcomb, Peirce, Barbarin and Schwarzschild—did take an interest in this line of reasoning, going back to Lobachevsky. However, while in the early years of the twentieth century it was realized that the curvature of space was indeed measurable, it was also realized that the kind of upper bound for the curvature that measurements allowed was ineffective to distinguish curved from flat space. Under these circumstances, it is no wonder that astronomers saw no reason to abandon the intuitively pleasing Euclidean space that had served their science so well in the past. Even should space be curved, the curvature radius would be so large that for all practical purposes it was infinite, that is, space could be considered Euclidean. So why bother? It seems that the main reason for the astronomers' reluctance to consider the consequences of space being non-Euclidean was just this: they had no need for the hypothesis.

10 EINSTEINIAN POSTSCRIPT

Although this review is limited to the pre-relativity era it would not be out of place to recall that the question of curved space entered a wholly new phase with Albert Einstein's (1879–1955) General Theory of Relativity. The observational evidence for curved space that was still missing at the time of Schwarzschild and Harzer first turned up in 1919 with the detection of the bending of starlight in the famous Eddington-Dyson solar eclipse expedition. Of course, this was a local curvature of space caused by the Sun's gravitational field and not a proof that global space is positively curved. Einstein's General Theory of Relativity revolutionized cosmology, but it did not and cannot provide an answer to the old question of whether cosmic space is closed or not, or finite or not. The present consensus view is that we live in a flat infinite space, yet (as Lobachevsky was already aware of) this is a view that can never be proved observationally. Another question that turned up in physical theory in the 1920s was the number of space dimensions, although this question was more discussed in the context of microphysics than in a cosmological context (Wünsch, 2010).

In early 1921 Einstein gave a brilliant address to the Prussian Academy of Sciences in which he reflected on the relationship between mathematics and the physical sciences (Einstein, 1982: 233). He famously stated that "… as far as the propositions of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality." Einstein distinguished between what he called 'practical geometry' and 'purely axiomatic geometry', arguing that while the first version was a natural science, the second was not, and

> The question whether the universe is spatially finite or not seems to me an entirely meaningful question in the sense of practical geometry. I do not even consider it impossible that the question will be answered before long by astronomy. (Einstein, 1982: 239).

Indeed, without this view of geometry, he continued, "I should have been unable to formulate the theory of [general] relativity." (Einstein, 1982: 235).

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THE WELLS CREEK METEORITE IMPACT SITE AND CHANGING VIEWS ON IMPACT CRATERING

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Abstract: Wells Creek is a confirmed meteorite impact site in Tennessee, USA. The Wells Creek structure was first noticed by railroad surveyors around 1855 and brought to the attention of J.M. Safford, Tennessee's State Geologist. He included an insert in the 1869 Geologic Map of Tennessee, which is the first known map to include the structure. The origin of the Wells Creek structure was controversial, and was interpreted as being either the result of volcanic steam explosion or meteorite impact. It was only in the 1960s that Wilson and Stearns were able to state that the impact hypothesis was preferred. Evidence for a Wells Creek meteorite impact includes drill core results, extreme brecciation and shatter cones, while a local lack of volcanic material is telling. Just to the north of the Wells Creek Basin are three small basins that Wilson concluded were associated with the Wells Creek impact event, but evidence regarding the origin of the Austin, Indian Mound and Cave Spring Hollow sites is not conclusive.

Keywords: Wells Creek, Tennessee, impact crater, extreme brecciation, shatter cones, J.M. Safford

1 INTRODUCTION

The state of Tennessee in the USA boasts two undisputed impact craters, Wells Creek and Flynn Creek, and two possible impact craters, the Dycus Structure and the Howell Structure (e.g. see Berwind, 2006, 2007; Deane et al., 2004; 2006; Evenick, 2006; Evenick et al., 2004; Milam et al., 2006; Mitchum, 1951; Roddy, 1977; Schedl et al., 2010; Schieber and Over, 2005; Stearns et al., 1968; and Woodruff, 1968). Of these, the Wells Creek site has played a major role in increasing our awareness of the nature of terrestrial impact cratering, and is referred to by Dietz (1963: 650), not as the 'prototype', but rather as the 'syntype' cryptoexplosion structure for the United States. As such, the knowledge gained from its recognition as an impact structure is worth revisiting.

Impact cratering was the dominant geological process in our Solar System, and was responsible for shaping surfaces on the terrestrial planets and their moons, and on the asteroids (Melosh, 1989). Shotts (1968: 459) points out that "For lunar craters, diameter and depth of floor can be measured, but neither true depth below the original surface nor depth of brecciation can be measured." These last two can be determined for terrestrial impacts, though, and the knowledge gained applied in studies of our Solar System. Despite the advances made in our understanding of Solar System impact cratering, it took many years before the idea that the Earth also was subjected to these bombardments was widely accepted by astronomers and geologists (e.g. see French, 2004; Reimold, 2003; Reimold and Koeberl, 2008).

In her catalog of meteorite impacts sites O'Connell (1965: 1) states that

> … the study of terrestrial craters and similar geological features of known and possible meteorite-impact origin … has become a major interdisciplinary effort carried on by astronomers as well as geologist and by other scientific specialists such as geophysicists and astrophysicists.
>
> But these books are written by, and primarily for, astronomers, whose main interest in terrestrial meteorite craters is their many analogies to lunar craters. Otherwise, information about terrestrial craters is widely scattered throughout the scientific and general literature, where it is presented in many forms ...

Accordingly, she prepared her 1965 catalog in an attempt to index "… this widely scattered and often elusive material … [in response to] the difficulties encountered in gathering material." Likewise, much of the material regarding the Wells Creek impact site is scattered through the seemingly-unrelated astronomical and geological literature. This paper reviews the compiled information on the Wells Creek structure generated by researchers during the past one hundred and fifty years.

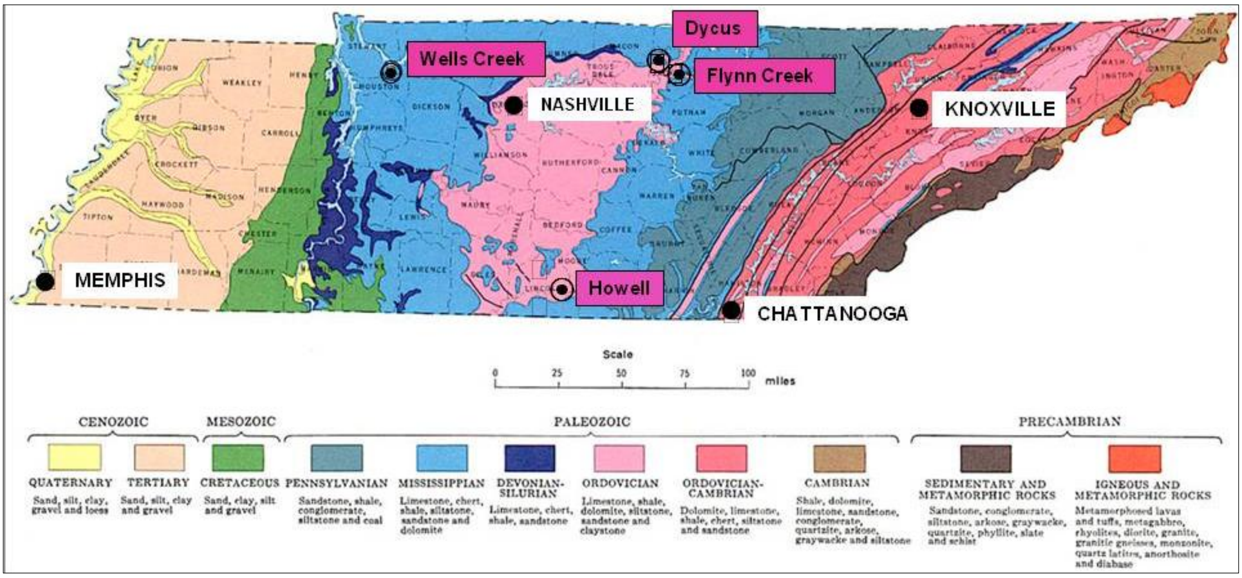

Figure 1: Generalized geological map of Tennessee showing the locations of the four largest cities (black dots) and the two confirmed and two suspected meteorite impact sites (small black dots with circles). These sites are located on the Highland Rim (Wells Creek), a Highland Rim outlier remnant (Howell), or on the Highland Rim escarpment (Dycus and Flynn Creek). The Highland Rim is the sky blue region on the map (base map after Tennessee Department Conservation, Division of Geology, 1966).

2 TENNESSEE GEOGRAPHY AND GEOLOGY

Situated in eastern, south-central USA, Tennessee is a long narrow state. Figure 1 is a geological map of the state and shows the four largest cities, and the locations of two confirmed impact sites, Wells Creek and Flynn Creek, as well as the two suspected impact sites, Howell and Dycus.

The Wells Creek structure (36°23′ N, 87°40′ W) is located about 210 meters above sea level in the northern part of middle Tennessee, in a region known as the Western Highland Rim. This forested area is characterized by rolling terrain and is graced by numerous creeks and streams. The Wells Creek Structure is about 13.7 km in diameter and is situated to the south of the Cumberland River. It is not easily discernible on aerial or satellite photographs (cf. Stratford, 2004: 10). This is not surprising as Dietz (1963: 653) notes that "Most structures of this type do not stand out on aerial photos."

However, Wells Creek does stand out as a 'bulls-eye' on geological maps of Tennessee (Miller, 1974: 9). Tennessee was covered by shallow seas during most of the Mississippian Period, 345 to 310 million years ago, and sediments were deposited then which now cover most of the Highland Rim. Rocks comprising the Knox Group, deposited earlier, during the Ordovician and Cambrian Periods, 500 to 425 million years ago, are exposed in only two locations in the Highland Rim, namely at the Wells Creek and Flynn Creek impact structures (Miller 1974: 19). Figure 1 shows the distribution of exposed rock units across the State and on the original version of this map the Wells Creek site is obvious, displaying uplifted older rocks surrounded by younger rock units.

3 HISTORICAL CONTEXT

In August 1854 the Memphis, Clarksville, and Louisville Railroad started work on a new railway line which would eventually run from Paris (Tennessee) to Guthrie (Kentucky) via the Wells Creek Basin (Price, 1991). Engineers and surveyors noted the area's strange, twisted rocks and tilted bedding planes which stood out in stark contrast to the region's usual horizontal stratigraphy.

Dr J.M. Safford's first report as State Geologist of Tennessee in 1855 included a geological map of the State, but did not show the Wells Creek structure. The structure, however, was included in his 1869 Tennessee geological map, with descriptions given on pages 147-148, 220, and 257 of his report. Figure 2 is the geological map of Tennessee that Safford

Figure 2: Safford's 1869 Geological Map of Tennessee (courtesy: Birmingham, Alabama Public Library Cartography Collection).

drew to go along with his 1869 report. In addition, a detailed geological map of the Wells Creek structure was placed in the corner of the main geological map of Tennessee (Wilson and Stearns, 1966: 37). Figure 3 shows this inset, which is titled "The Well's Creek Basin in Stewart Country". Safford (1869: iv-v) indicated in the report's preface that "A great amount of labor has been bestowed upon the Map … Aside from its Geology, the Map, so far as it goes, is the best geographical map of Tennessee yet published." In this report, Safford (1869: 147; his italics) states that there are exceptions to the generally-horizontal positions of the rock layers he located in Tennessee's Middle Division, and that

> The most interesting of these localities is in the region of Cumberland City, a small town on the Cumberland River, in Stewart County. This town is on the side of an elliptical area, or basin, containing six or seven square miles, and surrounded by hills. The river cuts through the northern end of the basin. Wells Creek enters it on the south and flows through it to the river. From this circumstance I have named it the *Wells Creek Basin*. Within this area the strata are highly inclined. We have here a very considerable upheaval of the formations. The strata were lifted in a high dome, the top of which has been worn and washed away.

Safford notes that the lowest strata have been elevated at least 760 meters and that the dip is found to be at high angles, even vertical at some points. He also points out that the Wells Creek disturbance is not confined to the Basin, but extends several kilometers beyond Cumberland City and that the rock lay-rock layers are folded, fractured and dislocated, and have inclinations at all angles (e.g. see Figure 4). This deformation is confined to the rocks of the Lower Carboniferous. Safford (1869: 220) refers to Wells Creek as the "… exceptional spot, in Middle Tennessee, showing outcropping Knox Dolomite …" and he notes that the basin is highly valued for farming. Furthermore, "The dome has a depression all around it – a ring of valleys, in which outcrops the Trenton, Nashville, and Niagara rocks." (ibid.).

J.B. Killebrew and Safford gave a more detailed description of the central part of the Wells Creek basin on pages 761-762 of their 1874 monograph:

> This is an area, nearly circular, containing six or seven square miles, and touching the Cumberland River. Wells' Creek runs through it, the rocks in the basin dip at a very great angle, and in some places are nearly vertical. *There are evidences of a terrible subterranean convulsion at one time*. (Our italics).

Between 1889 and 1893, based on the dates listed in their field notebooks, Safford, who was by then a Vanderbilt University Professor, and W.T. Lander, a Vanderbilt Graduate Fellow, mapped the structure in detail (Wilson and Stearns, 1966: 37). It was during this time that the actual size of the Wells Creek structure was recognized. Their circa 1895 manuscript based on this field work includes a geological map and cross sections. According to Wilson and Stearns (ibid.) "… this manuscript with its map and drawings is probably the first detailed geologic report on a cryptoexplosive (perhaps meteor impact) structure in the United States." Wilson (1953: 755) believes that Lander also "… prepared a detailed manuscript on the annular rings of faults that encircle the central uplift (ca. 1899)." We doubt that this manuscript has

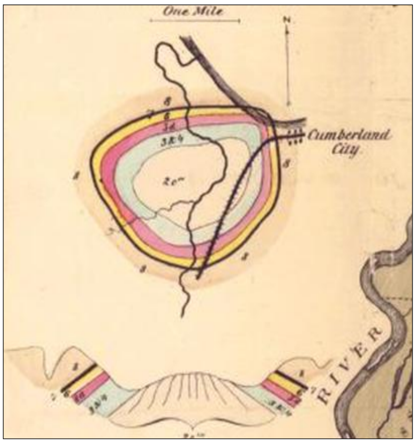

Figure 3: An enlargement of the small map inset on the upper left of Figure 2 (courtesy: :Birmingham, Alabama Public Library Cartography Collection).

Figure 4: A recent photograph illustrating "… the rock layers are folded, fractured and dislocated, and have inclinations at all angles." (photograph by the first author).

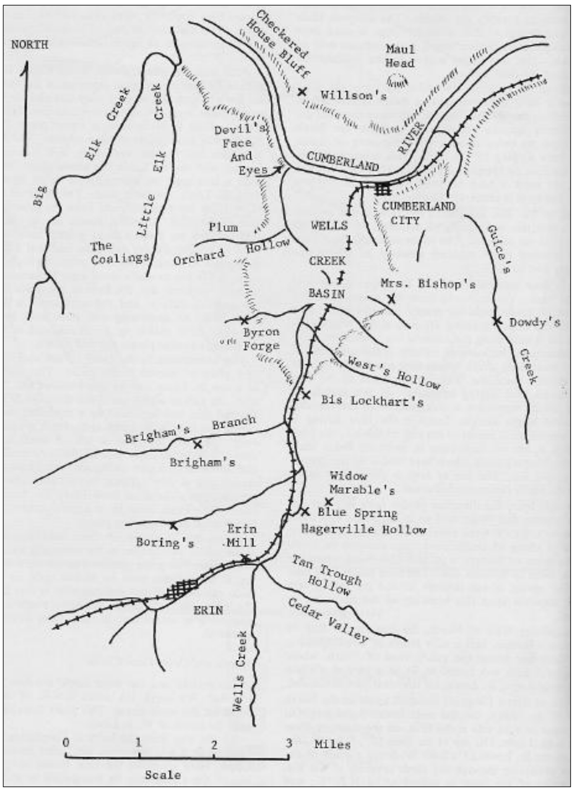

Figure 5: Safford and Lander's geographical map of the Wells Creek Basin (after Wilson and Stearns, 1966: 42).

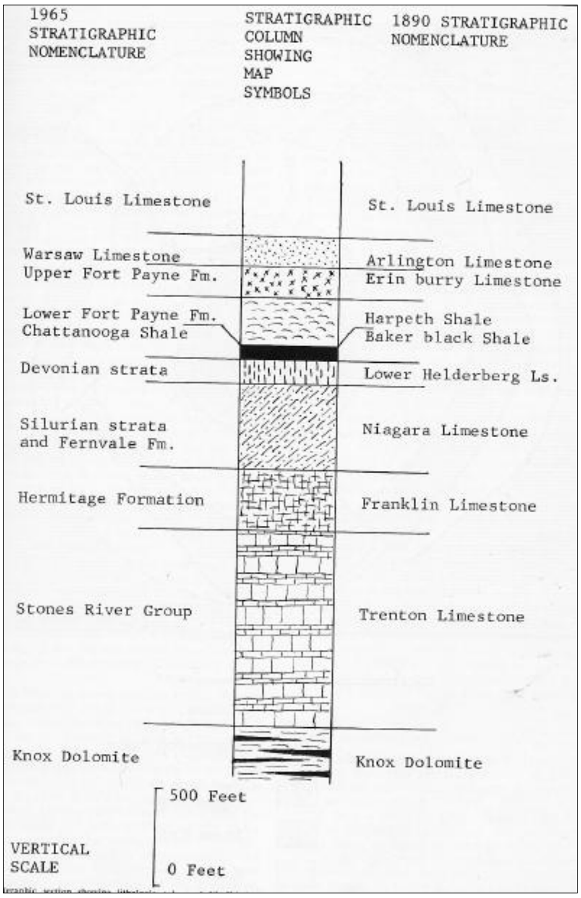

Figure 6: A stratigraphical section showing the lithological column with symbols as well as the stratigraphical nomenclature used in 1890 and 1965 (after Wilson and Stearns, 1966: 39).

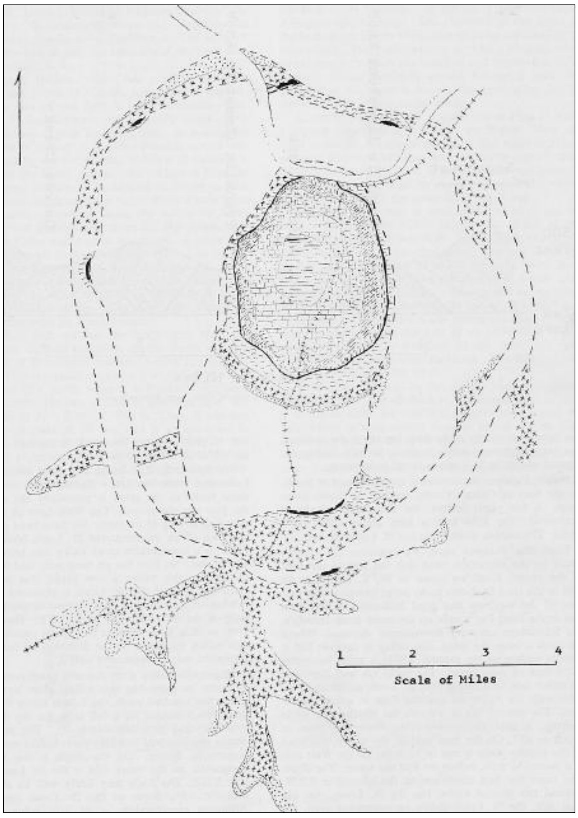

Figure 7: Geological map of the Wells Creek structure drawn by Safford and Lander circa 1895 (after Wilson and Stearns, 1966: 43).

survived as we were unsuccessful in locating it.

Figure 5 is a geographical map that shows the locations of the various features in Wells Creek that were studied and referred to by Safford and Lander in their 1895 manuscript (which was eventually published by Wilson and Stearns in 1966). Figures 1 and 3 record the formations of the Wells Creek basin they discovered. The nomenclature of the formations has changed over time and these changes in terminology are summarized in Figure 6.

Safford and Lander noted that the first five formations shown in Figure 6 were found to be confined to the central part of the Basin. The next five lay outside of and around the central part. It is in this outside area that the most striking faults were located (see Wilson and Stearns, 1966: 38). In an earlier publication, Killebrew and Safford (1874: 761-762) described their surprising findings:

> … a lower formation is never superimposed on a higher one without showing signs of great distress … This is precisely the case with the Wells' Creek basin. The center of the basin has been elevated by subterranean forces, and the elevation or cone swept away by abrasion. The surrounding rocks belong to the silicious group of the lower carboniferous formation; the other formations – the Black Shale of the Devonian, the lower Helderberg, and the limestone of the upper Silurian; the Nashville and Trenton limestones, and lastly, the Knoxville limestones of the lower Silurian, all appear in regular succession until the center of the basin is reached. Walking across the valley, all the formations are passed over twice, except the lowest – the Knoxville.

The Knoxville Dolomite marks the center of the Wells Creek structure and is the oldest geological formation.

Around 1895 Safford and Lander wrote that they "… found so many exposures of the Baker black shale on the rim of the Basin as virtually to make a continuous outcrop, evidently produced by the general Basin erosion …" (cited in Wilson and Stearns, 1966: 38). In their circa 1895 manuscript Safford and Lander stated:

> On locating these exposures, on the map, it was suggested that they were likely produced by a roughly circular fault surrounding the Basin. As the work continued, many observations and facts appeared to favor this view. But faults were found which could not be placed in this circle; so that it became manifest that, if there were one circle of faults, there must be two other concentric circles also. On the map, the three circles proposed are indicated, no fault being laid down except such as were carefully located …
>
> In defense of the proposition that there are three concentric circles of faults around the Basin, we not only offer a description of the faults found, but add that the position of most of them was predicted with satisfactory accuracy before they were visited; and furthermore, that no prediction as to the position of a fault was unverified, except in a few cases where no rocks were exposed to indicate the lay of the formation. (ibid.).

The geological map of the Wells Creek structure drawn by Safford and Lander around 1895 is shown in Figure 7 (after Wilson and Stearns, 1966: 43). Wilson and Stearns (ibid.) point out that "… the geology set forth is amazingly accurate, as anyone familiar with Safford's work would readily believe." It is interesting, though, to compare the map by Safford and Lander with the geological map of Wells Creek showing the fault patterns as they were understood in 1965 by Tiedemann, Marsh, and Stearns (see Figure 8). Figure 7 includes yet another main fault around the structure and shows that these circular faults define a set of concentric rings. Wilson and Stearns (1966: 47) note the excellent field work completed by Safford and Lander, but add that with the luxury of hindsight it is clear that

> … Safford and Lander found three faults everywhere around the structure. Unfortunately, they did not find the same three faults all the way around. They did not find the outermost fault in the northern portion of the structure … [where the] fault [is] difficult to see. In the southern part of the structure, they did not find the innermost fault, mainly because of unfavorable exposures.
>
> They connected the three faults known to them (through areas of scant exposure on the east and west sides of the structure) in such a manner that each fault on the north side connected with a fault of opposite vertical movement on the south side.

W.H. Bucher was the next to study the Wells Creek site, and he produced a geological map of the structure for the Tennessee Division of Geology that he included in his 1936 paper on cryptovolcanic structures. At the time this was the second known map of Wells Creek since Bucher did not know of the work of Safford and Lander; their circa 1895 manuscript was lost for sixty years, and was only published in 1966 (Stearns, 1988: 1). Wilson and Stearns (1968: 15) state that Bucher's (1936) paper and map "… showed his remarkable knowledge and understanding of the structure."

4 STRUCTURAL FEATURES

As Miller (1974: 55) points out, "The term crypto-explosion was first used in 1959 (Dietz) to designate a generally circular structure that was formed in some manner by a natural release of energy …" This energy was thought to come from either a crypto-volcanic steam explosion driving rocks upward and outward, or a meteorite impact. A high-velocity meteorite, which possesses a large quantity of kinetic energy before penetrating the Earth's surface, will explode after impact resulting in a great release of energy. Shock waves will move outwards from the focus of the meteorite impact, forming ring synclines and anticlines. Baldwin (1949: 101) states that the Wells Creek structure is similar to that seen in "… high-speed pictures of a drop of liquid falling into water." This type of structure is a complex crater with a central uplift and two fault rings surrounding the basin. Figure 9 shows Baldwin's (1963: 50) idealized cross-sections of simple and complex craters indicating distortions of rock layers and zones of brecciation.

In 1947 the Ordman Company cored the Wells Creek Basin in the belief that it was a salt dome. The core was given to the Tennessee Division of Geology and studied in 1951 by R.E. Hershey and C.W. Wilson with the following results:

> The core is essentially complete from a depth of 23 [7 m] to 2000 feet [610 m]. It started and bottomed in

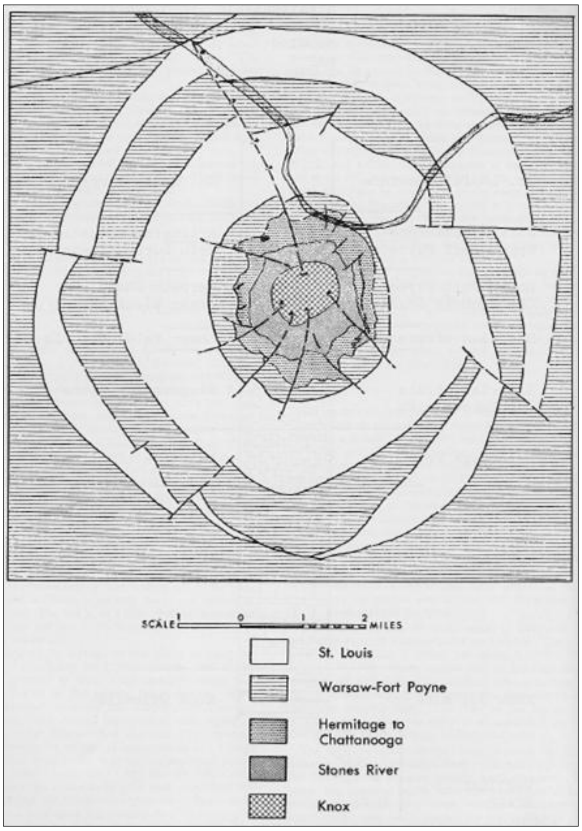

Figure 8: Geological map of Wells Creek Basin showing fault patterns as understood in 1965 (after Wilson and Stearns, 1966: 40).

> Knox dolomite …
>
> The injected breccia consists of a matrix of pulverized rock containing fragments of chert, limestone, and dolomite of great variety and usually less than half an inch [1.3 cm] in maximum dimension ... It is believed that the fragments in the breccia came from many of the formations present in the sequence …
>
> The examination of this core was an unusual privilege and in a way an eerie experience. The deep fingers of grotesque injection dikes and the intense, bizarre, ever-changing pattern of brecciation and deformation are awe-inspiring. Each new box of cores revealed new, strange, and different intricacies. (Wilson, 1953: 766).

Research on the Wells Creek Basin accelerated during the 1960s. The decision to undertake a series of manned landings on our Moon unleashed "… un-

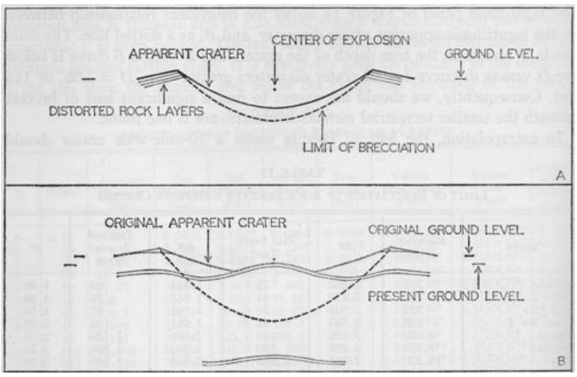

Figure 9: Idealized cross-sections through impact craters showing distortions of rock layers and zones of brecciation. At the top is the Odessa No. 1 crater, an example of a simple crater. Below is the Wells Creek Basin, an example of a complex crater (after Baldwin, 1963: 50).

heard-of levels of funding to research programs … and scientists in university, industry, and government labs were encouraged to do research on problems related to impact cratering." (Melosh, 1989: 11). Work on every aspect of impact cratering was stimulated. Accordingly, in 1963 NASA gave Vanderbilt University a grant to study the Wells Creek impact structure (Wilson and Stearns, 1968: 17), and most of the mapping and much of the information currently known and available concerning this site came from that study. Figure 10 is a map produced during this time showing the major structural features of Wells Creek (after Wilson and Stearns, 1968: 55).

Although Wells Creek is highly eroded, the structure's original faulting is still evident. The structure is about 13.7 km in overall diameter and Wilson and Stearns (1968: 3-4) describe it as having five structural subdivisions that are given below in order outwards from the center:

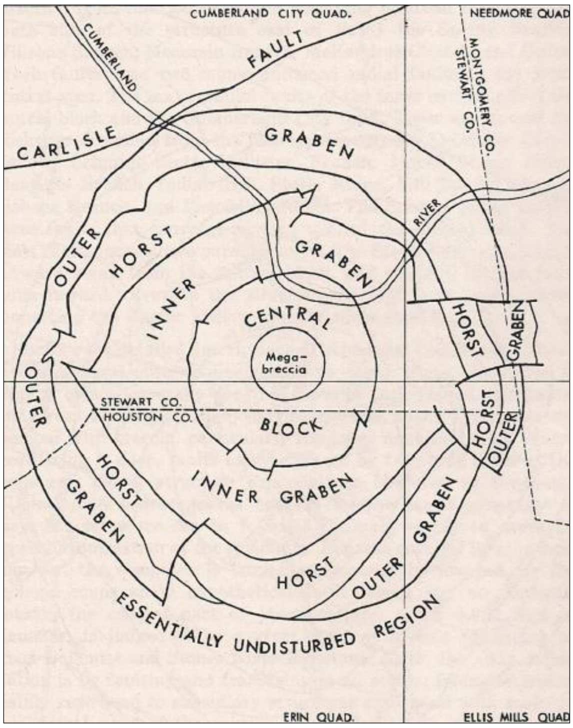

Figure 10: Map showing the major structural features of the Wells Creek structure (after Wilson and Stearns, 1968: 55)

(1) the circular central block – diameter 5.03 km, containing a circular core of megabreccia about 1520 m in diameter
(2) the annular inner graben, a downthrown block – width 1.83 km
(3) the annular horst, an upthrown block between two fault blocks – width 1.22 km
(4) the annular outer graben, a downthrown block – width 1.08 km
(5) the essentially undisturbed region surrounding the Wells Creek structure

The graben subdivisions dropped by as much as 170 m, while the rock at the center was uplifted by at least 760 m. The above dimensions were determined from surface measurements.

Wilson and Stearns (ibid.) also noted the structure's inward movement pattern. The dip of the outside fault of the outer graben is nearly vertical, but the inside fault dips outward from 30° to 60°. The result is that the outer graben narrows as the bounding faults converge with depth. Likewise, the dip of the outside fault of the inner graben is also nearly vertical; however, the inner fault dips steeply outward from 45° to 70°. Again the result is that the inner graben also narrows with depth. This means that the horst widens between the inner and outer grabens. Wilson and Stearns (1968: 89-92) note that although the outer edge of the central block does not appear to have moved from its original level as a result of the Wells Creek event, the cylindrical central block is uplifted in the center. 'Central Hill' rises some 137 meters near the center of the basin (Wilson and Stearns, 1968: 8). In this central block, a central zone 1.6 km in diameter is megabrecciated (Wilson and Stearns, 1968: 5). The conclusion is that the grabens dropped as material moved inwards when the central block was uplifted (Wilson and Stearns, 1968: 5-6).

Baldwin (1963: 108) points out that "…at larger impact structures, the anticline is itself bordered by a second ring syncline … and it is well developed at the Wells Creek Basin." He believes that the Wells Creek Basin structure originally was a 10 km diameter crater, and that it "… shows a definite ring syncline around it, and fragmentary indications of a ring anticline …" about 16 km in diameter (Baldwin, 1963: 109).

Wilson and Stearns (1968: 5) report that the uplifted central block consists of jumbled blocks of all sizes and megabreccia, and that it contains a core of Knox dolomite. The megabreccia includes both Knox and younger strata. They also note that "As well as can be measured, the volume of rock downthrown in the two ring grabens appears to be equal to the uplifted rock in the central block. This is consistent with the geophysical evidence that there is no intrusion at depth or uplift of basement rocks." (ibid.). Wilson and Stearns (1968: 4-8) believe that the horst and grabens are primarily exterior structures resulting from elastic rebound due to shock pressure following the impact and subsequent explosion. Hence, "The grabens occur where rock fell downward and outward into ring cracks; these ring cracks developed during inward movement of rock that formed the central uplift."

In his M.S. thesis S.M. Puryear (1968: 4) includes the following description of the Wells Creek structure. The outer graben is downfaulted 60 meters; the horst is basically level with the surrounding region, and the inner graben is downfaulted between 90 and 180 meters. The central cylinder of rock is uplifted at least 600 to 760 meters. The central uplift is topographically a 3.2 km basin. Puryear (1968: 27) believes there is a relationship between the general shape of the Wells Creek structure and two main joint sets that existed prior to the impact event, and he states:

> The Wells Creek structure demonstrates a pattern, especially the second and third concentric faults, which is "squarish" in shape. Shoemaker (1959) observed at Meteor Crater that "the regional jointing has controlled the shape of the crater, which is somewhat squarish in outline; the diagonals of the 'square' coincide with the trend of the two main sets

of joints." Like Meteor Crater, Wells Creek shows a relationship between the shape of the structure and the trend of the two major joint sets. The two major joint sets parallel the diagonals of the square. (Puryear, 1968: 25).

Miller (1974: 56) also notes that the roughly circular inner basin is about 3.2 km across and adds that "Some of these blocks are dropped down relative to others, indicating great uplift followed by differential subsidence of the earth in the vicinity of the structure." He describes the breccia in the central part of Wells Creek as consisting of highly-fragmented angular-edged pieces that have been strongly re-cemented. He also confirms the findings of Safford and Lander made 80 years earlier: the central uplift is a core of the older rocks, the Knox Group, located in the center of the basin, with younger rocks found progressively farther away from the center. Wilson and Stearns (1968: 8) agree, describing Wells Creek as a circular basin with 'Central Hill' near its center, rising some 25 m above "… a belt of prominent inner annular valleys." The central block contains Knox Dolomite, which is surrounded by concentric belts of "… post-Knox Ordovician, Silurian, Devonian and lower Mississippian formations." (Wilson and Stearns, 1968: 5).

A simple crater is a small, bowl-shaped crater, often with a raised rim, that originally had a depth that was as much as one quarter to one third its diameter before being partially filled with fallback breccias. A complex crater will display a central uplift, consisting of strata lifted above pre-impact levels, surrounded by a ring depression, or syncline. The syncline is usually filled with fragmented material, breccias, and is often surrounded in turn by a terraced rim. These larger craters experience the inward and upward movement of rock from below the crater as a result of the impact-produced central uplift. Figure 10 compares Baldwin's idealized cross-sections of the Odessa Crater number 1, a simple crater, and the Wells Creek Basin, a complex crater (after Baldwin, 1963: 50). Mark (1987: 162-163) points out that "… central uplifts are now considered analogous to the central peaks of lunar craters."

Fallback breccia and impact melt are concentrated toward the center of simple craters whereas in complex craters these deposits are thickest in a ring surrounding the central uplift. The original, transient crater walls in complex craters have most often been modified by collapse due to gravity, thus forming the terraced walls seen today. These structures are also much shallower in comparison to their diameters than simple craters. Wells Creek fits the description of a complex crater. This is as expected since Wells Creek is around 13.7 km in diameter and the transition from simple to complex craters occurs on Earth somewhere between 3 and 5 km, depending on whether the crater forms in sedimentary or crystalline rock (see Melosh and Ivanov, 1999).

Stratford (2004: 6) points out that "On geologic maps these … structures appeared as circular inliers of older rocks surrounded by concentric circular outcrops of successively younger rocks; this concentric pattern was, however, disturbed, and often disguised, by intense faulting." He also notes that the Wells Creek pattern of central uplift with radial faulting surrounded by concentric circular outcrops of rock is characteristic of terrestrial impact structures that formed in sedimentary terrains.

According to Milam and Deane (2005), brecciated material was found in significant amounts in the major faults at the Wells Creek site. They refer to these breccias produced along the major fault lines of the uplifted central area as 'fault breccias'. At Wells Creek the fault breccias contain pebble- to silt-size angular grains with many showing fine-grain outer margins surrounding course-grained centers. Some flow texture was noted along some of the outer margins.

5 THE AGE OF THE WELLS CREEK STRUCTURE

Since 325 Ma Mississippian rock is deformed at Wells Creek, the structure must have been formed after these rocks were deposited, and because the Cretaceous Tuscaloosa Formation (which dates to 75 Ma) has been found in the deformed area, the Wells Creek event must have occurred prior to the deposition of this Formation. No rock from any periods between these units have been found in any part of the structure, so on the basis of this geological evidence the age of the Wells Creek structure can only be estimated at 200 ± 100 million years. Referring specifically to the Wells Creek structure, Baldwin (1949: 103) points out that

> It is well to realize that, while this is the only method capable of dating these cryptovolcanic structures, the great discontinuities in geologic history as shown by the rock layers at any particular point leave tremensdous spans of time unaccounted for. Hence the dates of formation of these objects are uncertain usually by tens of millions of years and often by hundreds of millions.

Wilson (1968: 15) states that "… it is now believed that the Wells Creek structure is Late Mississippian in age rather than 'post-Eutaw, pre-Wilcox' (post-Late Cretaceous, pre-Eocene)."

6 THE 'CRYPTO CONTROVERSY'

Wells Creek is highly eroded. Erosion over long time periods will reduce the height of the crater wall and sediment will begin to fill the crater depression. The creek which gives this structure its name cuts through and erodes the basin on its way to the Cumberland River. However, Wilson (1953: 756) notes that some structural features at Wells Creek are still discernable, including the central uplift, since "… the relatively resistant Knox dolomite and chert form a low rounded hill in the center of the basin, above which it rises about 75 feet [23 meters]." Dietz (1959: 497-498) points out that "Meteorite craters are, of course ephemeral geologic features which are rapidly eroded away, but the jumbled mass of shattered rock which must extend for several thousand feet beneath an impact crater stands an excellent chance of geologic preservation."

The doctrine of catastrophism was not in favor during the early part of the twentieth century. The idea that the Earth had ever been impacted by meteorites large enough to pierce its surface and penetrate layers of subsurface rock seemed absurd to many in the scientific community (e.g. see Hoyt, 1987). W.H. Bucher (1936) became interested in the Wells Creek structure around 1930 and promptly applied the term 'cryptovolcanic' to it. Dietz (1959: 496) notes that "The term 'cryptovolcanic' is derived from the belief that these structures are formed by volcanic explosions, although the evidence of volcanism is hidden." This term was first used by Branca and Freas in 1905 (see Bucher, 1963a: 1241).

The largest structure included in Bucher's 1936 list of known cryptovolcanic structures in the United States is the Wells Creek structure (cf. Mark 1987: 66). Baldwin (1949: 110) includes Wells Creek, Flynn Creek, and Howell Tennessee in his list of the twelve best-known cryptovolanic structures. Bucher (1963a: 1243) states that Wells Creek stands out among American cryptovolcanic areas because of its size, the intensely broken-up condition of the rocks in the uplifted center caused by a subterranean explosion, and because of the "… distinct, anticlinal ring between the outer limits of the structure and the central uplift, suggestive of an elastic damped wave effect." (cf. Bucher, 1963b).

Several decades before Bucher made this statement, though, Boon and Albritton (1936: 7) described just such a scenario in a paper on meteorite craters.

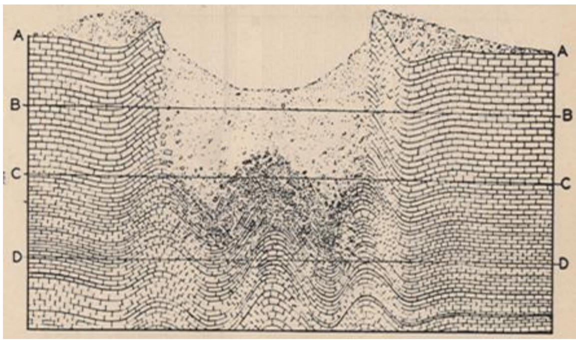

Figure 11: The probable structure beneath a typical meteorite crater (after Boon and Albritton, 1937: 57).

They recognized that identifying ancient impact structures would be difficult, and so they attempted to understand and describe what effect the impact would have at various depths. They hypothesized that when shocked, rock layers would behave in a fluid-like manner, and when the pressure lifted, the rocks would instantly freeze, and remain frozen in position:

> Therefore, as a result of impact and explosion, a series of concentric waves would go out in all directions, forming ring anticlines and synclines. These waves would be strongly damped by the overburden and by friction along joint, bedding, and fault planes. *The central zone, completely damped by tension factures produced by rebound, would become fixed as a structural dome.*
>
> The general and simplest type of structure to be expected beneath large meteorite craters would, therefore, be a central dome surrounded by a ring syncline and possibly other ring folds, the whole resembling a group of damped waves. (Boon and Albritton, 1936: 7; our italics).

Based on a similar interpretation of the impact process and its results, Boon and Albritton (1937: 57) drew the diagram shown in Figure 11 depicting the probable structure of a typical meteorite crater. The A-level in this diagram shows an impact site with an obvious crater of recent origin. The B-level represents an impact crater that has eroded to the point that it is barely discernable. The Level-C, however, shows the underlying strata of an impact structure becoming somewhat apparent as erosion continues. By the time that an impact structure has eroded to the D-level, the central uplift and ring folds have become conspicuous. This is the level that the Wells Creek structure has now reached. Over time, erosion will wear even this basement structure away and it will no longer be recognizable as an impact site.

Baldwin (1949: 101-103) notes that the Wells Creek structure clearly reveals the dominant pattern of a cryptovolcanic structure "… which arises from a sudden impulse, such as an explosion." He refers to the structure as having "… the appearance of damped waves …", with a central uplift that is "… surrounded by two pairs of up-and-down folds with diminishing amplitude …", and he notes that these damped waves appear to be nearly circular. Interestingly, Boon and Albritton (1936: 8) state that Bucher's assignment of Wells Creek to his list of cryptovolcanic structures was based on this very structure. But Boon and Albritton (1936: 9) conclude:

> It appears that some of the structures which have been assigned to volcanic origin are equally as well interpreted as meteorite structures. Certainly it can no longer be maintained that all explosion structures are necessarily volcanic. The meteorite hypothesis explains the occurrence of folds resembling damped waves, and evidences of violent explosion (breccias, shatter-cones, etc.) as well as does the cryptovolcanic hypothesis … It removes the embarrassing question as to the reason for lack of associated volcanic materials. Finally, it gives a tentative answer to astronomers who have long reasoned that large meteorites must have fallen [here on Earth] in the geologic past.

Giving further credence to the meteorite impact hypothesis Baldwin (1949:112) notes that in his 1941 study of the ordinary volcanic craters in Arizona, Hack "… was not able to find any deformation of the bedrock in the rims of the many volcanoes which he investigated." In addition, although the Wells Creek breccias were found to vary in texture, their mineral composition did not, and "… minerals generally considered indicative of elevated temperatures (e.g. calc-silicates such as wollastonite or diopside) are also apparently absent." (Stearns et al., 1968: 320).

Baldwin (1949: 108-110) notes that some researchers, while rejecting the idea of meteorite impact, still expressed reservations concerning a possible volcanic origin. The objections were based on "… the fact that no volcanic explosion of such a magnitude is known to have occurred anywhere on Earth." Yet the fact that the meteorite impact theory avoids this difficulty seemed to have made little difference in their opinions. Baldwin (1949: 112) concludes: "The meteorite-impact theory thus seems to fit the observed facts better than any other. It alone seems capable of supplying the vast amounts of energy which are needed to give the observed results."

Actually, it was D.M. Barringer's work (1905; 1914; 1924) concerning the impact origin of Meteor Crater in Arizona that played a key role in invalidating the old argument "… that there was no evidence that such [meteorite] impacts had ever occurred on the earth …" (see Hoyt, 1987: 184). W.H. Pickering (1920: 120), referring to terrestrial meteorite impact craters, asked "But why are there not more of them, or at least some evidence of their remains, since Earth is so much more massive than the Moon, has not been explained." By 1925, however, those promoting a volcanic explanation for lunar craters "… were clearly on the defensive." (Hoyt, 1987: 211). The tide had turned as was shown during the early part of the 1930s when the meteoritic origin of the Henbury cluster of craters in Australia was accepted almost immediately based on the criteria introduced by Barringer and other researchers at Arizona's Meteor Crater (see Hoyt, 1987: 246).

The origin of lunar craters also played a part in this evolving discussion. One of the key problems was to explain how meteorite impacts could result in crater formation, and this was addressed by the Estonian astronomer, E.J. Öpik, in a paper that was published in an obscure journal in 1916. He noted, based on the equation

$$E = 0.5mv^2 \quad (1)$$

(where E is the energy generated, m is the mass of the impactor and v is its impact velocity), that impacts on the lunar surface occurring at cosmic velocities would result in the release of huge amounts of energy and result in the formation of explosion craters that would be circular no matter the angle of incidence. Öpik (ibid.) also pointed out that only relatively small amounts of energy would be needed for the mechanical work of penetrating, shattering, and pulverizing target material before the explosion. Unfortunately, Öpik's paper remained unknown to most astronomers until it was publicized by Hoyt in his 1987 book.

The theme Öpik pursued was developed independently by New Zealand's A.C. Gifford (see Jenkinson, 1940) in a paper titled "The mountains on the Moon" that was published in the *New Zealand Journal of Science and Technology* in 1924. Gifford queried the volcanic explanation for the origin of lunar craters and the idea that the mechanical effects of impact could only result in a circular crater if the impactor's path was nearly vertical. He noted that most lunar craters are circular, yet only a small fraction of lunar impactors should have an incoming trajectory nearly perpendicular to the lunar surface. In supporting the 'meteoric hypothesis' Gifford argued that the circular lunar craters were the result of explosive impacts that transformed kinetic energy into thermal energy and thereby obliterated the pit just dug by the meteorite itself. Gifford later expanded on these ideas in a further paper, published in 1930. According to Hoyt (1987), Gifford later credited another New Zealand-based scientist, Professor A.W. Bickerton (see Burdon, 1956; Gilmore, 1982), with first suggesting this meteorite impact theory during discussions held at two successive meetings of the British Astronomical Association in London in 1915. Bickerton's original idea required supplementary volcanic action, but Gifford decided that impact alone was sufficient for explosive crater formation. Gifford's two papers appeared in a general scientific journal and, as in Öpik's case, they only reached a wide astronomical audience much later when they were discussed by Hoyt (1987).

Returning now to terrestrial impact craters, in 1959 Dietz suggested the term 'cryptoexplosion' to designate structures which were the apparent result of an explosive release of energy. Such structures are generally circular and show extensive folding, faulting, and brecciation of the target rock and are, in his opinion (which was definitely a minority opinion at the time), meteorite-impact scars. He continues:

> The writer prefers to call them "cryptoexplosion structures", since this term has less limited genetic implications … The term "cryptovolcanic" has tended to become a "wastebasket" term and now includes many structures which are unquestionably of volcanic origin. (Dietz, 1959: 496).

Dietz (1960: 1782; his italics) gives the definition of a cryptovolcanic structure as a "… deformation formed by a hidden explosion somehow considered to be related to volcanism although no direct evidence of this volcanism, such as volcanic rocks or hydrothermal alteration is found." He continues: "I prefer the term *cryptoexplosion structures* to *cryptovolcanic structures*, so as not to exclude the possibility of an extraterrestrial origin." He points out that the meteorite impact hypothesis, as originally developed by Boon and Albritton (1937; 1938), explains cryptoexplosion structures as explosion deformations produced by the explosive impact of crater-forming meteorites that are of asteroidal size (Dietz, 1959: 497).

Though a consensus was developing among researchers, the origin of impact structures was still being debated by some during the latter part of the twentieth century. Puryear (1968: 4) gives a description of the Wells Creek structure in his thesis and then concludes that it could be the result of volcanic explosion or meteorite impact. Miller (1974: 55) states that the most widely-accepted theory is that cryptoexplosion structures were created by comet or meteorite impact, but adds that many researchers still favor volcanic explosion as the cause, believing that "… upward moving steam drove the rocks outward …" to form the structure. Others disagreed. Sawatzky (1977: 462-463) included Wells Creek in his list of confirmed meteorite impact sites. But as late as 1991 a staff geologist at the Tennessee Division of Geology in referring to the Wells Creek structure stated: "The origin of this crater and similar features is still under debate …" (Price, 1991: 24). Even though no volcanic material had ever been found in the Wells Creek area, to his way of thinking the idea of a volcanic steam explosion was still considered plausible.

7 IMPACT MECHANICS

Barringer's original argument concerning the impact origin of Meteor Crater was made in 1906. He thought that the iron impactor was buried in the crater and planned to mine the metal. In 1911, M.E. Mulder also proposed impact by a meteorite, but with the interesting suggestion that meteorites could well explode just after impact and "… very little if any of the original meteoritic mass would remain in the crater itself, a circumstance which … Barringer and his associates might well consider." (cited by Hoyt, 1987: 192).

Many researchers have searched for some form of igneous rock or remnant of meteoritic material at the Wells Creek site in order to understand its origin.

Wilson (1953: 755) writes concerning his own research: "The writer studied the stratigraphy of the [Wells Creek] area for the [Tennessee] Division of Geology in 1940. About the same time he made a magnetic map of the region surrounding Wells Creek Basin. This map showed no magnetic anomaly associated with the structure." Some fifteen years later, Wilson and Stearns (1968: 7) noted that a "Lack of magnetic anomaly at the center is consistent with a lack of volcanic material and absence of a buried meteorite at depth, and with the idea that the basement is not uplifted beneath the structure." If this structure is indeed the result of a meteorite impact, then why is there a complete lack of meteoritic material on site or mixed in the breccia?

Boon and Albritton (1937: 54) point out that:

> It is difficult to comprehend the tremendous pressures which would be produced in the brief interval between impact and explosion of a large meteorite … these unprecedented pressures should be kept in mind, for they bring about the terrific explosions, the excavation of the craters, and the backfiring and shattering of the meteorites.

Dietz (1960: 1781) adds that "… meteorites have never been found in ancient rock, and this suggests that such fragments as are preserved from volitization during a hypervelocity impact weather rapidly." Miller describes a possible scenario in which the Wells Creek impactor would have penetrated to a depth of over 600 meters with the subsequent explosion resulting in a transient crater around 6.5 km across and 0.8 km deep. He also points out that "… a meteor presumably might be totally vaporized from the great heat involved in the impact." (Miller, 1974: 55). Dietz (1959: 498) says that "… it is physically naïve to expect the preservation of such a body; in fact, the preservation of any meteoritic fragments in ancient impact scars seems unlikely."

The shock wave resulting from meteorite impact will not only melt and vaporize target rock; it will impart a particle velocity to the shocked material. Velocities are radial in direction during compression, but then are deflected outwards and upwards by rarefaction wave interaction. These deflected particle motions are responsible for transient cavity growth during the excavation stage of an impact event (Grieve et al., 1977). As crater development moves from the excavation stage to the modification stage, the transient cavity rapidly readjusts to produce the final impact crater form. With increasing cavity size, collapse of the transient cavity leads to the formation of complex craters, such as Wells Creek, where the outer edge of the transient cavity rim has dropped down to form distinct annular grabens and the center of the cavity floor has experienced uplift (ibid.).

8 SHATTER CONES

One of the most important developments in the study of impact structures during the 1960s "… was the recognition of unique and geologically durable petrographic and mineralogical effects that could be used to unambiguously identify geologically old impact structures …" (French, 2004: 171). During impact, shock levels encountered in the rocks forming the central uplift of a complex structure such as Wells Creek cause the formation of characteristic microscopic planar deformation features in quartz and feldspars (Robertson and Grieve, 1977). Therefore, rather than requiring the discovery of associated meteoritic material to confirm an impact origin, shatter cones and planar deformation features [PDFs] in quartz became accepted as proof of impact since PDFs "… are uniquely produced by high shock pressures and their occurrence is restricted in nature to meteorite impact sites …" and shatter cones were found to be associated with PDFs in quartz (French, 2004: 171). In addition, the high-pressure polymorphs of quartz, coesite and stishovite, found in impact structures were shown to require pressures so high that only meteorite impact could account for their formation (ibid.). Coesite and stishovite "… have not been found in any natural environment that is clearly *not* related to a meteor impact." (Wilson and Stearns, 1968: 152).[1]

Wilson and Stearns (1968) found no evidence of coesite or stishovite in Wells Creek petrographic studies, though they note that the zone in which shock pressures were great enough to develop these minerals could have been removed by erosion. The most severe deformation Wilson and Stearns (1968: 153) noted in Wells Creek quartz was "… somewhat widely spaced fracturing …" They also state that the "… most pronounced evidence for severe deformation is distortion and fracturing and undulatory extinction in carbonate crystals …" which was observed in the Knox Dolomite and in calcite in the breccia (ibid.). Calcite crystals in the breccia were observed to be broken into platy fragments and Wilson and Stearns (ibid.) found that "Twinning is prominent in the calcite of this breccia but not in the dolomite of the central block …"

Shatter cones, however, are abundant in rocks of the Wells Creek central uplift. Shatter cones (see Figure 12) are distinctive fan-shaped features in rock with radiating fracture lines (see Sagy et al., 2004). They are not found in normal geological situations, and they do not seem to be formed by tectonic stresses, static loading or volcanic activity. Military explosives with high detonation velocity and shattering effect do form cones with surface marking similar to shatter cones, "… but not so perfectly formed as shatter cones …" (Baldwin, 1963: 75), while dynamite can produce "… rude cones … [but these] lack the surface markings of shatter cones." (Baldwin, 1963: 75).

Dietz (1960: 1781) explains that a primary effect of a meteorite impact and the resulting explosion is the generation of a high-velocity shock wave which spreads out from the point of impact and engulfs a large volume of rock before decaying into an elastic wave. He continues:

> Volcanic explosions are steam explosions involving not more than several hundred atmospheres, so it is extremely doubtful that a shock wave can be developed in rock as part of volcanic phenomena … It would seem, then, that if one can produce evidence that a large volume of rock has been intensely and naturally shocked, this would constitute definitive evidence of a meteorite impact. Fortunately, at least under favorable conditions, rocks when shocked appear to fracture into a curious pattern, forming shatter cones which are preserved and may be readily identified in the field.

According to Wilson and Stearns (1968: 108), shatter cones were first located in the United States by Bucher in the Wells Creek Basin. Back in 1959, Dietz wrote that "Shatter cones (striated percussion fracture cones), apparently formed by explosive percussion, are known only from four cryptoexplosion (i.e. "cryptovolcanic") structures, viz., Steinheim Basin, Wells Creek Basin, the Kentland deformation, and the Crooked Creek structure." (page 496). As early as 1946, Dietz had proposed shatter cones as a criterion for the identification of terrestrial meteorite craters, "… in the course of suggesting that cryptovolcanic, or cryptoexplosion structures were possibly related to craters on the moon." (Hoyt, 1987: 356). In fact, by the late 1950s, Dietz was convinced that they provided a definitive criterion for impact identification as a result of his successful search for shatter cones at other cryptoexplosion sites (see Mark, 1987). Shatter cones are now considered to be unambiguous shock features associated with meteorite impacts and are, in fact, the only shock indicators that can be seen with the unaided eye.

Dietz collected several compression fracture cones that were produced by high explosive detonation in a Nashville (Tennessee) limestone quarry and compareed one of these with a Wells Creek shatter cone, noting that the compression cone "… lacks striations, and is crude and irregular in form." (Dietz, 1959: 498). He also noted (Dietz, 1959: 500) that shatter cones are not found in rock that has been subject to known volcanic explosion. Explosions due to the expansion of compressed gases and steam, in his opinion, were not violent enough to produce an intense shock wave in the upper rock layers. Dietz (1963: 661) believes shatter cones are usually limited to the intensely-deformed center of cryptoexplosion structures, such as Central Hill in the Wells Creek structure, whereas the outer rings show only heaving, suggesting rapid decay of shock waves. Dietz (1960: 1782) adds that shatter cones have only been found in the USA in the central sections of structures that were identified as cryptovolcanic in the 1940 edition of the *Structural Map of the United States*. He also states that shatter cones have never been reported resulting from any other natural geological situation.

Mark (1987: 124) notes that "… as of 1959, they [shatter cones] were known only in ... three locations in the United States …", including the Wells Creek basin, and that these shatter cones are found in dolomite and show "… uniform orientation. The cones are interlaced, and new fractures of the rock reveal new shatter cones." Figure 12 shows

shatter cones found in the central uplift of Wells Creek, which is known for its fine, easily-located, and pro-fuse shatter cones. Perhaps this is due to the fact that the Wells Creek central uplift is composed of Knox Dolomite. Dietz (1960: 1781) indicates that shatter cones are usually found in carbonate rocks, but they have also been identified in shale and chert; he concludes: "Presumably, a fine-grained homogeneous rock like dolomite favors their development, but it is not an absolute requirement."

Figure 12: Wells Creek shatter cones (photograph by Andrew Tischler).

Dietz (1960: 1784) points out that in addition to indicating a meteorite impact, shatter cones provide an additional clue as to the origin of impact structures. The initial impulse delivered by a meteorite is carried into the target rock by stress waves, and so the shatter cones usually "… point toward the locus of pulse source." Dietz (1963: 661; his italics) states:

> I retain the conviction that shatter cones are truly indicative of intense transient shock loading, far in excess of any known volcanic forces. Their concentration in the bulls-eye of cryptoexplosion structures indicates a highly localized ground zero. And when the preferred orientation of the cones can be worked out, the apices *do* point toward the direction of oncoming shock wave. When definitely recognized, they seem to me a valid criterion for intense shock such as can be derived only from a cosmic impact.

Milton (1977: 704) also considers shatter cones to be a diagnostic feature of impact structures and states that "Shatter cones form during the compression stage, as is indicated by the occurrence of broken cones in breccia and also by the orientation of cones in place in the crater floor and central uplift …" Shatter cones were found in the Knox group rocks exposed in the Wells Creek central uplift and the orientation of these shatter cones indicates a point of explosion at about 610 meters below the surface at the time of the event, which strengthens the meteor impact theory (see Miller 1974).

In 1956, Gilvarry and Hill published a monograph on meteorite impacts which estimated pressures and temperatures during the early stage of an impact event. They stated that

> … the explosive pressures and temperatures are created in a time of the order of that required for the impinging mass to traverse a distance equal to its diameter. Hence the effective center of the explosion must lie within a depth below the impact surface of the order of a linear dimension of the impinging mass. (Gilvarry and Hill, 1956: 620).

Stearns et al. (1968: 335) note that "The Wells Creek structure has, at its center, a remarkable development of shatter cones …" on Central Hill. Wilson and Stearns (1968: 108) note that in the Wells Creek structure, "… all known shatter cones are in the Knox Dolomite." They continue by noting that "Shatter-cone orientation data support the interpretation of a meteorite penetrating from an ancient surface to such a depth that shock waves emanated mainly from near the top of the Knox Dolomite (a position at least 2,000 feet [610 m] underground at the time." (Wilson and Stearns, 1968: 130). Milton (1977: 704-706) continues:

> As measured, orientations show little pattern, but at those craters that formed in horizontal strata, displacements during the excavation stage can be determined and if shatter-coned outcrops are restored to their pre-impact position, cone axes point inward and upward toward a point near the original ground surface at the center of the structure. This is striking evidence for, beyond the basic hypothesis that cryptoexplosion structures are caused by impact, the formation of shatter cones during the compression stage with their axes normal to an advancing hemispherical shock front.

Shock wave reflections can explain multidirectional cones. Instead of experiencing a simple spherical spread, a shock wave would be expected to reflect from interfaces and discontinuities resulting in a complex shatter cone orientation. Dietz (1959: 501) writes that "According to J. Rinehart (personal communication), who has experimented extensively with shaped charges and high-speed impact phenomena, minor inhomogeneities, such as bedding planes and especially the bottoms of strata, can strongly reflect shock waves." Shatter cones formed by meteorite impact might tend to have a preferred orientation toward the explosion, but cones with their axes pointed in other directions are likely to occur as well. If an impact explosion-induced shock wave encounters a pebble, the pebble will in turn act as a secondary shock wave source forming a shatter cone, and this cone will point toward the oncoming shock wave. Cones pointing in other directions can be explained by reflection. Dietz (1963: 658-661) also states "I cannot agree with Bucher's interpretation that shatter cones pointing upward are explainable by a cryptovolcanic pulse coming from below."

Wilson (1963: 767) reports that he found shatter cones after studying a 610-m core drilled near the center of the Wells Creek structure, and states that he found three features that were especially significant:

(1) Deformation was instantaneous, and did not result from normal tectonic forces;
(2) Progressive downward dying out of deformation may be traced, in spite of the brecciation between 1743 and 1930 feet [530 and 590 meters];
(3) In the top 200 feet [60 m] of the core, the shatter cones are all horizontal, except for some that point obliquely upward.

He found horizontal shatter cones to be concentrated at a depth of 30 meters, and the few shatter cones found below 60 meters were not complete or well defined, except for a single exception located at a depth of 377 meters. He noted that "As the core was not oriented, it is impossible to state in which direction these cones pointed." (Wilson, 1963: 767). Some 200 meters to the south of this location, horizontal shatter cones were also located in an exposure. Wilson believes that these shatter cones "… were not formed by the impact of the meteorite, as such should be normal to the bedding and oriented stratigraphically up, but rather by the explosion of the rocks compressed beneath the penetrating meteorite." He also points out that this block was most likely moved from its original position when the meteorite impacted and penetrated the surface rocks just before the explosion. He concluded (ibid.) that these features "… present definite evidence that the deformative force came from above and not from below." After their formation, some shatter cones at the Wells Creek site were cut by faults and fault breccias, indicating that the target rock layers were displaced after the formation of shatter cones (Milam and Deane, 2005).

Although numerous shatter cones were found in the drilled core at Wells Creek, this did not reveal the presence of an igneous core. The fact that this core indicated that the structure appeared to die out with increasing depth emphasized its non-volcanic origin. Studies of impact structures show that, unlike volcanoes, there is a lower limit to the depth below the Earth's surface of disrupted rocks, indicating that the cause of the disturbance was not endogenic.

9 BILATERAL SYMMETRY

Both the cryptovolcanic and meteoritic hypotheses could explain the formation of the structures in question as the result of tremendous explosions. In the cryptovolcanic case, an explosive release of subterranean gases is considered to be the cause, while in the other case the explosion results from the impact of a massive high-velocity meteorite. Both of these could explain the existence of circular structures with central domes, surrounded by ring folds. Both could also explain the observed brecciation and faulting. However, Boon and Albritton (1937: 57) state that

> … the meteoritic hypothesis can account for two features which are unsatisfactorily explained by the alternate mechanism. These are (1) the distinctly bilateral structural symmetry found in several American examples, such as Wells Creek … and (2) the absence of volcanic materials and signs of thermal activity. It is more difficult to explain how an upwardly directed explosion alone could produce a bilaterally symmetrical structure … than it is to see how an obliquely impinging meteorite could produce a radially symmetrical structure.

In fact, Boon and Albritton (1936) regard bilateral symmetry as a basic criterion for the identification of an impact structure.

Baldwin (1949: 101) observes that Wells Creek "… exhibits a distinct bilateral symmetry." Safford and Lander also comment on this: "The fault circles are longer North and South than East and West, the direction of the long diameter being about N.N.E. and S.S.W." (see Wilson and Stearns, 1966: 38). Baldwin (1949) states that the proportion of those cryptovolcanic structures that show bilateral rather than radial symmetry is what should be expected if the structures were actually the result of meteorite impact, since the majority of impactors would come from some non-vertical angle. He continues: "Although the resultant surface craters probably would be very similar to those formed by impacts of bodies falling perpendicularly, the subjacent rocks would indicate both the fact that an angular fall had occurred and its direction." (Baldwin, 1949: 110).

Wilson and Stearns (1968: 5) also note this north-northeast axis of bilateral symmetry in the basically circular and symmetrical Wells Creek structure which "… is manifested by the linear occurrence of several structural features along this line and by the 'enantiomorphic pairings' of other structural features in reference to this line." Gravity patterns also show this bilateral symmetry which Wilson and Stearns (ibid.) believe to be related to trends of pre-existing joints and controlled by the north-northeast joint set.

10 THE WELLS CREEK 'SATELLITE CRATERS'

Meteoroids often break up as they travel through the Earth's atmosphere (see Baldwin and Sheaffer, 1971; Melosh, 1989; Pierazzo and Artemieva, 2005). Usually, only iron or tough stony-iron meteorites survive the aerodynamic atmospheric stresses and reach the Earth's surface intact without first breaking up. If a meteorite disintegrates in the Earth's atmosphere, the resulting cluster of separate fragments will continue to fall forming an elliptical strewn field or crater field upon impact, as illustrated in Figure 13. In these fields, the smaller fragments fall short of the larger ones due to air drag, causing the largest craters to be at the far end of the impact ellipse, as is shown in the Henbury and Odessa schematic maps. Note that some of the larger Henbury craters overlap.

In their discussion of the Wells Creek structural data, Wilson and Stearns (1968: 88) include the following interesting comments:

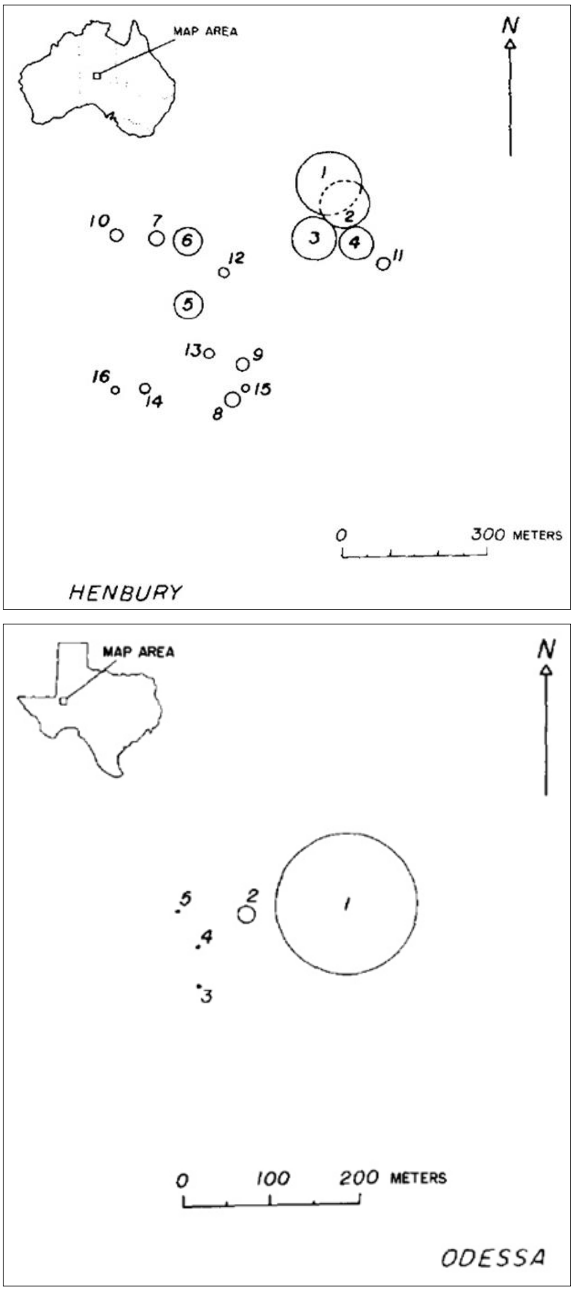

Figure 13: The Henbury, Australia (upper) and Odessa, Texas (lower) crater fields (after Passey and Melosh, 1980: 214, 217).

> If a line is projected north-northeastward from the center of the Wells Creek structure along the symmetry axis, it intersects the Indian Mound craters (6 miles [9.7 kilometers] north-northeast of the edge of the Wells Creek structure). These features have been interpreted as subsidiary meteor impact scars by Wilson (1953), and therefore their relationship to the Wells Creek structure is genetically significant.

Referring to Wells Creek, O'Connell (1965: 126) states that there are actually five different craters (cf.

Hey, 1966), and he includes their depths and diameters drawn from data included in Wilson (1953). Table 1 is based on this information, but note that Wilson (ibid.) stresses that the figures listed in the third column are minima. Figure 14 shows the locations of these deposit-filled satellite craters with respect to the main Wells Creek structure (after Wilson, 1953: 754). Note their alignment with the north-northeast axis of symmetry of the main structure.

Table 1: Wells Creek Basin, Tennessee, and its satellite craters (after O'Connell, 1965: 126).

Feature	Diameter	Depth
Wells Creek Basin	2 × 3 miles (3.2 × 4.8 km)	---
Little Elk Creek Deposit	---	---
Cave Spring Hollow	1 mi (1.6 km)	---
Indian Mound	2000 ft (610 m)	>263 ft (70 m)
Austin	375 ft (115 m)	>40 ft (12 m)

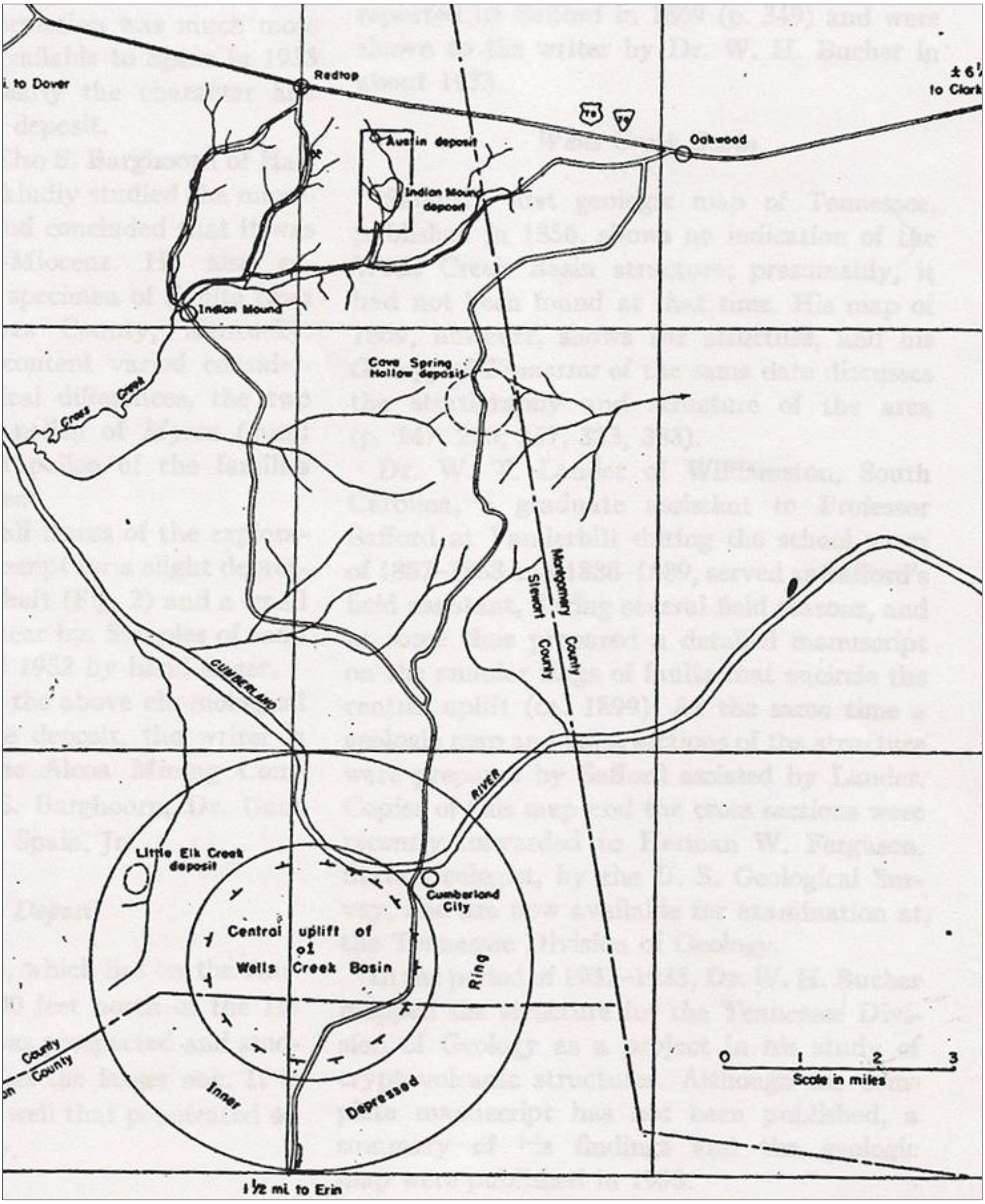

Figure 14: Map showing the locations of the Wells Creek Structure and the Little Elk Creek, Cave Spring Hollow, Indian Mound and Austin 'satellite craters' (after Wilson, 1953: 754).

Comparing the diameters given in O'Connell's table above with Wilson and Stearn's map shown in Figure 14, it is obvious that these craters show decreasing diameter with increasing distance from the main impact crater.

Wilson (1953) continues his discussion, noting that the four basins are all oriented along basically the same line within a relatively small distance, and that they contain similar sediments, in fact the only such deposits known in the Western Highland Rim. Wilson (1953: 753) describes these small craters as follows:

> Four small deposits of Wilcox sediments occur in Stewart County, Tennessee. One of these deposits is in the inner depressed ring, or crater, of the Wells Creek Basin structure. It is concluded that these four craters had a common post-Eutaw, pre-Wilcox age and common origin by impact and resulting explosions of fragments of a meteor.

What originally was the largest of these satellite craters is Little Elk Creek, which is located on the inner depressed ring of the Wells Creek structure that contains the central hill or uplift. Eight kilometers north-northeast of its northern rim is the much smaller Cave Spring Hollow basin, the extent of which is unknown. Almost five kilometers farther north is the Indian Mound basin, at least 610 meters in diameter and greater than 80 meters in depth, but with a central hill rising above the level of the floor of the basin (Baldwin, 1963). Classen (1977) lists the largest of the Odessa craters as having a diameter of 168 m. This gives the Indian Mound basin a diameter almost four times that of the largest of the Odessa craters. Around 520 meters farther north is the very small Austin basin, over 12 meters deep. Wilson (1953: 764) states that

> It seems logical that the four basins, or craters, had a similar origin at the same time. That origin would have been related to the phenomenon that formed the Wells Creek Basin structure."

Wilson (1953) believes that the Little Elk Creek deposit resulted from the explosion that formed the Wells Creek structure. He notes that several small deposits are exposed in a tributary of Little Elk Creek, and that these were first reported by Safford (see Safford, 1869: 349). Bucher showed Wilson these deposits around 1933.

The Indian Mound satellite crater was first investigated around 1930 when the first drilling and opening of shafts in this area occurred, as a result of Dr Gant Gaither's interest in the deposit (see Wilson, 1953). A Master's thesis for Vanderbilt University concerning the deposit was completed by Ernest Spain in 1933, but "… the findings of the preliminary exploration … were insufficient to reveal the full significance of the unique deposit." (Wilson, 1953: 754). The area was prospected in more detail during 1934 by the Alcoa Mining Company, and although the information obtained was not released for publication until 1948 it showed more clearly the characteristics and surprising thickness of the deposits (Wilson, 1953). Wilson (1953: 761) provides the following description of Indian Mound: "It is shaped like a doughnut with the central hill of chert occupying the 'hole' of the doughnut."

This central hill is puzzling since the diameter of Indian Mound is ~610 meters, and central uplifts are characteristic of complex craters which have diameters ≥2 km. Indian Mound has a diameter that is within the range of a simple crater and so should be bowl-shaped if it is the result of a meteorite impact. However, Wilson (1953: 764) states that

> No evidence of uplift was found, unless the loose blocks of Warsaw chert in the central area of residual chert are higher than their normal position. If the blocks are from the lower part of the Warsaw, then uplift of over 100 feet [30 meters] is possible.

An explanation may be found in the idea that

> … large simple craters often possess low central or near-central mounds … [which are] probably the result of the convergence and pileup of high-speed debris streams sliding down the walls and onto the crater floor. (Melosh 1989: 136).

The Cave Spring Hollow satellite crater is located 7.2 kilometers south-southeast of Indian Mound (Wilson, 1953). The deposit was prospected around the same time as Indian Mound, however "The indefinite limits of this deposit are based on local reports of where the drilling was concentrated." (Wilson, 1953: 755).

The Austin satellite crater is about 520 meters north of the Indian Mound deposit and although it was also studied and prospected at the same time, just one well was drilled, and this only went down 12 meters (ibid.). Wilson (1953: 764) notes that

> No structural disturbance was noted in the Austin and Cave Spring Hollow deposits, but again the bedrock is chert rubble yielding no information as to its structure.

According to Wilson (1953: 756) the Cave Spring Hollow deposit is just over 180 meters above sea level and the Indian Mound and Austin deposits are at an altitude of between 140 to 165 meters. He adds that "These deposits of clay do not affect the topography in any way, nor do they show up in the aerial photographs." (Wilson, 1953: 758). The rectangular area in the upper part of the Figure 14 map, which includes Indian Mound and Austin, is enlarged in the geological map shown in Figure 15.

Wilson summarizes the Wells Creek structure as follows. Around the central uplift the beds dip away from the center as expected, except for the Ross and Decatur formations which dip steeply southward toward the center of uplift for some 305 meters along the northern boundary of the structure. This asymmetry when superimposed upon the otherwise circular structure was also noted by Bucher and by Boon and Albritton. Lander and Safford also recognized this bilateral asymmetry. In fact, Lander's 1887-1889 manuscript included a sketch with the line of asymmetry plotted with a strike of N. 25°E. This axis, along with the southward-dipping Ross and Decatur formations on the northern side, point unerringly to the Indian Mound crater. Wilson (1953: 764) believes that

> … only two known forces could account for the origin of Indian Mound crater; (1) a local, abnormally deep sink hole; (2) the depression ring of an explosion crater. It seems to the writer that the sink hole can be eliminated when … it must have been cut: (1) 130 feet [40 meters] below the present level of bedrock in Cumberland River valley, and (2) through at least 200

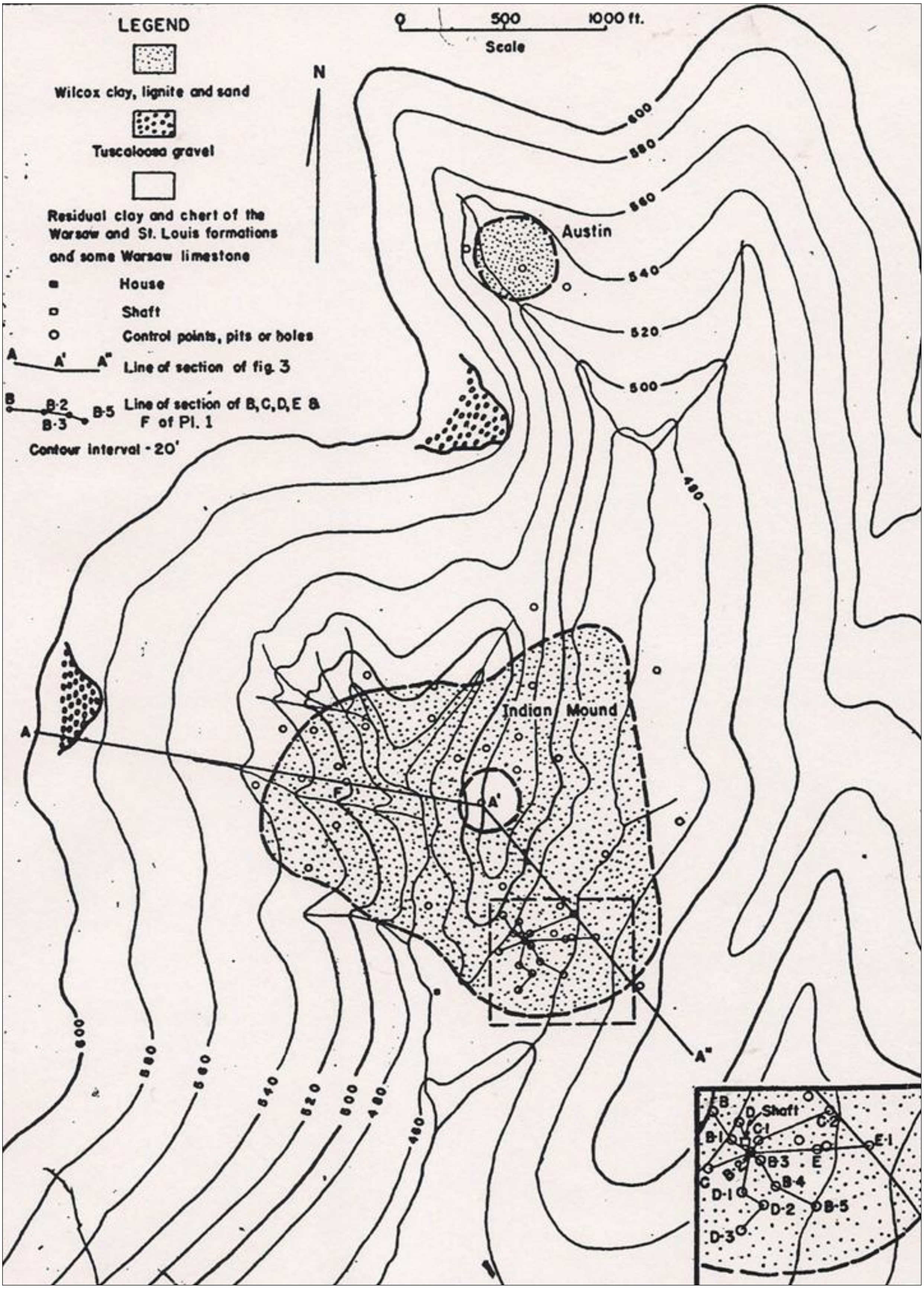

Figure 15: Geological map showing the presumed areal extent of the Indian Mound and Austin structures, based on shafts, pits and holes. The inset shows in detail the investigation of the southeastern section of the Indian Mound site (after Wilson, 1953: 759).

feet [60 meters] of Fort Payne and Ridgetop beds. These relatively insoluble beds are underlain by the Chattanooga shale and about 50 feet [15 meters] of Devonian Harriman chert, a sequence that would have prohibited, or made improbable, the cutting of such a deep sink hole … Austin and Cave Spring Hollow cra-

> ters represent small meteoritic pits, or craters ...
>
> It is concluded that a swarm of meteors approached the earth's surface from the south, or a single meteor fragmented into at least four pieces before striking the surface. The largest fragment struck at the present position of Wells Creek Basin, and the second in size struck at the Indian Mound locality. Smaller fragments ploughed into the earth to form the Austin and Cave Spring Hollow craters.

The son of D.M. Barringer recognized several small craters at Odessa, Texas, in 1922 (e.g. see Figure 13, lower map), that were associated with iron meteorites (see Barringer, 1967). Baldwin (1963: 19) describes the formation of the Odessa group of craters by a nickel-iron meteorite as follows: "Accompanying the main body were at least four smaller companions. They also struck, exploded, or partially exploded and formed lesser craters." (cf. Holliday et al., 2005). In addition to the main crater, Crater No. 2 is nearby, and

> Three other craters, much like No. 2 but smaller, have also been identified … many of the other recently discovered meteoritic craters occur in bunches … Usually there is one rather large crater and numerous smaller pits. (Baldwin, 1963: 21).

The similarity of this description to the structures found at Wells Creek is striking.

However, due to their distances from the Wells Creek structure, one has to query whether Cave Spring Hollow, Indian Mound and Austin can be explained as secondary craters produced by fragments from the explosive impact of a single large meteorite. Wilson's statement that the supposed approach of the fragmenting meteoroid was from the south is also puzzling, as the smaller fragments tend to fall first, yet the main impact site is to the south of Indian Mound. Nonetheless, Wilson (1953: 768) concludes that the

> … evidence combined with the occurrence of four aligned craters, of which the Indian Mound crater has critical depth and cross section, and the southward dip of the Ross and Decatur limestones on the north periphery of the uplift of Wells Creek Basin all harmonize to tell the same story of meteoritic origin.

Considering Indian Mound's critical depth and cross section, it is unfortunate that the depth of the Cave Spring Hollow deposit was not determined. Its larger diameter, 1.6 km compared to Indian Mound's 610 m, could indicate that its depth could be even greater than the 70-80 m determined for Indian Mound, making it a third structure in the Wells Creek group with critical depth and cross section.

McCall, however, has reservations regarding Wilson's conclusions. He refers to Wilson's paper when stating that

> Wilson (1953) believed that the deformation came from above and was produced by a group of objects approaching from the south. He believed that the five structures were more or less contemporary. (McCall, 1979: 279-280).

Then McCall (1979: 279-281) gives his own opinion:

> Wilson (1953) mentions also three small craters to the north and one inside the main structure. Of these satellite craters, Indian Mound is 80 m deep and contains a central knoll 650 m in diameter; Cave Springs Hollow is 1.6 km in diameter; and Austin is 120 m in diameter and 12 m deep. Little Elk, in the northwest quadrant of the main basin is reported to be 500 m in diameter …
>
> However, the alternative, that the craters are not contemporary with the main structure, seemed only compatible with endogenic theory, unless there was a remarkable overlap of impacts. If the Little Elk structure is a crater, it would represent a major problem in terms of impact theory for it is clearly absurd to suppose that a small contemporaneous crater could be superimposed in a deeply eroded structure such as the Wells Creek Basin … If these [craters] are related to the [Wells Creek] structure, it is difficult because of their smaller size, to reconcile them with a contemporaneous larger explosion 2500 ft [760 m] below the existing land surface, for much smaller scale impacts such as those would have fragmented at no significant depth and the traces of their impact would have been obliterated by erosion. It is probable that the Little Elk crater does not exist, but the others certainly do. They are either fortuitously related to the main basin, or must be explained in any hypothesis of the Wells Creek origin. (ibid.).

In contrast to McCall's view, Wilson (1953: 765) was of the opinion that "A fourth craterlet, the Little Elk Creek depression, lies within the Wells Creek Basin …" and that it was produced by a smaller meteoritic fragment that trailed behind and fell inside the main crater. It is worth noting that according to Bucher (1963b), similar small craters exist on the floor of the Ries Basin, a proven impact crater in Bavaria, Germany (Shoemaker and Chao, 1961).

In reference to the north-northeast axis of bilateral symmetry, it must also be pointed out that Wilson and Stearns (1968: 5) state that "A structure map drawn by projecting contours across the structure shows that the regional north-south trending highs and lows continued across the area before the [Wells Creek] structure was formed." This may be the cause of the structure's bilateral symmetry rather than the meteorite's direction of approach.

Bucher presents his own ideas. He believes Wells Creek to be aligned with the Hicks Dome and the Avon area, both of which he considers to be volcanic in origin. Hicks Dome is located some 145 km NNW of Wells Creek and the Avon Area is around 255 km NW of Wells Creek. Bucher (1963b, 626) notes that "… the Hicks Dome with its explosion breccia pipes … [is located] along the same, now curving, belt … [as] the Avon area of 78 volcanic breccia pipes ..." Bucher (1963a: 1243) also states that:

> About 145 km (90 miles) to the south-south-east of the Hicks Dome, three diminutive craterlets filled with Cretaceous sediments trend north-northwestward a short distance beyond the Wells Creek Basin, that is, essentially in the same direction as the basic dikes farther north, and, more important, in the direction of the anticlinal flexure zone. Dr. Wilson, who described them, called them impact craters, caused by small meteorite fragments running ahead of the master meteorite … it is assumed that a giant and baby meteorites hit the ground in line with the axis of an independent major flexure zone.
>
> About 168 km (105 miles) west-north-west of the Hicks Dome lies the Avon area …
>
> Here then, of three structures lying on a major flexure zone (of purely terrestrial origin), one is supposed to

be the product of meteorite impact, while the other two are undoubtedly volcanic in origin.

I cannot accept a hypothesis which holds that … multiple meteorites … struck a clearly defined terrestrial flexure zone so that their impact scars are aligned parallel to its axis and with structures of proved volcanic origin.

Dietz (1963: 654-655) responds to Bucher's objecttions:

The Wells Creek disturbance … makes a useful "syntype" for the United States … Bucher argues that the Wells Creek basin must be terrestrial in origin because of its regional associations. To me, this seems to be only a possibility rather than a probability. It is difficult to lay down any point upon the tectonic map of the United States without finding associated regional trends, etc. If we consider all of the cryptoexplosion structures, they seem to be randomly disposed …

In his description of Wells Creek, Baldwin states that the Wells Creek Basin structure is not alone and that during the post-Eutaw-pre-Wilcox (Cretaceous) interval, at least four basins were located in the region, the largest one being what we now know as the Wells Creek structure. He also concludes that the four basins were all formed by the Wells Creek event. Baldwin (1963: 92) concludes that this is a group of four associated meteorite impact structures around 100,000,000 years old. He also takes note of the fact that the rock layers along the structure's northern boundary dip southward toward the center, which is "… consistent with the idea that the meteorites approached from the south …", while the resulting axis of asymmetry "… points unerringly toward the Indian Mound Crater." (Baldwin, 1963: 89).

Finally, in their 1968 interpretation of the origin of the Wells Creek structure Wilson and Stearns dispute Baldwin's conclusion that the disintegrating meteoroid approached from the south. They note that the direction of approach of the impactor can be derived from the positioning of the shatter cones, and that these are found in greater abundance in the southern part of the Knox Dolomite. From this they conclude that the meteoroid came in from the north-northeast, resulting in a greater compression of this section of the impact site and causing more shatter cone development. They also suggest that

Perhaps lesser accompanying meteors were slowed sufficiently by the atmosphere that they fell more vertically and behind the main meteor to form the Indian Mound craters. (Wilson and Stearns, 1968: 177).

Unfortunately, the precise origin of these supposeed 'satellite craters' may never be determined as Wilson and Stearns noted in 1968 (page 166) that they "… unfortunately [are] now largely concealed …", although these authors do not reveal whether by erosion, deposition, pasture, human activity or some combination of these. Fortunately, the conclusion as to the origin of the main Wells Creek structure is much clearer.

11 CONCLUDING REMARKS

The Wells Creek structure was discovered in the late 1800s when a railway line was constructed from Tennessee to Kentucky and passed through the Wells Creek Basin. The first professional investigators simply described the structure's features, and did not include any suggestions about its origin in their manuscripts or field notes. Discussions during the 1930s concerning the structure's origin led to two strongly-opposing views: that it was either crypto-volcanic or cryptoexplosive (and therefore resulted from a meteorite impact). Detailed studies of the structure were completed during the 1960s in preparation for the first lunar landings. Our Moon is covered with craters, and NASA wanted to learn whether lunar craters were related in any way to these terrestrial structures. The primary investigators, Wilson and Stearns, came to prefer the meteorite impact hypothesis to explain the origin of the Wells Creek structure.

Evidence for a Wells Creek impact event includes: drill core results; extreme brecciation; and shatter cones oriented to indicate explosive force from above; while the lack of local volcanic material is telling. The fact that the shatter cones preferentially point to a location that would have been over 600 meters underground at the time of the structure's formation adds credence to the meteorite impact hypothesis. A volcanic origin would not have left space for rock to move inwards toward the center of the structure nor are volcanic pressures sufficient for shatter cone formation. The fact that meteoritic material has not been found is no longer seen as an issue given the fact that any fragments that could have survived the explosive event would have eroded away long ago.

The Wells Creek impact site is now recognised as the 'syntype' cryptoexplosion structure for the United States. Early investigators recognized that it revealed more clearly than most other structures the pattern of impact, presenting the appearance of damped waves and a conspicuous central uplift.

Dietz (1963: 663), an early advocate of the meteorite impact theory, has stated that "Astrogeology is a subject which must concern the earth, as well as the moon …", but we must now add the terrestrial planets, some of their moons, asteroids and cometary nucleii to this 'portfolio'. Over the passage of more than a century, Tennessee's Wells Creek structure has been a source of controversy and of knowledge as researchers slowly came to recognize that we do not live on a planet which is isolated from the rest of our chaotic Solar System (see Koeberl, 2009). In the opinion of at least one noted meteoriticist, "… future historians will accord the recognition of [terrestrial] impact cratering an equal importance with the development of plate tectonics." (Melosh, 1989: v).

12 NOTES

1. Apart from the presence of shatter cones, veins of pseudotachylyte containing coesite and/or stishovite (Dressler and Reimold, 2001) and planar deformation features (PDFs), undisputable proof of meteoritic impact is also afforded by planar fractures (PFs), crystallographic configurations of feldspars (Shoemaker, 1983) and by basal Brazil twinning and alteration in zircons (Kamo, Reimold, Krogh, and Colliston, 1996). Note, however, that some of these 'indicators' were unknown when Wilson and Stearns conducted their research at Wells Creek site.

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ON THE ORIGIN OF THE NAME OF THE MINOR PLANET (1441) BOLYAI

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Abstract: The nineteenth century mathematician János Bolyai was a founder of non-Euclidean geometry, and a minor planet discoverer wanted to honor him by naming an asteroid after him in 1939. However, most later sources give a mistaken justification for the origin of the name of minor planet (1441) Bolyai, claiming that it was named after his father, Farkas Bolyai. In this short paper we present a copy of the original naming of this minor planet after János Bolyai, and we explain why later scholars continued to erroneously associate it with Farkas Bolyai.

Keywords: minor planet names, (1441) Bolyai, János Bolyai, György Kulin

1 INTRODUCTION

Minor planet names reveal how the people who name them relate to our society and our world. These names identify not only the subjects of many scientific investigations, thus distinguishing one minor planet from another, but they also express many different things: our wish to honor different people; the discoverer's links to arts, sciences, nations and people; or the geographical distributions of the discoverers (because they like to name asteroids after their home towns, or well-known rivers, mountains or places, etc.). Sometimes they manage to smuggle politics into these minor planet names, or express what was important for the discoverer or the comittee which accepted the name-suggestion. We note that sometimes the naming is also important for nations, where members of the non-astronomical scientific community as well as the public are very happy to see the names of their scientists, actors, writers, places, etc. honored in the sky.

However, all of these require that the justifications for the names are the correct ones. Here we show that these justifications are not always correct, because sometimes—especially in the case of early discoveries—it is very hard to guess the real intention of the discoverer. In the case of minor planet (1441) Bolyai we found that an error has been repeated from source to source.

The excellent work *Dictionary of Minor Planet Names* (hereafter referred to simply as *Dictionary*), compiled by Lutz D. Schmadel, has the most complete list of the origin of the names of different minor planets. According to this work, the minor planet (1441) Bolyai was "Discovered 1937 Nov. 26 by G. Kulin at Budapest. Name proposed by the discoverer in honor of Farkas Wolfgang Bolyai (1775-1856), a Hungarian astronomer and computer." (Schmadel, 2003: 115-116). However, as we will document here, the claimed origin of this minor planet name is not correct. In the following Section we will show that in fact this minor planet was named after Farkas Bolyai's son, János Bolyai, a famous mathematician, who developed non-Euclidean geometry in the first half of the nineteenth century.

2 THE CORRECT ORIGIN OF THE NAME OF THE MINOR PLANET

György Kulin (Figure 1), the discoverer, published a note in the Hungarian language in *Csillagászati Lapok* (in English: *Astronomical Papers*) in 1939 (see Figure 2).

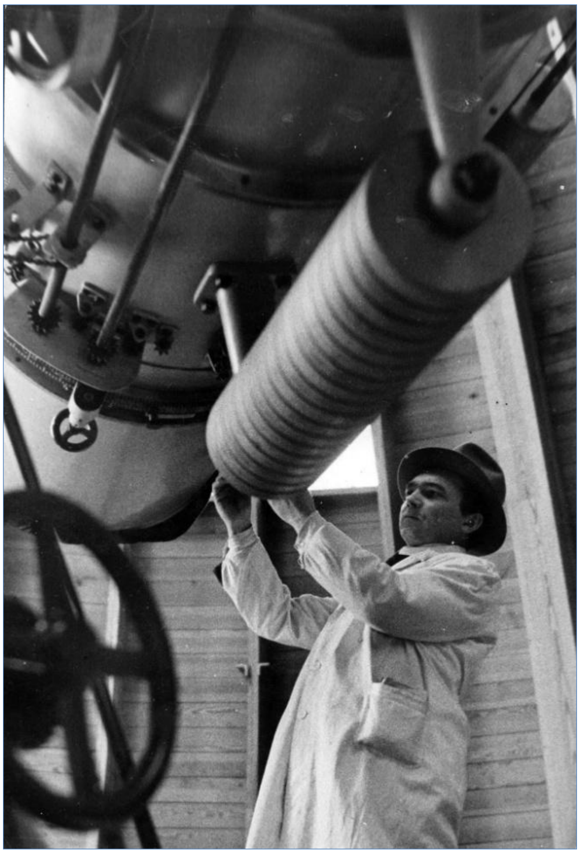

Figure 1: György Kulin using the 24-in reflector at the Konkoly Observatory. Kulin discovered (1441) Bolyai, and many other minor planets, with this telescope (courtesy: Hungarian Astronomical Association).

In this note, *Három új, magyarnevű kisbolygó* (translated as *Three new Hungarian named minor planets*) Kulin wrote clearly: "The minor planet numbered 1441, temporarily designated as 1937 WA, received the name *Bolyai* after the great Hungarian mathematician János Bolyai." (Kulin, 1939: 118; our English translation). Although various images purporting to be János Bolyai are on the web, these are suspect, and there are no known authentic portraits of him.

The former *Astronomischer Jahresbericht*, the annals of the Coppernicus-Institut Berlin, known as the Astronomisches Rechen-Institut today, listed the astronomical literature from year to year, and translated the titles of foreign language papers into German. In Volume 41 of *Astronomischer Jahresbericht* one finds the following title: "G. Kulin, Drei neue Kleine Planeten mit Ungarischen Namen. Csillagászati Lapok 2 118 (Ungarisch)." (*Astronomischer Jahresbericht*, 1941: 135). However, it


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Három új, magyarnevű kisbolygó. A svábhegyi Csillagvizsgálóban évek óta folyó kisbolygómegfigyelések eredményeképen számos új felfedezés történt. Az ezekre vonatkozó megfigyeléseket a Csillagvizsgáló Intézet külön kiadványban tette közzé. (7. sz. kiadvány.) A 40 új felfedezésű kisbolygó közül mindazoknak, melyeknél lehetséges volt, pályája is ki van számítva. Ezek között 8 olyan, melyeknek pályaelemei teljesen megbízhatóknak bizonyultak s így ezeket a berlini Copernicus Intézet a számozott, véglegesen elismert bolygók sorába felvette. Három kisbolygónak 1939-ben történt újraészlelésével — a szokás szerint — a felfedező elnyerte azt a jogot, hogy azoknak választása szerint nevet is adhatott. Ez volt az első alkalom, hogy magyar felfedezésű bolygó magyar vonatkozású nevet kapott.

Az 1441 sorszámú és 1937 WA ideiglenes jelzéssel ellátott bolygó a nagy magyar matematikus Bolyai János nevének megörökítésére a *Bolyai* nevet kapta. Ez a névadás egyrészt iránta érzett tiszteletünk és megbecsülésünk kifejezése, másrészt magyarságának büszke megvallása.

Az 1442 1937 YF kisbolygó Corvin Mátyásról nyerte a *Corvina* nevet. Arról a Mátyásról, akinek világszerte híres kódexei a Corvinák, a magyarság tudományos műveltségének jelképei.

Az 1445 1938 AF jelzésű kisbolygónak Wodetzky József professzorom javaslatára *Konkolya* nevet adtuk. Konkoly Thege Miklós nevét viseli a mai Svábhegyi Csillagvizsgáló Intézet is, minthogy ez az Ógyallai Csillagvizsgáló helyett és annak bizonyos mértékben utódaképen jött létre. Az Ógyallai Csillagvizsgáló megalapítása pedig Konkoly Thege Miklós áldozatos szellemének köszönhető. A névadás tehát reá vonatkozik és egyben annak a magyar nemesi szellemnek szól, amely saját ügyének ismeri el a magyar kultúra és művelődés ügyét s azért áldozatot hozni örömmel kész.

Kulin György.

Figure 1: The discoverer György Kulin's (1939: 118) article in *Csillagászati Lapok*. Kulin wrote about the denomination of (1441) Bolyai in the second paragraph: "Az 1441 sorszámú és 1937 WA ideiglenes jelzéssel ellátott bolygó a nagy magyar matematikus Bolyai János nevének megörökítésére a *Bolyai* nevet kapta." This translates as: "The minor planet numbered 1441, temporarily designated as 1937 WA, received the name *Bolyai* after the great Hungarian mathematician János Bolyai."

seems that this reference was not used to find the original explanation of the name. In the following Section we will investigate how it was possible that we have the same incorrect name explanation in two other fundamental works on the origin of the minor planet names: Paul Herget's *The Names of the Minor Planets* (1968) and Antonio Paluzíe-Borrell's *The Names of the Minor Planets and their Meanings*? (1963).

3 FARKAS BOLYAI AND JÁNOS BOLYAI: WHY FARKAS?

It seems likely that neither Herget nor Paluzíe-Borrell, nor the *Dictionary*, used the original Kulin reference.

Therefore, they had no other way of finding the correct explanation. The *Astronomische Nachrichten* mentioned only that the name of this minor planet is 1441 Bolyai, without explanation (Stracke, 1940). The *Beobachtungs-Zirkulare der Astronomischen Nachrichten* (Benennungen, 1939) and the *Zirkular No. 2011* of the Astronomisches Rechen-Institut (Planetenbenennungen, 1939) wrote the same. These sources did not list the given name, so one cannot identify whether it was Farkas or János Bolyai on the basis of these publications. No more information is available, and the only place where Kulin specified which Bolyai he named the celestial body after was his Hungarian note. But this does not explain why the afore-mentioned astronomers thought that Farkas was the person to whom the credit should be given rather than János.

Farkas Bolyai appears as "... astronomer and computer ..." in the *Dictionary*. This quotation is suspicious because—although he was a polymath—Farkas Bolyai primarily was a mathematician. His connections to astronomy are quite limited: he taught astronomy at his college in Hungary, but he was never considered to be an astronomer. Overall, the quotation in the *Dictionary* magnifies a negligible part of his interest, while the essence of his life—his mathematical work and results—is all but ignored. Therefore it is worthwhile to investigate the associated reference given in the *Dictionary*, which is *The Names of the Minor Planets* by Paul Herget (1968). In this book on page 130 we find: "Name proposed by the discoverer in honor of Farkas Wolfgang Bolyai (1775–1856), a Hungarian astronomer and computer." This is repeated word by word in the *Dictionary*. Herget (ibid.) states only that the author of this note was "RC", i.e. Robert C. Cameron. For minor planet entries in his book Herget often gives more precise references and cites papers in various journals, or he cites the *Minor Planet Circulars*. But sometimes there is only a name code as the origin of the information, and this is the case for the minor planet Bolyai.

Herget mentions in the Introduction to his book that he and his co-authors relied heavily on the work of Antonio Paluzíe-Borrell, who also started to compile a similar work. Paluzíe-Borrell published his own book in 1963 under the title of *The Names of the Minor Planets and their Meanings*. The *Dictionary* also uses the work by Paluzíe-Borrell. Paluzíe-Borrell (1963: 110) wrote the following about the origin of the name of (1441) Bolyai: "Farkas Wolfgang Bolyai (1775–1856), Hungarian astronomer who computed cometary orbits." Note that the meaning of the German given name 'Wolfgang' is quite similar to, but not exactly the same as, the Hungarian given name 'Farkas'. Farkas means 'Wolf'. Like R.C. Cameron, Paluzíe-Borrell does not give any reference for this explanation.

The similarity between these two explanations is so striking that one might suppose that Paluzíe-Borrell and Cameron used the same source. But there are other possibilities: that Herget's book erroneously repeated Paluzíe-Borrell's data, taking directly the mistakenly information from his book, or that it was a personal communication from Paluzíe-Borrell to Cameron, who shortened the explanation a little and somehow forgot to mention the original source.

Whatever the source of this information, we decided to check a possible original source. We took into account the fact that these authors would have checked on hundreds of persons after whom different minor planets were named, and they were interested only in the most important data relating to their lives (nationalities, birth and death dates, and their most important scientific contributions or results etc.—all in just one or two sentences). That is why they probably used some well-known, widely-accepted and well-respected encyclopedia or lexicon. Because of the nature of the minor planet names, they needed one or more appropriate biographies of scientists. These biographies had to be published prior to 1963, the publication date of Paluzíe-Borrell's book.

Of course many such biographies exist, and we can only speculate that they worked in this way, and it is even more speculative what kind of biography or biographies they used because they did not give their references. However, one can consider the well-known, widely-accepted series of biographies of scientists that initially was edited by J.C. Poggendorff. After his death, this series continued and was associated with his name. This *Poggendorff's*-series is called the *Biographisch-Literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften*. The first two volumes did not contain any information about Farkas or János Bolyai (Poggendorff, 1863), but in the second volume Poggendorff listed the names of those people who will be mentioned in the subsequent supplementary volume, including "Bolyai, W." (i.e. Farkas Wolfgang Bolyai). Volume Three was published in 1898 as *J.C. Poggendorff's Biographisch-Literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften*, but it did not include an article about János Bolyai. Instead, after his name there was only the following short notice: "Bólyai, Joh., s. [siehe] Farkas Bólyai Anm. [Anmerkung]." The English translation is: " Bólyai, János [in German, Johannes, which is abbreviated to Joh.], see Farkas Bólyai remark." (Feddersen, von Oettingen, 1898: 156). Elsewhere in this volume there was a detailed article about Farkas (Wolfgang) Bolyai, listing basic data about his life and books, and closing with the comment that the father "... berechnete auch mehrere Cometen ...", i.e. "... also calculated many cometary orbits." (Feddersen, von Oettingen, 1898: 156; our English translation). This is very strange because we do not know about any cometary orbit element calculations by Farkas Bolyai. We checked what is probably the most complete list of his publications (Gazda, 2007) and Sragner, et al.'s (2012) authoritative bibliography, which contains data on more than 59,000 astronomical works that were published in Hungary between 1538 and 2012, and neither of these volumes lists any cometary orbit-related work by the two Bolyais. Unfor-tunately, *Poggendorff's* 1898 edition does not in-clude a relevant reference in the article about Farkas Bolyai.

Now we can summarize the reasons why we assume that Paluzíe-Borrell and Cameron based their respective accounts on the same source, namely *Poggendorff's* 1898 biography, or on other works which took their data directly from this source. Firstly, the texts are very similar to each other. *Poggendorff's* German-language notice about the cometary orbit calculations shows a very good agreement with the English-language texts of Cameron and particularly of Paluzíe-Borrell. Secondly, *Poggendorff's* article includes a very rare biographical element relating to Farkas Bolyai—his cometary orbit calculations—and very probably this is the only place where one can find this statement. And this statement also is cited by Cameron and Paluzíe-Borrell. There is a third argument, too. *Poggendorff's* volume did not leave any real option of choice between the two Bolyais, and in fact the son, János Bolyai, appears only in the article about his father. Our fourth and final argument as to why the latter authors chose Farkas instead of János Bolyai is that *Poggendorff's* book mentions astronomy-related biographical elements throughout, so Cameron and Paluzíe-Borrell would automatically think that the person Kulin named the minor planet after also worked in astronomy. But, as we have shown in Section 2, this is not true. In addition, no other biography supports the *Poggendorff* claim that Farkas Bolyai did any cometary orbital calculations.

Another similar and known case of mis-identification relates to the minor planet (87) Sylvia. Paluzíe-Borrell (1963) and Herget (1955) explain that (87) Sylvia was named after the first wife of Camille Flammarion (Schmadel, 2003), but in 1866 the discoverer, N.R. Pogson, published a paper in *Monthly Notices of the Royal Astronomical Society* where he stated that the name was chosen in reference to Rhea Sylvia, the mother of Romulus (see Schmadel, 2006).

4 CONCLUSION

Our conclusion is that the entry for the minor planet (1441) Bolyai that appears in the *Dictionary of Minor Planet Names* relied upon previously-published references that contained incorrect information, and was not based on a thorough review of the relevant original literature. However, this can be understood considering the work-load involved in researching and assembling this monumental work and because the critical article by Kulin's was not easy to find.

Cameron as well as Paluzíe-Borrell (1963) identified only the person's surname, and this was mistakenly associated with Farkas instead of János Bolyai. We say this because we could safely assume that they found and reproduced the notice about Farkas Bolyai in the third volume of *Poggendorff's Biographisch-Literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften.* Unfortunately, the short footnote in *Poggendorff's* biography, namely that János Bolyai would be mentioned in the following volume, did not prove helpful.

In spite of the approach adopted by Cameron and Paluzíe-Borrell, we searched for the naming-intention of the discoverer of minor planet 1441 and we found it. This was contained in an Hungarian article by György Kulin, where he published the names and their associated explanations of the three minor planets that he discovered. There he clearly stated that (1441) Bolyai = 1937 WA was named after the great mathematician János Bolyai. We present a facsimile of the original text and an English translation in this article.

The cases of (1441) Bolyai and (87) Sylvia are examples which illustrate that in some cases the currently-available name-explanations for certain minor planets do not follow the original intentions of their discoverers.

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THE 1882 TRANSIT OF VENUS AND THE POPULARIZATION OF ASTRONOMY IN THE USA AS REFLECTED IN *THE NEW YORK TIMES*

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Abstract: After the disappointments of the 1761 and 1769 transits of Venus, the nineteenth century pair, in 1874 and 1882, offered astronomers the next opportunity to use these rare events in a bid to pin down a value for the solar parallax and hence that fundamental yardstick of Solar System astronomy, the astronomical unit. Only the 1882 transit was visible from the USA, and on the fateful day amateur and professional observers were scattered across the nation. While the value for the solar parallax derived from their combined observations was a significant improvement on the range of values obtained in the eighteenth century, there was considerable disquiet about the logic of using transits of Venus in this way when alternative approaches were available. In this paper we discuss some of the instruments that were used to observe the 1882 transit from American soil, review the scientific results from the overall American efforts and summarize the various reports that appeared in the pages of *The New York Times* and ultimately helped to generate a heightened public awareness of astronomy.

Keywords: 1882 transit of Venus, solar parallax, astronomical unit, *The New York Times*

1 INTRODUCTION

In the mid-1800s Sir George Biddell Airy (1801–1892, Britain's Astronomer Royal, described the determination of the astronomical unit, the mean distance of the Earth from the Sun, as "… the noblest problem in astronomy." (Airy, 1857: 208). Attempts at calculating this distance, up until the 1600s, led to figures much smaller than now known to be the case. The Greek Aristarchus of Samos (c. 310–230 B.C.) using clever geometry, with inaccurate data to implement it, concluded that the Earth-Sun distance was at least eighteen times, but not more than twenty times the Earth-Moon distance. Another Greek, Hipparchus of Nicaea (c. 162–126 B.C.), taking advantage of a solar eclipse in different degrees of totality at two different sites, applied trigonometry to the parallactic shift to calculate that the Earth-Moon distance was between sixty-two and seventy-four times the radius of the Earth. Using the radius of the Earth now known to be about 6,378 kilometers, Hipparchus' range for an Earth-Moon distance would be from 395,000 to 472,000 kilometers, a fair approximation for the time. The value of the radius of the Earth was well determined by the 1600s. Combining Hipparchus' determination with the premise of Aristarchus, the value of the astronomical unit could be calculated to be as low as (18 × 395,000) kilometers = 7,110,000 kilometers = 4,400,000 miles, lower by a factor of 20 than the actual value of 149,600,000 kilometers. Up until the first part of the seventeenth century, this value for the astronomical unit was commonly held.

It was in the seventeenth century that Johannes Kepler (1571–1630) stated his three truisms, later to be called 'laws', that provided a basis for a more accurate determination of the astronomical unit. According to his Third Law, for all the planets the squares of the periods of revolution are proportional to the cubes of the semi-major axes of their orbits. Therefore, if one could determine the absolute distance between any two members of the Solar System, one could further derive the distance between any two others, including that between the Earth and the Sun.

In 1627 Kepler published his *Rudolphine Tables* of planetary motion, named in honor of his patron, the Holy Roman Emperor Rudolph II of Prague. In these tables were predicted a transit of Mercury to occur on 7 November 1631 and a transit of Venus on 6 December 1631. Interestingly, and incorrectly, Kepler predicted that there would not be another Venus transit for 130 years. Transits of Venus are now known generally to occur in patterns of pairs about eight years apart, separated by about 105.5 and 121.5 years. Due to the 3.4 degree tilt of Venus' orbit with respect to that of the Earth, a transit can only occur when both planets are near the nodes of their orbits. Somehow Kepler missed the transit of 1639 in his calculations although ironically it would be the first

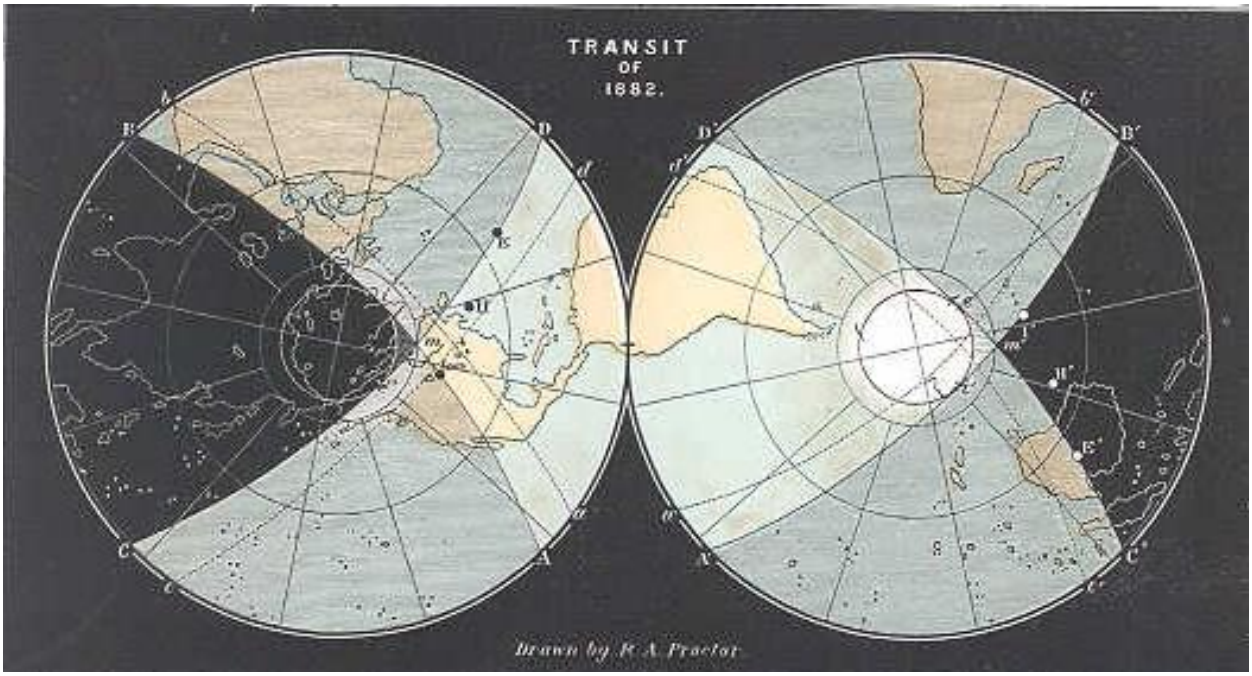


Figure 1: Visibility of the 1882 transit of Venus. The entire transit was visible from the pale blue areas, but only the ingress or egress phases from the darker blue areas. Those living in the black regions could not see the transit at all (after Proctor, 1882: Plate VII).

transit to be observed due to the efforts by the young Jeremiah Horrocks (1618–1641) who discovered Kepler's mistake (Proctor, 1882).[1]

It was Edmond Halley (1656–1742) who promoted the use of parallax observations during the next transits of Venus, to occur in 1761 and 1769, for the calculation of the Earth-Sun distance. In 1716 he wrote a proposal, which he contributed to the Royal Society:

> … scarce any problem will appear more hard or difficult than that of determining the distance of the sun from the earth, very near the truth; but even this, when we are made acquainted with some exact observations, taken at places fixed upon and chosen beforehand, will, without much labor be effected. And this is what I am now desirous to lay before this illustrious Society (which I foretell will continue for ages), that I may explain beforehand to young astronomers, who may perhaps live to observe these things, a method by which the immense distance of the sun may be truly obtained within a five-hundred part of what it really is. (cited in Proctor, 1882: 31-32).

Figure 2: German astronomers at Hartford in 1882 (after *Frank Leslie's Popular Monthly*, May 1883).

Halley described the method to be used though he knew he would not live to see the events himself. Halley's method and some variations thereof, notably that of Joseph-Nicolas Delisle (1688–1768), applied trigonometric interpretation to the apparent position of Venus on the disc of the Sun to determine a value for the solar parallax. Once this was known, a figure for the astronomical unit could be calculated. The many nations participating ultimately provided very discrepant values for the astronomical unit with documented parallax values ranging from 8.28″ to 10.60″ in 1761 and the somewhat tighter range of 8.43″ to 8.80″ in 1769 (Cottam et al., 2011: 226). Factors hindering the collection of accurate data were the difficulties in establishing longitude and latitude of the sites, and the unexpected presence of a 'black-drop effect' which blurred the image at the time of the internal contacts.

In 1874 the new tools of photography and spectroscopy were expected to be useful in providing a more accurate and precise value. This time the new nation of the United States would be participating in the efforts of the transit expeditions. The Americans launched eight expeditions, three in the Northern Hemisphere and five in the Southern Hemisphere. All the observing teams had some degree of success although there were some problems due to weather, and the 'black-drop effect' was not eliminated. It would be years before all the data were reduced. In fact, as late as 1880, Professor Charles A. Young (1834–1908) admitted, "The results of the transit of Venus observations have not yet been so fully published as might have been expected." (Young, 1880: 88) Indeed, the Americans did not publish any official result for the solar parallax from these efforts. However, David Todd (1855–1935), then of the National Almanac Office, published a 'provisional' value of 8.883 ± 0.034″, translating to a value for the

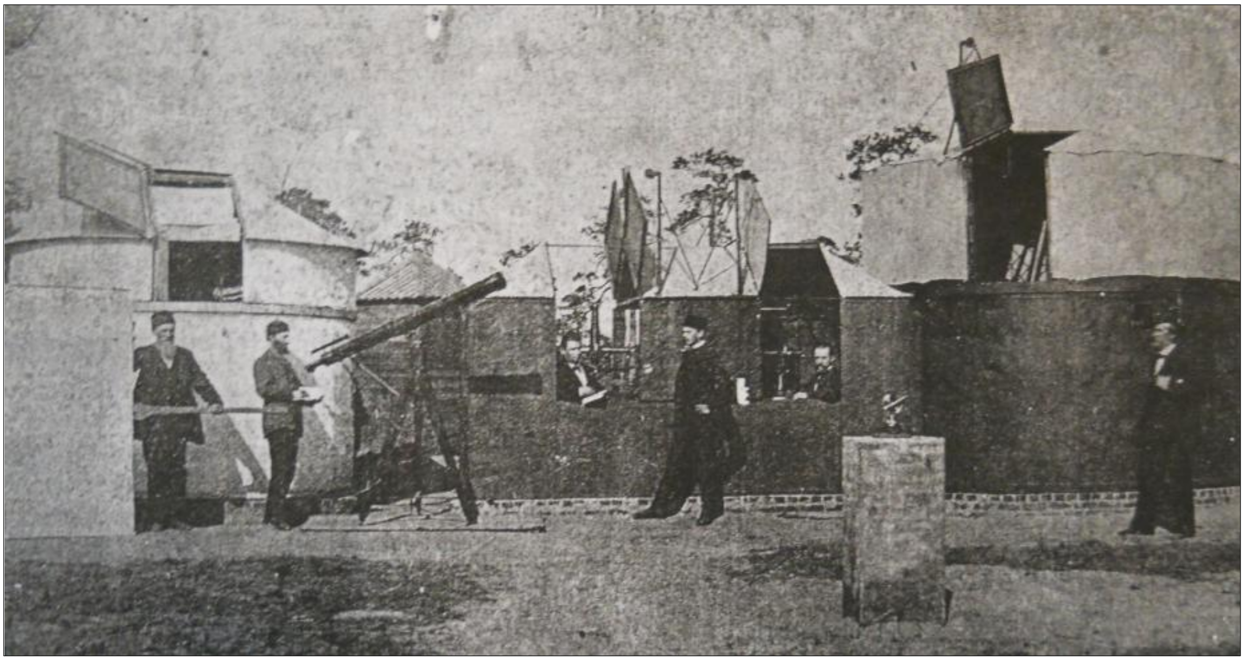


Figure 3: The 1882 German expedition site at Aiken (courtesy: Aiken County Historical Society). According to Duerbeck (2004: 14), Franz is second from the left.

astronomical unit of 92,028,000 miles, based upon data taken from *Observations*, *Part One*, *"General Discussion of Results"* of 1880 (see Todd, 1881).

2 THE 1882 TRANSIT OF VENUS

Disappointment in the results of the observations of the transit of Venus of 1874 might have dampened some of the enthusiasm for the upcoming transit of 1882 but there were reasons for renewed resolve. Weather permitting, this transit would be visible from much of Europe and the Americas (see Figure 1). It would last longer at about 6.3 hours, as opposed to the approximately 4.6 hours in 1874. This meant the area on the Earth where some part of the transit could be seen would be greater. But maybe most significant was the recognition that this would be the last transit for more than a century. If there was any doubt, this transit could not be ignored (Airy, 1880). The U.S. Congress therefore appropriated $177,000 for American efforts. Instruments would be improved and there would be expeditions this time both within and outside of American borders (see Dick, 1995).

2.1 Overseas Expeditions

In anticipation of the 1882 transit an international conference was held in Paris, in October 1881, to coordinate efforts. Fourteen nations participated (Orchiston and Buchanan, 1993). Discussion on methodology led to a general acknowledgement that photography had not led to satisfactory results in 1874, and as a result its use would be less significant in 1882.

Some countries, such as Portugal and Spain, that had not participated in previous transit parties, did attend the conference and would have their own parties in 1882. Some others, such as Norway and Chile, sent representatives to the conference but ultimately did not mount their own expeditions. Great Britain had a Transit Committee that decided to send numerous expeditions around the world, including to Canada in North America, and some sites that would not have access to all four contacts, such as South Africa (Koorts, 2004). Russia and the United States declined to participate in the Paris conference. America's Simon Newcomb did not have much faith in the established procedures, having been frustrated in his efforts in 1874 (see Tebbutt, 1883), while Russian astronomers had decided that observations of minor planets at opposition would be a less costly way of investigating the solar parallax than by using transits of Venus.

2.2 Foreign Expeditions to the United States

Although the United States did not attend the Paris conference, it would serve as host to transit parties from Belgium, France and Germany (see Duerbeck, 2004; Sheehan and Westfall, 2004).

Germany sent two expeditions to the United States, one going to Hartford in Connecticut (see Figure 2), and the other to Aiken in South Carolina (see Figure 3). Because of the disappointing results they obtained using the photographic method in 1874, the Germans decided to depend upon the planet's placement on the solar disk as measured with a heliometer. Here an object glass is divided diametrically into two halves, which can be manipulated by a screw in order to measure small angular distances between the focal images of two objects with a built-in micrometer used to bring the two objects into coincidence (Radau, 1874; Mauritius Expedition, 1874).

Expedition I, which went to the grounds of Trinity College in Hartford, Connecticut, was led by the astronomers Gustav Müller (1851–1925) and Friedrich Deichmüller (1855–1903) (Duerbeck, 2004). The morning of the transit the sky was overcast. Having missed the ingress contacts Müller reported

> … the ingress could not be observed, and only for one moment Venus was seen between first and second contact halfway in the Sun. Only after ingress the

Figure 4: The 1882 transit of Venus observatory structure at Aiken (photograph by the first author).

> clouds started to disperse with rapidity, and our mood started to rise. About one hour after external contact the clouds were so thin that we could start the heliometer measurements … Soon the sky improved, and remained quite good until the end …

They obtained eight full sets of heliometer readings (Knapp, 2004).

Julius Franz (1847–1913), Principal Astronomer at the Royal Observatory in Koenigsberg, headed Expedition II to Aiken, South Carolina. There the property of Henry Smith was selected, as it was far enough away from the railroad tracks to avoid the occasional jarring of the earth due to passing trains.

The public was very much interested in the goings-on at the Smith estate, but the Germans stationed guards to keep curious citizens away. It was said that even the Mayor of Charleston was kept away from the site of the scientific work taking place (Aiken and the transit of Venus, 1935). Aiken had been selected as a suitable site due to its usually fair climate, however unexpected rain prevented observation of the first two contacts. It did clear thereafter, allowing the Germans to make some satisfactory heliometric measurements for the duration of the transit. A total of forty-eight observations, three sets of sixteen each, were made. A marker, donated by the Germans, was placed at the site, the residence of Henry Smith, to commemorate the event. This marker was later donated by John Weems, then owner of the grounds, along with the observatory structures used, to the Aiken County Historical Museum (The transit of Venus, 1995), where it now stands with a descriptive plaque (see Figures 4-6).

This limestone slab of 27 × 31 inches, 4 inches thick, now cracked, contains the following inscription (with the English translation shown in brackets):

> Venus – Durchgang 1882 (The Transit of Venus 1882)
> Deutsches Station II (German Station No. II)
> 5h 26m 52s6 W 33° 31′51″ N

San Antonio, Texas, would host two expeditions. One of the four official American sites was on the grounds of what is now known as Fort Sam Houston. The Belgian nation would be participating in major scientific expeditions for the first time, here in San Antonio and in Santiago, Chile. Both Belgian parties were organized by Jean-Charles Houzeau (1820–

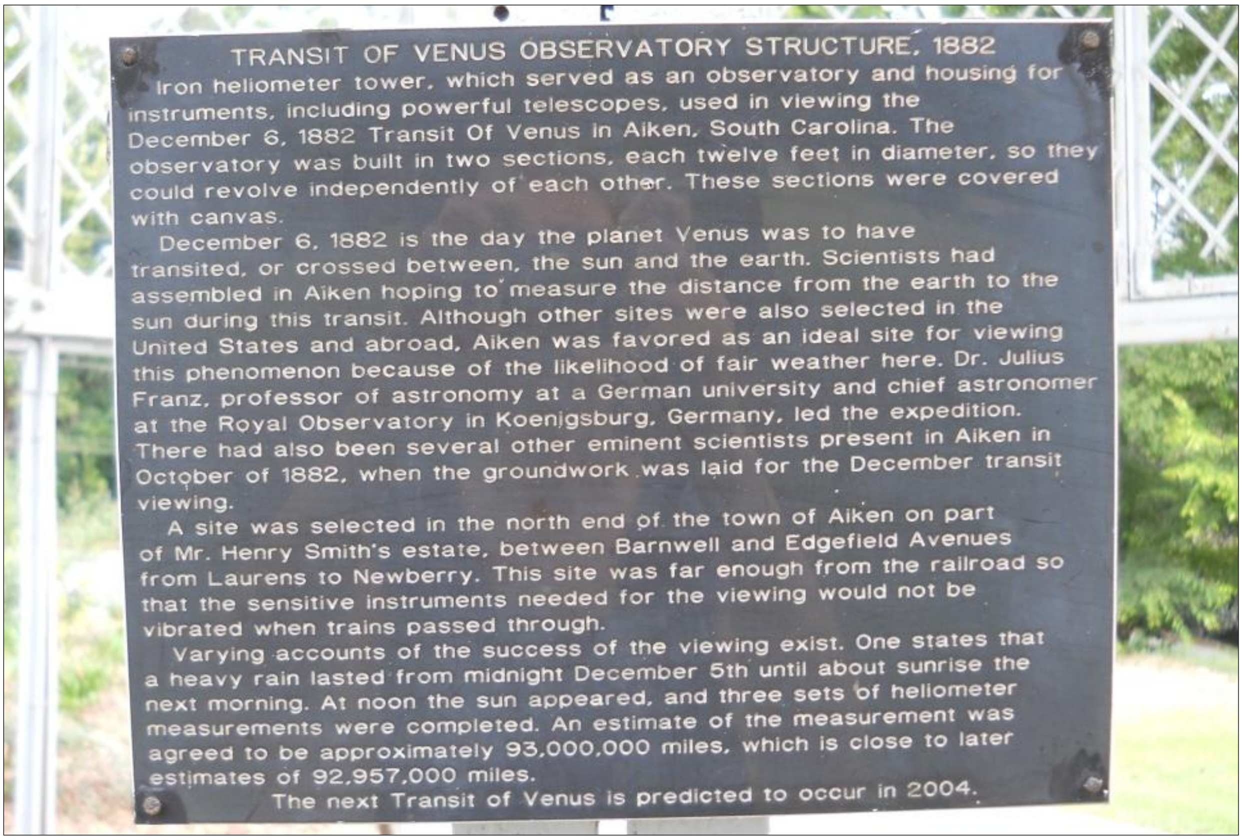


Figure 5: The plaque at the site of the 1882 German transit of Venus expedition at Aiken (photograph by the first author).

1888; Figure 7) who would himself head the party in San Antonio. The Belgians were about 500 meters to the west of the Americans, on private property. The methodologies of the two countries were different. Following the published instructions for all the official American expeditions, the Americans would be relying on the photographic method.

The Belgians at both sites would be using the invention of Houzeau, a heliometer with unequal focal lengths (see Figure 8). The instrument has two objectives of different focal lengths whereby large and small images of both the Sun and Venus are produced. A large solar image is projected on a screen (seen below the heliometer tube in the image below). A smaller solar image produced by the short-focus objective is made to coincide with that of Venus by micrometer adjustment. The difference in micrometer readings between the "… small Sun centred on crosshairs, being the centre of the large Sun … [and the] … small Sun centred on large Venus …" enables determination of the distance between the centers of both objects (Sterken and Duerbeck, 2004: 26). Houzeau's assistant, Albert Lancaster (1849–1908), reported on the progress of the day. At 6:15am Houzeau went to the American site to compare chronometers. Upon returning to the Belgian site there was early frustration as the first two contacts were lost due to cloud cover. Then at about 9:30am, 12 minutes before the minimum distance of the centers, the sky cleared and 124 micrometer readings were taken (Lancaster, 1882). When combined with the results obtained from the partner group in Chile—which enjoyed perfect weather—Houzeau was able to calculate a final result for solar parallax of 8.911 ± 0.084″ (see Sterken et al., 2004).

In October of 2005 an historical marker was inaugurated and placed at the Belgian transit of Venus observation site (see Figure 9). The original structure, a wooden house that was occupied by the party, is no longer extant and has been replaced by the Bullis House Inn (see Figure 10), a bed-and-breakfast that was built between 1906 and 1909, which is now in itself a Texas state historic landmark. Note the unfortunate error on the marker, which states that 124 photographic plates were taken. The Belgians only obtained micrometric data, and took no photographs (see Sterken, 2009).

The French also sent an expedition to the United States (*Passage de Vénus* ..., 1883). The report of their efforts at Fort Marion in Saint Augustine, Florida, was made by the three members, Colonel François Perrier (1835–1888), Commandant Bassot and Captain Gilbert Defforges (1852–1915). These three took separate readings on three different telescopes, an 8-inch, a 6-inch and a 3-inch respectively. They achieved a fair degree of agreement, especially for the time of the 4th contact.

Captain Defforges reported that 200 photographs were taken of the planet on the Sun. He was also responsible for establishing the longitude at the site, working with Preston of the Coast Survey, who communicated with him telegraphically from Savannah before the transit. They also made another series of confirmatory tests after the event. Commandant Bassot had already established latitude by means of the observation of a number of familiar stars (ibid.).

Figure 6: The cracked historical marker from the 1882 German transit of Venus expedition at Aiken (photograph by the first author).

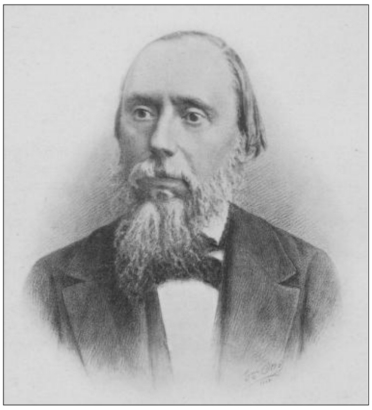


Figure 7: Jean-Charles Houzeau (after Sterken and Duerbeck, 2004: 25).

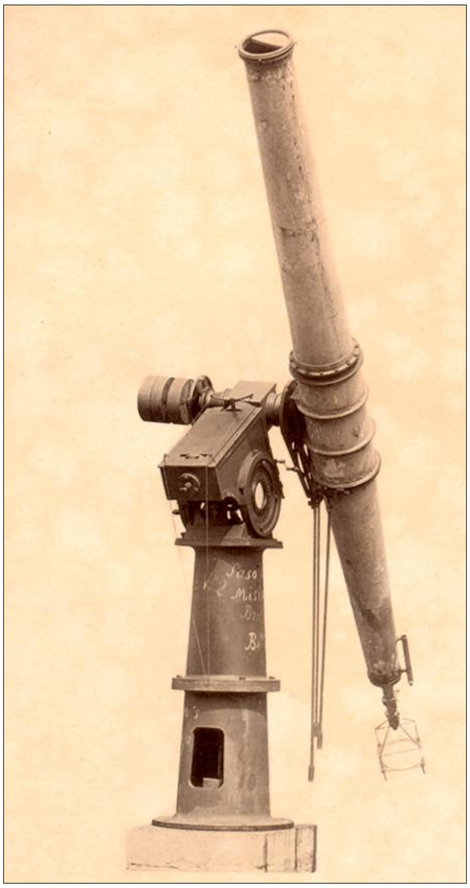


Figure 8: Heliometer with unequal focal lengths (adapted from Sterken et al., 2004: 26).

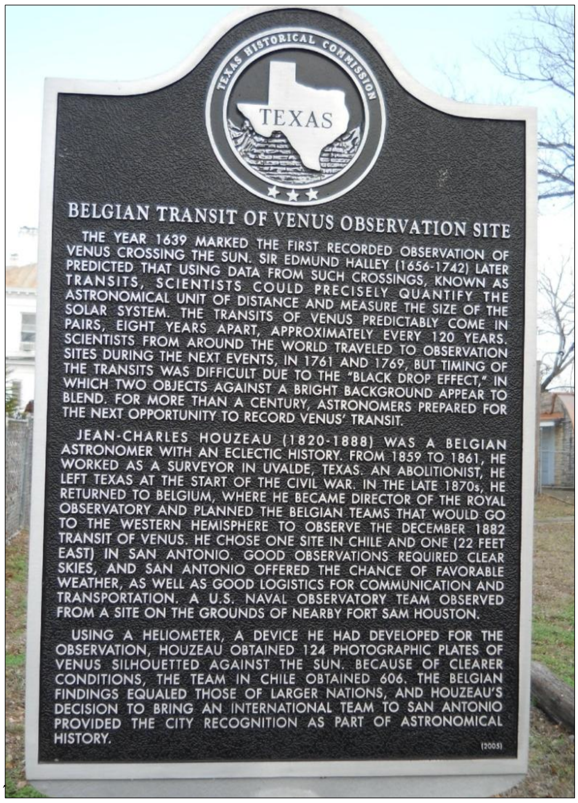


Figure 9: The historical marker for the 1882 Belgian transit of Venus expedition at San Antonio (photograph by the first author).

Figure 10: Bullis House Inn, site of the 1882 Belgian transit of Venus expedition at San Antonio (photograph by the first author).

The French enjoyed good weather for the entire transit and Colonel Perrier noted with satisfaction the arrival of encroaching clouds soon afterwards: "Le temps est à la pluie et à la tempête!!!" (ibid.).

2.3 The US Transit Program

The Americans organised several northern parties for the 1882 transit, and all of these were in their home country, at San Antonio (Texas), Cedar Keys (Florida), Washington (D.C.) and Fort Selden at Cerro Roblero, in the New Mexico Territory in the west.

2.3.1 Instrumentation

The horizontal telescope with a heliostat (Figure 11) and photographic plate-holder was the instrument favored by the Americans during the transit of 1874. It used a clock-driven mirror to bring the solar image to a long-focus objective lens in a stationary horizontal telescope. It could produce relatively large and distortion-free images which were photographed and measured (e.g. see Janiczek, 1983; Lankford, 1987).

Unlike most of the Europeans, the Americans had decided to stay with the photographic method, and although the equipment for the 1882 transit would be the same as in 1874, on this occasion the more convenient dry collodion plates would be used (Dick et al., 1998).

2.3.2 Expeditions and Results

The Americans at the San Antonio site were on the grounds of the current military base of Fort Sam Houston (see Figure 12) and under the leadership of Asaph Hall (1829–1907; see Figure 13) from the U.S. Naval Observatory. First contact should have occurred at about 7:20am but was missed due to clouds, as it was to the Belgians 500 meters away. The Americans captured their first photograph of Venus as the sky began to clear at about 10:17am. By the time the transit terminated at about 1:30pm they had obtained 204 photographs (Viewing Venus, 1882). Having sent a telegram shortly after the event, Professor Hall reported in more detail on his successes and frustrations in a letter to Admiral Rowan that he penned on 8 December 1882 (Hall, 1882).

Besides the standard membership of all American expeditions, Hall was able to take advantage of some on-site military personnel, who were not astronomers, as cited in his letter to Rowan,

> Major Clous and Capt. Livermore made observations of the diameter of Venus with our double-image micrometer. Lt. Shunk assisted Mr. Woodward [assistant astronomer] in managing the heliostat and chronograph and was of very good service. (ibid.).

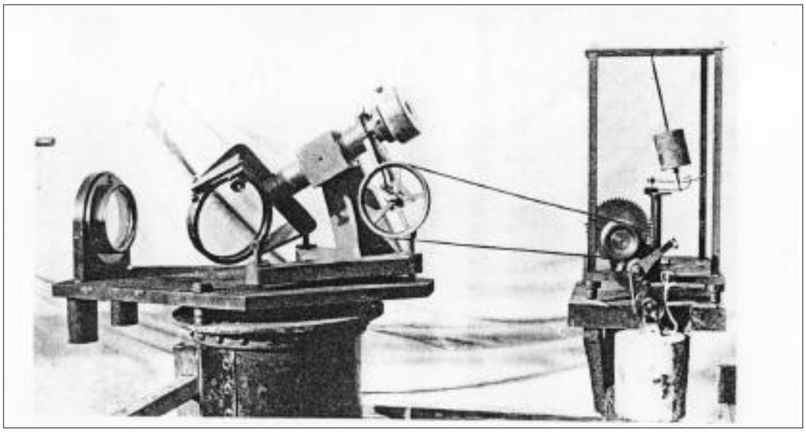


Figure 11: Close-up of the heliostat used at the Nagasaki site (after Janiczek, 1983: 58).

John Walter Clous was acting Judge Advocate in San Antonio at the time of the transit. Capt. William Roscoe Livermore was the base's Chief Engineer Officer, while William Alexander Shunk was a career military officer on a temporary assignment in San Antonio (Jacqueline Davis, personal communication, 2011). Hall and

Figure 12: Grounds of Fort Sam Houston, San Antonio (photograph by the first author).

his group would be remaining there for several more days to confer with the Belgians and to make other observations to assure the accuracy of their position and chronometers (Hall, 1882). An historical marker (see Figures 14 and 15) was dedicated on the grounds of Fort Sam Houston, near the American observing site on 3 December 2004 (Maley, 2005). The field where they made their observations is now an area of base officer housing. The marker is placed off a driveway a short distance from the precise location of their work, which is now in the grounds of a private residence (Jacqueline Davis, personal communication, 2011).

John Robie Eastman (1836–1913; Figure 16) from the United State Naval Observatory was the leader of the observing party at Cedar Keys, Florida (Prof. J.R. Eastman dies, 1913). As reported in his telegram (see Figure 17), the expedition at that site succeeded in catching the last three contacts. The circumstances were described in more detail in a letter of the same date to Vice Admiral S.C. Rowan, President of the Transit of Venus Commission. After the first contact the sky became so clear that many photographs were taken. The dry plates would soon be used up so it was decided to take some photographs using the wet process as well. "We then alternated groups of dry and wet plates until about five minutes before third contact we had exposed 150 dry plates

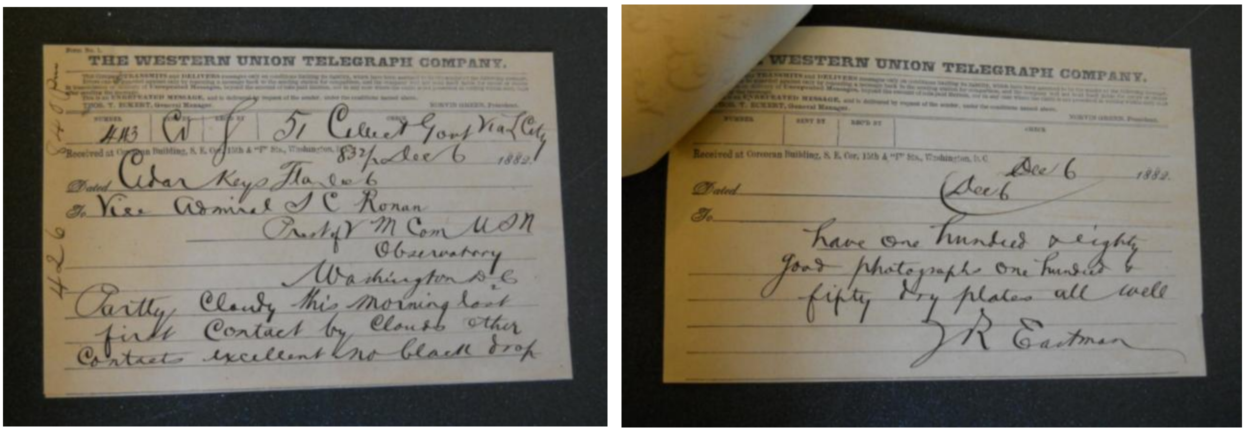
Figure 13: Asaph Hall (courtesy: usno.navy.mil).

Figure 14: Base Officer housing at Fort Sam Houston, with the historical marker just to the left of the tree (photograph by the first author).

Figure 15: Historical marker for the 1882 American transit of Venus expedition (photograph by the first author).

and 30 wet plates." Evidence of the degree of cooperation expected from all in this scientific endeavor was in the stated expectation that Eastman would communicate with both the Coast Survey party in Savannah and the French party at St. Augustine to help the French establish their longitude. However, as Eastman had yet to hear from either party he expressed his willingness to make this determination after the transit, and following the intense labors of the previous days he decided to take ten days vacation (Eastman, 1882).

Figure 16: John Robie Eastman (courtesy: photolib.noaa.gov).


THE WESTERN UNION TELEGRAPH COMPANY.
413 C. J. 51 Collect Govt Via L City
Received at Corcoran Building, S. E. Cor. 15th & "F" Sts., Washington, D.C. 832 pm Dec 6 1882
Dated Cedar Keys Fla 6
To Vice Admiral S C Rowan
Prest of V M Com USN
Observatory
Washington D.C
Partly cloudy this morning lost first contact by clouds other contacts excellent no black drop

THE WESTERN UNION TELEGRAPH COMPANY.
Received at Corcoran Building, S. E. Cor. 15th & "F" Sts., Washington, D.C. Dec 6 1882
Dec 6
have one hundred & eighty good photographs one hundred & fifty dry plates all well
J R Eastman

Figure 17: Eastman's telegram that was sent to Rowan (photograph by the first author; courtesy: National Archives, Washington D.C.).

George Davidson (1825–1911; see Figure 18) had charge of the American observing site at Fort Selden, New Mexico. On the day of the transit a telegram was sent to the Commission reporting complete success (see Figure 19): all four contacts were seen, measurements were taken of the diameter of Venus, and 216 "splendid" photographs were taken. On the same date Davidson also sent a short note to Julius Hilgard, Superintendant of the Coast Survey, conveying the same happy information (see Figure 20).

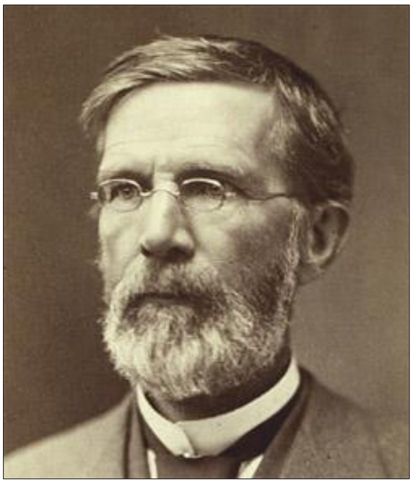


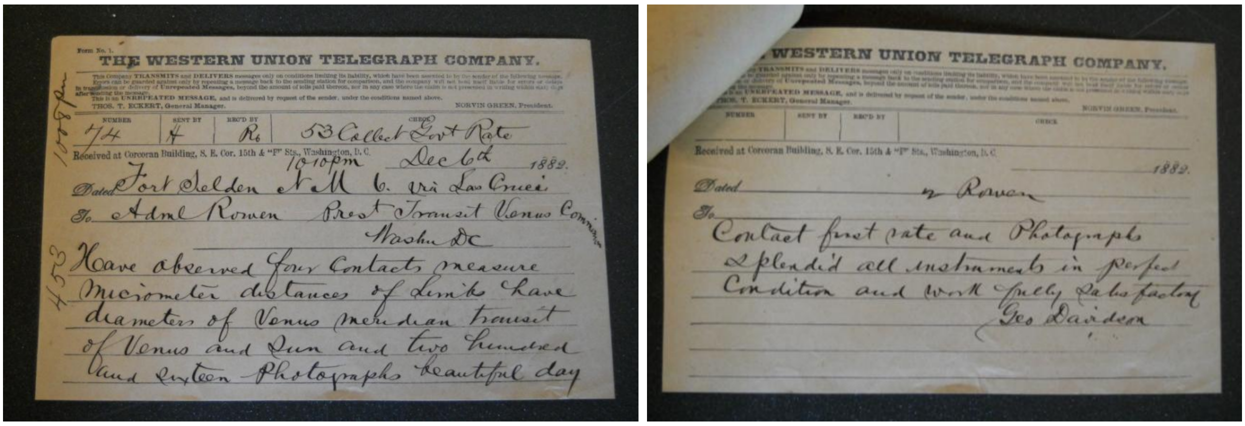
Figure 18: George Davidson (courtesy: history.noaa.gov).

William Harkness (1837–1903; see Figure 21) was in charge of the efforts in Washington, D.C. He was one of the only two remaining members of the American Transit of Venus Commission that had begun in 1871 and ended in 1891, anticipating the two transits of the century; the other person was Simon Newcomb (Dick, 2005). Harkness's party observed all four contacts at their site. The Americans never published a determination of the solar parallax based on their 1874 results but this time Harkness (1891) would do so:

> Professor Harkness, U.S.N., reports that the photographs of the last transit of Venus (more than 1400 photographs being available) lead to the following value of the solar parallax; $\pi = 8''.842 \pm 0.''0188$. With 3963.296 miles as the equatorial radius of the earth, the resulting mean distance of the sun is 92,455,000 miles, with a probable error of 123,400 miles. (*Report* ..., 1889).

In 1894 Harkness would publish an updated figure (Dick, 2005).

The four Southern Hemisphere sites selected were in South Africa, Patagonia, Chile and New Zealand. Simon Newcomb led the expedition to South Africa and established an observing station alongside the Huguenot Seminary for Girls at Wellington, where he encouraged local participation. Here only the first and second contacts would be visible. After the transit Newcomb left behind the instrument-mounting piers in the hope they would still be there at the time of the 2004 transit. They were not (Koorts, 2003). Lieutenant Samuel W. Very, U.S.N. was chief astronomer of the observing party that went Santa Cruz in Patagonia, where all four contacts were ob-


THE WESTERN UNION TELEGRAPH COMPANY.
474 H Ro 53 Collect Govt Rate
Received at Corcoran Building, S. E. Cor. 15th & "F" Sts., Washington, D.C. 10 10 pm Dec 6th 1882
Dated Fort Selden N M 6 via Las Cruces
To Adml Rowan Prest Transit Venus Comm
Washn DC
Have observed four contacts measure micrometer distances of limbs have diameters of Venus meridian transit of Venus and Sun and two hundred and sixteen photographs beautiful day

THE WESTERN UNION TELEGRAPH COMPANY.
Received at Corcoran Building, S. E. Cor. 15th & "F" Sts., Washington, D.C. 1882
to Rowan
Contact first rate and photographs splendid all instruments in perfect condition and work fully satisfactory
Geo Davidson

Figure 19: Davidson's telegram that was sent to Rowan (photograph by the first author; courtesy: National Archives, Washington D.C.).

served. Professor Lewis Boss (1846–1912) led the group to Santiago, Chile, where again all four contacts were observed. Edwin Smith (1851–1912), who led the 1874 US party to the Chatham Islands, was chief astronomer for the final group at Auckland, New Zealand, where only the two internal contacts were observed (Dick, 2003; Orchiston, 2004).

Not among the official eight expedition sites were those under Charles A. Young at Princeton and David Todd at the Lick Observatory at Mt. Hamilton, California. Todd, a Professor of Astronomy at Amherst College, was invited to observe the transit at Lick by Captain Richard S. Floyd. Todd accepted and the clear skies enabled him to obtain 147 photographs, 125 of which were deemed measurable. Princeton astronomer Charles A. Young stated that Todd's photographs may have been the best obtained (see Sheehan and Misch, 2004). In 2004 Misch and Sheehan found 142 of the original negatives in the Lick Observatory Plate Archive, and they constructed a movie of the event (ibid.). Young and Todd followed the instructions of the Commission and their data were included in the official report. Ultimately the southern US stations collected 587 measurable plates, and the northern stations (including Princeton and Lick) collected 793 (Dick, 2003). Most parties used the improved dry collodion emulsion plates. The Americans were generally fortunate with regard to weather conditions, and several parties, from both hemispheres, saw all four contacts. In all, seventeen hundred photographs were taken, the majority of which could be measured (Dick et al., 1998).

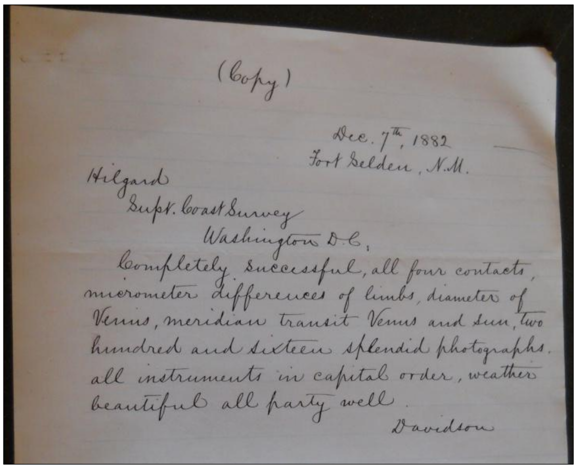
In America there was also cooperation from many established observatories across the country, as well as from private individuals. Instructions and time signals were available to anyone who was willing to contribute to the effort (ibid.).

Due to the high probability of inclement weather, the Harvard College Observatory had not been selected as a primary site by the Transit Commission. However, Edward C. Pickering (1846–1919) had some success there and reported his results to the American Academy of Arts and Sciences. Several of his observers recorded all four contacts (Pickering, 1882-1883). Maria Mitchell and her students observed from the grounds at Vassar College, as she had been denied participation in any Government expedition. Her group used a small version of the official photoheliostat, as well as an equatorial similar to those used by the U.S. expeditions, and succeeded in photographing the event (Sheehan and Westfall, 2004).

In 1882 the United States Transit of Venus Commission had published instructions for the observation of the upcoming transit. These were to be followed by all the official expeditions, to guarantee consistency in observing methods and the collection of data. It was also intended that they could be

> … adapted to the use of amateur observers who desire to be made acquainted with the methods by which they may make observations of value. (United States Transit of Venus Commission, 1882).

At the National Archives in Washington, D.C. there is a box containing 93 reports of observations



(Copy)

Dec. 7th, 1882
Fort Selden, N.M.

Hilgard
Supt. Coast Survey
Washington D.C.

Completely successful, all four contacts, micrometer differences of limbs, diameter of Venus, meridian transit Venus and Sun, two hundred and sixteen splendid photographs. all instruments in capital order, weather beautiful all party well

Davidson

Figure 20: Davidson's Letter to Hilgard (photograph by the first author: courtesy: National Archives, Washington, D.C.).

of the transit, submitted by those who were not on official Government expeditions. The majority of these people were amateur astronomers (Cottam, 2012: 208-209)

Once again reduction of data would be a time-consuming undertaking. The ligament that characterized the 'black drop' was often reported (Howlett, 1883), but not always (Horner, 1883; Todd, 1883). The presence of a Venusian atmosphere also was frequently reported (see Prince, 1883), but again not always (Howlett, 1883). These features would continue to complicate the accurate measurement of the photographs that was required for a valid interpretation of the event. By this time Simon Newcomb did not have much faith in the use of transits of Venus to solve the riddle of the astronomical unit, and in his 1895 monograph, *The Elements of the Four Inner Planets and the Fundamental Constants of Astronomy*, he ranked the value of results obtained by numerous methods above those obtained using transits of Venus (Newcomb, 1895: 166). In the previous year, William Harkness from the US Naval Observatory addressed the American Association for the Advancement of Science, and stated that his final best estimate for the solar parallax was 8.809 ± 0.0059″, which corresponds to a value of 92,797,000 ± 59,700 miles for the astronomical unit (Dick et al., 1998).[2] This result was closer to the parallax adopted by the International Astronomical Union in 1976 of 8.794148 ± 0.000007″ than the figure that Todd derived from observations of the 1874 transit (ibid.).

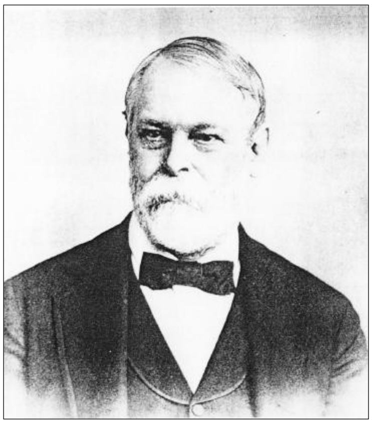


Figure 21: William Harkness, U.S. Naval Observatory (after Janiczek, 1983: 69).

3 The 1882 Transit of Venus and *The New York Times*

Since it was only eight years since the last transit, it was apparently deemed unnecessary by *The New York Times* to educate the public on the history and methodologies of such an event by means of lengthy articles, as had been done for the 1874 transit. However, there was some of this, on a smaller scale, as well as frequent updates on plans and expedition preparations in anticipation of the 1882 transit.

On 14 August 1881 *The New York Times* printed a short item describing the initial efforts in the selection of sites for the American parties. Help from the National Academy of Sciences was requested (The next transit of Venus, 1881). Later that month, on the 20th, the reader would learn that on the previous day Professor William Harkness read a paper titled "The Methods of Determining the Solar Parallax, with Special Reference to the Coming Transit of Venus" at the meeting of the American Association for the Advancement of Science in Cincinnati (General Telegraph News, 1881). On 3 February 1882, an article was reprinted from the *Providence Journal* which related that the upcoming transit would be visible throughout the Western Hemisphere and would last for six hours. Moreover, an "… intelligent observer … [with] … keen eyesight … with the use of smoked glass, might see the tiny dot on the planet with his naked eye." (The coming transit of Venus, 1882).

On 31 March 1882 *The New York Times* printed the speculation by Professor Daniel Kirkwood from the University of Indiana that the transit might provide an opportunity to watch for a satellite of Venus (General Notes, 1882a). On 3 August 1882 readers would learn that $75,000 was appropriated by the House of Representatives for the upcoming transit expeditions (Speech of President Curtis, 1882), and later that month, on the 22nd, there was an article listing all the American parties for transit observations that were subsidized by this appropriation. There were four northern hemisphere sites, all within the boundaries of the USA and its territories, and four in the southern hemisphere. The destinations of the expeditions and the members of all the parties were listed. The solar parallax and its significance were explained. There was also brief mention of some British, French and German parties (Gleanings from the mails, 1882).

On 27 November 1882 the *Times* reprinted another item from the *Providence Journal*, a general description of the transit and times it would be visible. Again all intelligent persons were reminded to observe this rare event "… with the aid of a piece of smoked glass ..." (The transit of Venus, 1882d). On 29 November 1882 there was a request from Professor Brooks of the Red House Observatory that prayers be made at all churches on Sunday, requesting clear skies for the observation of the transit (Prayers for astronomical science, 1882). On 5 December 1882, the day before the transit, there was an article with much information for the general public, the history of transits from the times of Kepler and Horrox (= Horrocks), the goals and methodologies of the observations, and some specifics about the parties. Readers were told how to prepare the smoked glass, and the times that the transit would be visible (Venus crossing the Sun's face, 1882).

This transit would find more cooperation among the various nations of the world, and *The New York Times* therefore would also report on foreign expeditions, as well as those sited on American soil.

On 30 January 1881, almost two years before the 1882 transit, readers of *The New York Times* could learn that the French Academy of Sciences had appointed an international Commission which, under the leadership of Monsieur Dumas, would prepare for the expeditions (Scientific gossip, 1881a). On 12 June 1881 one might further learn that the French Government was sending a scientific expedition to Cape Horn to study terrestrial magnetism, and this expedition would be accompanied by another party which would study the transit of Venus (Scientific gossip, 1881b). On 6 November 1881, French leadership in international cooperation in the observations of this transit became more apparent. Dumas, the President of the International Commission, would send instructions to all participating astronomers and observatories (Scientific gossip, 1881c).

On 28 November 1881 *The New York Times* reprinted an article from the Toronto (Canada) *Globe* of 25 November which expressed the opinion that their city could provide a favorable site for transit studies. The Canadians saw this as an opportunity to improve their standing in the astronomical scientific community (Preparing for the transit of Venus, 1881). On 14 November 1882 a reader could learn that Professor McCloud and Mr Payne were going to Winnipeg, Canada, to observe the transit (The transit of Venus, 1882c). On 6 December 1882 arrangements made at Kingston, Ontario, for observations at Queen's University Observatory were published (Little hope of seeing the transit, 1882). The next day a reader would learn that Canada was mostly cloudy during the time of the transit but occasional observations were made through gaps in the clouds (Across the Sun's face, 1882). On the other hand, on 29 December 1882 there was a 2-line article: "Toronto, Dec.28. -- Reports from various Canadian stations as to the transit of Venus have been received here. With one exception only they are considered very accurate." (The transit of Venus, 1882k).

On 12 April 1882 *The New York Times* reported that the French Government would send eight expeditions to study the transit, four to the northern hemisphere and four to the southern (Current foreign topics, 1882a). On 7 December 1882 it was reported that preparations for viewing the transit in Paris were fruitless due to the dark cloud cover (Across the Sun's face, 1882). On 23 December 1882 one could read that the results from the French party near the Straits of Magellan were awaited "… with great anxiety ..." (The late transit of Venus, 1882). Then on 4 January 1883 it was reported that "The French Commission telegraphed the Académie des Sciences that the results obtained in South America had exceeded all its hopes." (The South American states, 1883).

On 7 December 1882, in an article previously cited, *The New York Times* published preliminary results already received from many countries. It was noted that in London clouds and snow made obser-vations at the Greenwich Observatory impossible. The British had more favorable conditions at Cork, Durban and Portsmouth. At Penzance they could see the transit for two hours. At other English venues clouds interfered with all of the observations. However, there were good observing conditions in Cape Town, South Africa (Across the Sun's face, 1882).

On 17 September 1882 *The New York Times* noted that there would be four expeditions from foreign nations going to the western hemisphere: one to Costa Rica, one to the Straits of Magellan and two to the United States (Scientific gossip, 1882b). On 23 December 1882 a reader would learn that results from the Straits of Magellan were still awaited (The late transit of Venus, 1882).

The Belgians were quite successful in South America. One could have read in *The New York Times* on both 14 December 1882 (The transit of Venus, 1882j) and 4 January 1883 (The South American States, 1883) that they had made 606 observations.

On 6 December 1882 *The New York Times* reported that "The Mexican government has supplied instruments to scientific societies throughout the republic for making observations." (Little hope of seeing the transit, 1882).

On 10 December 1882 on the front page there was a short item received from Havana on the previous day:

> At Manzanillo both the internal contacts of Venus were observed. The external contacts were not seen on account of the interposition of clouds. The ingress of the planet was observed in Porto Rico, but her egress was hidden by clouds. (The transit of Venus, 1882g).

The transit of Venus of 1882 was the first where the United States, as a sovereign nation, could host scientific expeditions from other countries.

On 19 June 1882 *The New York Times* revealed that the Germans had selected Aiken, South Carolina, as one of its sites for the upcoming transit. Members from their Royal Observatory would arrive in late October (General notes, 1882b). The next month, in an article of 9 July readers would learn that the Germans also planned to observe from a second, as yet unnamed, site in the USA (Scientific gossip, 1882a). On 30 August 1882, it was reported that there would actually be four German expeditions going to the western hemisphere, and the two in the United States would be based at the afore-mentioned site in South Carolina and in Connecticut. Each German party would consist of "… two astronomers, a student, and an assistant." (Current foreign topics, 1882b). On 3 November 1882 an article announced the arrival of a German party that would observe from Hartford, Connecticut. The members of the party were identified (Arrival of German astronomers, 1882). On 6 December 1882 there was an item about the preparations of the Germans at Hartford that were made on the previous day, the last before the transit. Hopes were expressed for good weather:

> If the day is clear three telescopic observations of the contacts at ingress and egress will be made at the station of the astronomers at Trinity College, two by the Germans and one with the college refractor. (Little hope of seeing the transit, 1882).

Apparently there was some success at the Hartford site, as the Germans participated in the discussion of whether or not there was an atmosphere on Venus. On 8 December 1882 *The New York Times* reported that

> The German observers at Hartford are quoted as saying affirmatively that there were no indications of an atmosphere. (Article 2 – No title, 1882).

On 7 December 1882 *The New York Times* printed an article regarding the parties present in the San Antonio, Texas, area. Besides an American party, headed by Professor Asaph Hall, there was a Belgian party, headed by a Professor Houzeau (whose name was incorrectly reported as "Houzean"). The first two contacts were missed due to cloudy conditions but the sky cleared and observations were possible later. It was noted that Houzeau and his three assistants took no photographs, but they did obtain 120 (heliometers) measurements, which they wanted to compare with observations made by the Belgian party in Chile. Houzeau took his work very seriously during the transit, allowing no visitors, locking his gate, and using police to "… prevent an invasion …" However, he was quite cordial after the transit (see Fair success in Texas, 1882).

The United States also hosted an expedition party from France. On 6 August 1882 *The New York Times* related that

> The Secretary of War has granted permission to a party of French scientists to occupy Fort Marco, at St. Augustine, Florida, for the purpose of making observations of the transit of Venus. (Notes from Washington, 1882a).

On 8 December 1882 it was reported that the French party had clear weather and "… obtained good and complete observations …" (Watching the transit, 1882).

All four American government-subsidized observation sites in the northern hemisphere were within the boundaries of the United States and its territories. Besides these, there were many other observatories and private individuals who took an interest in the event and made what contributions they could to the effort.

The official northern sites for the Americans listed in *The New York Times* on 7 December 1882, included the Naval Observatory at Washington, D.C., under William Harkness; San Antonio (Texas), headed by Professor Asaph Hall; Fort Selden (New Mexico), headed by Professor Davidson; and Cedar Keys (Florida), headed by Professor Eastman (The Government's work, 1882). On 6 December 1882 there was an article about the preparations going on at several observatories around the continent. The Naval Observatory had prepared a similar set-up to that used by the American expeditions for the 1874 transit. A long-frame structure to convey the light to the camera had been built onto the side of the building. The apparatus was listed and the article stated that, with the cooperation of the weather, a successful observation was expected (Little hope of seeing the transit, 1882). However, on 7 December readers would learn that the weather did not cooperate, and although some measurements were taken and some photographs were obtained, overall the results were disappointing (The Government's work, 1882). Then on the next day Professor Davidson's report on the great success in New Mexico was published (Watching the transit, 1882). On 23 December 1882 readers learned that all of the government-subsidized parties employed the same apparatus and arrangements that had been used in 1874, and all of the parties, except for the one based in Washington, D.C., were quite successful (The late transit of Venus, 1882).

In addition, other observatories, colleges and individuals around the country participated in these efforts. Professor C.A. Young, who was active in keeping the public apprised of the various observations of this event, participated himself, using the facilities at Princeton University. When fears were reported by *The New York Times* on 12 November 1882 that a fire at a small building near the observatory at the University would not permit him to take any photographs (The transit of Venus, 1882b), Young quickly responded (on the 14th) stating that all had been restored and his party would be ready (Letters to the Editor – Messrs. Harper and Mr. Pym, 1882). Then on 7 December 1882 an article appeared which reported successful observations at Princeton. Equipment similar to that employed by the 1874 expeditions was used, as well as several other telescopes, and the Government provided photographic plates and emulsion. All four contacts were seen, and Young also conducted a spectroscopic examination of Venus' atmosphere (Fine results at Princeton, 1882).

On 22 November 1882 *The New York Times* reported that Harvard University did not expect to take any particular notice of the 1882 transit of Venus (A large spot on the Sun, 1882). However, on 7 December 1882 readers learned that many observations were made and data were collected there. All four contacts were observed (Good work at Harvard, 1882).

The New York Times reported on 16 July 1883 that the Litchfield Observatory in New York failed totally to observe the 1882 transit due to "… inexorable clouds ..." (Making celestial charts, 1883).

According to a short item in *The New York Times* on 4 December 1882, Lafayette College in Pennsylvania would make observations as directed by the Naval Department (The transit of Venus, 1882f). On 11 December 1882 Professor Coffin reported that all four contacts were seen. There was also mention of the 'black drop' effect that was apparent, and of a ring of light (atmosphere?) seen around the planet before the third contact (Observations of the transit, 1882).

On 8 December (Article 2 – No title, 1882) and 9 December of 1882 (The spot on Venus, 1882) *The New York Times* published Professor Langley's observations at Pittsburgh of a peculiar bright spot on the planet when it was halfway onto the disk of the Sun. No explanation was proposed. Langley was partially successful in his observation of the transit.

On 2 May 1880 *The New York Times* reported that the Winchester Observatory at Yale University ordered a heliometer that would be completed prior to the 1882 transit (Uniformity in time, 1880). On 3 December (The transit of Venus, 1882e) and 5 December of 1882 (The Yale astronomers, 1882) the members of their scientific party were identified and their preparations for the transit were described.

On 6 December 1882 *The New York Times* reported that Vassar College was making arrangements in Poughkeepsie, New York, to observe and photograph the upcoming transit (Little hope of seeing the transit, 1882). This party was led by Maria Mitchell, whose application to participate in an overseas expedition had been denied because of her gender (Sheehan and Westfall, 2004: 279).

On 7 December 1882 *The New York Times* described the efforts made at the Central High School in Philadelphia. Contacts were observed, but due to hazy conditions photographic, spectroscopic and micrometric observations were not attempted (Seen through a hazy sky, 1882).

The New York Times on 7 December 1882 reported that the four American transit of Venus parties in the southern hemisphere were based at Santa Cruz, (Patagonia), under Lieutenant Samuel W. Very; at the Cape of Good Hope (South Africa), under Professor Simon Newcomb; at Cordova (Chile), under Professor Boss; and at Auckland (New Zealand), under Professor Edwin Smith (The Government's work, 1882).

On 17 August 1882 *The New York Times* announced in its regular feature "Notes from Washington" that Lieutenant Samuel W. Very of the Navy would lead the transit party to Santa Cruz, Patagonia. They would leave from New York in a few days in the flagship *Brooklyn* (Notes from Washington, 1882b). A report was made on 4 January 1883 that observations there were marred due to rain (South American states, 1883). On 6 February 1883 readers learned of the progress of the returning party, which by then had reached Montevideo (Naval intelligence, 1883).

Reports on the expedition to Cape Town initially related to updates on the personnel. In the regular *New York Times* feature "Army and Navy News" readers learned on 15 August 1882 of the appointment of Lieutenant Thomas L. Casey, Jr., Engineer Corps to the Cape Town party (Army and Navy news, 1882a), and on 7 September 1882 of the appointment of Lieutenant E.W. Sturdy as Newcomb's temporary replacement as Superintendent of the Nautical Almanac Office during the latter's absence (Army and Navy news, 1882b). On 19 September 1882 *The New York Times* announced the departure of the expedition for the Cape of Good Hope on the steamship *Parthia* (Notes from the capitol, 1882). Two days later it was related that this, the first of the southern expeditions to leave for its site, would arrive at the Cape Town Observatory on about 1 November (The transit of Venus, 1882a). On 7 October 1882 it was announced that Professor Newcomb and his party left on the second leg of their journey, from Southampton to the Cape of Good Hope, on the steamer *Durban* (Current foreign topics, 1882c), while on 8 January 1883 *The New*

York Times related the success of the party, which reported good observations of the internal contacts. They obtained 236 photographs, more than 200 of which were measurable. They had landed at Plymouth on the previous day upon their return to the United States (Current foreign topics, 1883). In "Army and Navy Matters" on 12 September 1883, it was reported that Simon Newcomb had returned to the USA and resumed his duties at the Nautical Almanac Office (National capitol topics, 1883).

A second South American expedition was sent to Valparaiso in Chile. On 21 September 1882 it was noted in *The New York Times* that members at both of the South American venues selected would be able to observe the entire transit—weather permitting (The transit of Venus, 1882a). On 26 October 1882 readers learned that this expedition had departed from the USA on October 12 (South American affairs, 1882), and on 13 December 1882 there was the following short report:

> Panama, Dec. 12 -- Prof. Boss writes from Santiago, under date of the 9th inst., that the American observations of the transit of Venus were completely successful. The weather was splendid, and all the arrangements were carried out. The four contacts were observed, and the photographs and measurements taken were all satisfactory. (The transit of Venus, 1882h).

On 3 February 1883 there was an article subsequent to the return of Professor Lewis, who accompanied Boss, with his party. One learned of the courtesies extended them both by General Maturana of the Army as well as by the President of Chile. The circumstances surrounding the successful transit observations were described (The transit of Venus in Chili, 1883).

The remaining American expedition to foreign parts was sent to Auckland on the North Island of New Zealand. On 18 August 1882 *The New York Times* published the names of the members of this party, which was under the leadership of Edwin Smith from the Coast Survey. They would sail from San Francisco on 1 September (Notes from Washington, 1882c). On 3 September 1882, the reader learned that Smith would proceed to Japan after completing his transit work to make "… pendulum observations ..." (Notes from Washington, 1882d). In the article of 21 September 1882 which summarized the expeditions to the southern hemisphere, one would learn that only the egress contacts would be visible in New Zealand (The transit of Venus, 1882a). The summary article of 23 December 1882 told readers that the New Zealand party was successful in observing the last two contacts and that it took more than 200 photographs (The late transit of Venus, 1882).

The most complete article found in *The New York Times* dealing with the 1882 transit of Venus was printed after the event, on 23 December 1882. In this article of four-plus columns there was a summary of the goals and means of the various expeditions, and the following information summarizing the methods used, and the varying degrees of success in observing contacts. The following summary listing, including the "Key", is adapted from this article (ibid.):

KEY:

1,2,3,4 = numbers denoting contacts observed
P = photographs taken using standardized American methods (with the number of images in brackets)
P* = photographs taken by different method (ditto)
h = heliometer measures taken
h* = equivalent measures to the heliometers; but different means used
s = spectroscopic observations
p = photometric observations
m = micrometer measures of the planet's diameter

CANADIAN SITES:

1. Ottawa, Canada (1, 2, 3, 4)
2. Kingston, Canada (2, 3, 4)

US SITES:

3. Cambridge, Mass. (1, 2, 3, 4, s, p, m; several observers)
4. Providence, R.I. (2, P* (23))
5. Amherst, Mass. (3, 4)
6. South Hadley, Mass. (3, 4, s)
7. Hartford, Conn. (2, 3, 4, h, m; German Party)
8. New Haven, Conn. (1, 2, 3, 4, P* (150), h, m; several observers)
9. Helderburg Mountain, N.Y. (1, 2)
10. West Point, N.Y. (1, 2, 3, 4)
11. Poughkeepsie, N.Y. (3, 4, P* (9))
12. Brooklyn, N.Y. (1, 2, 3)
13. Columbia College, N.Y. (2, 3, 4)
14. Western Union Building, New York City (1, 2, 3, 4)
15. University City of New York, New York City (1, 2, 3, 4)
16. Elizabeth, N.J. (2, 3, 4)
17. Princeton, N.J. (1, 2, 3, 4, P (188), s, m; several observers)
18. Philadelphia, Penn. (1, 2, 3, 4)
19. Easton, Penn. (1, 2, 3, 4)
20. Allegheny, Penn. (1, 2, (?), s, m)
21. Pittsburg, Penn. (2, 3)
22. Wilmington, Del. (1, 2)
23. Baltimore, Md. (2, 3, 4; several observers)
24. Annapolis, Md. (2, 3, 4)
25. Naval Observatory, Washington, D.C. (1, 2, 3, 4, P (53), m; several observers)
26. Coast Survey, Washington, D.C. (2, 3, 4; several observers)
27. Signal Service, Washington, D.C. (1, 2, 3, 4)
28. Charlottesville, Va. (2, 3, 4)
29. Aiken, S.C. (3, 4, h, m; German Party)
30. St. Augustine, Fla. (1, 2, 3, 4, h*, P*(200), m; French Party)
31. Cedar Keys, Fla. (2, 3, 4, P (180), m; Government Party)
32. Chicago, Ill. (1, 2; several observers)
33. Madison, Wisc. (1, 2)
34. Northfield, Minn. (3, m)
35. Iowa City, Iowa (1, 2)
36. Ann Arbor, Mich. (4, m)
37. San Antonio, Texas (3, 4, P (200); Government Party)
38. San Antonio, Texas (3, 4, h*, m; Belgian Party)
39. Fort Selden, New Mexico (1, 2, 3, 4, P (216), m; Government Party)
40. Lick Observatory, California (2, 4, P (147), m)

FOREIGN SITES:

Potsdam, Prussia (1, 2, P*, s, m)
Jamaica (1, 2, 3, 4)
Pueblo, Mexico (1, 2, 3, 4, h*; French Party)
Chapultepec, Mexico (No contacts, P*(13))

Cape Town, South Africa (1, 2, P (?), American Government Party)
Durham, South Africa (1, 2)
Tasmania (3, 4, P (?); American Government Party)
Melbourne, Australia (3, 4, P (236[?]); American Government Party)
Santiago, Chile (completely successful, P (?); American Government Party)
Santiago, Chile (completely successful, h*, m; Belgian Party)

A comparison of this summary of Venus transit expeditions with information gleaned over the previous months would reveal that much of the information had been available to the public in previous articles, so interested readers could have followed and compared the relative successes of the different parties around the world and within the boundaries of their own countries. However, care was required as some of the information provided was wrong. For instance, neither Tasmania nor Melbourne, in Australia, hosted American transit of Venus parties in 1882—although in 1874 there were two different American parties in Tasmania, one in Hobart and the other in Campbell Town (see Orchiston, 2004; Orchiston and Buchanan, 1993; 2004).

Over the following months one would find other articles reflecting a degree of sustained interest in these scientific endeavors.

On the date of the transit itself, 6 December 1882, *The New York Times* printed an instance of a negative judgment on the various expeditions. The writer opined that Venus transits were just excuses for astronomers to request funds so that they could visit exotic places round the globe. The writer felt that during the 1874 transit the public had been misled when it was implied that transits only occurred about once in a century,[3] and he sarcastically remarked:

> No matter where an astronomer might live, the transit was never visible within a thousand miles of his home. The New-York astronomers had to go to Pekin; the Chinese astronomers had to go to Australia; and the Australian astronomers had to go to Europe. (The transit, 1882).[4]

On 17 December 1882 *The New York Times* published a compliment to American astronomers from the British popularizer of astronomer, Richard A. Proctor, reprinted from the *Gentlemen's Magazine*. Proctor was impressed with the Americans' use of photography and felt the results, once fully interpreted, would be very useful (A compliment to American astronomers, 1882).

On 31 December 1882, *The New York Times* reprinted an item from *Nature* which expressed the sentiment that the recent transits had awakened the intellectual world from "… the slumber of the ages ..." (The observations of 2004, 1882).

On 18 January 1883 *The New York Times* published a short item describing a social event at Delmonico's restaurant:

> There was a handsome display of flowers, the most notable of which was a design representing the transit of Venus. (The sheriff's jury, 1883).

On 10 February 1883 *The New York Times* printed another negative opinion on the profession of astronomy:

> An astronomer is a man who is sent at the cost of the nation on scientific picnics in connection with the transits of Venus, and who employs his time in between successive transits in discovering new asteroids. (Wiggins, 1883).

The New York Times on 13 June 1883 printed a short review of a new book by Richard A. Proctor, *Mysteries of Time and Space*, which included a chapter on the transits of Venus (see New publications, 1883).

Then on 27 June 1883 *The New York Times* printed the obituary of Stephen Alexander. Following the summary of his career as an educator and author was the following statement:

> For several years the aged astronomer had devoted his leisure hours to the study of the heavens from a small observatory in the rear of his residence, and there he observed the recent transit of Venus. (Obituary …, 1883).

4 DISCUSSION

During most of its existence in the second half of the nineteenth century *The New York Times* was typically only eight pages in length. The number of articles present in such a small publication that dealt with the 1882 transit of Venus was indicative of a significant interest in the subject, fostered by the popular appeal of the 1874 transit program (e.g. see Cottam et al., 2011). The reader was regularly updated on the failures and successes of the various 1882 parties—American and foreign—at the various venues. *The New York Times* printed a summary article later that year allowing its readers to compare achievements, and the means to these achievements. Later, after the transit, there were articles mentioning subsequent lectures and publications that might satisfy some lingering public interest in transits of Venus.

5 CONCLUSION

In the wake of the event of 1874 the general public in the USA was knowledgeable about the science and significance of transits of Venus. *The New York Times* delivered informative articles before, during and after the 1882 event. Readers were reminded of relevant lectures, and notified of publications written with a non-professional audience in mind. Letters to the Editor would reflect varying degrees of support in these costly endeavors. There was general interest in *The New York Times* articles regarding the various expeditions of different nationalities around the world, but in 1882 there was particular interest in the parties on their own soil, both American and foreign. The American public garnered pride in their country's abilities to contribute. As there would not be another transit of Venus for more than a century it was now to be seen if there was a lingering interest in other astronomical topics. Besides articles describing particular events such as eclipses and meteor showers *The New York Times* would begin to provide regular features on what celestial objects might be seen in the night sky. Such articles might contribute to the sustained interest and support of the public for future astronomical endeavors.

6 NOTES

1 Note that in addition to Horrocks, the transit was also observed by his friend, William Crabtree, (1610–1644; e.g. see Chapman, 2005).

2 Harkness' value was based on more than just the 1874 and 1882 transit results. As Dick et al. (1998: 247) relate, Harkness finally realized that the solar parallax was not an independent constant and treating it as such merely produced a mass of discordant values. In fact, the solar parallax

> … was inextricably entwined with lunar parallax, the constants of precession and nutation, the parallactic inequality of the Moon, the masses of the Earth and Moon, and the velocity of light, among others. He set about treating these constants as a system …

The result of his investigation was the value listed here.

3 But this is a totally unfair statement as numerous instances can be found in *The New York Times* where a full explanation was given of the frequency of transits.

4 These statements are equally ludicrous: no Chinese astronomers went to Australia to observe the 1874 or 1882 transit of Venus, and no Australian astronomers went to Europe to make their observations.

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ON THE RELIABILITY OF HAN DYNASTY SOLAR ECLIPSE RECORDS

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Abstract: The veracity of early Chinese records of astronomical observations has been questioned, principally based on two early studies from the 1950s, which suggested that political motives may have led scholar-officials at court to fabricate astral omens. Here I revisit the Han Dynasty (206 BCE-220 CE) solar eclipse reports to determine whether the charge has merit for those first four centuries of the imperial period. All 127 dated solar eclipses reported in the official sources are checked for accuracy against the "Five Millennium Catalog of Solar Eclipses" produced by Espenak and Meeus (2009). The Han Dynasty records prove remarkably accurate. Copyists' errors do occur, but there are only rare instances of totally erroneous reports, none of which is provably the result of politically-motivated manipulation.

Keywords: ancient China, Han Dynasty, solar eclipses, reliability.

1 INTRODUCTION

Studies by Wolfram Eberhard (1950) and Hans Bielenstein (1957; 1970) purporting to establish that early Chinese records of portents, including astronomical observations, were manipulated for political reasons continue to be cited as authoritative (e.g. see Steele, 2003; Stephenson, 1997). Surprisingly, until quite recently only their statistical methodology has been subjected to critical evaluation, despite the fact that in Eberhard's analysis omens of all kinds (astral anomalies, freakish weather, monstrous births, prodigies, etc.) were indiscriminately lumped together (Kern, 2000). Although Eberhard recognized that virtually any Han Dynasty (206 BCE-220 CE) official was entitled to report an omen and opine about its significance, he did not attempt to analyze separately the reliability of astronomical observations, or even just those emanating from the office of Grand Scribe-Astrologer (*Taishigong*). Nor was Bielenstein's analysis of the frequency of omens and portents methodologically adequate to support the conclusions he drew (Kern, 2000). In 1955, Homer H. Dubs (1938-1955, I: 289; III: 552) had already observed that "… during long periods all plainly visible eclipses were reported, while during other periods entire groups of eclipses were missed." In his analysis Bielenstein had assumed twenty years as the average length of reign of the Emperor, so that Dubs' observation already called into question Bielenstein's assertion about the deliberate suppression of reports during the reigns of individual rulers. Recently, Martin Kern has shown that contemporary manipulation of the records can be ruled out:

> Bielenstein's widely adopted conclusion that in Western Han times, such signs were invariably presented – or made up – by court officials in order to subtly admonish their ruler is too simple and flawed by its mechanical and ahistorical nature … First, Bielenstein, like other scholars, has been concerned only with negative and not with auspicious omens; yet only balancing the two will provide accurate figures of omen distribution … Second, the overall quotient, resulting from the number of omen reports relative to the years of a ruler, must be differentiated with respect to different phases of a ruler's reign … during which we observe shifts in the practice and ideology of rulership. Third, when considering the individual omens which are recorded in our historical sources, we need to take into account the historical moment at which a particular omen definition was actually determined as being calamitous … such interpretations often postdate by decades the reign during which the omen originally appeared; therefore, they cannot have been intended as admonishing the ruler whom they might have concerned directly. (Kern, 2000: 3).

Kern's second and third criticisms effectively vitiate Eberhard's and Bielenstein's conclusions. As a result I, too, no longer accept them.

Other scholars came to precisely the opposite conclusion from Eberhard and Bielenstein regarding the astronomical reports. For example, Needham and Wang (1959: 408) concluded:

> … certainly there was no question of a "fabrication" of an extraordinary event … occasionally there may have been a distortion of date for political reasons, as in the conjunction of –205 … but more often the records, when recalculated to-day, are found to be quite reliable, e.g., the occultation of Mars by the moon in –69 and of Venus in +361." (cf. Kiang, 1984; *Han shu* 26.1301).

Dubs was alluding here to the erroneous date for a planetary alignment recorded as "… 10th month of the First Year of Emperor Gaozu (206 BCE) …" in the *History of the Former Han Dynasty*. The actual planetary alignment occurred the following year in May 205 BCE. However, the *Han shu* date has long been known to be plainly impossible and an obvious interpolation. A century after the actual event, Sima Qian, in his *Grand Scribe's Records* (ca 100 BCE), had only written "… when Han arose." Whatever its cause, in the 5th century Gao Yun (390–487 CE) had already ridiculed the misdating, mordantly observing that in the 10th month the Sun would have been in Tail~Winnowing Basket (lodges #6-7, Sco-Sgr), not in Eastern Well (lodge #22, Gem) where the alignment actually occurred (*Wei shu* 48.1068).

It is sometimes even claimed that the Han astronomers did not believe that solar eclipses could occur only at the new moon, but this is flatly contradicted by both Liu Xiang (ca 77–6 BCE) and Zhang Heng (78–139 CE). In fact, Wang Chong (27–ca. 100 CE), who was not proficient in astronomy, even argued *against* the correct view (Needham and Wang 1959: 411, 414). With regard to 'doctored' reports, Dubs remarked:

> … it is probable that a solar eclipse [in 186 BCE] was

> fabricated in the early years of the Han as a warning to the unpopular Empress Lü (d. 180 BCE), and … certain observations of partial solar eclipses were not recorded during the reign of the popular Emperor Hsiao-Wen. (Needham and Wang 1959: 408).

Discussing the same false eclipse record, Rafe de Crespigny said:

> As Dubs remarks in discussing a similar false report of an eclipse in 184 [*sic*] B.C., the reporting of such a false portent, should it be discovered, would almost certainly be punished by death. It was most unusual for a false eclipse to be reported, and even in the second part of the reign of Emperor Huan, when criticism by portent was at its height, the critics contented themselves with the eclipses that actually took place. (de Crespigny, 1976: 45, n. 15).

Dubs had observed that, "… according to Chinese law it was a serious and capital crime to report falsely a prodigy (such as an eclipse of the sun) …" and cited an example of a high official who was imprisoned and executed "… for having falsely reported a lesser calamity – that a fire had damaged government buildings." (Dubs 1938-1955, I: 212; III: 555). Bielenstein, too. held that "… the records, while never falsified (except in the case of the empress just mentioned), were often left incomplete." (Needham and Wang 1959: 418).

As Martin Kern has shown, however, there is no evidence to suggest that even the erroneous solar eclipse report of 186 BCE was falsely reported *at the time*. The much greater likelihood is that it was interpolated later, so that Dubs, de Crespigny and Bielenstein were all mistaken. Sima Qian, in his review of early Han Dynasty astral anomalies in the "Treatise on the Celestial Offices" (*Tianguanshu*) in his *Grand Scribe's Records* (ca 100 BCE), says only: "… when the Lü clan rebelled, the sun was eclipsed and it grew dark in the daytime." This is a reference to the total eclipse of 4 March 181 (Table 1, #6), the only one recorded by Sima Qian in his account of the Empress Dowager Lü's reign:

> … on the *jichou* day [4 Mar] the sun was eclipsed and during the day it became dark. The Empress Dowager hated it and was displeased. She said to her attendants, "this is because of me". (Nienhauser et al., 2002).

The dubious report of yet another eclipse in the Empress Dowager's reign is obviously an interpolation postdating Sima Qian's *Grand Scribe's Records*. The record in the *History of the Former Han Dynasty*, compiled a century and a half after Sima Qian, is the sole demonstrably false report of an eclipse suspected of being politically motivated during the 400 years of the Han dynasty.

As Rafe de Crespigny noted, it was in the Later Han Dynasty that political portentology reached peak intensity. Nevertheless, F.R. Stephenson (1997: 230) stressed that

> … it should be emphasized that throughout Chinese history from the Han onwards, recorded dates of solar eclipses, when converted to the Julian calendar, usually agree precisely with the calculated dates of these phenomena."

2 ECLIPSE RECORDS FROM THE WESTERN (FORMER) HAN DYNASTY (206 BCE–5 CE)

The extreme destruction visited on the hated Qin Dynasty (221–206 BCE) by the rebellions that brought it down included the massacre of the populace of Xianyang and the burning of the capital, together with its palaces, administrative archives, and libraries. History records that Xianyang, founded in 350 BCE, burned for three months. This catastrophe, following the holocaust of pre-Qin writings instigated by Chancellor Li Si in 213 BCE, would have seriously hampered early Han efforts to reconstruct the imperial administration. Consequently, many Qin laws and ordinances remained in effect for years, including, at least initially, the prohibition against private ownership of books. Surprisingly, even the Qin calendar continued in use for a century before it was finally replaced in 104 BCE, after completion of lengthy work by an imperial commission (Cullen, 1993). Then, too, the Han founder, Liu Bang (ca 250–195 BCE) was a commoner, together with many of his supporters and military commanders, so that recruiting qualified men and reconstituting the administration of the empire presented a formidable challenge. This may explain why, in his account of the history of the office of Grand-Scribe Astrologer, Sima Qian mentions no holder of that office prior to the appointment of his father in 140 BCE. Under the circumstances, it is fortunate that any reliable astronomical observations at all survive from the first half of the 2nd century BCE.

Rather than simply relying on methodologically-flawed statistical studies, it seems advisable to probe the eclipse records themselves. In Table 1 below are catalogued all 127 solar eclipses recorded during the Han Dynasty in the standard historical sources: *Sima Qian's* the *Grand Scribe's Records* (ca 100 BCE), the *History of the Former Han Dynasty* (*Han shu*) compiled by Ban Gu (32–92 CE), and the *History of the Later Han Dynasty* (*Hou Han shu*) compiled by Fan Ye (398–445 CE).

In his study a half-century ago Homer H. Dubs analyzed all the solar eclipses during the Western Han Dynasty (1938-1955, III: 546-59). Dubs studied 98 eclipses potentially visible from some part of China during the 200 years of the Western Han (206 BCE-8 CE) and the two decades of the usurper Wang Mang's Xin Dynasty (9-23 CE). Fifteen of these were either invisible at the Han capital or too small to be observed. Of the remaining eighty-three eclipses, fifty-five, or two-thirds, were recorded in the official sources. More than two-thirds of these again, or thirty-eight eclipses, were recorded correctly. Dubs's figure of two-thirds coverage of observed and recorded eclipses in the Western Han period agrees favourably with the 68% result computed for Chinese observation of transient objects during the 1,000 years from 600 to 1600 (Strom, 2011). Dubs concluded:

> Considering the length of time since the HS [*Han shu*] was written in the first century A.D., and the many opportunities for mistakes, both by astronomers and annalists before the *HS* was compiled and the opportunities for errors in transmitting the *HS* text, this is an excellent record. Fourteen other eclipses can be fitted into the actual dates, usually by only slight changes in the text. Only at most three recordings are hopelessly erroneous; two of these are due to errors in transmission of the data. When we consider how very easy it is to write mistakenly the number of a month or the cyclical day, the essential correctness of the *HS* is a marked evidence of the care that was exercised in compiling it and in preserving and copying faithfully its

text (Dubs 1938–55, III: 551).

Several of Dubs's specific conclusions are worth noting; namely, (i) in several cases it can be shown that the dating errors occurred before the *HS* was compiled; (ii) in other cases, minor changes have plainly been made since the *HS* was composed, as shown by quotations in other texts; (iii) the capital of Chang'an was *not* the only place from which eclipses were observed; (iv) during long periods all plainly-visible eclipses were reported (over half a century in two cases), while during other periods entire groups of eclipses were missed; (v) the Chinese clearly used special techniques to observe eclipses and must have kept a watch in advance, allowing them to spot eclipses of small magnitude; (vi) differences in the recorded magnitudes of eclipses indicate that those found in the "Treatise on the Five Elemental-Phases" (*Wu xing zhi*) were observed by astronomers at the capital, while some in the "Basic Annals" (*Benji*) were witnessed outside the capital, although in a few cases the "Treatise" also specifically identifies certain reports as coming from elsewhere; (vii) there is no evidence that the Chinese calculated any eclipse recorded during the Former Han period; (viii) based on the reported positions of the Sun among the twenty-eight lodges, it is clear these have been calculated based on the recorded dates of the eclipses, some of these calculations possibly having been done by Liu Xiang (77 BCE–6 CE) in about 27 BCE, by which time the dates of many eclipses were probably already in error.

Table 1: Han Dynasty Eclipses Recoded in the Official Souces.

	Western Han Eclipses (–205 to 5 CE)				Comments
	Emperor	Dubs, *History of the Former Han Dynasty* vol., page	Espenak & Meeus eclipse mag. (*ital.* Dubs)	Espenak & Meeus number	Notations in order: reign year, month, cyclical day number (conversion of Chinese dates for the Common Era are given by Academia Sinica, "2000-Year Chinese-Western Calendar Conversion" http://sinocal.sinica.edu.tw/; 晦 *hui* "last day of the month" or 朔 *shuo* "first day of the month"; lodge location if given (known to be interpolated); "^{d}" are Chinese *du*, or 0.9856 degrees (for the boundary stars of the 28 lodges, see Cullen [2011]); [S + page] eclipse studied in Stephenson (1997); [K + page] eclipse studied in Kawabata et al. (2003); ✓record matches Espenak & Meeus, http://eclipse.gsfc.nasa.gov/SEcat5/SEcatalog.html
	Gaozu				
WH#1	–204 Dec 20	I, 165, i	[~*0.51*]	04277	III.10 甲戌 [11] (DIPPER 20^{d}); observable at Chang-an. ✓
2	–200 Oct 8	I, 165, ii	0.284 prov. report?	04286	III.11 癸卯[40]晦; should be VI.8 癸未[20]; too small to be observable at Chang'an, but mag. 0.467 at Changchun, Yan Province (43.8134° N 125.2905° E); likely provincial report. (?)
3	–197 Aug 7	I, 166, iii	0.957	04292	IX.6 乙未[32]晦 'total' (SPREAD 13^{d}) [S 238][K 6]; observable at Chang'an. ✓
	Huidi				
4	–191 Sep 29	I, 188, i	0.223	04304	VII.5 辛丑[38]朔; scribal error, should be III.9 and 酉 for 丑; observable at Chang'an. (?)
5	–187 Jul 17	I, 189, ii	0.926/1.007	04315	VII.5 丁卯[4]晦; 'total' and 'almost total' (STARS 'initial degrees') [S 234][K 6]; observable at Chang'an, but totality in a path NW to SE across western China confirms divergent 'total' and 'almost total' comments. ✓
	Empress Lu				
6	–180 Mar 4	I, 212, ii	1.013	04331	VII.1 己丑[26]晦 'total' (HALL 13^{d}) [S 234] [K 6]; observable at Chang'an. ✓
	Wendi				
7	–177 Jan 2	I, 284, i	*[~0.20]*	04337	II.11 癸卯[40]晦 (GIRL 1^{d}); observable at Chang'an. ✓
8	–177 Dec 22	I, 284, iii	0.385	04339	III.10 丁酉[34]晦 (DIPPER 23^{d}); observable at Chang'an. ✓
9	–175 Jun 6	I, 284, iv	0.276	04342	III.11 丁卯[4]晦 (VOID 8^{d}), possibly IV.5 辛卯[28]朔; observable at Chang'an.
10	–160 Aug 17	I, 286, v	[*0.349]* prov. report?	04379	*Houyuan* IV.4 丙寅[3]晦 (WELL 13^{d}) possibly III.6 庚申[57]晦; unobservable before sunrise at Chang'an, but mag. 0.349 at Zhangye in Beidi Commandery (38.8929° N 100.5054° E). (?)
11	–154 Oct 10	I, 286, vi	0.244	04395	VII.1 辛未[8]朔 ; should be Jingdi 2nd year, 9th month, 乙酉[22]晦; small partial eclipse observable at Chang'an. ✓
	Jingdi				
12	–153 Apr 5	I, 335, i	0.803 prov. report?	04396	III.2 壬子[49]晦 (STOMACH 4^{d}); unobservable at Chang'an before sunrise, confirmed provincial report. ✓
13	–149 Jan 22	I, 336, iii	0.691	04405	VII.11 庚寅[27]晦 (VOID 9^{d}); observable at Chang'an. ✓
14	–148 Jun 7	I, 336, iv	0.427	04408	*Zhongyuan* I.12 甲寅[51]晦; possibly I.5 壬子[49]晦; sunset eclipse at Chang'an. ✓

15	–146 Nov 10	I, 337, vi	0.763 prov. report	04413	III.9 戊戌[35]晦 ‘almost total’ (TAIL 9^d) [S 235] [K 6]; observable at Chang’an, but mag. 0.947 at Zhangshan Kingdom; probable provincial report of near totality. ✓
16	–144 Mar 26	I, 336, vii	0.506 prov. report?	04417	IV.10 戊午 [55]晦; possibly V.2 庚申[57]晦; no eclipse at Chang’an, mag. 0.506 at Sun Temple, eastern tip of Shandong (37.2408° N 122.4316° E), mag. 0.638 at coastal Kuaiji Commandery (30.9225° N 121.951° E). (?)
17	–143 Sep 8	I, 336, viii	0.619	04420	VI.7 辛亥[48]晦 (CHARIOT 7^d); observable at Chang’an. ✓
18	–142 Aug 28	I, 339, ix	0.625	04422	*Houyuan* I.7 乙巳[42]晦 (WINGS 17^d); observable at Chang’an. ✓
	Wudi				
19	–140 Jul 8	II, 136, i	0.277 prov. report?	04427	*Jianyuan* II.2 丙戌[23]晦 (GHOST 14^d); alternatively Jingdi, *Houyuan* III.5 丙寅[3]朔; no eclipse at Chang’an, mag. 0.277 at Guangzhou, Nanhai Commandery. (?)
20	–137 Nov 1	II, 136, ii	0.558	04435	III.9 丙子[13]晦 (TAIL 2^d); observable at Chang’an. ✓
21	–135 Apr 15	II, 136, iii	0.277	04438	V.1 己巳[6]朔; should be 3rd month; no eclipse at Chang’an, mag. 0.277 at Jiuquan Commandery (Inner Mongolia).
22	–134 Apr 5	II, 137, iv	prov. report?	04441	*Yuanguang* I.2 丙辰[53]晦; possibly *Jianyuan* VI.3 壬戌[59]晦; no eclipse at Chang’an, very small partial eclipse from Liaodong Commandery and eastward to the Pacific coast. (?)
23	–133 Aug 19	II, 138, v	0.709	04445	I.7 癸未[20]晦-1 (WINGS 8^d); observable at Chang’an. ✓
24	–126 Apr 6	II, 138, vi	0.502	04460	*Yuanshuo* II.2 乙巳[42]晦; observable at Chang’an. ✓
25	–122 Jan 23	II, 139, vii	0.645	04469	VI.11 癸丑[50]晦 should be 12th month; observable at Chang’an. ✓
26	–121 Jul 9	II, 139, viii	0.966	04472	*Yuanshou* I.5 乙巳[42]晦 (WILLOW 6^d); observable at Chang’an. ✓
27	–111 Jun 18	II, 139, ix	0.798	04496	*Yuanding* V.4 丁丑[14]晦 (WELL 23^d); observable at Chang’an. ✓
28	–107 Apr 6	II, 139, x	0.335	04505	*Yuanfeng* IV.6 己酉[46] 晦; should be 3rd month 乙酉[22] 晦; scribal error 6 for 3, 己 for 乙; observable at Chang’an. ✓
29	–95 Feb 23	II, 141, xi	0.881	04534	*Taishi* I.1 乙巳[42] 晦; intercalation confirmed by archaeological discovery of a calendar; observable at Chang’an. ✓
30	–92 Dec 12	II, 141, xii	0.844	04542	IV.10 甲寅[51]晦 (DIPPER 19°; observable at Chang’an. ✓
31	–88 Sep 29	II, 141, xiii	0.912	04551	*Zhenghe* IV.8 辛酉晦[58] LT15-17 ‘not total, like a hook’ (NECK 2^d) ‘at the hour of *fu* [LT=15-17h] the eclipse began from the northwest; towards the hour of sunset it was restored’ [S 235] [K 6]; observable at Chang’an. ✓
	Zhaodi				
32	–83 Dec 3	II, 178, i	0.643	04564	*Shiyuan* III.11 壬辰[29]朔 (DIPPER 9^d; observable at Chang’an.✓
33	–79 Sep 20	II, 178, ii	0.787/1.001 prov. report	04574	*Yuanfeng* I.7 乙亥[12]晦 (SPREAD 12^d) [S 235] [K 6]; “total” and “almost total”; observable at Chang’an, but total 1.001 at Beijing, Zhangshan Kingdom; confirms divergent ‘total’ and ‘almost total’ comments. ✓
	Xuandi				
34	–67 Feb 13	II, 275, i	0.435	04602	*Dijie* I.12 癸亥[60]晦 (HALL 15^d); observable at Chang’an (?), but 0.435 at Guangzhou, Nanhai Commandery. ✓
35	–55 Jan 3	II, 275, ii	0.92 prov. report	04632	*Wufeng* I.12 乙酉[22]朔 (GIRL 10°); no eclipse at Chang’an, mag. 0.92 at Lelang Commandery = Pyongyang (39.0328° N 125.7275° E). ✓
36	–53 May 9	II, 276, iii	0.815	04637	IV.4 辛丑[]晦 (NET 19°); observable at Chang’an. ✓
	Yuandi				
37	–41 Mar 28	II, 354, i	0.729	04667	*Yongguang* II.3 壬戌[59]朔 (PASTURE 8^d); observable at Chang’an. ✓
38	–39 Jul 31	II, 354, ii	0.519	04674	IV.6 戊寅[15]晦 (SPREAD 7^d); observable at Chang’an.. ✓
39	–34 Nov 1	II, 355, iii	0.825	04685	*Jianzhao* V.6 壬申[9]晦 ’partial, like a hook, then set’; should be IV.9 丁丑[14]晦 [S 236] [K 6]; sunset eclipse observable at Chang’an. ✓
	Chengdi				
40	–28 Jan 5	II, 419, i	0.66	04700	*Jianshi* III.12 戊申[45]朔 (GIRL 9^d); mag. 0.66 at Chang’an. ✓
41	–27 Jun 19	II, 419, ii	0.927	04703	*Heping* I.4 己亥[36]晦 ‘not total, like a hook’ [S 236] [K 6]; observable at Chang’an. ✓
42	–25 Oct 23	II, 419, iii	0.786	04710	III.8 乙卯[52]晦 (CHAMBER); observable at Chang’an.✓
43	–24 Apr 18	II, 420, iv	0.557	04711	IV.3 癸丑[50]朔 (MANE); observable at Chang’an.✓
44	–23 Apr 7	II, 420, v	0.106	04713	*Yangshuo* I.2 丁未[44]晦 (TAIL); very small partial eclipse

					at Chang'an. ✓
45	–15 Nov 1	II, 420, vi	0.08	04736	*Yongshi* I.9 丁巳[54]晦; extremely small partial eclipse at Chang'an ('small magnitude shows prior watch was kept.' – Dubs).✓
46	–14 Mar 29	II, 421, vii	0.864	04737	II.2 乙酉[22]晦; observable at Chang'an. [S 231]; provincial report. ✓
47	–13 Mar 18	II, 421, viii	0.424	04739	III.1 己卯[16]晦; observable at Chang'an. ✓
48	–12 Aug 31	II, 421, ix	0.218	04742	IV.7 辛未[8]晦; observable at Chang'an. ✓
49	–11 Jan 26	II, 422, x	[*0.07*]	04743	*Yuanyan* I.1 己亥[36]朔; observable at Chang'an? ('small magnitude shows prior watch was kept.' – Dubs).✓
	Aidi				
50	–1 Feb 5	III, 43, i	0.855	04769	*Yuanshou* I.1 辛丑[38]朔 'not total, like a hook' (HALL 10d) [S 237] [K 6]; observable at Chang'an. ✓
51	0 Jun 20	III, 43, ii	[*0.06*]	04772	II.4 壬辰[29]晦; should be 壬戌[59] (scribal error of 辰 for 戌); observable at Chang'an? ('small magnitude shows prior watch was kept.' – Dubs).✓
	Pingdi				
52	1 Jun 10	III, 87, i	0.733	04775	*Yuanshi* I.5 丁巳[54]朔 (WELL); observable at Chang'an. ✓
53	2 Nov 23	III, 87, ii	0.904	04778	II.9 戊申[45]晦 'total' [S 238][K 6]; observable at Chang'an. ✓
	Wang Mang's Xin Dynasty Eclipses				
54	6 Sep 11	III, 544, i	0.924	04787	*Jushe* I.10 丙辰[53]朔; should be 7th month; observable at Luoyang. ✓
55	14 Apr 18	III, 544, ii	0.524	04807	*Tianfeng* I.3 壬申[9]晦； observable at Chang'an. ✓
56	16 Aug 21	III, 545, iii	0.833	04813	III.7 戊子[25]晦; observable at Chang'an. ✓
	Eastern Han Dynasty Eclipses (26 – 220 CE)		Magnitude	Number	Comments
					Certain eclipses not observed at the capital are explicitly recorded as 'reported by' with the location noted; R before the month number indicates intercalary month. *Sources*: "Basic Annals" and "Monograph on the Five Elemental-Phases" in the *History of the Later Han Dynasty*: Fan (1965), VI.18. 3357.
	Guangwudi				
EH#1	26 Feb 6		0.697	04838	*Jianwu* II.1 甲子[1]朔 (8d in ROOF); observable at Luoyang (34.6255° N 112.4451° E). ✓
2	27 Jul 22		0.519	04841	III.5 乙卯[52]晦 (14d in WILLOW); observable at Luoyang. ✓
3	30 Nov 14		0.653	04849	VI.9 丙寅[3]晦 (8d in TAIL); 'not observed by scribe-astrologer officials, reported by a commandery'; mag. 0.653 at Luoyang, mag. 0.996 at Guangzhou, Nanhai Commandery. ✓
4	31 May 10		0.721	04850	VII.3 癸亥[60]晦 (5d in Net); observable at Luoyang. ✓
5	40 Apr 30		~0.5	04874	XVI.3 辛丑[38]晦 (7d in MANE); prior to sunrise at Luoyang, large early morning eclipse at Changchun, Liaodong Commandery. ✓
6	41 Apr 19		0.789	04876	XVII.2 乙未[32]晦 (9d in STOMACH); observable at Luoyang.✓
7	46 Jul 22		0.167	04889	XXII.5 乙未[32]晦 (7d in WILLOW); very small partial eclipse at Luoyang, mag. 0.479 at Dunhuang Commandery (40.1333° 94.6362° E). ✓
8	49 May 20		0.744	04897	XXV.3 戊申[45]晦 (15d in NET); observable at Luoyang. ✓
9	53 Mar 9		0.713	04905	XXIX.2 丁巳[54]朔 (5d in E. WALL); observable at Luoyang. ✓
10	55 Jul 13		0.266	04912	XXXI.5 癸酉[10]晦 (5d in WILLOW); observable at Luoyang. ✓
11	56 Dec 25		0.64	04915	*Zhongyuan* I.11 甲子[1]晦 (20d in DIPPER); observable at Luoyang.✓
	Mingdi				
12	60 Oct 13		0.701	04924	*Yongping* III.8 壬申[9]晦 (2d in BASE); observable at Luoyang. ✓
13	65 Dec 16		0.94/1.007	04938	VIII.10 壬寅[39]晦 'total' (11d in DIPPER) [S 240][K 6]; observable at Luoyang. ✓

14	70 Sep 23		0.889	04948	XIII.10 甲辰[41]晦; (7^d in Tail); *Annals* miswrites 壬 for 甲, *WXZ* is correct; observable at Luoyang,. ✓
15	73 Jul 23		0.828	04956	XVI.5 戊午[55]晦 (15^d in Willow); observable at Luoyang, total 1.007 at Guangzhou. ✓
16	75 Jan 5		------	04959	XVIII.11 甲辰[41]晦; (21^d in Dipper) day is wrong should be 己酉[46] *; E&M show the eclipse as not visible from China.*
	Zhangdi				
17	80 Mar 10		0.269	04927	*Jianchu* V.2 庚辰[17]朔 (8^d in Wall); observable at Luoyang. ✓
18	81 Aug 23		0.279	04975	VI.6 辛未[8]晦; 未 error for 卯 [28]晦 (6^d in Wings); observable at Luoyang. ✓
19	87 Oct 15		0.863	04990	*Zhanghe* I.8 乙未[32]晦 (4^d in Base) 'not observed by scribe-astrologer officials, reported by other officials'; sunset eclipse at Luoyang. ✓
	Hedi				
20	90 Mar 20		0.277	04996	*Yongyuan* II.2 壬午[19] (8^d in Stride) 'not observed by scribe-astrologer officials, reported by Zhuo Commandery 涿郡 (Hebei, 39 29.1 N 115° 58.5' E)'. ✓
21	92 Jul 23		0.661	05002	IV.6 戊戌[35]朔 (2^d in Stars); observable at Luoyang. ✓
22	95 May 22		0.927	05010	VII.4 辛亥[48]朔 (in Beak); observable at Luoyang, ✓
23	100 Aug 23		0.459	05022	XII.7 辛亥[48]朔 (8^d in Wings); observable at Luoyang. ✓
24	103 Jun 22		0.794	05030	XV.R4 甲子[1]晦 (22^d in Well); observable at Luoyang. ✓
	Andi				
25	107 Apr 11		0.434	05038	*Yongchu* I.3 癸酉[10] '2^{nd} day of the month' (2^d in Stomach); observable at Luoyang. ✓
26	111 Jan 27		0.77	05048	V.1 庚辰[17]朔 (8^d in Void); observable at Luoyang. ✓
27	113 Jun 1		0.923	05055	VII.4 丙申[33]晦 (1^d in Well); mag. 0.923 sunset eclipse at Luoyang.✓
28	114 Nov 15		0.556	05058	*Yuanchu* I.10 戊子[25]朔 (10^d in Tail); observable at Luoyang. ✓
29	115 Nov 4		~0.08	05060	II.9 壬午[19]晦 (4^d in Heart); mag. ca 0.08 sunset eclipse at Luoyang. ✓
30	116 Apr 1		0.956	05061	III.3 辛亥[48] '2^{nd} day of the month' (5^d in Pasture); 'not observed by scribe-astrologers officials; reported by Liaodong 遼東 [bordering Korea]'; no eclipse at Luoyang, mag. 0.956 at Changchun, Liaodong Commandery. ✓
31	117 Mar 21		0.186	05063	IV.2 乙巳[42]朔 (9^d in Stride) 'not observed by scribe-astrologer officials, reported by seven commanderies'; no eclipse at Luoyang, mag. 0.186 at Guangzhou, Nanhai Commandery. ✓
32	118 Sep 3		0.557	05066	V.8 丙申[33]朔 (18^d in Wings) 'not observed by scribe-astrologer officials, reported by Zhangye 張掖 [Gansu, 38° 55.5' N 100° 26.96 E]'; after sunset at Luoyang, mag. 0.535 at Zhangye Commandery. ✓
33	120 Jan 18		0.988/1.013	05071	VI.12 戊午[55]朔 'almost total, like twilight on the ground' (11^d in Girl) [S 237] [K 6]; observable at Luoyang, total 1.013 60 km south at Pingdingshan (33.7623° N 113.1702° E) within the capital commandery. *Hanji* says 'the stars all appeared' signifying totality, probably within the capital commandery; Stephenson (1997: 238). ✓
34	120 ® 121 Jul 2				*Yongning* I.7 乙酉[22]朔 (15^d in Spread); should be *Yongning* II.7 辛亥[48]晦; 'not observed by scribe-astrologer officials, reported by Jiuquan 九泉 Commandery (Gansu, 39° 43.9' N 98° 29.7' E)' [S 237].' In fact, not observable at Jiuquan, but mag. 0.193 at Kunming, Yizhou Commandery (23.7492° N 100.9424° E). ✓
35	123 Nov 6		<0.1	05080	*Yanguang* III.9 庚申[57]晦; very small partial eclipse visible only from the coast north of Korea. ✓
36	125 Apr 21		1.0063	05083	IV.3 戊午[55]朔 (12^d in Stomach) 'Longxi 隴西 35° 0.3' N 104° N 38.1' E, Jiuquan, and Shuofang 朔方 [near Baotou, Inner Mongolia] reported the event, the scribe-astrologer officials did not notice it'; mag. 0.314 at Luoyang, mag. 0.5 at Jiuquan Commandery, mag. 0.504 at Baotou, Shuofang Commandery. ✓
	Shundi				
37	127 Aug 25		0.961	05089	*Yongjian* II.7 甲戌[11]朔 (9^d in Wings) (a doublet is

					misdated *Yangjia* II); mag. 0.961 at Luoyang. ✓
38	135 Sep 25		0.267	05108	*Yangjia* IV.R8 丁亥[24]朔 (5^{d} in HORN) 'scribe-astrologer officials did not observe it, Lingling 零陵 [Guangxi/Hunan] reported'; no eclipse at Luoyang, mag. 0.394 at Lingling Commandery in the south. ✓
39	138 Jan 28		------	05115	*Yonghe* III.12 戊戌[35]朔 (11^{d} in GIRL; possibly 晦); 'not observed by scribe-astrologer officials, reported by Kuaiji 會計 Commandery (eastern Jiangsu)'; *E&M show eclipse not visible farther east than Ukraine.*
40	139 Jan18		0.115	05117	V.1 己丑[26]晦 '1st month' 正 miswritten as '5th month 五' (33^{d} in WELL); no eclipse at Luoyang, mag. ~0.115 at Guangzhou, Nanhai Commandery. ✓
41	140 Jul 2		0.538	05120	VI.9 辛亥[48]晦 (11^{d} in TAIL) ('6th year' should be '5th year') observable at Luoyang. ✓
	Huandi				
42	147 Feb 18		0.611	05136	*Jianhe* I.1 辛亥[48]朔 (3^{d} in HALL) 'scribe-astrologer officials did not observe it, reported by commanderies and kingdoms'; no eclipse at Luoyang, mag. ~0.611 at coastal Yangzhou Province (28.394° N 121.619° E). ✓
43	149 Jun 23		0.594	05143	III.4 丁卯[4]晦 (23^{d} in WELL); mag. 0.594 sunrise eclipse at Luoyang.✓
44	152 ® 157 Jul 24		0.161	05161	*Yuanjia* II.7 庚辰[17]朔 (4^{d} in WINGS); 'scribe-astrologer officials did not observe it, reported by Guangling 廣陵 Commandery [near Shanghai]'; scribal error; should be '*Yongshou* 3rd year, 庚辰[17]晦'; no eclipse at Luoyang, mag. ~0.161 at Yangzhou, Guangling Commandery. This is possibly a doublet of #46, also visible from eastern Jiangsu.[1] ✓
45	154 Sep 25		0.721	05154	*Yongxing* II.9 丁卯[4]朔 (5^{d} in HORN); mag. 0.721 at Luoyang. ✓
46	157 Jul 24		0.277	05161	*Yongshou* III.R5 庚辰[17]晦 (2^{d} in STARS) 'scribe-astrologer officials did not observe it, reported by commanderies and kingdoms'; no eclipse at Luoyang, mag. ~0.277 at Guangzhou, Nanhai Commandery. ✓
47	158 Jul 13		0.768	05163	*Yanxi* I.5 甲戌[11]晦 (7^{d} in WILLOW); observable at Luoyang. ✓
48	165 Feb 28		0.401	05178	VIII.1 丙申[33]晦 (13^{d} in HALL); observable at Luoyang. ✓
49	166 Feb 18		0.634	05181	IX.1 辛卯[28]朔 (3^{d} in HALL) 'scribe-astrologer officials did not observe it, reported by commanderies and kingdoms'; just prior to sunrise at Luoyang. ✓
50	167 Jul 4		0.582	05185	*Yongkang* I.5 壬子[49]晦 (1^{d} in GHOST); observable at Luoyang. ✓
	Lingdi				
51	168 Jun 23		0.33	05187	*Jianning* I.5 丁未[44]朔 (doublet mistakenly appears under Xiandi "25th year" in *Basic Annals*); unobservable at Luoyang, mag. 0.33 at Guangzhou in coastal Nanhai Commandery. ✓
52	168 Dec 17		0.71	05188	I.10 甲辰[41]晦 (with no prior intercalation this year) ; observable at Luoyang.✓
53	169 Dec 6		~0.60	05190	II.10 戊辰[5]晦 'reported by Youfufeng 右扶風 Commandery (34.3679° N 107.8816° E)'; scribal error 辰 for 戌; observable at Chang'an.✓
54	170 May 3		?	05191	III.3 丙寅[3]晦 'reported by the governor of Liang 梁 [Kingdom, eastern Henan]' (34.4248° N 115.6428° E).✓
55	171 Apr 23		0.219	05193	IV.3 辛酉[58]朔; sunrise eclipse at Luoyang. ✓
56	174 Feb 19		0.337	05199	*Xiping* II.12 癸酉[10]晦[Feb 18] (2° in VOID); mag. 0.337 at Luoyang. ✓
57	177 Dec 8		0.417	05208	VI.10 癸丑[50]朔 'reported by the Governor of Zhao 趙 [Hebei] (38.1783 N 114.3457 E)'; should be '11th month'; no eclipse at Luoyang. ✓
58	178 Mar 7		------	?	*Guanghe* I.2 辛亥[48]朔 in "Monograph"; "Basic Annals" has '5th month'. *E&M show no eclipse. Failed prediction?*
59	178 Nov 27		0.378	05210	*Guanghe* I.10 丙子[13]晦 (4^{d} in BASKET); observable at Luoyang. ✓
60	179 May 24		0.895	05211	II.4 甲戌[11]朔; observable at Luoyang. ✓
61	181 Sep 26		0.886	05216	IV.9 庚寅[27]朔 (6^{d} in HORN); observable at Luoyang, ✓
62	186 Jul 4		0.283	05227	*Zhongping* III.5 壬辰[29]晦; observable at Luoyang. ✓
63	189 May 3		0.7	05234	VI.4 丙午[43]朔; aka Shaodi, *Guangzi* 1st year; observable at Luoyang. ✓
	Xiandi				

64	193 Feb 19		0.549	05242	IV.1 甲寅[51]朔 (4^{d} in Hall); observable at Luoyang.✓
65	194 Aug 04		0.936	05245	*Xingping* I.6 乙巳[42]朔; mag. 0.936 sunrise eclipse at Luoyang. ✓
66	200 Sep 26		0.646	05259	*Jian'an* V.9 庚午[7]朔; mag. 0.646 sunrise eclipse at Luoyang.✓
67	201 Mar 22		0.387	05260	VI.2 丁卯[4]朔; mag. 0.133 at Luoyang, mag. 0.387 at Hanoi, Jiaozhi Commandery. ✓
68	208 Oct 27		0.749	05278	XIII.10 癸未[20]朔 (12^{d} in Tail); observable at Luoyang. ✓
69	210 Mar 13		0.816	05281	XV.2 乙巳[42]朔; sunrise eclipse at Luoyang. ✓
70	212 Aug 14		0.832	05286	XVII.6 庚寅[27]晦; observable at Luoyang. ✓
71	216 Jun 3		0.802	05295	XXI.5 己亥[36]朔; sunrise eclipse at Luoyang, ✓
72	219 Apr 2		0.512	05301	XXIV.2 壬子[49]晦; observable at Luoyang. ✓
	Concluding summation from the "Monograph on the Five Elemental-Phases," (*Hou Han shu*, VI.18.3372): "total eclipses = 72; first day of the month *shuo* = 32; last day of the month *hui* = 37; 2^{nd} day of the month = 3."				

3 ON THE QUESTION OF RECORDS ORIGINATING FROM OUTSIDE THE CAPITAL

Some Western Han Dynasty eclipses records certainly originated outside the capital (Dubs, 1938-1955, III: 552). Apart from the political units called commanderies under the direct administration of the imperial court, two-thirds of the Han Empire comprised powerful, quasi-autonomous kingdoms ruled by Lord-Kings (imperial relatives) with their own courts (Figure 1). Many of these were nominally successors to the kingdoms annihilated during the course of the Qin conquest campaigns lasting more than a century, which culminated in the unification of all of China proper in 221 BCE.

From an aristocratic tomb (closed ca. 168 BCE) in the most southerly kingdom, the Kingdom of Changsha, came the trove of Mawangdui silk manuscripts discovered in the 1970s, including the most 'important' astronomical/astrological 'texts' ever unearthed: the *Prognostications of the Five Planets*, the *Xing-De*, the *Diverse Prognostications on the Heavenly Patterns and Formations of Materia Vitalis*, and an illustrated *Cometary Atlas*. From the Kingdom of Huainan, also far to the south, comes the encyclopedic *Huainanzi* (139 BCE), which documents observation of celestial phenomena during the Qin and Former Han dynasties. By then astral prognostication had been practiced for centuries. Indeed, thirty-six solar eclipses are accurately reported in the *Spring and Autumn Annals* chronicle from the court of the eastern state of Lu, far from the Eastern Zhou Dynasty capital in Luoyang, the only one of its kind to have survived intact. Moreover, several of the most important scribe-astrologers active in the Warring States of the pre-imperial period are identified by name in Sima Qian's "Treatise on the Celestial Offices" in the *Grand Scribe's Records*. The most important observational astronomer who participated in the Grand Inception calendar reform of 104 BCE, Luoxia Hong, actually hailed from Ba (Sichuan) in the far southwest.

There can be no doubt, therefore, that solar eclipses were being closely observed in kingdoms and commanderies far from the capital, as explicitly stated in several Eastern Han Dynasty records assembled in Table 1. Ample evidence exists that numerous provincial observatories were in operation during the Song Dynasty (1127–1279) and later, so the assumption that observations were not being made at them is misguided (Pankenier, 1998: 32).

4 RECORDING ERRORS IN THE PARAMETERS (REIGN, YEAR, MONTH, DAY) OF WESTERN HAN ECLIPSE REPORTS

Here, the fifteen cases of recording errors in the Western Han records excerpted from Table 1 are examined in more detail. Five elements are considered to comprise the dating parameters because the day-date is composed of two separate characters A+B, each susceptible to scribal error.

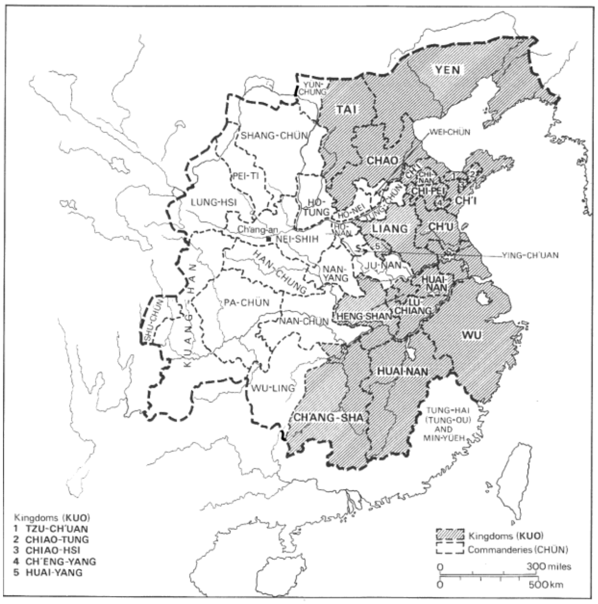

Figure 1: The Western Han Empire in 163 BCE. The shaded regions are the semi-autonomous kingdoms (after Fairbank and Twitchett, 1986: 138).

4.1 One Parameter Error

The errors, as shown in Table 2:

WH#*21, 25, 54* — 3rd month (三) miscopied as 5th (一); 12th month (十二) miscopied as 11th (十一); 7th month (七) miscopied as 10th (十), all common errors.

WH#*51* — day element B is miswritten, a common copyist's error of 辰 for 戌.

Explainable transcription errors: the observations are confirmed.

4.2 Two Parameter Error

The errors, as shown in Table 3:

#28 — mont*h* miswritte*n* a*s 6* (六*)* fo*r 3* (三)*,* day element B is miswritten 己 for 乙.

#19 — month and element B are wrong; copyist's error of year 2 (二) for 3 (三) and day 丙戌 [23] for day 丙寅 [3]; reign and year at the time of observation correctly attributed to Emperor Jing, who died in the 1st month of 141 BCE (*Houyuan* III). Emperor Wu's accession year, *Jianyuan* 1, was later variously identified as either 141 or 140, possibly causing confusion.

Explainable transcription errors: one observation confirmed, one requires corroboration of misdating.

4.3 Three Parameter Error

The errors, as shown in Table 4:

#2 — year, month, and day element B wrong; possible scribal error of day [40] *guimao* for [20] *guiwei*.

#4 — year, month, and element B wrong: possible copyist's error of 7 (七) for 3 (三), five (五) for nine (九), and day *xinchou* 辛丑 [38] for *xinyou* 辛酉 [58].

#9 — year, month, and day element A wrong; possible scribal error of day [4] for [28].

#11 — situation similar to WH*#19*; month and day are wrong, reign and year at the time of observation correctly attributed to Wendi who died in the 6th month of *Houyuan* VII. This is Espenak and Meeus (2009) 04395; mag. 0.244 at Chang'an.

#14 — day wrong, reign and year correct.

Nos. 11, 14 confirmed, nos. 2, 4, 9 require corroboration.

4.4 Four Parameter Error

The errors, as shown in Table 5:

#16 — reign, year, month and day all wrong, but the eclipse was significant on the east coast.

#39 — reign name and description correct, year, month, day all wrong; major observable sunset eclipse confirmed.

No. 39 confirmed; no. 16 possible provincial report but problematical unless corroborated.

4.5 Five Parameter Error

The errors, as shown in Table 6:

#10, 22 — reign, year, month, day all wrong.

Nos. 10, 22 unconfirmed without corroboration.

Table 2: One parameter errors.

No.	Date	Comments
WH 2	–200 Oct 8	III.11 癸卯[40]晦; should be VI.8 癸未[20]; too small to be observable at Chang'an, but mag. 0.467 at Changchun, Yan Province (43.8134° N 125.2905° E); likely provincial report.
4	–191 Sep 29	VII.5 辛丑[38]朔; scribal error, should be III.9 and 酉 for 丑; observable at Chang'an.
9	–175 Jun 6	III.11 丁卯[4]晦 (VOID 8^{d}), possibly IV.5 辛卯[28]朔; observable at Chang'an.
11	–154 Oct 10	VII.1 辛未[8]朔 ; should be Jingdi 2nd year, 9th month, 乙酉[22]晦; small partial eclipse observable at Chang'an. ✓
14	–148 Jun 7	*Zhongyuan* I.12 甲寅[51]晦; possibly I.5 壬子[49]晦; sunset eclipse at Chang'an. ✓

Table 3: Two parameter errors.

No.	Date	Comments
28	–107 Apr 6	*Yuanfeng* IV.6 己酉[46] 晦; should be 3rd month 乙酉[22]晦; scribal error 6 for 3, 己 for 乙; observable at Chang'an. ✓
19	–140 Jul 8	*Jianyuan* II.2 丙戌[23]晦 (GHOST 14^{d}); alternatively Jingdi, *Houyuan* III.5 丙寅[3]朔; no eclipse at Chang'an, mag. 0.277 at Guangzhou, Nanhai Commandery.

Table 4: Three parameter errors.

No.	Date	Comments
2	–200 Oct 8	III.11 癸卯[40]晦; should be VI.8 癸未[20]; too small to be observable at Chang'an, but mag. 0.467 at Changchun, Yan Province (43.8134° N 125.2905° E); likely provincial report.
4	–191 Sep 29	VII.5 辛丑[38]朔; scribal error, should be III.9 and 酉 for 丑; observable at Chang'an.
9	–175 Jun 6	III.11 丁卯[4]晦 (VOID 8^{d}), possibly IV.5 辛卯[28]朔; observable at Chang'an.
11	–154 Oct 10	VII.1 辛未[8]朔 ; should be Jingdi 2nd year, 9th month, II.9 乙酉[22]晦; small partial eclipse observable at Chang'an. ✓
14	–148 Jun 7	*Zhongyuan* I.12 甲寅[51]晦; possibly I.5 壬子[49]晦; sunset eclipse at Chang'an. ✓

Table 5: Four parameter errors.

No.	Date	Comments
16	–144 Mar 26	IV.10 戊午 [55]晦; possibly V.2 庚申[57]晦; no eclipse at Chang'an, mag. 0.506 at Sun Temple, eastern tip of Shandong (37.2408° N 122.4316° E), mag. 0.638 at coastal Kuaiji Commandery (30.9225° N 121.951° E).
39	–34 Nov 1	*Jianzhao* V.6 壬申[9]晦 'partial, like a hook, then set'; should be IV.9 丁丑[14]晦 [S 236] [K 6]; sunset eclipse observable at Chang'an. ✓

Table 6: Five parameter errors.

No.	Date	Comments
10	–160 Aug 17	*Houyuan* IV.4 丙寅[3]晦 (WELL 13^{d}) possibly III.6 庚申[57]晦; unobservable before sunrise at Chang'an, but mag. 0.349 at Zhangye in Beidi Commandery (38.8929° N 100.5054° E).
22	–134 Apr 5	*Yuanguang* I.2 丙辰[53]晦; possibly *Jianyuan* VI.3 壬戌[59]晦; no eclipse at Chang'an, very small partial eclipse from Liaodong Commandery and eastward to the Pacific coast.

Table 7: Three problematical cases.

No.	Date	Comments
16	75 Jan 5	XVIII.11 甲辰[41]晦; (21[d] in DIPPER) day is wrong should be 己酉[46] *; E&M show eclipse not visible farther east than Caspian Sea.*
39	138 Jan 28	*Yonghe* III.12 戊戌[35]朔 (11[d] in GIRL; possibly 晦); 'not observed by scribe-astrologer officials, reported by Kuaiji 會計 Commandery (eastern Jiangsu)'; *E&M show eclipse not visible farther east than Ukraine.*
58	178 Mar 7	*Guanghe* I.2 辛亥[48]朔 in "Monograph"; "Basic Annals" has '5[th] month'. *E&M show no eclipse. Failed prediction?*

4.6 Summary

Re-examination of the original records from the Western Han and checking with Espenak and Meeus's catalogue of solar eclipses hardly affects Dubs's sixty-year-old conclusions. Virtually all the matches Dubs was able to make between the Chinese records and actual eclipses are confirmed (in some cases refined), as are his general conclusions. Of the fifteen erroneous records seven are too problematical to be accepted without further corroboration. The other eight contain one or more common scribal errors, but each provides sufficient information to confirm that the record corresponds to an actual observation. Detailed examination of the mistakes in the defective records mainly points to copyists' errors in transmission. One record (WH#*19*) provides convincing evidence of a distant observation subsequently reported to the capital (as do WH#*5*, *12*, *15*, *21*, *33* and *35* in Table 1). In two cases (WH#*11* and *19*) confusion about the date may be attributable to the fact that the eclipse was observed and recorded during the partial last year of an emperor, which year was subsequently also attributed by some to his successor. In the one surprising case (WH#*39*), the unique comment that the Sun set during a large eclipse observable throughout western and central China is enough to show that the event was certainly witnessed, even if the record contains numerous errors. In only two cases (WH#*22* and *10*) are all five parameters (reign, year, month, day elements A+B) wrong.

In an Appendix in his translation of *The History of the Former Han Dynasty*, Dubs (1938-1955, III: 559) concluded:

> The outstanding impression left by the Chinese recordings of eclipses in the Former [Western] Han period is their high degree of fidelity to fact. The Chinese were not to any great extent interested in fabricating eclipses as portents and it was dangerous to do so. They had not yet begun to predict eclipses. They watched for eclipses, at times with great pertinacity, and succeeded in observing eclipses that were quite small and required the use of special means to be seen. It is but natural that the original records should have suffered errors of transmission; as a whole they are surprisingly correct. This fact constitutes an unimpeachable testimony of the fidelity of the HS [*Han shu*] …

5 ECLIPSE RECORDS FROM THE EASTERN (LATER) HAN DYNASTY (26–220 CE)

The records in the "Monograph on the Five Elemental-Phases" in the *History of the Later Han Dynasty* are usually more complete by comparison with the "Basic Annals" of the individual emperors. With very few exceptions, all the Later Han records can be readily matched with eclipses observable in China. By comparison with the surviving Western Han accounts one notices significant improvement in the records in terms of accuracy and fidelity of transmission.

Remarkably, three reports during the usurper Wang Mang's Xin Dynasty (9–23 CE) and all eleven from the reign of Emperor Guangwu (25–57), first emperor of the Eastern Han, are entirely free of error. This seems to indicate that the bureaucracy survived the interregnum more or less intact.

In individual cases the Eastern Han records prove illuminating, especially where the observation is noted as coming from far afield. In a number of cases (e.g., EH#19, 20, 30-32, 36, 38, 44, 53, 54 and 57) the report states explicitly that the eclipse was not witnessed at the capital of Luoyang, but at some distant location, even as far west as Jiuquan in Gansu, over 1,700 km from the capital in Luoyang, and as far north as Shuofang (near present-day Baoding) in Inner Mongolia. Officials in Liao-dong, near the border with present-day North Korea, 1,300 km to the northeast, must have reported the eclipse of 116 CE (EH#30), even though this is not indicated in the record. But numerous other observations, which were also probably made outside the capital, are not specifically identified as such (e.g., EH#5, 13, 35, 40, 42, 46, 49 and 51). The eclipse of 2 July 121 (EH#34) was not visible from the recorded location of Jiuquan in the far west, but only from Yunnan in the far south (Kunming and southward). A clustering of remote records during the reigns of Andi (107–125), Shundi (126–133), and Huandi (147–167), suggests that remote reporting during those sixty years was particularly accurate. This illustrates how easily one can be led astray by the presumption that reports derive exclusively from observations made at the capital, even when this is not stated explicitly (Stephenson, 2012).

Three cases, EH#16, 39 and 58 (see Table 7), are most problematical, since no eclipse was visible in China on those dates. Of course, one could dismiss these out of hand, as has been done in the past. A focused look at the records indicates that deliberate misrepresentation is unlikely. Report EH#58 may simply be a garbled record of one of the other observations from years I-II of the *Guanghe* reign period (178–183). Records EH#16 and EH#39 are quite different, however, and bear closer scrutiny since the two January observations are strikingly similar. The dates are found to correspond to actual eclipses, the first (EH#16) is miswritten as 31 December 74 although the new moon actually occurred on 5 January 75. The second date, 28 January 138 (EH#39), is correct even though the observation is mistakenly attributed to Kuaiji Commandery on the east coast. Figures 2 and 3 show the tracks of these two total eclipses, both of which ended in Eastern Europe. The contradiction cannot be explained by false reporting. Even if fabrication had been rampant, the sheer improbability of invention by a court official resulting in a correct eclipse date effectively rules out that possibility. The most likely explanation is that both cases represent unsuccessful predictions.

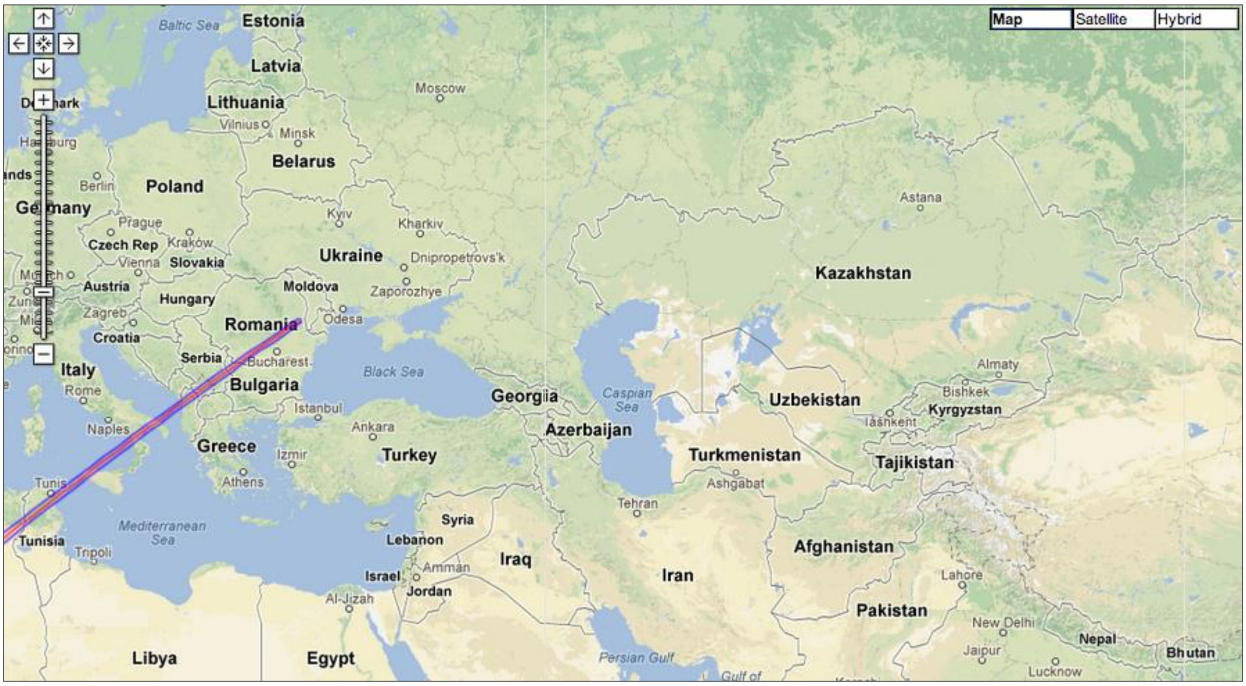

Figure 2: The path of the total eclipse of 5 January 75. The map shows the eastern end of the eclipse track across Earth's surface. (Eclipse Predictions by Fred Espenak (NASA's GSFC); map from Google Earth).

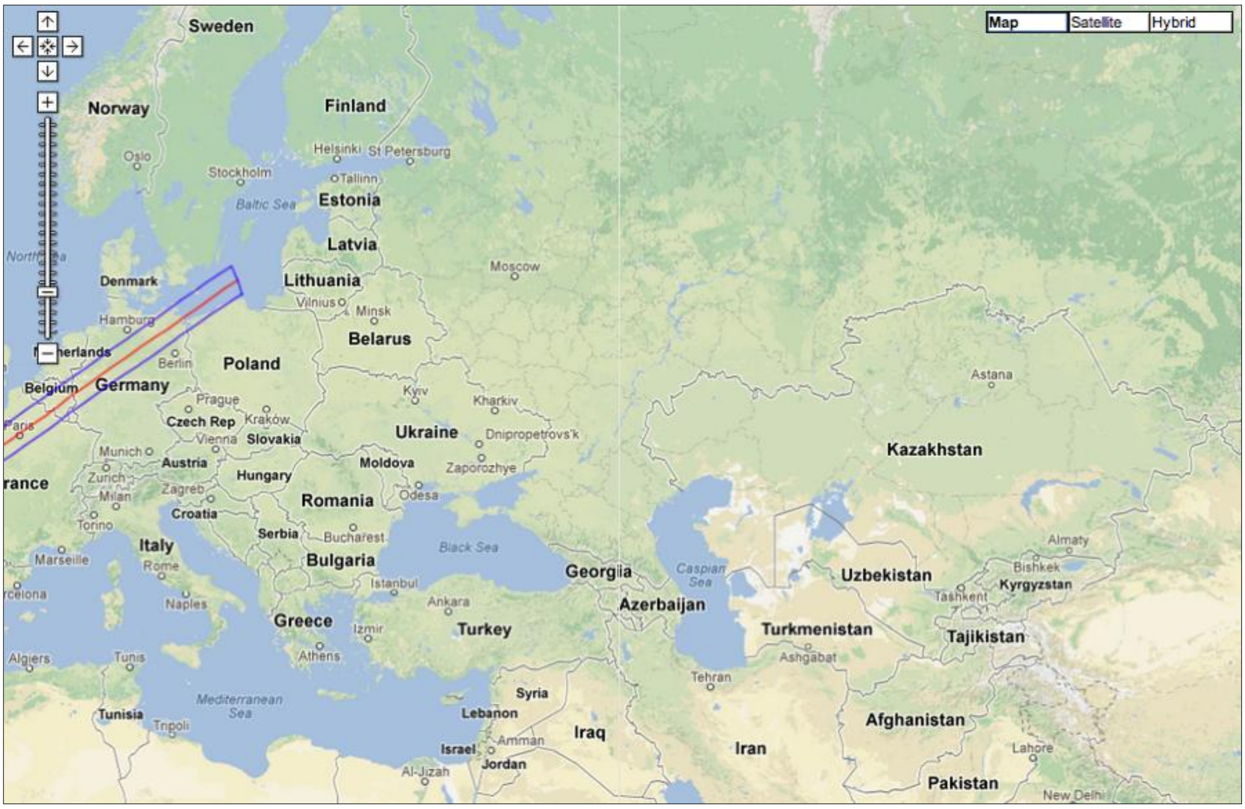

Figure 3: The path of the total eclipse of 28 January 138. (Eclipse Predictions by Fred Espenak (NASA's GSFC); map from Google Earth).

This is especially true in the case of the eclipse of 28 January 138. At precisely this time one of China's greatest polymaths, Zhang Heng (78–139 CE), was active and serving in an official capacity at court for the second time in his career. Zhang was exceptionally accomplished both as an astronomer and a mathematician, having already served during Emperor Shun's reign (126–133) as Chief Astronomer/Astrologer. He was famous for his persistent criticism of the inaccuracies of the calendar in the face of opposition at court, as well as for his unrelenting advocacy of new, more rigorous computational methods. An error of a few hours in the calculated time of the 28 January 138 eclipse would be well within the realm of possibility at this date. This is a plausible hypothesis worth pursuing.

Those who would claim reports with multiple dating parameter errors are fictitious, lacking in scientific value and may be dismissed out of hand, need to address two fairly straightforward questions: if those records are fabrications, how can it be that even when four or five dating parameters are wrongly recorded an observable eclipse actually did occur in China in the year in question? Or, put another way, how can it be that only two reports (138, 178 CE) are found for dates when no eclipses could possibly have been observed anywhere in China, but in both those cases eclipses did occur just a few hours to the west? The most likely explanation is not that they are faked, but rather that they give evidence of failed predictions. If so, the reports offer valuable historical evidence of the ability of the Chinese astronomers to calculate eclipses. In any case, the above results show that arbitrary invention can be ruled out. The default assumption should be that texts have simply become corrupted, not that erroneous records are deliberate fabrications. That is the premise this study was designed to put to the test.

No doubt, as we have seen, there is much more of interest to be gleaned from the records, even the defective ones, as well as from the associated astrological prognostications in the "Monograph on the Five Elemental-Phases." But here my primary purpose was to re-examine the assumption that eclipses were falsely reported for political reasons. Simply dismissing outliers as fakes or assuming on no good evidence that the records refer only to eclipses observable at the capital is not only ahistorical, but also forecloses the possibility of discovering historically-interesting developments. There are numerous mistakes in the reports, but it is well to remember that these are not pristine texts archaeologically excavated in recent years, like the Mawangdui silk mss. These records were copied and recopied for over 1,000 years, and then carved and re-carved in mirror image onto wooden printing blocks (often by illiterates) for another 1,000 years. As Dubs (1938-1955, III: 559) observed: "… it is but natural that the original records should have suffered errors of transmission; as a whole they are surprisingly correct."

6 ON THE LIKELIHOOD OF FALSE REPORTING

My "Planetary portent of 1524" (Pankenier, 2009) provides an illustration of how, in 1524, under one of the most repressive regimes in Chinese imperial history, even an invisible (!) five-planet cluster in February of that year was duly reported as ominous, based on the prognostication manuals (*zhan shu*). Le Huo (*jinshi* degree 1522), a scholar-official in the Bureau of Astrology and the Calendar in 1524, was banished by Emperor Shizong (1507–1567) simply for honestly reporting that the planetary massing was inauspicious. Interpretations calculated to flatter the emperor were generally offered by those who were *not* serving in the Bureau (Wu, 1990). Accurate reporting of the observations themselves was the norm. Indeed, as both de Crespigny (1976) and Dubs (1938-1955) point out, it would have been suicidal to attempt to fake a report of an eclipse, comet, or nova—all of which were easily detectable. Sunspots on the face of the Sun would also reflect on the rulership and might be easier to fake, and yet, as Joseph Needham remarked about the records:

> … if they were not more accurate than would appear from some of their severest critics, it would have been impossible to find known periodicities in them, as has been done, e.g., in the case of the sun-spot cycle. (Needham and Wang, 1959: 419-420, 435).

Attempting to deceive the emperor was always a capital crime, and factional rivalry at court virtually guaranteed that any attempt at deception would be exposed. Furthermore, the fact that solar eclipses were simultaneously observed and reported from distant locations means that any falsification would have required a nation-wide conspiracy, which is an impossibly far-fetched assumption.

De Crespigny (1976) demonstrates that it was the *interpretation* of astral anomalies that was manipulated, not the fact of their occurrence. Often the motive for such 'spin' was to deflect ominous implications arising from the standard prognostics. As Martin Kern (2000) and Yi-yi Wu (1990) show, such controversy could arise in the context of political debates long after the fact. It was in the very nature of policy arguments at court that illustrative precedents had to be cited to support one's position. No proponent of a new policy proposal would submit a memorial and expect it to be taken seriously on its own merit. Justification had to be based on documented historical precedent or the Confucian canon. There were periods in Chinese imperial history when the quality of record-keeping deteriorated due to cronyism, laxity, political unrest, and so on, most of which episodes are familiar to historians. It was standard practice for the official history of a dynasty to be compiled by its successor, so that scholars selected and edited records from archival material long after the observations were made, and in some cases would 'correct' the records or render moral judgments. In later periods one can find, for example, that even if predicted eclipses were sometimes recorded without always being identified as such, the record shows that at times conscientious officials also took pains to correct miscalculations in the record (e.g., see Xu et al., 2000: 40-41). A record of a failed prediction does not mean there was intent to deceive (Stephenson, 1997). Moreover, the inclusion of unmarked predictions among the eclipse reports, like the interpolations of the position of the Sun in the Western Han records, could simply be the result of inadvertent inclusion of interlinear comments years later, a common enough occurrence in ancient Chinese texts. Copyists possessed neither the technical skill nor the motivation to check centuries-old reports for accuracy.

7 CONCLUSION

We conclude, then, that the solar eclipse records for all four centuries of the Han Dynasty as a whole are remarkably accurate. In view of the above, it is misleading to generalize from the few instances of inaccuracy among 127 observations that the astronomical records in the early Chinese dynastic histories were freely manipulated for political reasons (Steele, 2000), especially when noted historians like Bielenstein, Dubs and de Crespigny had concluded that the records were never falsified.

Given the overall quality of the observational records, if an erroneous report like that of 178 CE is alleged to be a deliberate fabrication, the onus is on the critic to provide proof of misrepresentation based on historical evidence. In view of the methodological problems with the statistical studies, the typical transcription errors found in the records and their high degree of fidelity even when political portentology peaked in the Eastern Han Dynasty, it is unacceptable simply to assume that false reporting was common, all the more so when based on the faulty assumption that the recorded observations were all made at the capital.[2] Certainly, the official who was executed for falsely reporting an omen in the Western Han Dynasty, Gao Yun, who ridiculed erroneous dating in the 5th century and Le Huo, who suffered banishment for proffering an honest opinion in the 16th century, would all be shocked to learn that in their 'cultural context' faking reports was "… obviously perfectly acceptable …" (Steele, 2004: 347).

8 NOTES

1. Regarding this observation de Crespigny remarks 'the false report of 152 is a remarkable exception to the general reliability of Chinese observations' (de Crespigny, 1976: 45). I suspect this entry is just a garbled duplicate of the record documenting the eclipse of 157 CE.
2. This assumption is probably fundamental to N. Foley's 1989 survey as well, though I have not seen it, since as of this writing theses and dissertations are not available from the University of Durham.

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HIGHLIGHTING THE HISTORY OF JAPANESE RADIO ASTRONOMY. 1: AN INTRODUCTION

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Abstract: Japan was one of a number of nations that made important contributions in the fledgling field of radio astronomy in the years immediately following WWII. In this paper we discuss the invention of the Yagi-Uda antenna and the detection of solar radio emission in 1938, before reviewing radio astronomical developments that occurred between 1948 and 1961 in Osaka, Nagoya, Tokyo and Hiraiso. In order to place these early Japanese experiments in a national and international context we briefly review the world-wide development of radio astronomy in the immediate post-War years before discussing the growth of optical astronomy in Japan at this time.

Keywords: Japan, radio astronomy, Yagi-Uda antenna, Dellinger Effect, 1948 solar eclipse, Osaka University, Osaka City University, Nagoya University, University of Tokyo, Tokyo Astronomical Observatory, Radio Research Laboratories of the Ministry of Posts and Telecommunications; optical astronomy in Japan.

1 INTRODUCTION

Radio astronomy started in Japan in 1948, about the same time it was launched in France (see Orchiston et al., 2007; 2009), and just two years after the first post-war solar radio observations were made in Australia, England and Canada (see Covington, 1973; Orchiston, et al., 2006; Sullivan 2009). But as Tanaka (1984: 335) points out, "… considering the difficult social circumstances arising from the nation's defeat in World War II, its development was not so slow." Part of the reason for this was the ready availability of suitable equipment:

> Radar was also intensively developed in Japan during World War II, although it was not as technically advanced as that of the Allies. Once the defeated nation began to recover in the 1940s, however, radio physicists could draw not only on domestic stores, but also on American radar parts, readily available from War surplus dealers. (Sullivan, 2009: 225).

By the early 1950s researchers in Hiraiso, Osaka, Toyokawa and Tokyo were actively involved in solar research, and the last two groups went on to make important contributions to international radio astronomy. For Japanese localities see Figure 1.

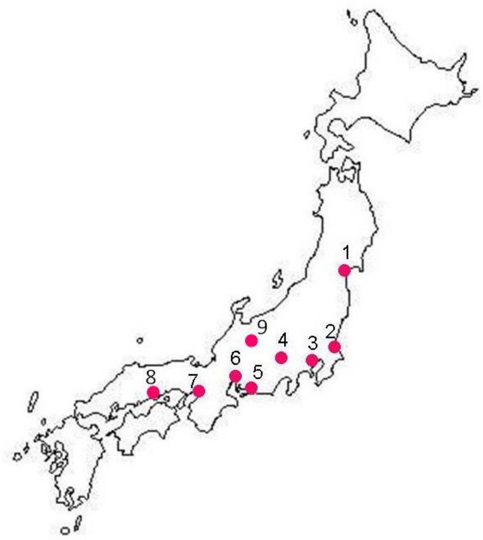

Figure 1: Japanese localities mentioned in the text. Key: 1 = Sendai; 2 = Hiraiso; 3 = Tokyo Astronomical Observatory (Mitaka); 4 = Nobeyama; 5 = Toyokawa Radio Observatory; 6 = Nagoya; 7 = Osaka; 8 = Okayama Astrophysical Observatory; 9 = .Norikura Solar Observatory.

Figure 2: Hidetsugu Yagi with one of the early antennas that he and Uda developed (after: microwaves101.com).

This is the first paper in a series aimed at providing a detailed account of these developments in Japanese radio astronomy from 1948 through to the mid-1960s,[1] and it builds on the foundations laid by Tanaka and by Ishiguro and Orchiston[2] in their review papers of 1984 and 2013 respectively. In his paper, Tanaka (1984: 347) was quick to point out that "… my selection is far from complete, and not a few hidden topics have been left out." These "hidden topics" (or gaps in documentation) are identified in the Ishiguro and Orchiston review paper, and will be discussed in more detail in this paper and others that will follow it in this series on 'Early Japanese Radio Astronomy'.

But before commencing the post-War review of Japanese radio astronomical achievements we will examine the development of the Yagi-Uda antenna, which was to play a critical role in the early development of radio astronomy worldwide, and we will also discuss an experiment conducted in 1938 that led to the detection of solar radio emission. After reviewing the activities of the active post-War radio astronomy groups in Osaka, Toyokawa, Tokyo and Hiraiso we will place these Japanese efforts in international context by examining overseas trends in radio astronomy during these critical post-War years. We will also review developments that occurred in optical astronomy in Japan between 1930 and 1960 so that these pioneering local efforts in radio astronomy can be seen within the overall framework of Japanese astronomy during this era.

Figure 3: Shintaro Uda, the initial inventor of the Yagi-Uda antenna (after: ieeecincinnati.org).

2 THE INVENTION OF THE YAGI-UDA ANTENNA

One of the most widely-used aerials in early radio astronomy world-wide was the Yagi-Uda antenna (Figure 2), more commonly but incorrectly termed the 'Yagi antenna', which was invented by the Japanese radio engineer Shintaro Uda (1896–1976; see Figure 3) in 1925 (see Uda, 1925) with some assistance from his Professor, Hidetsugu Yagi (1886–1976). Both worked at the Tohoku Imperial University in Sendai (see Figure 1) and they published an account of the new antenna design in a Japanese journal in February 1926, but it was Yagi who brought it to an international audience when he published a description in the *Proceedings of the Institute of Radio Engineers* two years later (Yagi, 1928). The design became widely known as the 'Yagi Antenna', but Yagi was always quick to acknowledge Uda's pivotal role in its invention and development. Consequently, we will refer to it here as the Yagi-Uda antenna.

In its simplest form the Yagi-Uda antenna is a directional aerial that comprises a driven element (which is typically a half-wave dipole or a folded dipole) plus a parasitic reflector, but generally the reflector also is accompanied by one or more directors. The reflector is usually about 5% longer than the driven element whereas the directors are a little bit shorter than it, and these are placed at specified locations along the antenna axis. The dipole is the only element that is directly excited, and is connected electrically to the feed-line. These antennas only operate effectively over a narrow band-width and were ideal for radio astronomical investigations.

Yagi-Uda antennas were used extensively by the Allies and Germans during WWII (Brown, 1999), but

> … many Japanese radar engineers were unaware of the design until very late in the war, partly due to rivalry between the Army and Navy. The Japanese military authorities first became aware of this technology after the Battle of Singapore when they captured the notes of a British radar technician that mentioned "yagi antenna". Japanese intelligence officers did not even recognise that Yagi was a Japanese name in this context. When questioned, the technician said it was an antenna named after a Japanese professor. (Yagi-Uda Antenna).

Soon after the War, Yagi-Uda antennas were used by radio astronomers in Australia, Britain, Japan, New Zealand and the USA for solar and/or non-solar investigations (Sullivan, 2009; Orchiston, 2005a).

3 A MISSED OPPORTUNITY: THE DELLINGER EFFECT AND SOLAR RADIO EMISSION

The 'Dellinger Effect' was defined by its discoverer as:

> … the occurrence of a very sudden change in ionization of a portion of the ionosphere. It manifests itself by the complete fading out of high frequency radio transmission for a period of a few minutes to an hour or more, and by perturbations of terrestrial magnetism and earth currents. The effect was discovered in 1935. (Dellinger, 1937: 1253).

In 1936 and 1937 respectively, Daitaro Arawaka and J. Howard Dellinger (1886–1962) reported that "… a kind of 'grinder' like noise sometimes appeared almost simultaneously with [the] Dellinger phenomena in short-wave telecommunication receivers." (Tanaka, 1948: 335).

In 1938 Drs Minoru Nakagami and Kenichi Miya (1915–2004) from the International Telecommunication Co. Ltd. in Tokyo (see Figure 1) were interested in the origin of this 'grinder' noise, so they erected two horizontal half-wave dipoles, one at h = λ/2 and the other at h = 5λ/4 above the ground, and compared their outputs. With this arrangement they could measure the incident angles of the incoming radiation if it was >70° (Nakagami and Miya, 1939). From April through to September 1938 they monitored telecommunication signals, watching the output meters and writing down the observed values in a notebook every minute. Their patience was rewarded on 1 August when they noted a short-term increase in noise that coincided with a Dellinger phenomenon. This is shown in Figure 4, where the noise

> … suddenly increased to 40-50 dBμV as soon as the communication signal from station PLJ at 14.6 MHz faded out. The noise decreased rapidly in five minutes. They were surprised that the noise received by the h = 5λ/4 antenna was more than 10dB stronger than the one received by the h = λ/2 antenna, which clearly showed that the incident angle was more than 70 degrees, as plotted in the upper part of the Figure. As the Sun was then placed at about 70 degrees in elevation angle, Miya believed naturally that the noise came directly from the sun. However, his senior Nakagami was too cautious to accept the young Miya's simple idea, and imagined that the noise originated around the E-layer, connected with a Dellinger disturbance of the atmosphere. In the end, the possibility of direct noise from the Sun was not mentioned in their paper. (Tanaka, 1984: 336-337).

Dr Kenichi Miya, who provided Tanaka with this account, subsequently became the President of the International Telecommunications Installation Co. Ltd., and made many research contributions in the fields of radio waves and satellite communications. At one time he was the President of the International Satellite Communications Society (ISCS), and he was involved in ionospheric research during the International Geophysical Year. In honor of his many achievements, he received the IEEE Award in International Communications in 1987. He died in 2004 at the age of 89 (see Smith, 2004).

Finally, it is of interest to note that although these pre-War Japanese observations were carried out in isolation, 'ham radio operators' in England and a number of other countries also recorded anomalous noise during the 1930s (e.g. see Ham, 1975). While some of them also assumed that the noise was of solar origin, they were unable to take the vital step and attribute it directly to radio emission from the Sun (for an excellent overview see Sullivan, 2009:

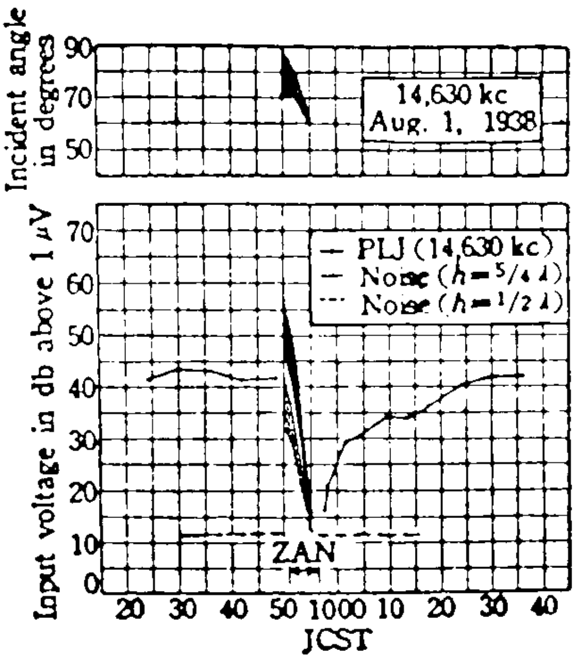

Figure 4: Observations conducted on 1 August 1938 showing (lower) the fade-out of the telecommunications signal (ZAN), and (upper) a simultaneous increase in noise received by the two antennas, which we can now associate with solar radio emission (after Nakagami and Miya, 1939: 176).

85-89). Sullivan (2005: 89) appropriately refers to the Nakagami and Miya episode as "Another near-miss …"

4 EARLY DEVELOPMENTS IN JAPANESE SOLAR RADIO ASTRONOMY: AN OVERVIEW

4.1 Koichi Shimoda and the Solar Eclipse of 1948

Tanaka (1984) claims that Japanese solar radio astronomy began in 1949, but he was not aware of an earlier investigation which was conducted by Koichi Shimoda (1919–; Figure 5) in 1948. Nor does this investigation feature in Sullivan's (2009) encyclopaedic history of early radio astronomy.

Figure 5: Dr Koichi Shimoda, Japan's first radio astronomer (osahistory.org).

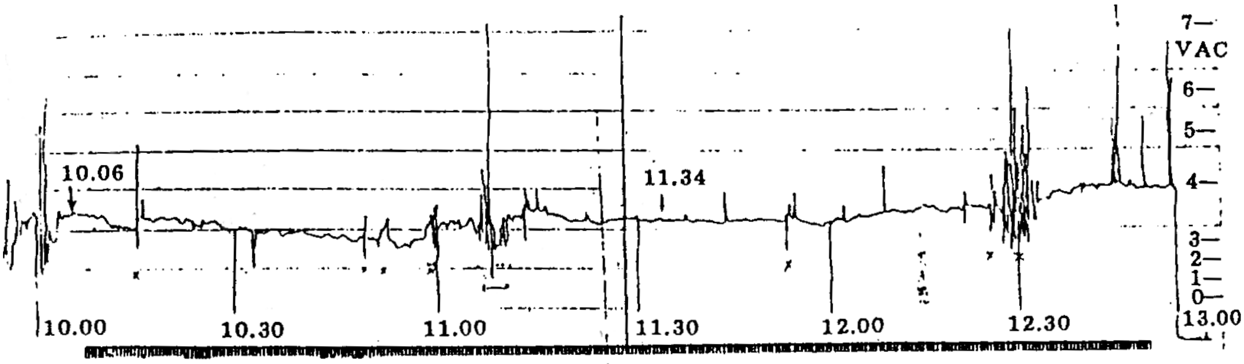

Figure 6: A copy of the oscilloscope display during the 3000 MHz observations of the 9 May 1948 partial solar eclipse, as observed from Tokyo. This little-known pioneering observation marked the start of Japan's early radio astronomy program (after Shimoda, 1982: 33).

The foundations for Shimoda's 1948 experiment can be traced back to 1930 when two 2-m parabolic reflectors were manufactured for the Aeronautical Research Institute (ARI) at the University of Tokyo (Shimoda, 1982). Following WWII the Institute of Science and Technology was established in 1947 to replace the ARI, and after completing his graduate studies in physics at the University of Tokyo (Figure 1) Koichi Shimoda began research at the Institute. He then discovered one of the two 2-m antennas among the relics of the ARI, and promptly installed a micro-wave feed at the focus and attached the dish to a 3 GHz radar receiver and observed the partial solar eclipse of 9 May 1948 (see Figure 6). This was the first radio astronomical experiment made in Japan, and it and subsequent Japanese and overseas observations of solar eclipses will be the focus of the next paper in this series on early Japanese radio astronomy.

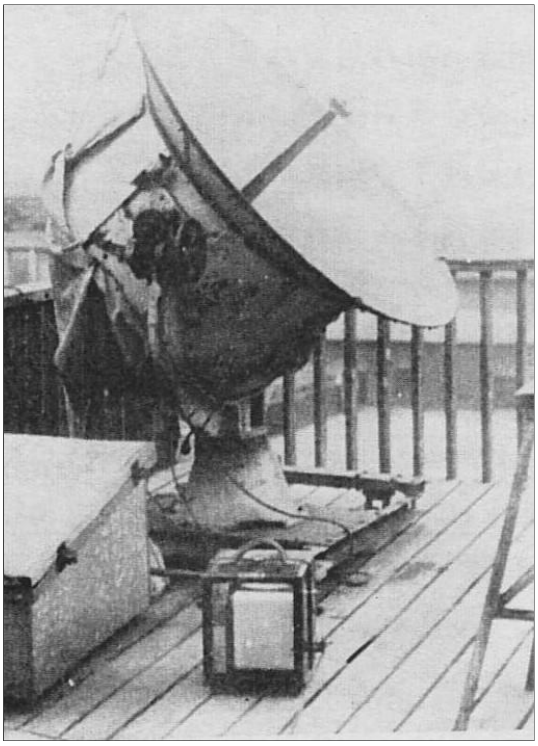

Figure 7: A photograph taken after the move to Osaka City University, showing the refurbished radio telescope which now features a solid-surface parabolic reflector in place of the horn. This instrument was used to monitor solar radio emission from April 1950 to July 1951 (after Takakura, 1985: 163).

As it turned out, Shimoda's antenna was just beginning its career in radio astronomy, for in 1951 it was transferred to the Tokyo Astronomical Observatory by Kenji Akabane and went on to do good service in the name of Japanese solar radio astronomy Shimoda, 1982).

4.2 Observations by Oda and Takakura in Osaka

After Shimoda's exploits, the next experiment in Japanese radio astronomy occurred in November 1949 when Minoru Oda (1923–2001) and Tatsuo Takakura (1925–) from the Physics Department at Osaka University (see Figure 1) observed solar noise at 3300 MHz using a hand-made metallic horn on a searchlight mounting (Tanaka, 1984). They then moved to Osaka City University, where the horn was replaced by a small parabolic dish (see Figure 7). From April 1950 Oda and Takakura (1951) used this radio telescope to monitor solar radio emission during the next 15 months.

4.3 The Solar Radio Astronomy Group at the Tokyo Astronomical Observatory

Radio astronomy began at Tokyo Astronomical Observatory (henceforth TAO) at Mitaka, Tokyo (Figure 1), in September 1949 under Professor Takeo Hatanaka (1914–1963), who was assisted by Fumio Moriyama (b. 1927) and Shigemasa Suzuki (1920–2012). They received strong support from the Director of the Observatory, Professor Yūsuke Hagihara (1897–1979), who realised the potential of this new line of research. Most of these individuals are shown in the 1954 photograph reproduced here as Figure 8.

The first radio telescope at the TAO was a 5 m × 2.5 m broadside array that operated at 200 MHz and was installed in 1949 (see Figure 9). Soon 60 and 100 MHz Yagi antennas were erected, and multi-wavelength observations of solar bursts began. This program was expanded in 1952 when a 100-140 MHz spectrometer became operational, and at the same time the radio telescope that Shimoda had used during the 1948 eclipse was set up at Mitaka for observations at 3000 MHz.

The collection of instruments was expanded further in 1953 with the completion of two more rhombic antennas (thereby allowing solar spectral observations from 200 to around 700 MHz), and with the erection of a 10-m equatorially-mounted parabolic dish that could operate at both 200 and 3000 MHz.

Figure 8: Meeting of the Japanese National Commission V of URSI held at Toyokawa Observatory in 1954. Front row (left to right): Professor A. Kimpara (Director, Institute of Atmospheric Physics, Nagoya University) and Professor Y. Hagihara (Director, Tokyo Astronomical Observatory). Back row (left to right): H. Jindo (Toyokawa), K. Akabane (TAO), T. Takakura (TAO), T. Kakinuma (Toyokawa), H. Tanaka (Toyokawa), S. Suzuki (TAO) and T. Hatanaka (TAO) (after Tanaka, 1984: 345). The only other Japanese radio astronomers active at this time, but missing from the photograph, were F. Moriyama (TAO) and T. Takahashi (Hiraiso) (after Tanaka, 1984: 345).

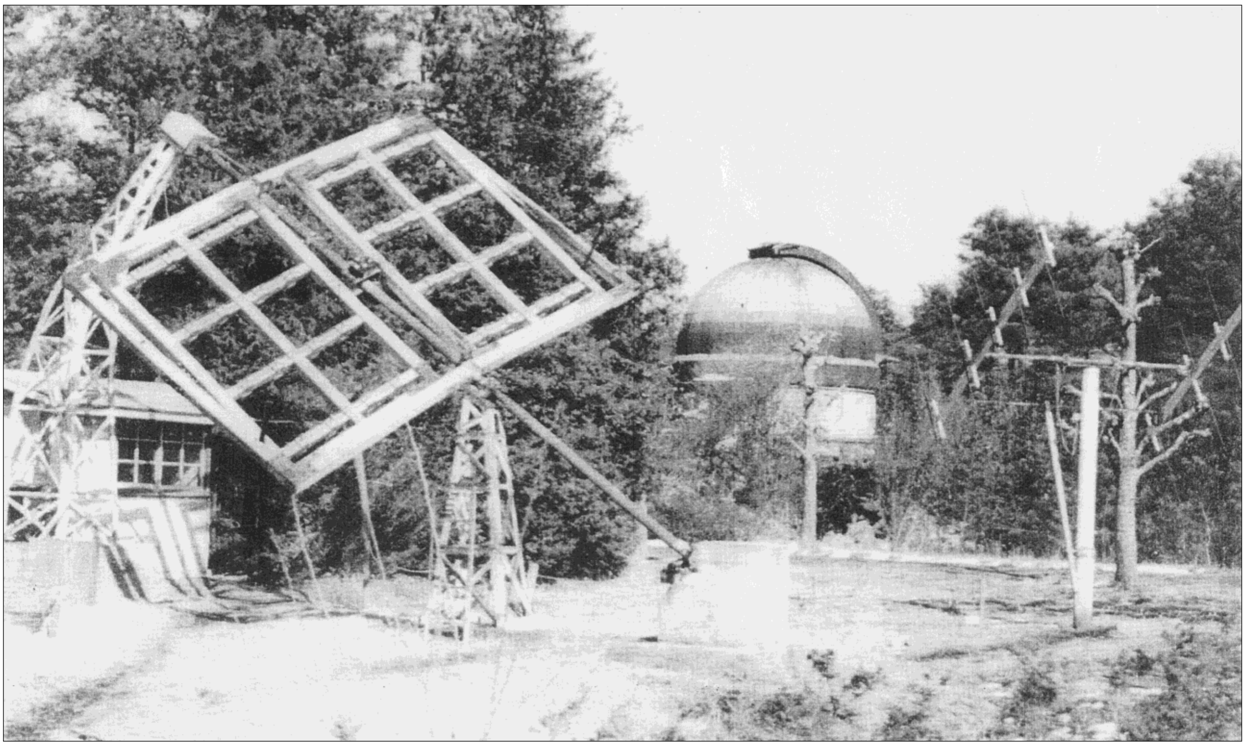

Figure 9: On the left is the broadside array that was the first radio telescope erected at the Tokyo Astronomical Observatory. From September 1949 it was used to monitor solar radio emission at 200 MHz. On the far right is a 2-element 60 MHz Yagi antenna that was installed in 1950 (courtesy: National Astronomical Observatory of Japan Archives).

At the time, this was the second-largest radio telescope of this type in the world. This dish is shown in Figure 10.

In 1954 Suzuki (1959) installed a four-element interferometer which was designed to investigate the positions of the sources of 200 MHz solar bursts. Fin-

Figure 10: The 10-m equatorially-mounted dish erected at the TAO in 1953, which was used to monitor solar radio emission at 200 and 3000 MHz (courtesy: National Astronomical Observatory of Japan Archives).

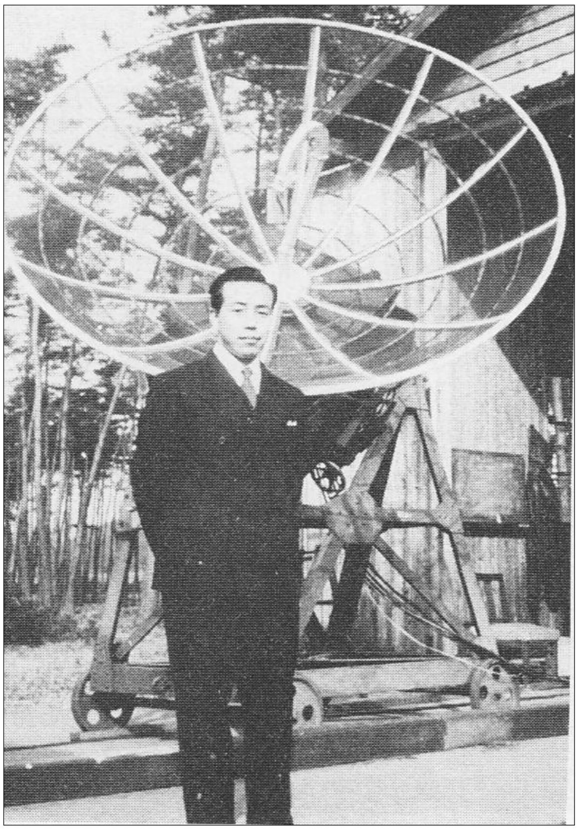

Figure 11: Haruo Tanaka and the first radio telescope installed at Toyokawa in April 1951. The parabola was 2.5 m in diameter and recorded solar emission at 3750 MHz (after Tanaka, 1984: 344).

ally, in 1957 a 1.2 m dish was erected to detect solar emission at 9500 MHz.

Most of these instruments were installed in a dedicated 'radio astronomy precinct' near the south-western boundary of the Observatory grounds, far from the main buildings and their associated electrical interference. After a short hiatus, further instruments were added from 1963 onwards.

The initial research at 200 MHz by the Tokyo Astronomical Observatory radio astronomers focussed on the relationship between solar bursts and sunspots and calcium plages (Hatanaka, Akabane, Moriyama, Tanaka and Kakinuma, 1955) and the polarization of these bursts (e.g. see Hatanaka, Suzuki and Tsuchiya, 1955a; 1955b). With the construction of the Suzuki 4-element interferometer research turned to the positions and heights of the sources responsible for the 200 MHz bursts. Given access to this new instrumentation and the low-frequency spectrometers, from 1961 Kai and Morimoto began an investigation of specific types of solar bursts, with emphasis on the characteristics, polarization parameters and source heights of Type 1, Type III and Type IV bursts (Kai, 1962; 1963; 1965; Morimoto, 1961; Morimoto and Kai, 1961; Takakura and Kai, 1961; see, also, Tsuchiya, 1963). Meanwhile, observations conducted at 3000 and 9000 MHz centred on long-term variations in solar emission at these higher frequencies (Hatanaka and Moriyama, 1953), and included observations of a partial eclipse in 1955 which produced a model for the region assumed to be responsible for the emission (see Hatanaka, Akabane, Moriyama, Tanaka and Kakinuma, 1956). By 1960 about ten TAO staff were actively studying solar radio emission.

4.4 The Solar Radio Astronomy Group at the Toyokawa Observatory

The Research Institute of Atmospherics at Nagoya University was established in June 1949 under the Directorship of Professor A. Kimpara, and a radio astronomy field station was established at Toyokawa, a former naval arsenal and radio-quiet site 60 km south-east of Nagoya (see Figure 1). The plan was to observe the Sun at high frequencies in connection with the ionospheric disturbances that impact on radio communications and terrestrial radio noise.

At the end of 1949 H. Tanaka was appointed to lead a radio astronomy group, and nearly one and a half years later he was joined by T. Kakinuma. In 1951 the first Toyokawa radio telescope was completed. This was a 2.5 m dish connected to a 3750 MHz receiver (Figure 11) and operated as a total power radiometer.

Tanaka (1984: 339) describes what happened next:

> After the completion of our first radiometer at 3750 MHz, we designed a one-dimensional grating interferometer and applied for funds for construction in 1951. The frequency of the interferometer was 4000 MHz … The budget was partly approved in 1952, and the first 5-element interferometer was completed in March 1953 …

The dishes were 1.5 m in diameter (see Figure 12). The following year this interferometer was expanded to eight elements (Tanaka and Kakinuma, 1953b). It

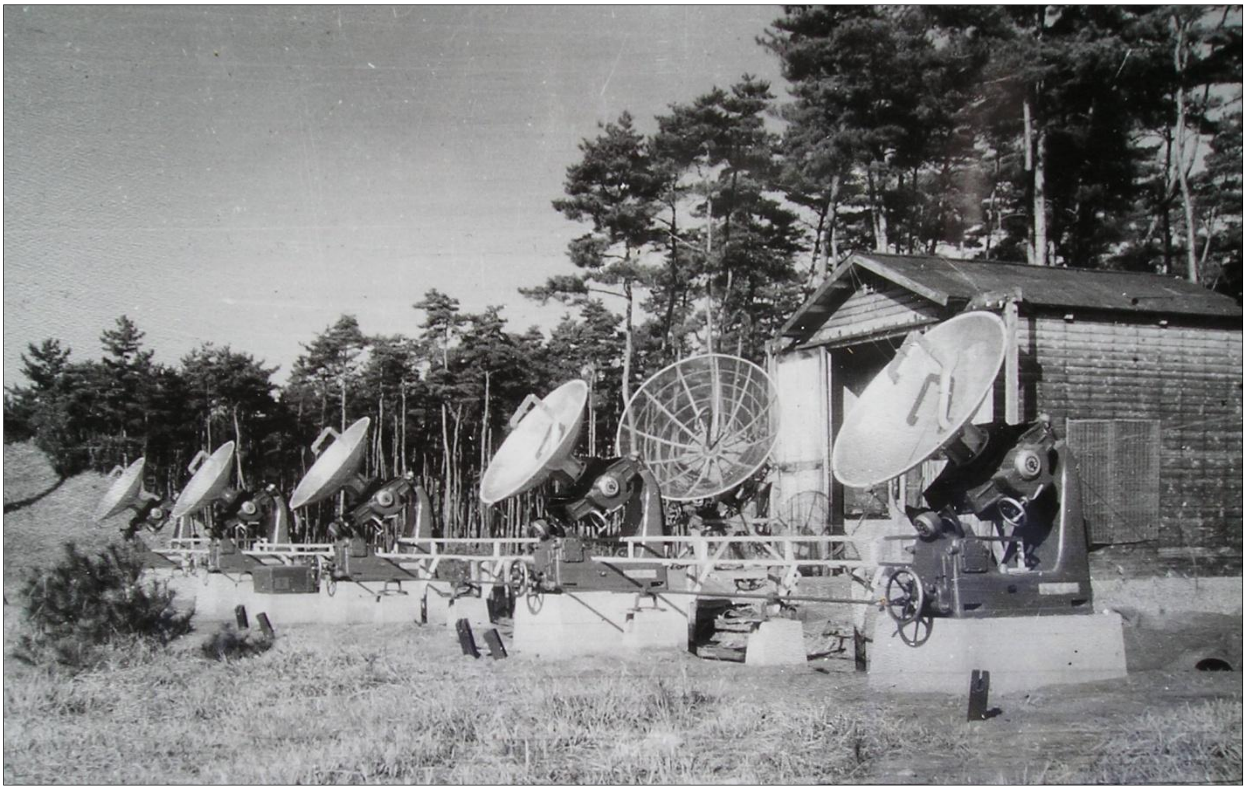

Figure 12: The 5-element E-W grating interferometer in 1953, with the original Toyokawa antenna in the background (courtesy: Tanaka Family).

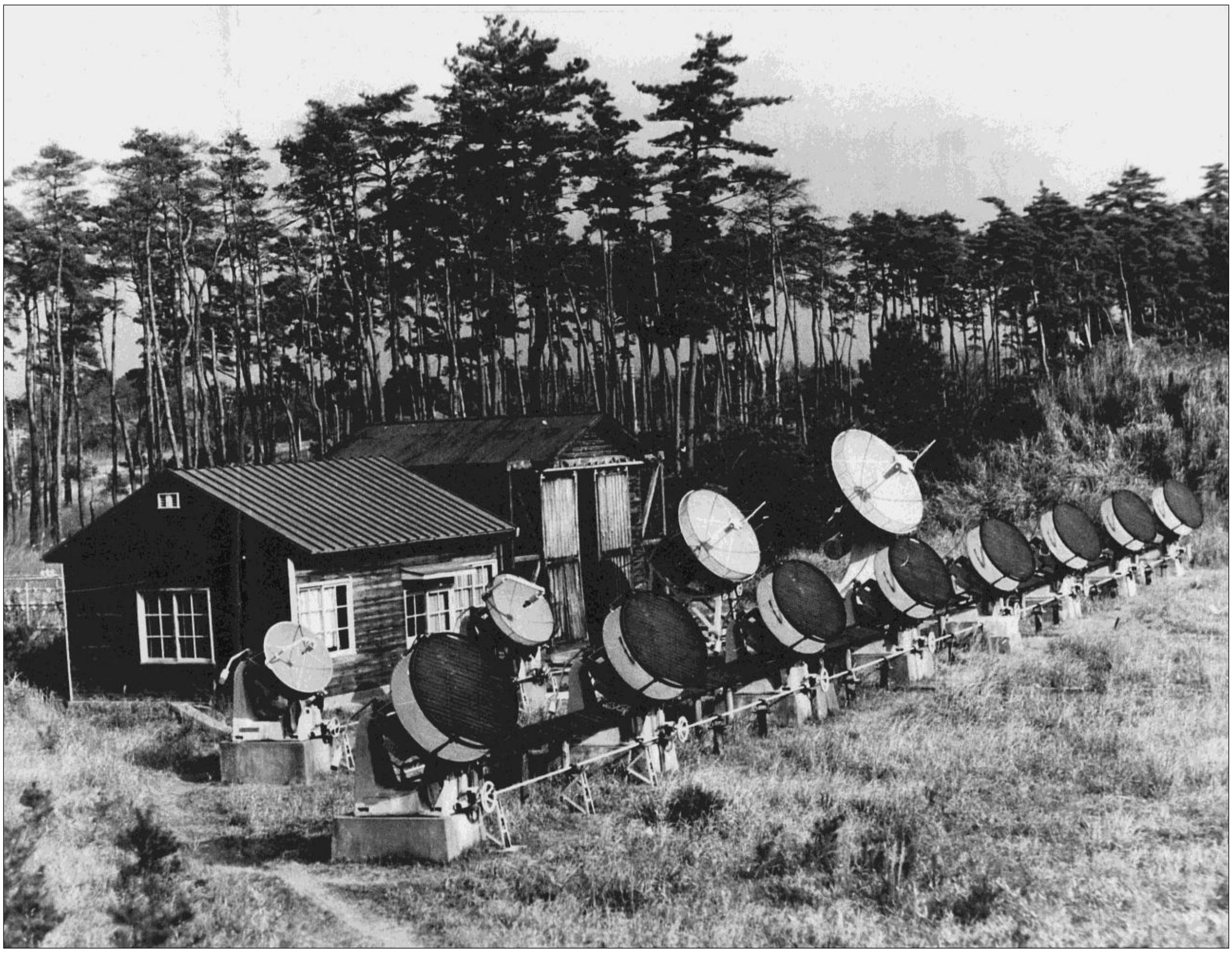

Figure 13: The expanded 8-element solar grating array, complete with polarisation screens, and behind it the four total power radiometers that monitored the Sun at 1000, 2000, 3750 and 9400 MHz (courtesy: Tanaka Family).

is important to remember (see Tanaka, 1984: 340) that this grating interferometer was planned and built quite independently of the one at Potts Hill in Sydney which was constructed by W.N. Christiansen at about the same time (see Wendt et al., 2008b). In 1954, polarization screens were added to the Toyokawa dishes (see Figure 13).

The next phase in the development of the Toyokawa Observatory involved the construction of three dishes with diameters of 3 m, 2.2 m and 1.2 m, which operated at 1000 MHz, 2000 MHz and 9400 MHz respectively (Tanaka and Kakinuma, 1956a). These were used as total power radiometers in conjunction with the original 2.5 m dish (which continued to record at 3750 MHz). These four radiometers are shown in Figure 12, behind the 8-element grating array.

The final phase in the development of the pre-1961 instrumentation at Toyokawa occurred in 1959 when another 8-element grating array was constructed, but this one utilized 1.2 m dishes and operated at 9400 MHz. During the 1960s, a two-dish antenna, another grating array, two compound interferometers and a radioheliograph were constructed.

The Toyokawa radio telescopes were used to study the characteristics of radio plages at 4000 and 9400 MHz (Kakinuma, 1956; Tanaka et al., 1956) and the intensity and polarization of bursts at these two frequencies and at 2000 and 1000 MHz (Kakinuma, 1958; Kakinuma and Tanaka, 1961; Tanaka and Kakinuma, 1956b; Tanaka and Kakinuma, 1962). Tanaka and Kakinuma (1958) also used multi-frequency observations of the partial annular solar eclipse of 19 April 1958 to examine the brightness distribution over the solar disk. International collaborative programs were also undertaken with Australian, Canadian, Indian and U.S. colleagues (e.g. see Christiansen et al., 1960; Kakinuma and Swarup, 1962; Swarup et al., 1963). By 1960 there were eleven staff members and a few students from the Faculty of Engineering at Nagoya University involved in radio astronomical research at Toyokawa.

4.5 The Solar Radio Astronomy Program of the Radio Research Laboratories, Ministry of Posts and Telecommunications

The Radio Research Laboratories of the Ministry of Posts and Telecommunications was interested in monitoring solar noise in connection with Japan's international telecommunications network (Obayashi, 1954), and maintained a field station at Hiraiso on the east coast of Japan about 150 km northeast of Tokyo (see Figure 1). In 1950 an experimental broadside array was installed, but in 1952 this was replaced by the new 200 MHz array shown in Figure 14, and regular solar monitoring began (see Takahashi et al., 1954).

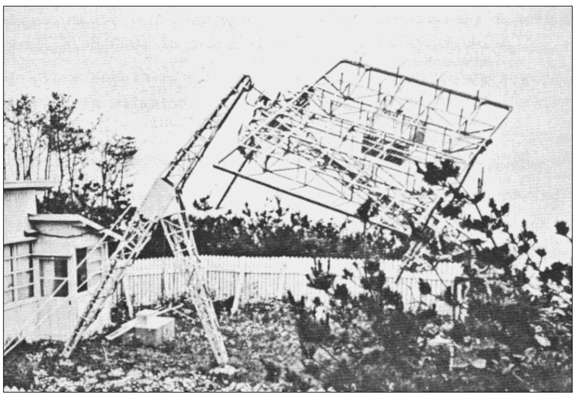

Figure 14: The broadside array at Hiraiso that was used to monitor 200 MHz solar emission from 1952 in connection with the overseas telecommunications network (after Tanaka, 1984: 347).

5 NON-SOLAR RADIO ASTRONOMY

Soon after beginning solar radio astronomy the Toyokawa group observed the background sky temperature at 3750 MHz in a bid to calibrate solar flux density at that frequency. They obtained a result of 0-5 K, which was reported in Tanaka et al. (1951), but only the abstract was written in English. Two years later a full English-language version was published (Tanaka and Kakinuma, 1953a), fourteen years before Penzias and Wilson reported the discovery of the 3 K cosmic microwave background.

Non-solar radio astronomy in Japan only began in earnest in 1963 when a 24-m spherical transit dish was erected by Kenji Akabane at the TAO and serious research began on 1420 MHz H-line emission. Then three years later (in 1966) a 10-m altazimuth-mounted parabolic antenna was erected at Toyokawa so that the Nagoya University radio astronomers could launch a serious non-solar research program.

6 DISCUSSION

6.1 Japanese WWII Radar and the Possibility of the War-time Detection of Solar Radio Emission

One of the remarkable features of WWII was the independent discovery of solar radio emission by radar operators in Norway (Schott, 1947), England (Hey, 1946), Australia (see Orchiston and Slee, 2002; Orchiston, Slee and Burman, 2006) and New Zealand (Alexander, 1946; see Orchiston, 2005). In addition, Grote Reber (1944) also detected solar radio emission at this time in the course of his study of galactic radiation (see Sullivan, 2009). All of these war-time detections were made at meter-wavelengths, but Southworth (1945) was also successful in detecting solar radio emission at cm wavelengths.

Since the radar detections in Norway, Britain, Australia and New Zealand were at first mistaken for interference or some ingenious jamming mechanism developed by the enemy, the question arises as to whether solar radio emission was ever recorded by Japanese WWII radar operators. To our knowledge, there are no published accounts of this occurring, but since reasonable numbers of metre-wave land-based radars were operated by the Imperial Japanese Army and the Imperial Japanese Navy around the coasts of Japan during the latter stages of the War (Nakagawa, 1997; Nakajima, 1988) and potentially these were capable of solar detections, a systematic examination of Japanese war-time radar records is justified. The Sun was active at this time, so we estimate that the prospects of a successful search are reasonably high. The Imperial Japanese Navy also maintained reasonable numbers of microwave radars (see Wilkinson, 1946), and in light of Southworth's detections, their records also deserve to be scrutinized.

6.2 Instrumentation: The Original Idea of the Solar Grating Interferometer

In the course of their solar observations at the Osaka City University, Oda's group developed the concept of a grating interferometer that would operate at 4000 MHz and would be used to identify the locations of the sources responsible for the solar noise. The interferometer would consist of 25 circular horns each 50-cm in diameter arranged in the configuration illustrated in Figure 15. While this interesting concept was presented at the annual assembly of the Physical Society of Japan in 1950 (see Ojio et al., 1950), it was never acted on. Had it been, then Japan rather than Australia would have hosted the world's first solar grating array. However, Tanaka was inspired by this idea, which led him to construct the grating interferometers at Toyokawa mentioned above in Section 4.4.

6.3 Early Japanese Radio Astronomy in International Context

It is notable that all of Japan's early (pre-1961) radio astronomical investigations focussed on the Sun, and even Tanaka and Kakinuma's measurement of what we would now term the 'cosmic microwave background' was motivated by solar observations. However, as Sullivan (2009: 225) has pointed out, this solar pre-occupation is easy to understand considering "Japan's long tradition of research on the ionosphere and radio communications …, [which was] natural for an island nation …" Note that the solar program at Hiriaso was linked to Japan's telecommunications efforts, and this was also the motivation for the early initiatives at Toyokawa (although this was soon to change).

Let us now focus on international solar radio astronomy. Table 1, which is adapted and developed from Stewart, 2009: 263-265 and Stewart, Wendt, Orchiston and Slee, 2011: 618-621, lists the most significant developments that occurred in instrumentation between 1948 and 1960. While it is apparent that Australia was at the forefront of solar radio astronomy during this period (e.g. see Sullivan, 2005; 2009; Orchiston and Slee, 2005; Stewart, Wendt, Orchiston and Slee, 2011), both France and Japan played very prominent roles.

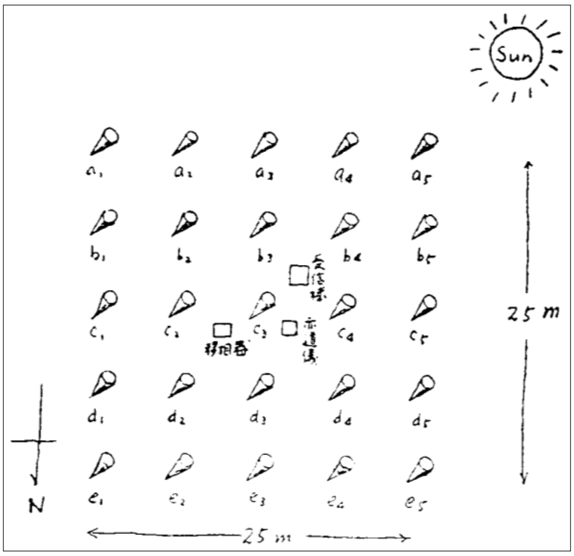

Figure 15: The solar grating array that was designed by Oda's group in 1950 but was never built (after Tanaka, 1984: 338).

The vibrant Solar Group in the CSIRO's Division of Radiophysics in Sydney was responsible for developing the world's first radio-spectrograph (1949), position interferometer (1949), solar grating array (1951) and crossed-grating interferometer (1957), but Japan was quick to follow: its first radio-spectrograph was installed at Mitaka in 1952; its first grating array was constructed at Toyokawa in 1953 (quite independently of the Australian initiative); and a position interferometer was operational at Mitaka by 1954. In each case, Japanese radio astronomers were the first, after their Australian colleagues, to construct these innovative types of radio telescopes and use them to investigate the nature of burst emission and the 'slowly-varying component'.

Table 1: Significant Developments in International Solar Radio Astronomy, 1949-1960. Japanese entries are shown in red print.

1948

• In Australia, solar observations at 18.3, 19.8, 60, 65 and 85 MHz using Yagi-Uda antennas were commenced at the Division of Radiophysics Hornsby Valley field station on the northern outskirts of Sydney (Payne-Scott, 1949; see also Goss and McGee, 2010).

• In Australia, solar observations at 24,000 MHz commenced at the Division of Radiophysics Headquarters in cental Sydney using a recycled WWII searchlight dish (Piddington and Minnett (1949).

• In Australia, solar observations at 200, 600 and 1200 MHz using a recycled experimental WWII radar antenna commenced at the Division of Radiophysics Georges Heights field station in suburban Sydney (Lehany and Yabsley, 1948; see also Orchiston, 2004).

• In France, solar observations at 555 MHz using 7.5-m Würzburg antennas began at Meudon (Laffineur and Houtgast, 1949); they were extended to 255 MHz in 1949 (Laffineur, 1954; see also Orchiston et al., 2007).

• In the Netherlands, solar monitoring at 75 MHz with a corner reflector and at 140 and 200 MHz with a 7.5-m Würzburg antenna began at three sites; extended in 1951 to a world-wide network for the study of solar effects on the ionosphere (de Voogt, 1952; see also Strom, 2005).

• In the New Zealand, solar monitoring at 100 MHz with a twin Yagi-Uda antenna at Auckland University College leads to the world's first graduate thesis on solar radio astronomy (Maxwell, 1948; see also Orchiston, 2005a).

1949

• In Australia, the world's first radio-spectrograph operating at 70-140 MHz was constructed at the Division of Radiophysics Penrith field station, in Sydney. Observations led to the classification of Type I, II and III bursts (Wild and McCready, 1950; see also Stewart et al., 2010).

• In Australia, the world's first swept-lobe interferometer was installed at the Division of Radiophysics Potts Hill field station in Sydney, Australia, to measure source positions and polarizations of solar bursts at 97 MHz (Little and Payne-Scott, 1951; see also Wendt et al., 2011).
• In Japan, solar monitoring with a 200 MHz broadside array commenced at the Tokyo Astronomical Observatory, Mitaka (Tanaka, 1984; see also Ishiguro and Orchiston, 2013).
• In Japan, solar monitoring at 3,300 MHz using a horn mounted on a recycled searchlight mounting commenced at Osaka University; the horn was replaced by a 1-m dish in 1950 at Osaka City University (Oda and Takakura, 1951; see also Ishiguro and Orchiston, 2013).
• In Russia, metre wavelength studies of the Sun were begun by FIAN in the Crimea (Chikhachev, 1950; Salomonovich, 1984).

1950

• In Japan, solar monitoring with 60 and 100 MHz Yagi-Uda antennas commenced at the Tokyo Astronomical Observatory, Mitaka (Tanaka, 1984; see also Ishiguro and Orchiston, 2013).

1951

• In Australia, the world's first solar grating array was installed at the Division of Radiophysics Potts Hill field station to investigate the one-dimensional distribution of radio brightness across the solar disk at 1420 MHz. This consisted of 32 x 1.7 m antennas on a 213 m east-west baseline; a north-south array was added in 1953 (Christiansen, 1953; Christiansen and Warburton, 1955; see also Wendt et al., 2008b).
• In Canada, 2,800 MHz strip-scans of the Sun commenced at Goth Hill using a slotted waveguide array (Covington and Broten, 1954).
• In Japan, solar monitoring at 3,750 MHz using a 2.5-m dish commenced at Nagoya University's Toyokawa Observatory (Tanaka, 1984; see also Ishiguro and Orchiston, 2013).

1952

• In Australia, a new radio-spectrograph operating at 40-240 MHz was installed at the Division of Radiophysics Dapto field station to the south of Sydney, Australia. This led to the first detection of harmonic structure in Type II and III bursts (Wild, Murray, and Rowe, 1954; Wild, Roberts and Murray, 1954; see also Stewart, Orchiston and Slee., 2011).
• In Japan, a 100-140 MHz radio-spectrograph was installed at Mitaka for research on solar bursts; in 1953 this was joined by a 200-700 MHz radio-spectrograph (Tanaka, 1984; see also Ishiguro and Orchiston, 2013).

1953

• In Japan, a 10-m diameter equatorially-mounted dish was erected at Mitaka and used for solar monitoring at 2000 and 3000 MHz (Tanaka, 1984; see also Ishiguro and Orchiston, 2013).
• In Japan, a 4,000 MHz 5-element grating interferometer was constructed at the Toyokawa Observatory for one-dimensional solar mapping (Tanaka and Kakinuma, 1953b); this array was extended to 8 elements in 1954 (Tanaka, 1984; see also Ishiguro and Orchiston, 2013).

1954

• In Australia, the east-west grating array at the Division of Radiophysics Potts Hill field station was converted to 500 MHz and used to measure the one-dimensional distribution of radio brightness across the solar disk and evidence for limb-brightening (Swarup and Parthasarathy, R., 1955; see also Wendt, Orchiston and Slee, 2008b).
• In Japan, a 201 MHz four-element multi-phase interferometer was installed at Mitaka to measure the positions of the sources of solar bursts (Suzuki, 1959; see also Ishiguro and Orchiston, 2013).

1955

• In France, a two-element variable-baseline interferometer was set up at Nançay for synthesis mapping of solar active regions at 9350 MHz (Kundu, 1959; see also Orchiston et al., 2009).

1956

• In France, at Nançay, the 169 MHz Grande Interferometer consisting of 32 x 5 m antennas on a 1600 m east-west baseline began observations (Blum et al., 1957); a north-south arm was added in 1959 (see also Pick et al., 2011).
• In Canada, regular one-dimensional solar mapping began at 3,000 MHz using a compound interferometer at Goth Hill (Covington and Broten, 1957); in 1959 this was converted to a 4-element array (Covington, 1984).
• In Japan, three single-dish polarimeters were installed at 1,000, 2,000 and 9,400 MHz at the Toyokawa Observatory (Tanaka, and Kakinuma, 1956; see also Ishiguro and Orchiston, 2013).
• In the USA, Harvard University's radio-spectrograph at Fort Davis, Texas, began recording solar bursts over the 100-580 MHz band (Maxwell et al., 1958; this was extended to 25-580 MHz in 1959 and 2100-3900 MHz in 1960 (Thompson, 1961; see also Thompson, 2010).

1957

• In Australia, the world's first crossed-grating interferometer was installed at the Division of Radiophysics Fleurs field station to generate daily two-dimensional isophote maps of solar emission at 1423 MHz (Christiansen and Mathewson, 1958; see also Orchiston and Mathewson, 2009).
• In Australia, a swept-frequency interferometer operating at 40-70 MHz was installed at the Dapto field station to investigate the positions of the sources of solar bursts (Wild and Sheridan, 1958; see also Stewart et al., 2011).
• In France, one-dimensional solar mapping began at 9,350 MHz using a 16-element east-west array at Nançay (Pick and Steinberg, 1961; see also Pick et al., 2011).
• In the USA, the University of Michigan's 100-580 MHz radio-spectrograph began observing solar bursts (Haddock, 1958).
• In the USA, an east-west grating array constructed by the Department of Terrestrial Magnetism at the Carnegie Institute of Washington began observations at 340 MHz and 87 MHz (Firor, 1959, Kundu and Firor, 1961).

1958

• In Australia, the Dapto radio-spectrograph was extended to 25-210 MHz (Sheridan et al., 1959).

• In Russia, 35,000 MHz solar observations began at Puschino (Salomonovich, 1984).

1959

• In the Netherlands, a 254 MHz interferometer and a 200 MHz polarimeter at NERA were used to study noise storms (Fokker, 1960; Cohen and Fokker, 1959).
• In Japan, an 8-element grating array operating at 9400 MHz was installed at the Toyokawa Observatory (Tanaka, 1984; see also Ishiguro and Orchiston, 2013).
• In Norway, a high-time and high-frequency resolution spectrograph operating over the 140-170 MHz and 310-340 MHz bands was installed by the University of Oslo (Elgaroy, 1961).
• In the USA, a radio-spectrograph operating at 500-900 MHz began observations at Owens Valley (Young et al., 1961)
• In the USA, a 15-38 MHz radio-spectrograph and two-element interferometers operating at 18 and 38 MHz set up at the High Altitude Observatory, University of Colorado, were used to study Type I noise storms and Type III bursts (Boischot, and Warwick, 1959).

1960

• In Australia, the Dapto radio-spectrograph was extended to 15-210 MHz (Sheridan and Trent, 1960).
• In the USA, solar mapping began at 3,260 MHz using the Stanford University compound interferometer crossed array (Bracewell and Swarup, 1961; see also Bracewell, 2005).

By the end of the 1950s, less than two decades after the full-frontal attack on solar radio astronomy mounted by Australia and Canada immediately following WWII, other nations had joined the challenge, and apart from the French and Japanese efforts notable contributions were being made by the Netherlands, Norway, Russia and the USA (see Sullivan, 2009). Japanese solar radio astronomers were in good company, and their research was highly valued. The fact that they generally published in English (unlike their French colleagues) was one of the reasons for this. As a result, their research results were widely available to the international solar radio astronomy community.

While Japan may have made an important international contribution to solar radio astronomy in the decade and a half following WWII, what is puzzling is that there was no attempt at this time to observe 'radio stars'. These discrete localized sources of intense radio emission were reported in the international literature by British and Australian radio astronomers and "… long remained the most mysterious and hotly-debated [objects] in astronomy." (Sullivan, 2009: 101). Cygnus-A, the first radio star, was announced in *Nature* by J. Stanley Hey, S. John Parsons and James W. Phillips in 1946, and by 1950 when three Japanese groups were actively involved in radio astronomy, the number of confirmed discrete sources had grown to seven (see Sullivan, 2009: Table 14.1 on page 316), and Sydney-based John Bolton, Gordon Stanley and Bruce Slee (1949) had correlated three of them with distinctive galactic and extragalactic optical objects, namely the Crab Nebula (Taurus-A), Messier 87 (Virgo-A) and NGC 5128 (Centaurus-A). Two of these identified sources, and Cygnus-A, were ideally located in the northern sky and the Japanese certainly had the requisite instrumentation to join in the investigation of these enigmatic objects but chose not to, and even when the steerable 10-m dish was erected at the NAO in 1953 its very obvious non-solar potential was all but ignored.

6.4 Early Japanese Radio Astronomy in the Context of the Development of Optical Astronomy in Japan Between 1930 and 1960

In other parts of the world, following an initial period of caution, even mistrust, of radio astronomers by optical astronomers (e.g. see Jarrell, 2005; Sullivan, 2009), at a national level the growth of radio astronomy and astrophysics often went hand-in-hand. In Japan, however, the emergence of radio astronomy appears to have occurred in comparative isolation, with little if any inspiration from developments that were occurring in Japanese optical astronomy at the time, as the following review indicates.

The development of twentieth century optical astronomy in Japan is discussed by Nakamura (2008; 2013) and his colleagues (Nakamura et al., 2008) and by Tajima (2011), and it is significant that although the Tokyo Astronomical Observatory (TAO) completed its move to the dark-sky Mitaka site on the outskirts of Tokyo in the mid-1920s and acquired a 65-cm Zeiss refractor with a 38-cm guide scope in 1929, there was little effort to redirect Japan from classical astronomy to astrophysics at this time. Nakamura (2013) explains why:

> In spite of astronomers' expectations for this first large telescope, this telescope thereafter did not bring about any conspicuous scientific outcomes. The reasons for the failure are considered to be due to the large chromatic aberration of the objective lens of the telescope, and the world trend in astrophysical studies [by this time] had already shifted to using 1m-class reflectors; a refractor as small as 65cm in diameter obviously was insufficient for up-to-date astrophysical observations.

Nevertheless, in 1939 Sekiguchi et al. published a short paper on the spectra of 30 A-B stars obtained using the Zeiss telescope in order "… to make a quantitative analysis of their hydrogen absorption lines …" (ibid.), but in the overall context of Japanese optical astronomy these were to remain isolated and anachronistic astrophysical observations until the advent of the Okayama Astrophysical Observatory (OAO; see Figure 1) in 1960—at the very end of the period under review.

Yet the origin of the OAO can be traced back to the immediate post-WWII period when Japanese radio astronomy also was experiencing its first awakening. In 1948 Yusake Hagihara, the Director,

> … reorganized the TAO, creating new posts for researchers and introducing a series of new research divisions … Japanese astronomers wanted to embark on front-line astrophysical studies of stars and galaxies, but they had no telescope which was capable of

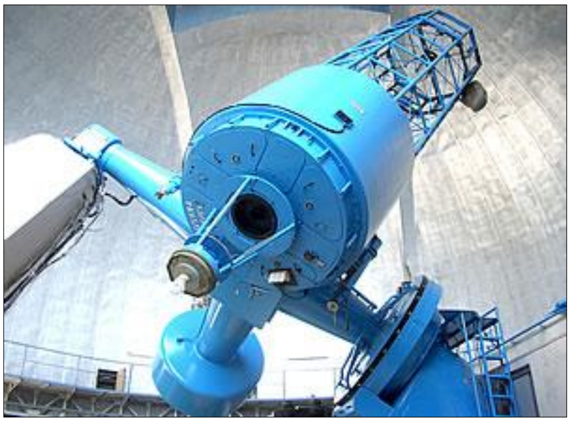

Figure 16: The 1.88-m Grubb Reflecting Telescope at the Okayama Astrophysical Observatory (after (Tajima, 2011: 220).

> carrying out detailed spectroscopic investigations of such objects. (Tajima, 2011: 218-219).

With support from the Science Council of Japan the TAO started lobbying for a large telescope, which ultimately resulted in the acquisition of a 74-in (1.88-m) Grubb reflector (Figure 16). When the OAO opened in 1960 Japanese astronomers finally had access to a locally-based telescope designed for astrophysical research, and "A whole new generation of astronomers and technicians was trained through the operation of the OAO, which greatly enhanced the growth of the community of Japanese optical astronomers." (Tajima, 2011: 220).

While the rapid early development of radio astronomy in Japan did not foster galactic and extragalactic astrophysical research, the story was more promising for solar astronomy, but only after an abortive start. In 1922 Einstein visited Japan, and this possibly sparked the concept of constructing an Einstein Tower[3] at the TAO modelled on the original one at the Potsdam Astrophysical Observatory in Germany. Japan's Einstein Tower (Figure 17) was completed in 1930, but it was only in the post-WWII era that it began to contribute to solar physics (Nakamura, et al., 2008). However, optical solar astronomy in Japan really came of age with the advent of the Norikura Solar Observatory (NSO; see Figure 1):

> From its foundation in 1949, observations using its 10 cm and 25 cm aperture coronagraphs were conducted continuously until it was closed in March 2010 … this observatory played an important role in the development of solar astronomy in Japan … (Tajima, 2011: 217).

The NSO was an outstation of the TAO, and perhaps the fact that about half of Japan's early radio astronomers also were employed by the TAO might explain their overwhelming preoccupation with solar radio astronomy, even if—initially—there was little research collaboration between the two groups.

Finally, we may conclude that through the combined efforts of the early Mitaka and Toyokawa solar radio astronomers Japan was able to dramatically increase its international visibility in solar physics at a time when most Japanese optical astronomers were struggling to break free from the long-entrenched shackles of classical astronomy in order to embrace the 'new astronomy', astrophysics

6.5 Heritage Issues: The Survival and Preservation of Japan's Early Radio Telescopes

One of the projects of the IAU Working Group on Historic Radio Astronomy is to compile a worldwide inventory of all surviving pre-1961 radio telescopes, and—where relevant—lobby for their preservation. It is a sad fact that none of the early Japanese radio telescopes described in this paper has survived, although a full-scale replica of the initial 200 MHz TAO broadside array, incorporating the original polar axis from Mitaka, has been erected at the Nobeyama Radio Observatory (Figure 1) and is accessible to visitors (see Figure 18).

A field examination of the original 'radio astronomy precinct' at Mitaka (Figure 19) in December 2011 failed to reveal any vestiges—even founda-

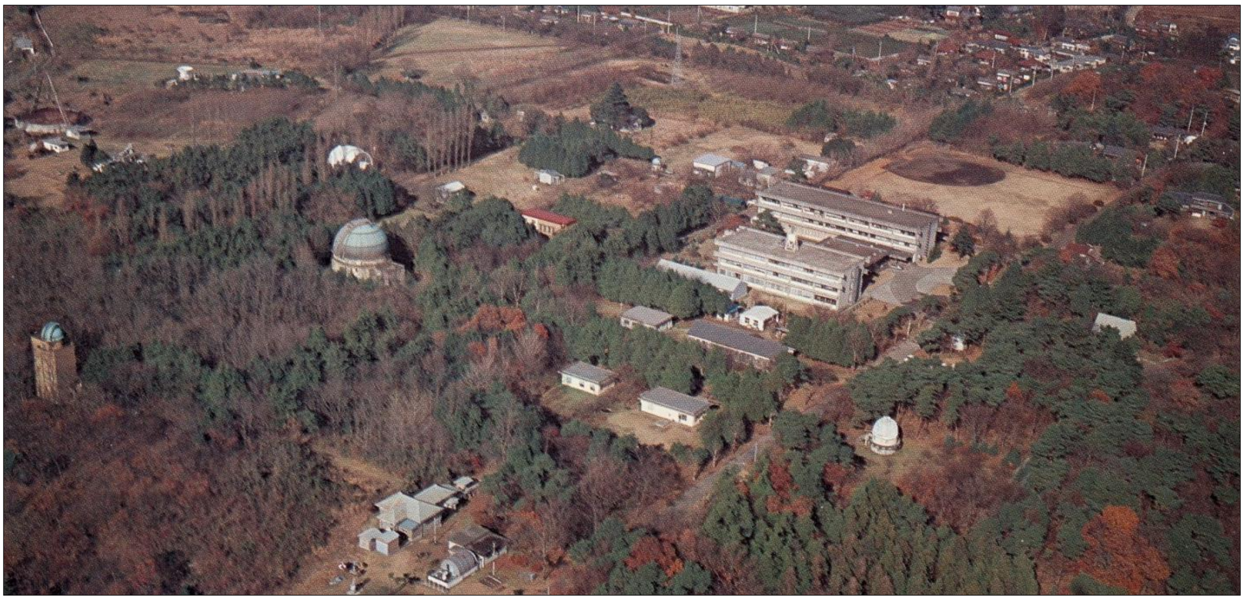

Figure 17: A later aerial view of the Mitaka precinct with the brown brick green-domed Einstein Tower on the extreme left of the image. Directly above it, just beyond the wooded area, is the radio astronomy precinct (courtesy: National Astronomical Observatory of Japan Archives).

Figure 18: A replica of the original TAO broadside array on display at the Nobeyama Radio Observatory. Only the polar axis is from the original radio telescope of 1949 (photograph: W. Orchiston).

tions—of the original instruments at this site, but early in 2012 an interpretative display panel was installed at the site of the 10m parabolic antenna (see Figure 20). As Japan's foremost radio astronomical institution it would be appropriate for the National Astronomical Observatory of Japan to develop this historic precinct further by erecting a full-scale replica of the 200 MHz broadside array, modelled on the one now on display at Nobeyama.

Likewise, a visit by the authors to the Toyokawa Observatory site in December 2010 failed to reveal remains from any of the antennas discussed in this paper, but just prior to the visit a number of rusting antennas belonging to a T-shaped solar grating array erected by the first author of this paper in the 1970s were discovered during a detailed examination of the area by Dr T. Watanabe. Two of these antennas are shown in Figure 21, surrounded by dense vegetation.

Apart from a number of buildings in varying stages of preservation (e.g. see Figure 22) and the rusting antennas of the T-array, the site of the Toyokawa Radio Observatory contains no other surviving evidence of its pivotal role in early Japanese radio astronomy. But what the site does contain is an amazing assemblage of tunnels, bunkers, earthworks and other field evidence that reflects its important military associations during the 1930s and through into WWII (see Figure 23). Now that the site is no longer required by the Nagoya University we hope that it will be developed as a heritage park where its important military and radio astronomical associations can be interpreted for the benefit of future generations. If such a program proves impossible to implement then we recommend that *all* of the surviving rusting antennas of the 1970s T-array be removed and relocated to the historic radio astronomy precinct at the National Astronomical Observatory of Japan in Mitaka.

7 CONCLUDING REMARKS

If we discount Nakagami and Miya's examination of the Dellinger Effect in 1938 we can conclude that Japanese radio astronomy began in 1948 when Shimoda observed the 9 May solar eclipse from the University of Tokyo. By the early 1950s, small groups at the Osaka City University, the Radio Research Laboratories at Hiraiso, the University of Nagoya (Toyokawa) and the Tokyo Astronomical Observatory (Mitaka) were actively involved in solar radio astronomy. Healthy competition between the two largest groups, at Toyokawa and Mitaka, was very effective in promoting the development of radio astronomical research in Japan, and inspired the construction of an impressive range of instruments designed to investigate solar radio emission between 60 MHz and 9000 MHz. Eventually this culminated in the merger of the two groups and the construction of the Nobeyama Radioheliograph. In this way, Japan was able to play an important part in the early development of international solar radio astronomy, but the value of its overall contribution was even more significant if we allow for the pivotal role that the Yagi-Uda antenna played in the early development of both solar and non-solar radio astronomy world-wide.

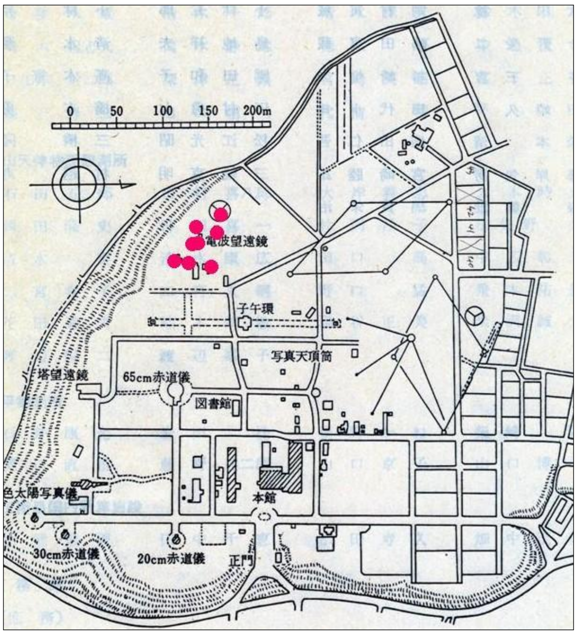

Figure 19: Map of the TAO grounds showing the Main Building and Library wing (the hatched buildings on the eastern side of the site, near the entrance gate) and to the west of them the radio astronomy precinct, where the various red dots mark the positions of different radio telescopes. The largest and most westerly of these was the 24m transit antenna (courtesy: National Astronomical Observatory of Japan Archives).

Figure 20: A recent photograph showing the interpretative panel installed at the site of the 10-m parabolic antenna at Mitaka (courtesy: NAOJ).

Figure 21: Two of the surviving antennas of a T-shaped array erected in the 1970s, discovered overgrown by dense vegetation just prior to a visit to the Toyokawa Observatory site by the first two authors of this paper in November 2010 (photograph: W. Orchiston).

Figure 22: A photograph of the main research building at the Toyokawa Observatory site taken in November 2010. This majestic building is now no longer used by the University of Nagoya and is now surplus to requirements, but is still in sound condition and could easily be utilised in any future development of the site as a heritage precinct (photograph: Wayne Orchiston).

Figure 22: One of the tunnels and bunkers which reflects the original military role of the site, photographed in November 2010. Some of these bunkers were used to house the radio telescope receiving equipment (photograph: Masato Ishiguro).

Initially solar research was the mainstay of Japan's early involvement in radio astronomy, but with the passage of time the fledgling non-solar radio astronomy community grew rapidly and its efforts finally crystallised in the construction of the Nobeyama Millimeter Array, the 45-m Radio Telescope at Nobeyama and eventually the Atacama Large Millimeter/ submillimeter Array (ALMA). Japan is now seen as a leading international contributor to solar and non-solar radio astronomical research.

8 NOTES

1. This ambitious international project is conducted under the auspices of the IAU Working Group on Historic Radio Astronomy, and follows the succession completion of a similar project that documented early French radio astronomy through a series of seven papers that were published in this journal between 2007 and 2011.
2. Parts of this first paper in the series on early Japanese radio astronomy draw heavily on the review paper by Ishiguro and Orchiston (2013) that was prepared recently for the book *The History of Astronomy and Development of Astrophysics in Asia* which will be published by Springer in 2013.
3. The original 'Einstein Tower' at the Potsdam Astrophysical Observatory was erected in 1924 to facilitate research on the solar spectrum (see Hentschel, 1997). Hermann Brück (2000: 123) describes this famous solar telescope:

> The instrument used two mirrors of a coelostat … to send the Sun's light vertically down on to a lens of 60-cm aperture and focal length 14.5 metres. The solar beam, turned into a horizontal direction by an auxiliary mirror, was then thrown into a large prism or gating spectrograph with a collimator of 12 metres focal length … The Einstein Tower was acclaimed as a truly modern and effective instrument which would lead to significant advances in the field of relativity theory and of solar physics in general.

While the Einstein Tower at the Tokyo Astronomical Observatory was inspired by the Potsdam prototype, its rather rustic building lacks the architectural charm of its German counterpart.

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Dr Masato Ishiguro is a Professor Emeritus of the National Astronomical Observatory of Japan (NAOJ). He started his research in radio astronomy at Nagoya University in 1970 where he investigated radio interferometry techniques. In 1980, he moved to Tokyo Astronomical Observatory of the University of Tokyo to join the project to construct large millimeter-wave telescopes at the Nobeyama Radio Observatory (NRO) where

he was in charge of constructing the Nobeyama Millimeter Array. He was the Director of the NRO from 1990 to 1996 and contributed to the open use of the telescopes. While doing research at the NRO, he worked over a plan of a large array at millimeter and submillimeter wavelengths. Since 1998, he had been leading the Japanese part of the international project to construct the Atacama Large Millimeter/submillimeter Array (ALMA) in Chile. He was a Professor at the NAOJ from 1988 until he retired in 2009. He is now a member of the IAU Working Group on Historic Radio Astronomy.

Dr Wayne Orchiston is an Associate Professor of Astronomy at James Cook University in Townsville, Australia, but for much of 2012 has been a Visiting Professor at the National Astronomical Research Institute of Thailand in Chiang Mai and moves there in January 2013 following the close-down of astronomy at James Cook University. In his earlier years Wayne worked in the CSIRO's Division of Radiophysics in Sydney and later in its successor, the Australia Telescope National Facility, and he has published extensively on the history of radio astronomy. Wayne was the founding Chairman of the IAU's Working Group on Historic Radio Astronomy, and he currently serves as its Secretary.

Dr Kenji Akabane is a Professor Emeritus of National Astronomical Observatory of Japan (NAOJ). He started solar radio observations at a microwave frequency at the Tokyo Astronomical Observatory (TAO) of the University of Tokyo in 1952. He stayed in the United States from 1957 to 1960, studying at Cornell University and the University of Michigan. After coming back from the US, he showed a great interest in the field of cosmic radio astronomy and led a project to construct the 6-m millimeter wave radio telescope on the campus of TAO, which was completed in 1970. After that he was motivated to construct the largest millimeter wave radio telescope in the world, and was appointed as the founding Director of the Nobeyama Radio Observatory (NRO) in 1982. He was a Professor at the NAOJ from 1970 until he retired in 1987. He then worked at the University of Toyama from 1987 to 1992, and then was the President of Matsusho Junior College for seven years, from 1992.

Professor Norio Kaifu has a D.Sc. from Tokyo University, and specializes in radio astronomy, infrared astronomy and star formation. He was one of the founders of the Nobeyama Radio Observatory, led the construction of the Nobeyama 45-m mm-wave radio telescope, and found many exotic organic molecules in dark clouds. Then he was involved in the construction of the 8-m aperture Subaru Telescope on Mauna Kea as Director, before becoming Director General of the National Astronomical Observatory of Japan in 2000. As Director General he led NAOJ to be one of the three major international partners in the ALMA Project. Since his retirement as NAOJ Director, he has served the Science Council of Japan as President of the Natural Science & Engineering Division, and also was a Professor of the Open University of Japan until 2012. Currently he is President of the International Astronomical Union. In addition to 150 papers in astronomy he has published some 30 books in Japanese, both for students and for general readers. Well-known books among these are: *From Galaxy to the Universe* (Shin-Nihon-Shinsho, 1972), *Cosmic Radio Astronomy* (co-authored with K, Akahane and H. Tabara, Kyoritsu Shuppan, 1986 and 2012), *Poetries of the Universe* (Chukou Shinsho,1999), *101 Books to Understand the World* (Iwanami Shoten, 2011), etc. Besides several honors in science, Norio Kaifu was awarded the Mainichi Book-Review Award in 2011, and was honored when minor planet 6412 Kaifu was named after him.

Dr Masa Hayashi is Director General of the National Astronomical Observatory of Japan (NAOJ). He trained in radio astronomy at the Nobeyama Radio Observatory of NAOJ and became an Assistant Professor at the University of Tokyo in 1987. He moved to the Subaru Project at NAOJ in 1994, became a Professor of NAOJ in 1998, and served as Director of the Subaru Telescope from 2006 to 2010. He then was a Professor of Astronomy at Tokyo University from 2010 to 2012, before accepting his current position.

Dr Tsuko Nakamura is a Professor at the Teikyo-Heisei University in Tokyo, where he has taught information sciences since 2008. His research interests lie in observational statistics of very small asteroids and Japanese history of astronomy, and he has published about forty papers in the second field. His books include *Five Thousand Years of Cosmic Visions* (2011, in Japanese, co-authored by S. Okamura), *Mapping the Oriental Sky*, Proceedings of the ICOA-7 Conference (2011, co-edited by W. Orchiston, M. Sôma, and R. Strom), and *Highlighting the History of Astronomy in the Asia-Pacific Region*, Proceedings of the ICOA-6 Conference (2011, co-edited by W. Orchiston, and R. Strom). Tsuko is now Vice-chairman of the IAU Working Group on Historical Instruments.

Dr Ronald Stewart worked as a Principal Research Scientist on the CSIRO Radioheliograph and Australia Telescope projects before retiring. Since then he has written several research papers on the history of Australian radio astronomy, as well as a novel titled *Ravi's Karma* (2012).

Hiromitsu Yokoo lectures in history of science and physics at Chiba University of Commerce and at Housou University. His main research field is the history of astronomy, especially of SETI, and he wrote the book *A History of Ideas on the Extraterrestrial Civilizations* (1991, Koseisha; in Japanese). Another of his research areas is biological astronomy, and in 1978 he and Professor T. Oshima published a paper in *Icarus* (38, 694), where they proposed a new biological message carrier printed in the virus phi-X174.

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THE UNIVERSITY OF TEXAS MILLIMETER WAVE OBSERVATORY

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Abstract: This is an account of the Millimeter Wave Observatory (MWO), a 4.9 m diameter antenna facility that pioneered continuum observations of planets and spectroscopy of interstellar molecules from 1971 to 1988. The circumstances of its founding, development of its instrumentation, and major research contributions are discussed. The MWO role in training of personnel in this new field is illustrated by a listing of student and postdoctoral observers, with titles of PhD theses that included MWO data.

Keywords: Molecular clouds, star formation, interstellar molecular spectroscopy, planetary brightness temperatures, holographic millimeter-wavelength antenna evaluation.

1 INTRODUCTION

The discovery in the late 1960s of interstellar ammonia, water, and hydroxyl, leading to the discovery in 1970 of interstellar carbon monoxide, opened an era of galactic exploration that continues to the present time. The detection of many molecular species, now well over one hundred, revealed a rich and complex chemistry. Molecular spectroscopy proved to be a powerful tool for probing physical conditions in the Galaxy's dark, dense, star-forming interstellar clouds. Although the presence of three molecular radicals in tenuous interstellar gas had been known since the 1930s, spectroscopy of galactic molecular gas did not develop as a major field in astronomy until the exploitation of the radio spectrum, and in particular, the millimeter spectrum. As this development was largely unanticipated, astronomers had to adapt existing telescopes, built for other purposes, to this new area of research. One such telescope was the 4.9-meter diameter millimeter-wave antenna of the Electrical Engineering Research Laboratory (EERL) at the University of Texas (UT) in Austin. For seventeen years, from 1971 to 1988, the Millimeter Wave Observatory (MWO), made pioneering studies of interstellar molecular phenomena. The MWO was able quickly to come on line, a consequence of a number of happy circumstances, not the least being the existence of the antenna itself.

2 THE ELECTRICAL ENGINEERING RESEARCH LABORATORY

The Electrical Engineering Research Laboratory (or EERL) was founded in 1942 as an organized research unit of the University of Texas at Austin (UT). Funding for the EERL came primarily from the Department of Defense, which provided generously for over twenty years of research in radio communications, atmospheric propagation, scattering and general electromagnetics. E. Hamlin was the founding Director, but was soon succeeded by Archibald (Archie) Straiton; both were Department of Electrical Engineering faculty members. Straiton's research was guided, in part, by the general trend to higher frequencies in radio communications and applications. By about 1960 he had acquired an interest in astronomy. Perhaps he was inspired by the launch of Sputnik, perhaps by the growing prominence of McDonald Observatory (McD) at the University. It is also possible that he heard about radio astronomy at conferences; many of the first radio astronomers were electrical engineers. Whatever the inspiration may have been, he applied to NASA for support for the construction of a high-quality antenna to study the planets at millimeter wavelengths.

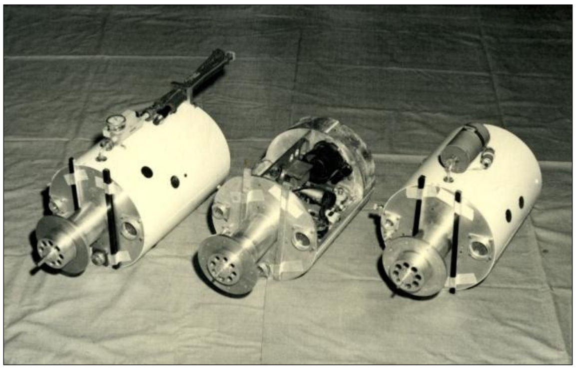
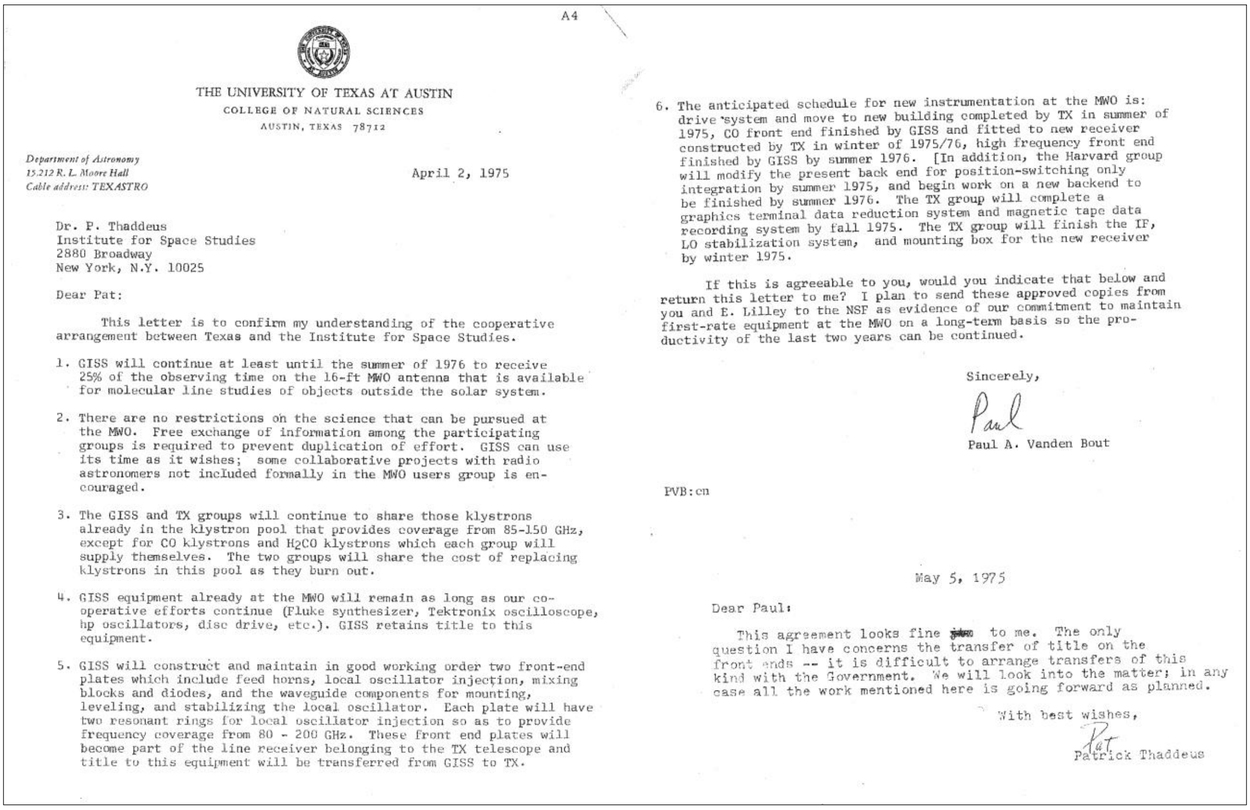


Figure 1: The first receivers of the 4.9-meter antenna, which were used to make planetary brightness measurements from the original site of the antenna, at the Balcones Research Center in Austin (courtesy: files of the EERL).

In 1961 the EERL received two NASA grants, a small ($7,000) grant supporting planetary observations and a larger ($444,000) grant for the antenna. The Western Development Labs, Philco Corp., Palo Alto, California, a subsidiary of the Ford Motor Co., received the contract to build the antenna, and in June of 1963 its construction at the UT Balcones Research Center, on what was then the northern border of Austin and where the EERL was located, was complete. Charles Tolbert, who directed the antenna's construction and its early research program, wrote a description of the new facility (Tolbert, et al. 1965). The first planetary observations at 35, 70, and 94 GHz were of Venus, the brightest of the planets (Tolbert and Straiton, 1964). The receivers, used at prime focus, are shown in Figure 1. Observations of



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THE UNIVERSITY OF TEXAS AT AUSTIN
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Department of Astronomy
15.212 R. L. Moore Hall
Cable address: TEXASTRO

April 2, 1975

Dr. P. Thaddeus
Institute for Space Studies
2880 Broadway
New York, N.Y. 10025

Dear Pat:

This letter is to confirm my understanding of the cooperative arrangement between Texas and the Institute for Space Studies.

1. GISS will continue at least until the summer of 1976 to receive 25% of the observing time on the 16-ft MWO antenna that is available for molecular line studies of objects outside the solar system.

2. There are no restrictions on the science that can be pursued at the MWO. Free exchange of information among the participating groups is required to prevent duplication of effort. GISS can use its time as it wishes; some collaborative projects with radio astronomers not included formally in the MWO users group is encouraged.

3. The GISS and TX groups will continue to share those klystrons already in the klystron pool that provides coverage from 85-150 GHz, except for CO klystrons and H_2CO klystrons which each group will supply themselves. The two groups will share the cost of replacing klystrons in this pool as they burn out.

4. GISS equipment already at the MWO will remain as long as our cooperative efforts continue (Fluke synthesizer, Tektronix oscilloscope, hp oscillators, disc drive, etc.). GISS retains title to this equipment.

5. GISS will construct and maintain in good working order two front-end plates which include feed horns, local oscillator injection, mixing blocks and diodes, and the waveguide components for mounting, leveling, and stabilizing the local oscillator. Each plate will have two resonant rings for local oscillator injection so as to provide frequency coverage from 80 - 200 GHz. These front end plates will become part of the line receiver belonging to the TX telescope and title to this equipment will be transferred from GISS to TX.

6. The anticipated schedule for new instrumentation at the MWO is: drive system and move to new building completed by TX in summer of 1975, CO front end finished by GISS and fitted to new receiver constructed by TX in winter of 1975/76, high frequency front end finished by GISS by summer 1976. [In addition, the Harvard group will modify the present back end for position-switching only integration by summer 1975, and begin work on a new backend to be finished by summer 1976. The TX group will complete a graphics terminal data reduction system and magnetic tape data recording system by fall 1975. The TX group will finish the IF, LO stabilization system, and mounting box for the new receiver by winter 1975.

If this is agreeable to you, would you indicate that below and return this letter to me? I plan to send these approved copies from you and E. Lilley to the NSF as evidence of our commitment to maintain first-rate equipment at the MWO on a long-term basis so the productivity of the last two years can be continued.

Sincerely,

Paul

Paul A. Vanden Bout

PVB:cn

May 5, 1975

Dear Paul:

This agreement looks fine to me. The only question I have concerns the transfer of title on the front ends -- it is difficult to arrange transfers of this kind with the Government. We will look into the matter; in any case all the work mentioned here is going forward as planned.

With best wishes,

Pat

Patrick Thaddeus

Figure 2: Copy of the letter of agreement for the MWO partnership, signed by P. Thaddeus (from the personal papers of R. Loren).

Mars, Jupiter, and Saturn followed (Tolbert, 1966). Observations of other astronomical sources were made with the 35/70 GHz receiver: Tau A and Sgr A (Tolbert and Straiton, 1965) and the Crab (Tau A) and Orion Nebulae (Tolbert, 1965). An article summarizing observations of the Moon at 35 and 70 GHz over six months of lunar phases (Clardy and Straiton, 1968) was the last publication of Solar System studies with the antenna from the Austin location. Experience had made it clear that a dryer site was required for millimeter wavelength astronomy.

3 THE MILLIMETER WAVE OBSERVATORY

It was decided to move the antenna to McDonald Observatory (McD) in West Texas, to a location on the western side of Mt. Locke, at an elevation of 2070 meters. NASA provided funding and the Western Development Labs received a contract for the move. The antenna did not become part of McD, but remained an EERL facility. Straiton and Harlan Smith, McD Director, agreed to this arrangement on Mt. Locke because the antenna would continue to be operated by electrical engineering staff and there was no one in the Department of Astronomy at the time who was interested in millimeter wavelength astronomy. The antenna became a 'satellite observatory' on Mt. Locke, called the Millimeter Wave Observatory (MWO).

A serious problem was encountered in bringing the antenna back into operation. Measurements of the antenna gain showed a substantial loss, which was traced to astigmatism in the beam pattern. The antenna had a polar mount, with the backup structure for the reflecting surface attached to each of the two forks by four bolts. The contractor had either failed to preserve the original shim thicknesses for these bolts or had somehow warped the reflector in transporting it. A long program of beam pattern measurements using a transmitter installed at the nearby Davis Mountains State Park eventually resulted in a properly-shimmed mount and restored antenna gain. (During the restoration of the antenna gain, a Japanese solar astronomer, Wu-Hung Su, used the daytime hours to observe the Sun. The angular size of the Sun meant the broadened antenna beam was of no concern.) The techniques developed in the course of this work, as applied to the MWO antenna, became the PhD thesis of John Davis, supervised by John Cogdell. The work appeared in two publications (Cogdell and Davis, 1973a; 1973b). For an account of the installation at Mt. Locke, a description of the antenna performance, and similar descriptions of the performance of other operating millimeter antennas as the time see Cogdell et al. (1970).

Once the antenna was operational, planetary observations resumed. Although the site was clearly superior to Austin, the poor noise figure of the receivers made for long integration times and the work was slow. Cogdell and Davis, the only observers, could not man the telescope full time; teaching and family duties severely restricted operations. It was during one of these downtimes in the spring of 1971 that two visitors to McD, wandering around the mountain in the late afternoon, happened on the antenna standing idle. One of them, Patrick Thaddeus from Columbia University and the Goddard Institute of Space Studies (GISS), knew something of the antenna, in particular, that it was of high quality. He also knew that Arno Penzias and his group at Bell Telephone Labs (BTL), who had recently discovered

Figure 3: Photograph of the MWO 4.9-meter antenna and astrodome following dome renovations and installation of the error-correcting subreflector and high-frequency receiver box (courtesy: McDonald Observatory).

interstellar CO (Wilson et al., 1970), were unhappy about what they considered the small amount of time they were getting to pursue their discovery at the National Radio Astronomy Observatory's (NRAO) 36-ft Radio Telescope. At the end of their observing run, Thaddeus went home to propose to Penzias that he bring his receiver to the MWO in exchange for observing time. The other observer (P. Vanden Bout) went back to Austin, where he was teaching in the Department of Astronomy, to propose to Cogdell and Davis that they make the MWO available for interstellar molecular astronomy in exchange for using the much superior BTL receiver for their planetary work. The missing element for interstellar molecular spectroscopy was a spectrometer. In due course, Vanden Bout organized a four-way partnership: UT provided the antenna, BTL a receiver, GISS reference oscillators, and the Harvard College Observatory (HCO) a spectrometer. The observing time not required by the EERL for planetary work was split equally between the UT group and each of the other three partners for interstellar spectroscopy. The agreement was merely verbal at the start. It was formalized in writing in 1975, apparently, to satisfy the NSF. The letter, signed by P. Thaddeus, is reproduced in Figure 2.

The first funding for the new project was from McD, in the amount of $1000, to buy parts for a sidereal clock. Additionally, A. Straiton, who was then serving as Acting Dean of the Graduate School, provided $5000 for the purchase of a spectrum analyzer, vital to the construction and testing of the local oscillator system. The UT group received its first grant from the National Science Foundation (NSF) in 1972. Without the strong support of NSF program officer James Wright, the entire enterprise could have failed. The head of astronomy at NSF regarded the partnership as unmanageable, but at Wright's urging he gave his reluctant approval. NSF funding to UT for the MWO continued until 1988. Wright was the NSF program officer until the early 1980s when Kurt Weiler replaced him. The last NSF program officer was Vernon Pankonin, who took over in 1986. A grant for instrumentation was provided by the Research Corporation, and used to purchase mixer diodes fabricated by Robert Mattauch from the Department of Electrical and Computer Engineering at the University of Virginia. P. Vanden Bout received support from the Welch Foundation.

Observations with the new equipment began in the fall of 1972. Figure 3 shows the antenna and astrodome as equipped in the 1980s. One of the striking features of the antenna can be seen in Figure 3—the surface is gold plated. The thin film of gold was intended to protect the thicker silver paint that was the actual conducting surface. Unfortunately, the silver migrated through the gold and tarnished. Although this had no apparent effect on performance, it was thought to be prudent to re-plate the surface from time to time. Obtaining University and federal approval to do this was a challenge and the group endured many jokes about their 'gold-plated' telescope.

The partnership proved very beneficial to the planetary program, which continued to receive strong support from the NASA program officer, William Brunk. Using the new equipment and an improved transmitter and gain calibration system, it was possible to make accurate absolute measurements of planetary brightness temperatures. Results were reported in a series of papers (Ulich, et al. 1973; Ulich 1974; Ulich, et al. 1980) that remained fundamental references for absolute planetary brightness temperatures until supplanted by recent space mission observations. Planetary observations at the MWO stopped in 1977 with the end of NASA funding.

The interstellar spectroscopy programs flourished, enjoying the advantages of good receivers and, compared to what was available elsewhere, large amounts of observing time. The partnership worked remarkably well. Each partner needed the others, and their scientific interests were to a certain degree different. Their sources of funding were also different: NSF funded the EERL to operate the MWO. BTL was supported by AT&T. NASA funded GISS. And HCO had an endowment. At least in the early days, the amiable relations at the MWO stood in contrast to the NRAO 36-ft Radio Telescope, where astronomers engaged in a vigorous competition to gain what was typically a few days of observing time, often to search for a new interstellar molecule. The much smaller MWO telescope lacked the sensitivity easily to discover new molecules; all the strong emitters had already been found. Instead, the observing programs were largely devoted to using the strongest molecular lines to address questions posed by the discovery of an entirely new phase of the interstellar medium, for example, to determine the nature of the molecular clouds and probe their physical conditions.

4 THE MWO STAFF

The MWO staff was typical of university research facilities, consisting of faculty and graduate students, supplemented by a minimal number of support positions. After the move of the antenna from Austin to Mt. Locke, responsibility for research and operations moved from Straiton, assisted by Tolbert, to Cogdell and his graduate student, Davis. They, together with another graduate student, Bobby Ulich, conducted the NASA-funded program of measuring planetary brightness temperatures. Responsibility for technical matters remained in Electrical Engineering throughout the history of the MWO. Davis was a research associate after graduation in 1970 and joined the faculty in 1978. His graduate students, Charles Mayer and Heinrich Foltz, made major contributions to holographic antenna evaluation. The Department of Electrical Engineering was a source of skilled labor: graduate students Natalino Camileri and Rodney Barto assembled a receiver and a paged memory system for the NOVA computers, respectively. Wan Ho, another electrical engineering student, also worked on receivers. Wolf Vogel took over the propagation studies research program at EERL in 1969, and was a valuable resource for technical matters at the MWO. Vogel was honored as a Fellow of the IEEE for his work on propagation modeling in 1991. William Wilson worked with the Texas group as a faculty member in Electrical Engineering during 1976-1977. Anthony Edridge was a receiver engineer in 1984-1985 and he built a 2mm receiver, and as a postdoc Steve Laycock worked on a 350 GHz receiver.

The Department of Astronomy at UT gave the MWO research program a major boost in 1975 with the appointment to the faculty of Neal Evans, who introduced an emphasis on the physics of molecular clouds to the research program. Frank Bash moved to the MWO group in 1980, following the completion of the operational phase of the Texas All-Sky Survey, which had been conducted near Marfa, Texas, by the Jim Douglas group. Bash had responsibility for the MWO in its final years of operation. Another Department of Astronomy faculty member, Dan Jaffe, was part of the group in 1988. His experience in submillimeter astronomy was helpful in developing the possibilities for converting the MWO antenna to a submillimeter facility. Herbert Pickett, a molecular spectroscopist in the Department of Chemistry, was associated with the MWO in the mid-1970s. Other UT faculty that encouraged the development of the MWO included James Browne of the Department of Computer Engineering and James Boggs of the Department of Chemistry, who were theoretical and experimental molecular spectroscopists, respectively. They helped launch a series of seminars to introduce MWO group members to molecular quantum structure.

The support staff was a part of the EERL. Wanda Turner handled all administrative affairs from basic secretarial support to purchasing and accounting, working at the EERL from the days of Straiton to the closure of the MWO. A.J. Walker provided mechanical engineering support as the group machinist over the same period. He was critical to the maintenance of the radio telescope and dome as well as all mechanical components of the MWO. Charles McEvoy served as an electronics technician at the EERL, building professional quality electronics for the MWO for several years. The remote location of the telescope required on-site care. Carlos Garza, an electronics technician with multiple skills, provided that for many years. Garza was a Navy veteran, where he had been a radar technician. Larry Strom succeeded Garza. Strom had had a TV cable installation business in Dallas, Texas. On closure of the MWO, Strom became the on-site technician for the Caltech Submillimeter Observatory in Hawaii. Loren joined the MWO staff on completion of his graduate research. After aiding in the construction of a filter bank in the summer of 1977 and taking it to the telescope for installation, he remained there, becoming, in time, the 'man on the mountain', a 'go-to person' for all observers, as well as a key contributor to MWO science.

Critical technical support came from the partners, in particular, Robert Wilson (BTL), Keith Jefferts (BTL), Anthony Kerr (GISS), and Hays Penfield (HCO). Guest observers who brought receivers to the MWO included Glenn White (Queen Mary College, London), Dick Plambeck and Paul Goldsmith (University of California, Berkeley), and Tom Phillips (BTL). Significant technical help with receivers was provided by Neal Erickson (Five Colleges Radio Astronomical Observatory, University of Massachusetts) and with local oscillators by John Payne (NRAO) and John Carlstrom (Caltech).

It is a sad but curious fact that the incidence of Parkinson's Disease appears to be anomalously high among senior radio astronomers. Overall, Parkinson's Disease strikes one in a thousand men over the age of 70 (Van Den Eeden, et al., 2003). At least ten older radio astronomers out of a population that cannot exceed roughly one hundred suffer from Parkinson's. To date, this includes only one of the observers at the MWO. If there is an environmental factor at work here, it may lie in the laboratory, where some pioneering interstellar spectroscopists spent much of their time, rather than at the telescope.

5 THE SCIENTIFIC PROGRAM AND INSTRUMENTATION

5.1 The Early Days

As with any telescope, the research is limited by the instrumentation. The MWO antenna had an aperture of 4.9 m, giving it a beam size (FWHM) of 2.6′ at the frequency of the CO (J=1-0) transition (115 GHz), by far the most heavily-observed molecular line. It operated at prime focus, limiting the physical size of receivers. Its surface accuracy of 90 μm rms provided good efficiency, and it enjoyed extraordinary thermal stability, due to the construction of its surface panels and backup structure in Invar, a very low thermal expansion metal alloy. Observing with the surface half illuminated by the Sun made no measureable difference in the antenna gain. The absolute pointing accuracy was 22″ and the tracking accuracy was 7″. An astrodome provided protection from the wind and weather, but it had to be closed in winds over 56 km/h or risk the doors being lifted out of their tracks. This limit was tested by Bobby Ulich, who, ignoring the rule, observed on a windy day and had one of doors unseated by an 80 km/hr gust. Initially, the rotation of the dome was under manual control.

The drive system used opposing torque motors on both the polar and declination axes, with position read by shaft encoders. To track a source, one drove the telescope under manual control to the position the source would have at an upcoming even minute of time. At the right instant, one pushed a switch to start the tracking. Ephemerides had to be printed out in advance for every object to be observed. Because the antenna had been built to track Solar System objects, the clock ran on ordinary solar time. One of the first improvements was to build a sidereal clock. The absolute rms pointing accuracy was within a quarter beam width provided one had taken care to measure offsets in Right Ascension and Declination by looking at one or two planets that might be visible.

The Bell Labs receiver was built out of waveguide and used a Schottky-barrier diode mixer. The diodes were mounted in Sharpless wafers and the receiver was tuned using three micrometers, two for the cavity ends, and one for the backshort. Local oscillator (LO) power came from klystrons. Tuning the receiver was a long stroll through a multi-parameter space, adjusting the micrometers and LO power, measuring the response to hot and cold loads, and repeating the procedure until the maximum response had been achieved. By modern standards, the receiver was primitive, but for its day it defined the state of the art. Typical double-sideband noise temperature on the sky was ~1000 K. Their unique feature was the use of wafers that held the smallest area Schottky-barrier diodes available at the time.

The reflex klystrons for the LO operated at high voltage, had limited lifetimes, produced limited power, and were very expensive. A division of Varian Corp. in Canada produced the millimeter frequency units that were required. The market was small and eventually loss of key personnel at Varian meant no klystron could be purchased for use at frequencies above 100 GHz that would actually work. The supply of klystrons in hand carried the MWO to the end of its operation. Alternatives to klystrons were considered. In particular, backward wave oscillators known as Carcinotrons, which produced significant power up to THz frequencies. Thijs de Graauw brought a Carcintoron to the MWO for a test, but the cost of these systems precluded their routine use at the MWO.

All LO chains were phase-locked to a frequency reference. At first, the MWO used a tunable frequency synthesizer to generate a reference signal near 100 MHz, which was then fed into a resonant cavity to reject all but the 20th harmonic. In turn, a signal typically lying between the 55th to 75th harmonic of the 2 GHz signal was used to phase-lock the klystron. The usual observing mode used frequency switching, accomplished by feeding the klystron phase locking circuit with two alternating reference signals, separated by 80 MHz. Calculating the correct observing frequency to account for the source velocity in the local standard of rest and the motion of the telescope on the surface of the Earth was done by hand. It did not take long to acquire the computer program used at NRAO. Similarly, if power was lost and the sidereal clock needed to be reset, a hand calculation using the U.S. Naval Almanac was required.

An L-band parametric amplifier was used to amplify the intermediate frequency. It operated at 1.4 GHz with a bandwidth of 100 MHz. So-called paramps were the curse of radio astronomy until cooled-transistor amplifiers replaced them. The problem was gain stability. Some progress was made at the MWO by mounting the paramp to a water-cooled metal plate, limiting thermal drift.

The first spectrometer, built by Hays Penfield of HCO, had two sets of (40) channels, one of width and spacing equal to 250 kHz and one to 2 MHz. The voltages on the capacitors that accumulated the signal were read out digitally (and very slowly).

Figure 4 shows an early CO dark cloud spectrum.

The data were recorded on punched paper tape for offline data reduction. For several years, Amber Woodman of McD made plots of spectra from punched paper tapes on an x-y plotter located in the 107-inch Telescope dome. The software that produced the paper tapes was written by Bob Wilson and called 'BTL'. To initiate an observation, one pressed the Spectral Line Observe (SPLOBS) button. This needed to be done for each individual integration. Failure promptly to start a new integration wasted observing time and P. Thaddeus posted a sign reading "DON'T THINK - INTEGRATE". In time, a thumbwheel was added to the SPLOBS button that set the number of integrations desired. Using a high number kept things going, and integrations were only stopped for emergencies: grass fires that threatened the facility, a tropical storm remnant that took out the power lines in flash flooding, and the all-too-frequent lightning strikes in summer.

The observed line intensities were calibrated using a chopper wheel technique developed by the BTL group (Penzias and Burrus, 1973). The system produced a calibration signal proportional to the difference between an ambient temperature mm absorber and the sky. Conveniently, the technique did not require knowing the atmospheric opacity. The technique was refined in an early paper by the Texas group (Davis and Vanden Bout, 1973) to take account of differences in gain and atmospheric absorption between the two sidebands of the receiver. For molecular clouds observed at low elevation and for certain receiver setups these effects could be significant.

The inefficiencies and limitations of the equipment were compensated for by the strength of the CO lines, the relatively large amounts of observing time available, and the high quality of the receiver. The MWO was a CO-mapping machine, most particularly in its early days of molecular line observing, which began in the fall of 1972. The antenna was just the right size, large enough to identify the locations of CO hot spots, where the very youngest stars are embedded, but small enough to map a galactic cloud in a reasonable time.[1]

The first publications reported mapping CO in nebulosity associated with Herbig Be/Ae stars (Loren et al., 1973) and in the Orion Nebula (Tucker et al., 1973). An exception for that first observing season was the detection of a new transition in SO (Gottlieb and Ball, 1973) by members of the Harvard group, who had discovered interstellar SO with the NRAO 36-ft Radio Telescope. Overall, approximately two-thirds of the observing time was devoted to CO, the rest to studies of other molecules. A CO map of L43, shown in Figure 5, was typical of those made in the early years (Elmegreen and Elmegreen, 1979).

5.2 The MWO Reaches Maturity

5.2.1 Facility Development

The success of the MWO in its early years, and the demands of heavy observing schedules for more reliability, led to a program of improvements that was carried out over a number of years. Central to this program was the construction of a small building adjacent to the telescope dome that could house the control system and all electronics aside from the receiver. The old control room was located in the dome behind the telescope support piers. It was extremely crowded; no more than three people could fit inside at any one time. Figure 6 shows a student observer, E. (Betsy) Green, in the new control room. UT provided the funds, through McDonald Observatory. Support from the Vice-President for Research, Gerhard Fonken, was the key to getting the building and to making improvements in the dome itself. Archie Straiton wanted to use the dedication of the building as an excuse to get a distinguished Texan to visit the MWO. He had Lady Bird Johnson in mind. But Harlan Smith, McD Director, felt that Lady Bird

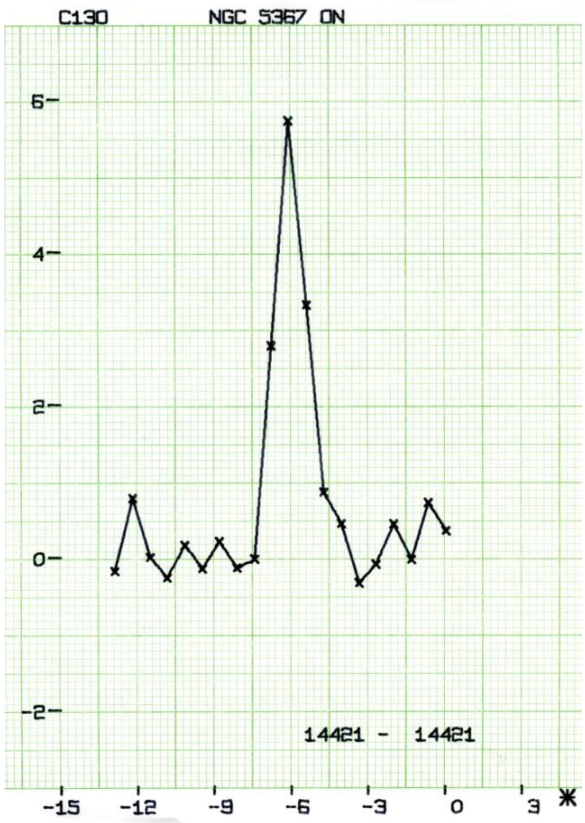

Figure 4: A ^{13}CO (J=1-0) spectrum in NGC5367, as plotted on the x-y plotter in the McD 107-inch Telescope control room. This was the only data display available to the observer during an observing run in the early years (from the personal papers of R. Loren).

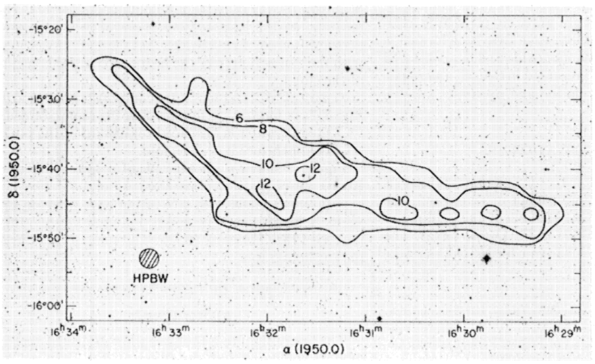

Figure 5: Map of the dark cloud L43 made in the CO J=1-0 transition. The CO contours are superimposed on the red plate of the Palomar Sky Survey. (after Elmegreen and Elmegreen, 1979. © American Astronomical Society. Reproduced by permission).

Figure 6: Photograph of the new MWO control room. The student observer is Betsy Green (from the personal papers of R. Loren).

deserved to cut the ribbon for something bigger than the little MWO electronics house, and after an exchange of memoranda the idea was dropped. It is ironic that Lady Bird had previously enjoyed a visit to the still smaller transmitter hut in the Davis Mountains State Park that was used to map the antenna pattern.

Replacing the wing doors to the dome with a roll-up door allowed the wind limit to be raised to 88 km/hr. This significantly increased the amount of observing time on the windy MWO site. Spring brought high winds, with gusts recorded as high as 160 km/hr. One such gust brought down the power lines. The observer, L. Mundy, and site technician, L. Strom, were forced to turn the dome to face into the wind by hand, using a so-called ‘come along’, a device for making barbed wire fencing taut.

During this period the entire electronics system was replaced and the software upgraded. The filter bank was expanded to 256 channels each of 62.5 kHz and 250 kHz, and eventually 512 channels of 1 MHz. The new filter bank was pipelined (with what would be called an embedded processor today), so that while all of the channels were being read a new integration was under way. The software upgrade consisted of automating several functions that had previously been handled manually by the observer, such as the dome positioning, and keeping the antenna within its pointing limits.

In contrast to major optical and radio telescopes which were operated by night assistants and telescope operators, the MWO was a strictly do-it-yourself facility. The observer alone did all the operations required. For this mode of operation to be successful required a very user-friendly computer program, at a time when computers were not all that friendly. Bill Peters and John Davis designed the computer software and hardware interfaces. The control program was called NIMBUS and ran on a single NOVA computer. It handled all telescope and dome functions, controlled the receiver frequency, and acquired the data. Another program called ABACUS, running on a second NOVA computer, was used for data reduction. The two computers shared a single hard drive. These computer programs were notable for their reliability and simplicity of operation. After a brief introduction, first-time novice student observers could take full control of the telescope, acquire data, and reduce it while taking more data.

The MWO antenna drive featured two rate zones to optimize observing time. A very rapid slew rate was used when the antenna position was changed by a large amount. As the final source position was approached, the drive shifted into a critically-damped regime to arrive at the source at the exact tracking velocity. The antenna drive consisted of software embedded in NIMBUS that drove linear amplifiers directly connected to opposing DC torque motors.

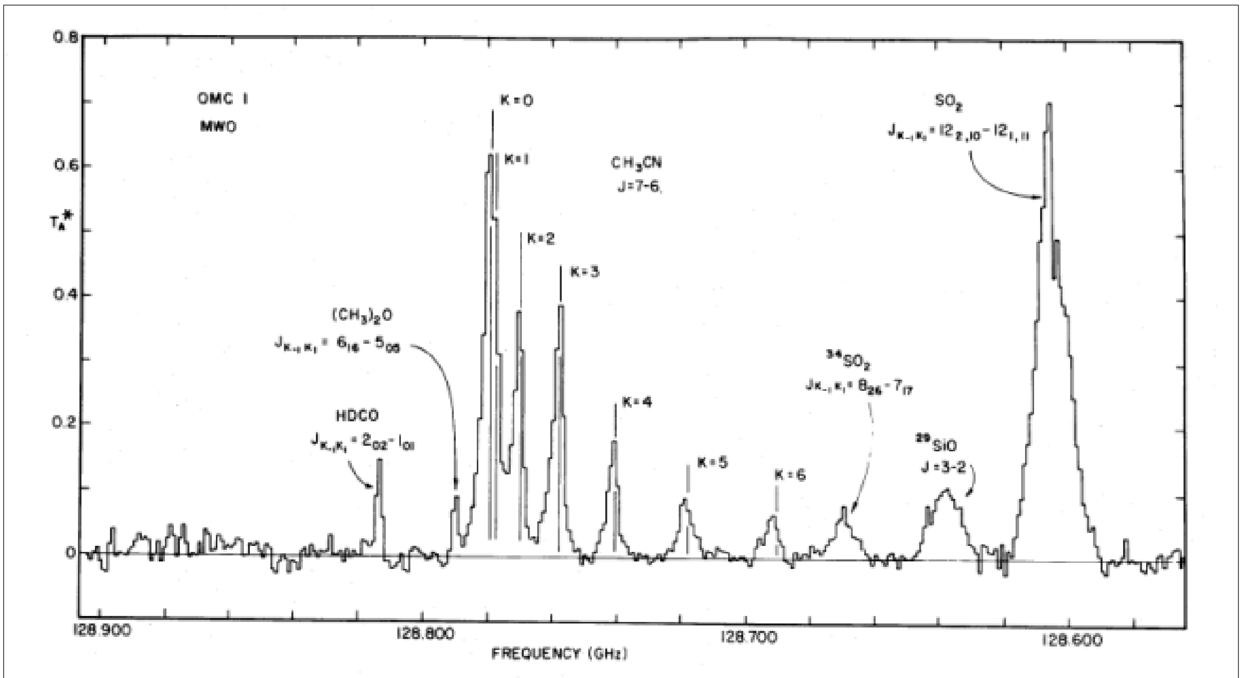

Figure 7: Spectrum of the Orion molecular cloud in the vicinity of 128 GHz, showing the K-ladder lines of CH_3CN (J=7-6), and lines of SO_2, SiO, $(CH_3)_2O$, and HDCO (after Loren and Mundy, 1984. © American Astronomical Society. Reproduced by permission).

The priorities for science-related improvements were always higher than those for creature comforts. For example, the two mobile homes used for observer lodging, obtained as surplus government property, were never kept at anything more than the bare minimum needed for shelter. One spring a wind gust of 160 km/hr removed the shade roofing over the trailers and wrapped it around the power lines. On other occasions, observers were forced to nail down loose siding to prevent it being torn off in a storm. Mt. Locke was plagued with insects. No observer could forget the seasonal invasion of moths, let alone the spiders and scorpions. Some observers refused to use the poorly-sealed and poorly-insulated mobile home accommodations and stayed instead in the McD Transient Quarters. Others found solace in a diet of steak and beer, supplemented by Cuban cigars that were occasionally brought in by French observers (see Section 7). The ban on alcohol in UT facilities was widely ignored at McD, which was over 600 km from the University in Austin.

Simply travelling to the MWO was difficult, due to its remote location. Some observers drove from the nearest airport, either Midland/Odessa (250 km) or El Paso (320 km). Others drove all the way from Austin. Travelers always checked to see if there was a spare seat on the plane chartered by McD for weekly trips between Austin and Marfa, a town 50 km from the Observatory with a landing strip that had been used to train WWII glider pilots. The most economical mode of travel was to take the Greyhound bus to either Alpine or Kent. The latter was no more than a very isolated gas station in the tumbleweeds off the highway between Houston and El Paso. Someone from the MWO would pick up and drop off travelers at these spots. Exiting a bus after the gas station in Kent had closed for the night was done with trepidation if the MWO driver was late; one waited in the dark with only the howling of the wind and coyotes for company.

5.2.2 Scope of the Research

The MWO produced over 250 papers published in refereed journals and conference proceedings, as well as 23 PhD dissertations. Only a small fraction of these can be discussed here. There was a wide range of topics, illustrated by these papers: the interaction of supernova remnants with molecular clouds (Wootten, 1977; 1981); the enormous extent of molecular cloud complexes (Elmegreen and Lada, 1976; Kutner et al., 1977; and Lada et al., 1978); molecular clouds associated with giant HII regions (Lada, 1976); the dynamics of CO clouds and galactic density waves (Bash and Peters, 1976; Bash et al., 1977); circumstellar shells (Lambert and Vanden Bout, 1978; Clegg and Wootten, 1980; Sahai et al., 1984); a limit on the abundance of oxygen in molecular clouds (Liszt and Vanden Bout, 1984); high galactic latitude molecular clouds (Blitz et al., 1984; Magnani et al., 1985); CS (J=5-4) line profiles towards Sgr A at 245 GHz (Sandqvist, 1989); molecular rotational constants of CCH (Ziurys et al., 1982); and molecules in comets (Irvine et al., 1984). Figure 7 shows a small portion of the rich molecular spectrum from the Orion molecular cloud, obtained in a study of CH_3CN (Loren and Mundy, 1984).

In the Texas group, star formation was a topic of central interest. To understand star formation, one needed to know the physical conditions of the dense molecular cloud cores where stars formed. Neal Evans defined a research program that continued for many years to probe the physical conditions of cloud cores. In a series of papers, the energetics of molecular clouds was examined (Evans et al., 1977, 1981; 1982; Blair et al., 1978; Evans and Blair, 1981), comparing the energy input from newly formed stars with cooling by molecular line emission and far-infrared emission by dust. The optically thick CO line gave the gas kinetic temperature. Cloud cores were located by mapping ^{13}CO, which peaked in emission strength on the densest gas and also gave a rough location of potential-embedded stars. Near infrared observations were made to characterize the stars, and published far-IR data were used to determine the dust luminosity. Observations of both the 2 mm and 2 cm formaldehyde lines at the MWO and with the NRAO 140-ft Radio Telescope, respectively, gave a good estimate of core densities. The utility of CS and H_2CO as density tracers for modeling clouds was demonstrated in a series of papers (Snell et al., 1984; Mundy et al., 1986; 1987).

5.2.3 Toward Submillimeter Observing, the 1mm Band

At the outset, observations at the MWO were limited to frequencies between 110 and 150 GHz. But it was clear that a niche for the MWO was the 1mm frequency band. Higher frequencies were unexplored, as receivers at these frequencies did not exist. The first experience at higher frequencies at the MWO was with receiver components developed by the innovative radio astronomy group at the University of California, Berkeley. The receiver was tuned to respond to the second harmonic of the LO reference signal. In 1979, Richard Plambeck's diplexer mixer was used to make the first observations of the J=2-1 transition of CO in a bipolar outflow source, showing that temperatures in the outflow were up to a factor of three hotter than the surrounding cloud (Snell et al., 1980). It was also used to detect the 3_{12}-2_{11} line of H_2CO at 1.3 mm (Evans et al., 1979).

Experience with so-called second harmonic receivers prompted the Texas group to examine the effect of second harmonic response in a standard receiver tuned to the fundamental of the LO (Vanden Bout et al., 1985). Because the effect was usually small, observers tended to ignore it. Everyone in mm astronomy at the time knew and accepted the seemingly unavoidable systematic uncertainties in calibration. In time, the advent of mm wavelength interferometers and single-sideband receivers allowed line calibration to be more precise. The Atacama Large Millimeter/Submillimeter Array promises an intensity accuracy of 5% in line images.

The University of Massachusetts' N. Erickson (1977) from the Five Colleges Radio Astronomy Observatory built the first quasi-optical receiver using a Martin-Puplett interferometer for local oscillator injection and a clever refocusing of the prime focus spot to form the quasi-optical beam. For a description of this system see Goldsmith (1988). Producing a local oscillator signal for frequencies above 150 GHz was a challenge, accomplished by using the 2nd, 3rd, or even 4th harmonic of a klystron tuned from 80 to 115 GHz. Subsequently, J. Davis and C. Mayer mapped the antenna surface error pattern with a holography receiver they developed (Mayer et. al., 1983). They made a folded Gregorian optical system with an error-correcting secondary mirror, which was installed into the beam path in 1983. This optical system is shown in Figure 8. The error corrector significantly improved the antenna efficiency in the 1mm band. An illustration of the power of this new receiver/optics system is the study Mangum et al. (1990) made of H_2CO in OMC-1 (all 14 ΔJ=1 lines of H_2CO between 211 and 363 GHz, 8 lines of $H_2{}^{13}CO$, and 4 lines of $H_2C^{18}O$). The higher excitation of lines from levels in the K=2 and K=3 ladders, which only occur at higher frequencies, are especially useful for determining the physical conditions in the hot core and plateau regions of the cloud. They combined these data with VLA observations to determine the physical distribution of H_2CO in the kinematic components of OMC-1.

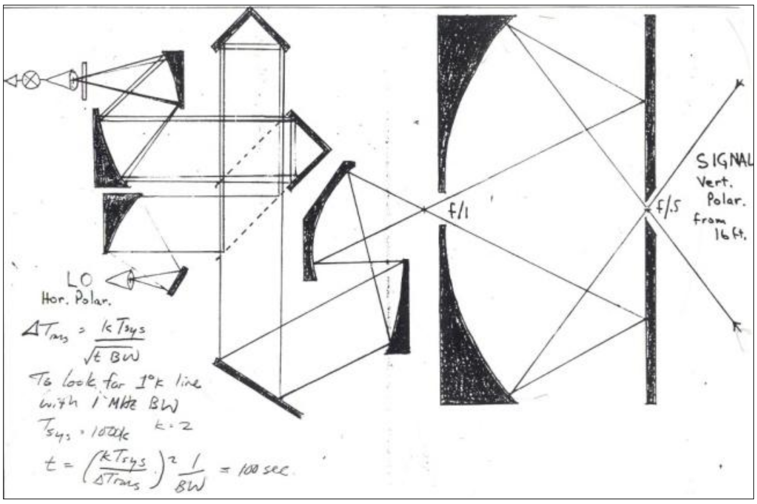

Figure 8: Drawing by Mayer and Davis of the folded error-correcting secondary and associated receiver optics. The errors in the primary surface were cut (with the opposite sign) into the mirror just behind the f/0.5 primary focus. The mirror is smaller than the primary by a factor of ~70, and was made on a numerically-controlled milling machine driven by a program derived from the holography map (see Figure 10). Sketches such as this were typical of documents used in construction of equipment; the interactions with technicians and machinists were close and informal (drawing from the files of the EERL).

In 1985 observations were pushed to true submillimeter wavelengths (301–352 GHz) with cooled Schottky diode mixers developed in-house by N. Camilleri. The receiver used Erickson's quasi-optical injection system, with tripled and quadrupled klystron fundamental frequencies for the LO. Over twenty-four new lines were detected in a host of molecules: H_2CO, SO, SO_2, CS, $C^{34}S$, CH_3OH, CN, CCH, SiO, and $H^{13}CN$, all in OMC-1 (Loren and Wootten, 1986).

Studies of deuterated molecules, a program of interest to the Texas group, were aided by the high frequency capability. In contrast to atomic deuterium, which has only recently been detected in the interstellar medium of our Galaxy, deuterated molecules are relatively easy to detect. Snell and Wootten (1977) detected DNC. Combes et al. (1985) detected CCD. Using an InSb bolometer receiver, Beckman et al. (1982) detected the 2_{11}–2_{12} line of HDO at 242 GHz and estimated the HDO/H_2O abundance ratio. Molecular ions like HCO^+ recombine with free electrons. Guelin et al. (1977) and Wootten et al. (1979) used the DCO^+/HCO^+ ratio to find upper limits for X_e of order 10^{-7} to 10^{-8} in a number of Galactic dark clouds. Wootten et al. (1982) confirmed strong temperature dependence for DCO^+ fractionation in a wide range of clouds, obtaining results similar to those of Snell and Wootten (1979) for DNC. DCO^+ became a marker for cold, star-forming cloud cores. Loren et al. (1990) mapped DCO^+ emission in a cluster of twelve such cores in the ρ Ophiuchus molecular cloud. The cores contained infrared sources having steep spectra characteristic of the youngest protostars, indicating a brief phase of evolution during which DCO^+ molecules exist before stellar heating destroys them.

5.3 Significant Results and Discoveries

The principal advantage of the MWO was that it had sufficient observing time to allow for extensive map-ping of molecular emission. This led to what are its most highly cited results: the discovery of mass out-flows from newly-formed stars; a means of measuring molecular cloud mass; evidence for the dark matter halo of our Galaxy; and the discovery of an interstellar maser. The most significant technical achievement at the MWO was the development of holographic antenna evaluation.

5.3.1 Bipolar Outflows

It was widely assumed in the early years of milli-meterwavelength astronomy that star formation occurred in molecular clouds, and much research was devoted to establishing that connection. Looking for kinematic evidence for the collapse of molecular gas onto a new star was an early interest of Bob Loren. Among the first clues for collapse was the observation of broad CO line wings in the cores of the Mon R2, R CrA, and LkHα198 clouds (Loren et al., 1974). Mon R2 was particularly intriguing (Loren, 1977). It's CO line shows a self-absorption feature shifted by 1 km/s with respect to the ^{13}CO line, indicating motion of cold outer gas toward the center of the cloud. The CO lines showed a bipolar structure that was aligned with the rotation axis determined from ^{13}CO mapping. The conclusion of the paper, reflected in its title, was that the flow was inward, but the data in hand could not rule out an outward flow in the bipolar structure.

That bipolar flows were outward was established from observations of the cloud L1551, published in what is now seen as an iconic paper (Snell et al., 1980). Their CO map, reproduced in Figure 9, shows red- and blue-shifted emission extending 0.5 pc in both directions from a central star, IRS-5, visible only in the infrared. The blue-shifted lobe contains knots of nebulosity, HH28, HH29, and HH102, whose optical radial velocities and proper motions (Strom et al., 1974; Cudworth and Herbig, 1979) yield a true space motion away from IRS-5. Identifying these knots as associated with the blue lobe implies outflow from the star. The model presented in their paper has come to be the standard picture of a stage in star formation when the collapse of the molecular cloud core has formed a rotating protostellar disk with a stellar wind/shock wave that flows out along the rotation axis. These outflows are ubiquitous signatures of newly-formed (low-mass) stars and have been studied in great detail (see the review by Lada, 1985). Bipolar outflows are arguably the most significant discovery made at the MWO.

5.3.2 CO to Molecular Cloud Mass

The hydrogen molecule, principal cloud constituent, is not easily observed and the mass of molecular clouds is typically estimated using observations of CO, which is easily detected at millimeter wavelengths. The X-factor converts the brightness temperature of the CO emission line to a column density of H_2. The original determination of X(CO) began with work done at the MWO by Bob Dickman, a student of Thaddeus, who compared the strength of the ^{13}CO (J=1-0) line in so-called 'dark clouds' to their visual extinction. He inferred the amount of dust along the line of sight from the reddening, and, in turn, calculated the column density of H_2 using the dust to H_2 ratio from independent UV satellite measurements. Because a cloud's mass is a fundamentally important property, this paper (Dickman 1978) is one of the most significant results from the MWO.

5.3.3 Kinematics and Dynamics of the Milky Way

The discovery of interstellar CO provided a new means for mapping the structure of our Galaxy. Early work by Leo Blitz (1979) at the MWO showed the utility of CO for such work by obtaining a rotation curve for the outer Galaxy from observations of HII regions in the second and third quadrants. He showed that our Galaxy has a flat rotation curve to a large distance from the Galactic Center, as had been seen in external galaxies from HI observations. This result is among the earliest evidence for our Galaxy's dark matter halo. As an aid to further work, he published a catalog of CO observations towards essentially all optically visible HII regions in the Milky Way. The catalog included nearly all the HII regions in the Sharpless Catalog as well as 65 additional HII regions. This catalog (Blitz et al., 1982) has proven to be of high utility.

5.3.4 SiO Maser Lines

Early results from the MWO included detections of rotational transitions of vibrationally-excited (v=1) SiO. Snyder and Buhl (1974) had seen a set of unidentified, narrow lines in the Orion molecular cloud and suggested that they could be the J=2-1, v=1 SiO line. The Texas/GISS groups then observed the v=1, J=3-2 SiO line at the same velocity (Davis et al., 1974). They then detected the v=1, J=1-0 line at this velocity (Thaddeus et al., 1974) using a receiver specially built for the purpose. These observations confirmed the suggestion of Snyder and Buhl, and established the existence of a new interstellar molecular maser.

5.3.5 Development of Holographic Antenna Evaluation

The 1970s saw the transition from mechanical to electronic techniques for mapping the surface accuracy of reflector antennas. Scott and Ryle (1977) were the first to use the so-called 'holographic technique' for an interferometer. Bennett et al. (1976) demonstrated the technique on a single dish of 3 m diameter, making partial surface maps that established a proof of concept.

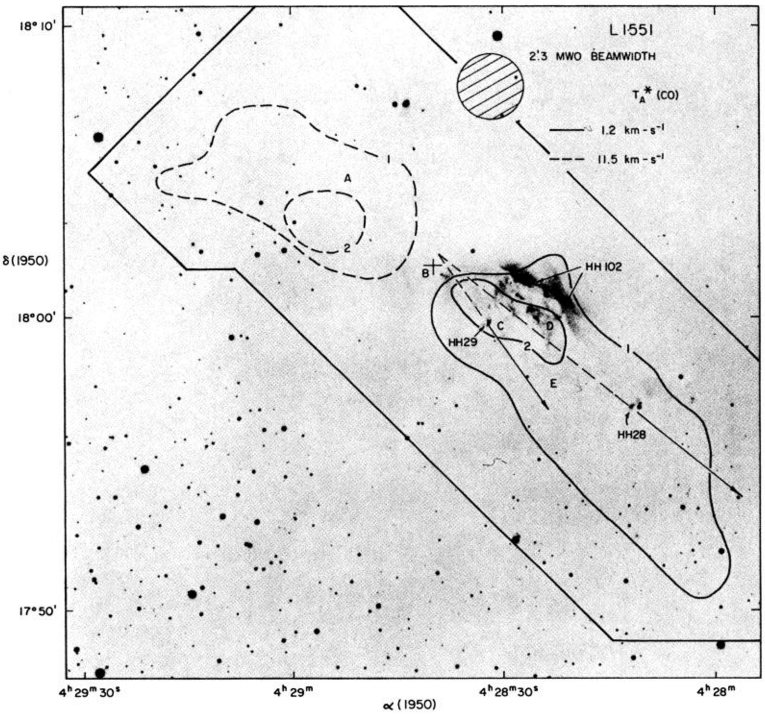

Figure 9: Contours of the CO emission in L1551 superimposed on an optical image, showing the blue and red shifted lobes to the SW and NE, respectively. The blue lobe engulfs the HH objects (after Snell, Loren, and Plambeck, 1980. © American Astronomical Society. Reproduced by permission).

To the best knowledge of the authors, the first holographic map of a large aperture reflector surface, done with sufficient accuracy and resolution to be useful, was that made by Mayer et al. (1983) of the MWO. The surface map is shown in Figure 10. The results of their holographic receiver system were used to design the error-correcting optics that greatly enhanced the MWO performance at 1mm wavelength. Collaboration with NRAO led to the installation of a similar system on the NRAO 12-m Radio Telescope. Holographic evaluation of antenna surface accuracy has become the standard technique in use today.

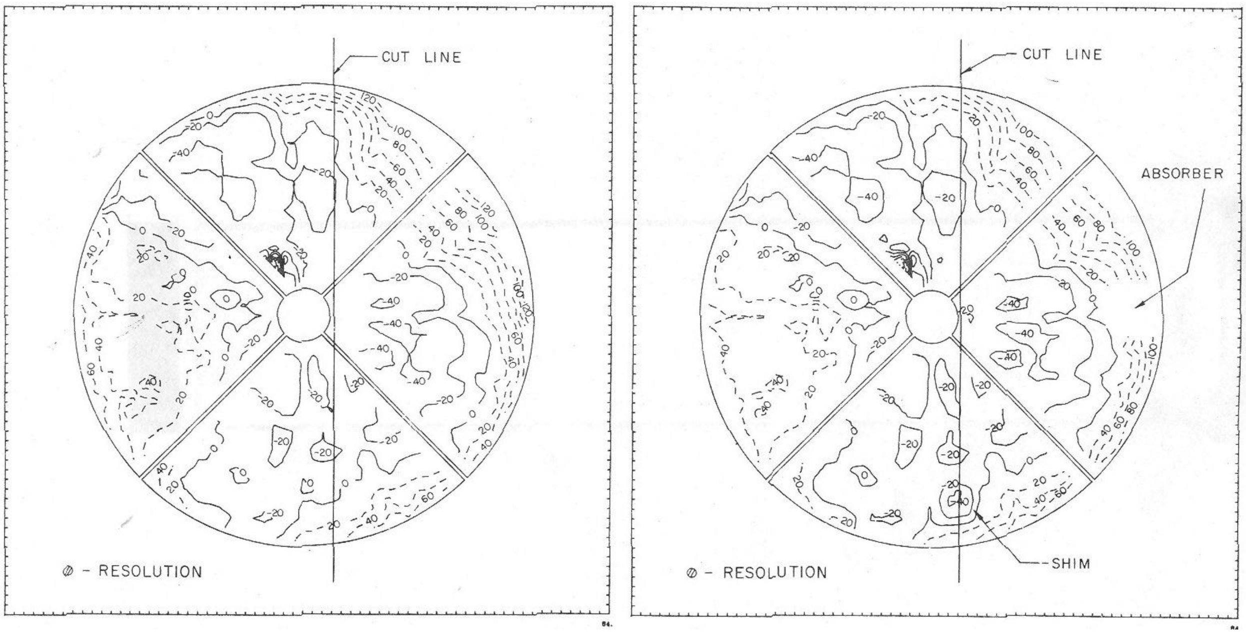

Figure 10: Holographic maps of errors in the surface of the 4.9 m MWO antenna. Left: The normal surface. Right: A sheet of absorber placed on the surface can be seen as an absence of contours. A metal shim taped to the surface is easily seen and serves to confirm the sign of the surface errors with respect to a perfect parabola (after Mayer, et al. 1983. © IEEE. Reproduced by permission).

6 STUDENTS, POSTDOCTORALS, AND SABBATICAL VISITORS

Students played a major role at the MWO, where sufficient observing time was available to do large PhD projects. It also contributed to the training of many other students who came to observe for research projects outside their PhD. A list of students who observed at the MWO, with titles of 23 PhD dissertations that included MWO data is given here:

> Greg Baran, Guy Blair ("Millimeter Molecular Line and Infrared Observations of Dense Clouds Associated with Small Hα Emission Regions"), Leo Blitz, Elizabeth Bozyan, Ron Buta, Harold Butner ("Dense Cores and Young Stellar Objects"), John Caldwell, John Carr, Fabienne Casoli, David Chance, Gordon Chin, Françoise Combes ("Dynamics and Structure of Galaxies"), Dan Clemens, Hong-Ih Cong, Jacques Crovisier ("Contribution to the Study of the Interstellar Medium by Observation of the 21-cm of Neutral Hydrogen in Absorption"), John Davis ("The Evaluation of Reflector Antennas"), Robert Dickman ("The Ratio of Carbon Monoxide to Molecular Hydrogen in Interstellar Dark Clouds"), Debra Elmegreen, Robin Frost, D. Garrett, Maryvonne Gerin ("Molecular Clouds and Dynamics of Interacting Galaxies"),[2] David Gilden, Betsy Green, Stephane Guilloteau, Paul Ho, John Howe, Frank Israel, Marshall Joy, Charlie Lada ("Observations of Dense Molecular Clouds"), Elizabeth Lada ("Global Star Formation in the L1630 Molecular Cloud"), David Leisawitz, Emmanuel Lellouch, Russell Levreault ("Molecular Outflows and Mass Loss in Pre-Main-Sequence Stars"), Harvey Liszt ("Carbon Monoxide Studies of Hydrogen-II Regions"), Bob Loren ("Millimeter Wavelength Molecular Emission Associated with the Massive Young Herbig Be and Ae Stars"), Robert Lucas ("Study of the Formation of Millimeter Molecular Lines in Interstellar Clouds"), Paul Makinen, Loris Magnani ("Molecular Clouds at High Galactic Latitudes"), Jeff Mangum ("The Throes of Star Formation"), Charlie Mayer ("Microwave Antenna Metrology by Holographic Means"), Marshall McCall, Lee Mundy ("The Density and Molecular Column Density Structure of Three Molecular Cloud Cores"), Anneila Sargent ("Molecular Clouds and Star Formation"), Michael Scholtes, David Slavsky, Ron Snell ("A Study of Interstellar Dark Clouds"), Bobby Ulich ("Absolute Brightness Temperature Measurements at Millimeter Wavelengths"), Peter Wannier ("Isotopic Abundances in Interstellar Clouds"), Bruce Wilking, Diane Wooden, Al Wootten ("A Study of Molecular Clouds Near Supernova Remnants") and Shu-Dong Zhou ("Small Scale Structures and Density of Star-Forming Regions").

The MWO had a good number of observers who came as postdoctorals. Their motivation often went beyond the obtaining of research data to include practice and training in making observations at millimeter wavelengths. Postdoctoral visitors included: John Beckman, John Black, François Boulanger, Jorge Canto, Bruce Elmegreen, Pierre Encrenaz, Steve Federmann, Edith Falgarone, Carl Gottlieb, Elaine Gottlieb, Thijs de Graauw, Michel Guelin, Marc Kutner, Richard Linke, Gillian Knapp, John Mather, Mark Morris, Phillip Myers, Antonella Nata, Peter Phillips, Jean-Loup Pujet, Mark Reid, Luis Rodriguez, Nick Scoville, and Ken Tucker. Howard Van Till was a visitor for the 1974-1975 academic year. He came on an NSF-funded sabbatical to gain experience in astronomical research in support of his new program in astronomy at Calvin College.

7 INTERNATIONAL OBSERVERS

The MWO had a significant number of international observers. The largest contingent by far was from France, as can be seen from the list of students given above. It began when Pierre Encrenaz took a postdoctoral position at GISS with Thaddeus, funded by the National Research Council. Encrenaz pioneered the study of the molecular cloud in ρ Ophiuchus (Encrenaz, 1974) at the MWO. Somewhat later Michel Guelin spent a postdoctoral at GISS. They encouraged colleagues in France to apply for time, and a number of them, most prominently, Alain Omont, came with their students. Alain Castets spent a sabbatical with the Texas group in 1984, visiting from the University of Grenoble.

Glenn White from Queen Mary College, London, brought his group on two occasions, once to observe CO with a MWO receiver, and again with a cooled InSb bolometer receiver for submillimeter wavelength observations. Aa Sandqvist came from Sweden and Frank Israel and Thijs de Graauw from the Netherlands. Luis Rodriguez and Jorge Canto were visitors from Mexico.

8 CONCLUDING REMARKS

By the 1980s the original partnership had faded away. The Bell Labs group had built its own radio telescope. The Harvard observers were pursuing other interests, among them use of the nearby 14-m millimeter radio telescope of the Five Colleges Radio Astronomy Observatory. And the GISS group had turned to surveying Galactic CO with a 'mini-telescope' of aperture 1m on the roof of the physics building at Columbia University. Larger, more sensitive millimeter wavelength telescopes, built on better sites, were making the MWO less competitive. The discussion of the future of the MWO focused on using it at higher frequencies. The very stable surface combined with the error-correcting subreflector would support such observations. But Mt. Locke lacked the atmospheric transparency for submillimeter observations. In 1985 the University of Texas administration was informed of a plan to move the MWO to Mt. Graham in Arizona, where the University of Arizona was locating telescopes, including a submillimeter telescope of its own. The University of Texas sought private funding to support the move, but was unsuccessful. The next plan was to move the MWO to Mauna Kea, next to the James Clerk Maxwell Telescope, also as a step to submillimeter interferometry. Initial inquiries led to an alternative plan —an agreement with Caltech that the University of Texas become a partner in the Caltech Submillimeter Observatory, which was already sited next to the James Clerk Maxwell Telescope. The MWO would not be moved. The Keck Foundation supported the University of Texas' capital contribution to the Caltech Submillimeter Observatory, and NSF grants supported the research there by the University of Texas. The University of Texas contributions to the operation of the CSO came from the State of Texas through University and the McDonald Observatory. In 1988 the MWO was closed. Later, the University of Texas gave the antenna and astrodome to a research group from the University of Mexico for studies of solar activity and galactic masers. The antenna is to be installed on Sierra Negra, elevation 4600m, a dormant volcano 100 km east of the city of Puebla. Sierra Negra is also the site of the new Large Millimeter Telescope (LMT), a partnership between the University of Massachusetts and the Instituto Nacional de Astrofísica, Óptica y Electrónica in Mexico. With a 50-m aperture, the LMT is the largest single-dish millimeter telescope on Earth. The site is a fitting place for one of the oldest millimeter telescopes to close out its service to the scientific community.

9 NOTES

1. Extragalactic CO clouds required larger aperture telescopes; Françoise Combes failed to detect CO in an external galaxy at the MWO and then succeeded using the NRAO 36-ft Radio Telescope.
2. Maryvonne Gerin did not observe in person. Thesis data from the MWO were taken by P. Encrenaz and F. Combes.

10 ACKNOWLEDGEMENTS

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SIGURD ENEBO AND VARIABLE STAR RESEARCH: NOVA GEMINORUM 1912 AND THE RV TAURI STARS

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Abstract: Sigurd Enebo made two important contributions to variable star research in 1912: the serendipitous discovery of Nova Geminorum II and the introduction of RV Tauri stars as a new class of variables. Based on recently-discovered source material and literature sources, we describe Enebo's variable star program from 1903 to 1942 and highlight some results. Enebo was a meticulous observer who contributed extended time series for several types of variable stars. He determined periods for a large number of them, and was the discoverer of 2 eruptive, 7 long period, and 2 Algol variables.

Keywords: variable stars, Nova Geminorum 1912, RV Tauri stars, Sigurd Enebo (Einbu)

1 INTRODUCTION

Sigurd Enebo (Figure 1) was born in rural Lesjaskog, Norway, on 5 November 1866. He was the third child in a flock of eight. He became a teacher and organist. He established himself at Dombås, Norway, in 1896, the same year he opened a lifelong correspondence with the astronomers at the University of Oslo. Jens Fredrik Wilhelm Schroeter, who was First Assistant (*Observator)* from 1890 to 1919 and Professor from 1919 to 1927, became a supervisor and mentor for Enebo's self-education in astronomy, and eventually directed his efforts towards observations of variable stars. Enebo studied Gauss' treatise on motions in the Solar System and quickly realized the need to study more advanced mathematics. In 1902 he borrowed a powerful set of binoculars (magnification 20×) and set out to detect Neptune by recording its motions among the stars.

Schroeter realized that Enebo had determination and talent as an observer and in a letter dated 15 December 1902 suggested that he should begin systematic monitoring of variable stars. In 1903 Enebo borrowed a 7-cm homemade refractor (Figure 2). He made a tripod mounting and began variable star observations in November 1903. From the University Observatory in Oslo he borrowed the *Bonner Duchmusterung* star maps which he copied manually onto transparent paper, including different symbol sizes for the nine different magnitudes shown. He could now identify variable stars and comparison stars for his observing program.

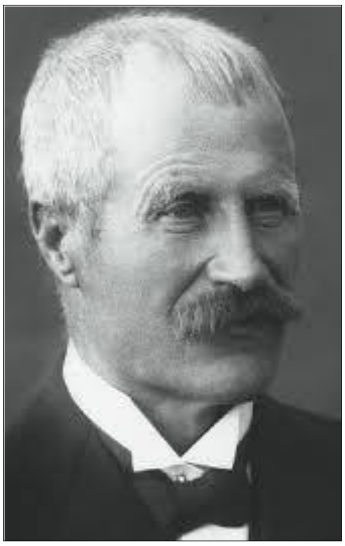

Figure 1: Sigurd Enebo (Einbu), 1866–1946 (courtesy: *Yearbook*, 1947).

At Dombås, Sigurd Enebo became acquainted with a fellow-teacher, Helga Eriksen, and they married in 1905. Two years later he bought the property Brennøygarden from her parents, and outside the house he established an observing site modeled after Tycho Brahe's Stjerneborg. During the first few years Enebo determined the periods of a number of variable stars and he discovered new Algol variables and long period variables. The results were published in *Astronomische Nachrichten*, initially with assistance frrom Schroeter, but from 1906 Enebo submitted his own contributions to the journal. He also collected his observations in a series of 14 papers that were published between 1906 and 1944 in *Archiv for Mathematik og Naturvidenskab* and in the *Publications from the Norwegian Academy of Sciences*. As Enebo's mentor, Schroeter was a scrutinizing reviewer of the observation tables and manuscripts (in German), as well as the facilitator of the printing and its funding. He also applied to the Academy of Sciences to obtain funds from the Fridtjof Nansen Foundation to acquire a larger telescope for Enebo. Upon the advice of Camille Flammarion a 10.8-cm refractor was acquired from Bardou of Paris (see Figure 3). Enebo made his first observations with this new instrument (of RW Aur) on 22 December 1906.

During the summer of 1907 Enebo was visited by astronomy professor Hans Geelmuyden. This led Geelmuyden to submit an application to the Parliament for an annual stipend to Enebo so that he could devote himself entirely to astronomy. The Parliament awarded Enebo an annual grant in 1908. He reduced his amount of teaching and increased his observing program. Two years later the grant was increased considerably. Enebo became a state-supported full-time astronomer, and continued as such for the rest of his life.

A new house at Brennøygarden was completed in 1913, and Enebo established an observing room in the attic, equipped with a rotating conical roof (see Figure 4).[1] He continued his extensive observing program until 1940, with a few stars being observed until 1942. His last paper was published in 1944.

Figure 2: The homemade 7-cm refractor (photograph by the author).

Sigurd Enebo changed his family name to Einbu in 1925, which reflects in his authorship.

Recently a box containing about 2000 postcards and letters addressed to Enebo was discovered in the attic of his abandoned house. This source material allows his astronomical career to be studied in greater detail than before. This paper is partly based on these unpublished sources and partly on the scientific publications by Enebo. We will now review his efforts and results as an observer and discoverer of variable stars between 1903 and 1942.

2 THE INITIAL OBSERVATIONS

In the fall of 1903 Enebo employed Argelander's step method on increasingly fainter stars. During his first winter season 1903/04 (Nordic summer nights are too bright for photometric observations) he had begun monitoring twelve stars relative to sets of nearby comparison stars. Four of the stars showed no immediate variability. He continued to observe them for three seasons, and they proved to be constant.

Figure 3: The 10.8-cm Bardou refractor (photograph by the author).

Figure 4: The house at Brennøygarden with the roof-top observatory. Sigurd Enebo is posing in the foreground (courtesy: Urd 27 March 1915; photographer: H.H. Lie).

Table 1 reveals that the standard deviations of his visual estimates were (slightly) better than 0.1 mag-

Table 1: Non-variable stars observed by Sigurd Enebo.

Star	Year	Instrument	No. of obs.	Magn ± σ
32 Vul	1903-1906	binoculars	45	5.16 ± 0.08
BD+33°4056	1903-1906	7 cm refractor	47	8.66 ± 0.05
BD+49°3239	1904-1906	7 cm refractor	52	9.27 ± 0.03
BD+45°3271	1904-1906	7 cm refractor	101	8.81 ± 0.09
RT Tau	1906-1907	7 cm refractor	22	9.33 ± 0.09
RT Tau	1907-1909	11 cm refractor	91	9.33 ± 0.08

nitude. RT Tau, which he added to his program in 1906, was not detected as a variable by Enebo. It was observed with two of his instruments and may thus serve to check the consistency of his observations. The last two lines of Table 1 yield equal results for both instruments. RT Tau is listed as constant in the *General Catalogue of Variable Stars*.

When Enebo selected comparison stars for a new variable to be added to his program, he carefully compared telescope views to *Bonner Durchmusterung* maps. During such exercises he noted several deviations. He alerted Schroeter that BD+39º1963 was seen much fainter than on the map. Schroeter confirmed that he could just see it with the 19-cm Merz refractor of the University Observatory in Oslo. In April 1905 Schroeter advised Enebo to keep this under surveillance, as it may prove to be a variable star.

Schroeter mailed astronomical journals to Enebo from time to time. He could borrow them for a few weeks before they had to be returned to the Observatory library. Enebo selected variable stars and suspects from journal articles and observing reports. On 16 November 1905 he added BD+41º851 to his program. He monitored it throughout the winter season, sometimes estimating magnitudes several times per night. On four occasions the star was too faint to be detected in his 7-cm refractor. Enebo deduced from his initial data that the period was about 13 days and that the star was an Algol variable (Schroeter, 1906). At the end of the season he refined the period to 13.196 days (Enebo, 1906c), close to the value of 13.199 days obtained later from 301 photographic plates at Harvard College Observatory (Pickering, 1906). Four years later the period was further refined to 13.1989 days (Enebo 1910c). The first variable star that Enebo discovered was named RW Per.

At this time Enebo had about two dozen variable stars on his program. Many were Miras or semi-regulars for which several years would be required to determine the period. Successful period determinations were achieved for Algols, Cepheids, and RR Lyra stars during these initial years. The number of program stars more than doubled in 1907 when his new 11-cm refractor allowed fainter stars to be monitored. Each of the following years would see an annual increase of a dozen stars, bringing the program to 110 stars by 1912.

3 THE CONTRIBUTIONS OF 1912

Nova Geminorum II was discovered on 12 March 1912 by Sigurd Enebo. It was a serendipitous discovery by an experienced variable star observer. He realized immediately what he saw and rushed to alert astronomers at the University Observatory in Oslo. He was advised to telegraph the astronomical central bureau in Kiel which informed observatories worldwide. As it turned out, Enebo had discovered the Nova before it reached maximum luminosity so the object plays the role of the first nova to be studied spectroscopically in all phases of development.

A couple of months later he published three papers where he classified five variable stars as possible members of the RV Tau type. He selected the members of this class (Enebo, 1912b) by a light curve characterized by a periodic β Lyr-like variability, superimposed on a much slower background variation with a time scale of perhaps several years. The periods observed put RV Tau type stars between short period variables and long period variables. Enebo (1908e) had studied RV Tau itself in 1906-1908 and had identified the characteristics and periods of the variability. The β Lyr-like variations showed two unequal minima (0.6 and 0.3 magnitude) with a period of 78.6 days. He attributed the 3-year background variability of 0.8 magnitude to a slow variation of one of the components of what he thought was a β Lyr eclipsing binary. The long term periodicity would later become a distinguishing characteristic between subclasses RVa and RVb.

4 THE VARIABLE STAR PROGRAM

Sigurd Enebo published results for 125 stars throughout his career. Half of them (52%) were observed for 10 years or longer. The majority of these (78%) were irregular or long period variables.

During the first half of the twentieth century the most cited classification scheme for variable stars was due to Edward C. Pickering. It was an observational approach which separated the variables according to the nature of their brightness variations. There were five classes:

1. Novae
2. Short period variables
3. Long period variables
4. Irregular variables
5. Eclipsing variables

The stars on Sigurd Enebo's observing program are compiled in Tables 2-5. We list the modern variable classification in column 2 from the *General Catalogue of Variable Stars* (GCVS) (Samus et al., 2012). Enebo's classification is noted in column 4. The Tables also contain information on the total time span of Enebo's observations (column 3), the periods determined from these observations (column 4), and the literature sources for the data (column 5). The latter are identified by the issue number of *Astronomische Nachrichten* (e.g. AN 4188) and by the running number of Enebo's publication series of original observations, named *Beobachtungen Veränderlicher Sterne angestellt auf Dombaas (Norwegen)* (e.g. B # 2). These designations are attached to the relevant publications in the reference list.

4.1 Novae and Eruptive Stars

Table 2 lists novae and other eruptive stars observed by Enebo. The discovery of Nova Geminorum II = DN Gem is the single one serendipitous discovery that made his name known beyond contemporary variable star observers. At this time he was still observing from the yard outside his home with instant visibility of the entire sky. A year later he had completed a small tower observing room on top of his house, equipped with a revolving conical roof. Enebo later remarked that he would have made earlier detections of other novae (i.e. Cygni 1921 and Herculis 1936) had it not been for the limited sky view through the slit, which prevented easy visual scanning of the skies (Einbu, 1944a).

Table 2: Novae and eruptive stars observed by Sigurd Enebo.

Star Designation	GCVS Type	Observing Interval	Result	Literature sources
RW Aur	T Tau	1906-1939	Irregular behavior, but maxima at P=3,430 d	AN 4188, B # 2, 3, 4, 5, 6, 8, 9, 12
SS Aur	U Gem	1908-1939	Multiple outbursts	AN 4307, 4506, 4596, 4727, 5206, 5521, B # 4, 6, 8, 9, 10, 11, 12
SV Cep	Rapid irregular	1909-1939	Annual outbursts	AN 4497, 4596, 5206, B # 8, 12
SY Gem	U Gem?	1904-1916	Outbursts	AN 4229, B # 9
DN Gem	nova	1912	Discovery!	AN 4562
DQ Her	nova	1934	One observation	AN 6078
X Per	γ Cas	1903-1906		B # 1, AN 4207
UV Per	U Gem	1923-1926	Two outbursts	B # 10, AN 5521

Table 3: Short period pulsating variable stars observed by Sigurd Enebo.

Star Designation	GCVS Type	Observing Interval	Period	Literature sources
SY Aur	C	1907-1939	P=10,140 d	AN 4238, 5521, B # 3, 5, 8, 10, 12
RW Cam	C	1907-1908	P=16,4 d	AN 4223, B # 2
RZ Cam	RR	1909-1916	P=0,480 d; RR	AN 4497, B # 6, 12
RW Cas	C	1906-1907	P=14,80 d confirmed	AN 4207
SW Cas	C	1907-1911	P=5,44 d; C	AN 4223, B # 2, 3, 5
SZ Cas	C	1914-1918	P=13,604 d confirmed	B # 14
XY Cas	C	1922-1923	P=4,50 d confirmed	B # 14
XZ Cyg	RR	1905-1920	P=0,467 d; RR	AN 4094, B # 1, 2, 14
SU Dra	RR	1907-1911	P=0,660 d; RR	AN 4223, B # 5
RZ Gem	C	1908-1914	P=5,530 d; C	AN 4300, B # 3, 5, 13
Z Lac	C	1907-1912	P=10,89 d	AN 4223, B # 2, 3, 6
RR Lac	C	1907-1912	P=6,412 d	AN 4223, B # 2, 3, 6
SV Per	C	1907-1914	P=11,128 d	AN 4223, B # 2, 3, 8
SX Per	C	1907-1911	P=4,290 d; C	AN 4300, B # 5
U Tri	RR	1911-1915	P=0,447 d conf.; RR	AN 4595, B # 14

Another remarkable observation of a potential eruptive star, perhaps a dwarf nova, was made by Enebo in 1904. Upon consulting the *Bonner Durchmusterung* star maps on 4 March 1904 he noted that BD+31º1380 (magnitude 9.2) was not visible in his 7-cm refractor. He informed Schroeter at Oslo University Observatory who subsequently checked the field with larger telescopes. On 20 April 1904 he, too, could not see BD+31º1380. Enebo continued his monitoring for a year, but could never detect it. A letter from Schroeter dated 22 April 22 1905 reports that he saw it on 18 April 1905, slightly fainter than BD+31º1379. He estimated the magnitude at 9.5. Enebo's notebook reports that he saw it as very faint on 24 October and 28 November 1905, and again on 16 March 1906. These observations are reported with question marks in Enebo (1917). On Christmas Eve 1906 Enebo saw the star clearly at magnitude 9.5 in his new 11-cm refractor, 0.1 magnitude fainter than BD+31º1379 (Enebo, 1908a).

Enebo continued monitoring BD+31º1380 = SY Gem for the next ten years. He estimated his detection limit with the new refractor at magnitude 12.5 in the majority of cases, and again and again he noted that the star as invisible. On a few occasions he suspected he saw it very faintly, but recorded the observations as uncertain (Enebo, 1917). Numerous other observers have searched for SY Gem, both visually and photographically, without success. Enebo (1917) suggested that it might be a U Gem star with rare and short-lived flare ups. Meanwhile, a search for a nova remnant did not reveal any candidates (Downes and Szkody, 1989). A mystery remains about the 1856 *Bonner Durchmusterung* observations and later ones. Enebo is listed by Müller and Hartwig (1918) as the discoverer of SY Gem.

Other erupting stars in Table 2 are currently classified as a flaring T Tau-star (RW Aur) and an X-ray pulsar with an IR excess (X Per).

4.2 Pulsating Variables with Short Periods

Table 3 lists the short period variables observed by Enebo. There are eleven Cepheids and four RR Lyr stars, identified as C and RR, respectively, in column 2. He determined the periods in column 4 for eleven of the stars, and confirmed the period determined by other observers for four stars. He was the first to classify the four RR Lyr stars (e.g. see Figure 5)[2] and three of the Cepheids, based on his own light curves. XZ Cyg, which he observed extensively during the winter of 1905/1906, was the first variable star for which he published a light curve (Enebo, 1906b; 1906c).

4.3 Irregular and Long Period Variables

Table 4 lists the irregular and long period variables observed by Enebo. The type in column 2 is from Samus et al. (2012) where M indicates Mira variable, SRA/SRB/SRC/SRD indicate subgroups of semi-reg-

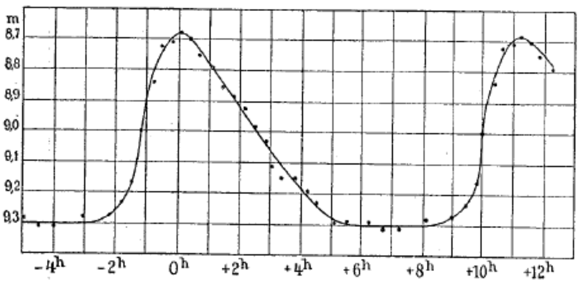

Figure 5: Sigurd Enebo's light curve for the RR Lyr variable XZ Cyg (after Enebo, 1906b).

ular (SR) variables, and RVa/RVb indicate sub-groups of RV Tau stars. LB indicates slow irregular variables. Column 4 lists the periods determined by Enebo. If a period could not be determined, he classified the star as irregular. Table 4 contains twenty-six Mira variables, of which Enebo determined a period for twenty-four stars. For sixteen Mira stars the period values determined by Enebo deviate less than 1% from the modern values listed in the GCVS. Three stars deviate more than 4%, i.e. TT Cas, W Dra, and Z Peg. Table 4 contains forty semi-regular variables, of which Enebo determined periods for thirty. Many of them are reported to show variable or multiple periods in the GCVS, which produces large deviations between current period values and 15 of those determined by Enebo. Two stars, i.e. RV Lac and RX UMa, have had the light curves re-interpreted and the periods revised by an integer number. Table 4 contains four RV Tau stars, of which Enebo determined a period for three. SS Gem later had its period doubled. There are eight irregular variables, in agreement with Enebo except that CY Cyg appeared constant during his (short) observing series. However, Enebo classified a total of seventeen stars in Table 4 as irregular, some no doubt due to the short time intervals over which he observed them.

Enebo persistently monitored forty-four stars for more than two decades and twenty-one stars for more than three decades. Three stars, i.e. RR Cyg, AD Cyg, and SW Gem, were monitored for thirty-five years. This last-mentioned star was the first long period variable discovered by Enebo (1906a) from observations conducted during 1904-1906. It took an additional four years before the period estimate was set at 698 days. This was refined to 680 days only when the observations ended in 1939 (Einbu, 1943). Further discoveries of long period variables were SW Per (Enebo, 1908a), SY Per (Enebo, 1908g), TT Cas (Enebo, 1909a), AI Cyg (Enebo, 1910b), RY Lac (Enebo, 1911b), and AF Peg (Enebo, 1914b).

Table 4: Irregular and long period variable stars observed by Sigurd Enebo.

Star Designation	GCVS Type	Observing Interval	Period	Literature sources
RV And	SRA	1914-1916		B # 13
SZ And	M	1907-1938	P=342 d	AN 4272, 5206, B # 11, 12
TU And	M	1909-1938	P=315 d	AN 4596, 4727, 5206, 5521, B # 8, 10, 12
TV And	SRB	1908-1939	P=115 d	AN 4323, 4506, 4596, 5521,B # 4, 6, 9, 10, 11, 12
TX And	M	1910-1938	P=234 d	AN 4596, 4727, 5206, B # 6, 11, 12
TY And	SRB	1910-1938	P=151 d	AN 4596, 4727, 5206, 5521, B # 6, 11, 12
UW And	M	1911-1938	P=236 d	AN 4596, 4727, 5206, 5521, B # 11, 12
RR Aur	M	1906-1939	P=308 d	AN 4207, 4506, 4596, 5206, 5521, B # 4,9,10,13
RS Aur	SR	1906-1939	P=170 d	AN 4207, 4506, 4596, 4727, 5206, B #2,3,6,11,12
RU Aur	M	1911-1939	P=466 d	B # 12
RV Aur	SRB	1914-1917	Irr. ?	B # 13
TV Aur	SRB	1908-1938	P=183 d	AN 4416, 4506, 4596, 5521, B # 5, 10, 12
TW Aur	SRB	1908-1939	P=148 d	AN 4416, 5206, 5521, B # 5, 9, 10, 12
TX Aur	LB	1911-1925	Irr.	AN 4727, B # 12
UZ Aur	SRB	1908-1916	Irr.?	B # 9
VW Aur	SRB	1911-1936	P=213 d	AN 4595, 4727, 5521, B # 9, 10, 12
ρ Cas	SRD	1903-1908	Irr.?	B #1, 2
Z Cas	M	1907		AN 4207
RV Cas	M	1906-1912	P=333 d	AN 4207, 4596, 4727, B #2
SS Cas	M	1914-1916	P=142 d	B # 14
TT Cas	M	1908-1939	P=372 d	AN4277,4323,4506,4596,4727,5521,B #5,8,10,12
X Cnc	SRB	1904-1910	P=362 d	AN 4207, B # 4
RR Cyg	SRB	1904-1939	Irr.	B # 1, 2, 3, 14, AN 4506
SV Cyg	LB	1904-1906	Irr.	B # 1, 2
AB Cyg	SRB	1907-1931	P=522 d	AN 4272, 4323, 4727, B # 4, 11
AD Cyg	LB	1907-1942	Irr.	AN 4416, 4506, 4596, 4727, 5206, B # 5, 11, 14
AF Cyg	SRB	1910-1913	P=94 d	AN 4596, 4727, 5206, B # 6, 13
AH Cyg	SRB	1909-1932	P=100? d	AN 4497, 4727, B # 8, 11
AI Cyg	SRB	1910-1942	P=141 d	AN 4400, 4497, 4596, 4727, B # 6, 11, 14
AV Cyg	SRD	1910-1940	P=88 d	AN 4595, 4727, 5206, 5521, B # 8, 10, 13
CY Cyg	LB	1904-1906	constant	B # 1
W Dra	M	1906-1927	P=260 d	AN 4207,4506,4596,4727,5206,5521, B #2,3,5,10
X Dra	M	1907-1938	P=257 d	AN 4207, 4506, 4596, 4727,5206,5521,B # 3,5,12
SV Dra	M	1909-1939	P=258 d	AN 4416, 4506, 4596, 5206, 5521, B # 5, 10, 13
TT Dra	SRB	1914-1918	P=95 d	AN 5206
TY Dra	LB	1907-1927	Irr.	AN 4497, 4727, B # 6, 10
UU Dra	SRB	1907-1927	P=234? d	AN 4497, 4596, 5521, B # 8, 10
SS Gem	RVa	1908-1911	P=45 d	AN 4323, 4506, B # 5
SW Gem	SRA	1904-1939	P=680 d	AN 4092, 4272, 4416, 4497, 5521, B #1,2,5,10,13
g Her	SRB	1903-1906	Irr.	B # 9
RY Her	M	1906		AN 4207
U Lac	SRC	1904-1908	Irr.	B # 1, 2
RS Lac	SRD	1908-1926	P=237 d	AN 4323, 4596, 4727, 5206, 5521, B # 4
RU Lac	M	1911-1927	P=203 d	AN 4596, 4727, 5206, 5521, B # 10
RV Lac	SRB	1909-1932	P=137 d	AN 4416, 5521, B # 10, 11
RY Lac	SRB	1910-1932	P=122 d	AN 4473, 4497, 4596, 4727, B # 6, 11

T Lyr	LB	1904-1906	Irr.	B # 9
X Lyr	LB	1904-1909	Irr.	B # 3
SZ Lyr	SRA	1909-1939	P=133 d	AN 4497, 4727, 5206, 5521, B # 6, 9, 10, 13
TX Lyr	M	1913-1939	P=223 d	AN 4715, 5207, 5521, B # 10, 13
Z Peg	M	1905-1911	P=320 d	AN 4207, 4506, B # 3
SS Peg	M	1907-1939	P=419 d	AN 4272,4323,4506,4596,4727,5206,5521, B # 5, 10, 14
ST Peg	SRB	1907-1939	P=101 d	AN 4272, 4727, B # 4, 10, 11, 14
SU Peg	M	1909-1939	P=198 d	AN 4416, 4506, 4596, 4727, 5206, 5521, B # 14
SW Peg	M	1911-1939	P=396 d	AN 4596, 4727, 5206, 5521, B # 10, 14
SX Peg	M	1910-1927	P=306 d	AN 4727, 5206, 5521, B # 9, 10
UY Peg	LB	1907-1914	Irr.	AN 4595, B # 8
AF Peg	SRB	1913-1932	P=52 d	AN 4726, 5206, 5521, B # 10, 11
SW Per	SRB	1905-1932	P=84 d	AN 4229, 4272, 4506, 4596, 4727,B # 2,5,8,10,11
SY Per	SRA	1907-1940	P=472 d	AN 4271, 4323, 5206, 5521, B # 8,10,14
TW Per	M	1911-1927	P=337 d	AN 4727, 5206, 5521,B # 10
TX Per	RVa	1911-1940	P=77 d	AN 4595, 4727, 5206, 5521, B # 8, 10, 11, 14
UZ Per	SRB	1911-1940	Irr. ?	B # 9, 10, 14, AN 5206, 5521
VV Per	SRB	1913-1927	P=220? d	AN 4715, B # 10
VW Per	M	1913-1936	P=278 d	AN 5206, B # 14
R Sge	RVb	1914-1917	Irr. ?	B # 14
RV Tau	RVb	1906-1912	P=78,7 d	AN 4188, 4243, B # 2, 4, 6
TV Tau	SRA	1912-1913	P=120 d	AN 4727
TX Tau	SRA	1911-1940	Irr.	B # 11, 14
S Tri	M	1909-1939	P=248 d	AN 4506, 4596, 4727, 5206, 5521, B # 5, 10, 14
T Tri	M	1911-1940	P=320 d	AN 5206, B # 14
Y UMa	SRB	1906-1909	Irr.	AN 4207, B # 2, 3
Z UMa	SRB	1906-1908	P=206 d	AN 4207, B # 2
RS UMa	M	1906-1939	P=260 d	AN 4207, 4280, 4506, 4596, 4727, 5206, 5521, B # 2, 3, 8, 10, 14
RX UMa	SRB	1907-1919	P=64 d	AN 4272, 4323, 4506, B # 4, 6, 11
RZ UMa	SRB	1908-1913	P=133 d	AN 4323, 4506, 4727, B # 5
SV UMa	SRD	1910-1932	P=76 d	AN 4596, 4727, 5521, B # 10, 11
V UMi	SRB	1910-1913	P=72 d	AN 4497, 4596, 4727, B # 6

Table 5: Eclipsing variables observed by Sigurd Enebo.

Star Designation	GCVS Type	Observing Interval	Period	Literature sources
TT And	EA	1907-1913	P= 2,764 d	AN 4232, B # 14
UU And	EA	1910-1920	P=1,486 d	AN 4502, B # 12
RY Aur	EA	1907-1925	P=2,725 d	AN 4232, B # 7, 13
SX Aur	EB	1907-1939	P=1,210 d conf.	AN 4238, B # 3, 4, 6, 9, 12
TT Aur	EB	1907-1926	P=1,333 d	AN 4272, 4300, B # 3, 9, 13
ε Aur	EA	1903-6; 1928-30	A minimum	B # 1, 13, 14
SS Cam	EA	1909-1936	P=4,824 d	AN 4497, B # 9, 12
SX Cas	EA	1907-1909	P=36,564 d	AN 4238, 4241, B # 3
SX Dra	EA	1909-1913	P=5,169 d	AN 4386, 4502, B # 7
UZ Dra	EA	1907-1925	P=3,261 d	AN 4595, 5206, B # 7, 13
RX Gem	EA	1907-1913	P=12,209 d	AN 4232, 4407, B # 7
SV Gem	EA	1908-1920	P=4,006 d	AN 4386, B # 7, 13
SX Gem	EA	1908-1930	P=1,367 d	AN 4497, B # 7, 13
u Her	EA	1903-1906	P=2,051 d conf.	B # 1, AN 4363
RT Lac	RS CVn	1908-1910	P=5,073 d	AN 4319, 4416, B # 4
RW Lac	EA	1909-1916	P=5,185 d	AN 4400, 4410, 4502, 5206
TT Lyr	EA	1911	P=5,244 d	AN 4497
RV Per	EA	1905-1910	P=1,974 d	AN 4173, 4207, 4407, B # 4
RW Per	EA	1905-1910	P=13,199 d	AN4078,4407,B # 1
ST Per	EA	1907-1910	P=2,648 d	AN 4223, 4407, B # 4
SV Tau	EA	1908-1913	P=2,167 d	AN 4319, 4407, B # 4, 7
RW UMa	EA	1907-1913	P=7,328 d	AN 4272, 4502, B # 7
RR Vul	EA	1907-1908	P=5,051 d	AN 4272, 4300
RS Vul	EA	1908-1909	P=4,477 d	AN 4386

4.4 Eclipsing Variables

Table 5 lists the eclipsing variables observed by Enebo. There are twenty-one Algol variables, two β Lyr type, and one RS CVn star, identified in column 2 by EA, EB, and RS CVn, respectively. Enebo determined the periods in column 4 for twenty-one of the stars, and confirmed the periods determined by other observers for two stars. He classified nineteen Algol stars and one β Lyr star based on his own light curves.

RW Per was the first variable star discovered by Enebo (Schroeter, 1906). Enebo (1906c) classified it immediately as an Algol and determined the period to be 13.196 days. This closely matched the photographic result of 13.199 days (Pickering 1906) and was further improved by Enebo (1910c) to 13.1989 days.

Enebo also discovered the Algol variable RW Lac. It served initially as a comparison star to RV Lac, but a faint appearance and subsequent rapid rise on 11 December 1909 led Enebo (1910b) to suspect Algol variability. He suggested a period of 5.18 days and refined the value just a month later to 5.1874 days (Enebo, 1910d). Further observations a year later (Enebo, 1911d) and a final minimum in 1915 brought the period to 5.18453 days (Enebo, 1923). The modern value in the GCVS (Samus et al., 2012) is 10.36922 days, due to Martinov (1938) who decided from a photographic light curve that the secondary eclipse did not occur exactly at phase 0.5. Lacy et al. (2005) concluded from photoelectric and spectroscopic observations that RW Lac consists of two very similar main sequence G-stars with masses close to 0.9 solar masses. They confirm a slightly eccentric orbit and suspect synchronous rotation. Comparison with evolutionary models suggests an age of 11 Gyr.

The period determination of some stars would prove challenging. SX Aur initially led to P = 1.53 days but with a growing time series the number of

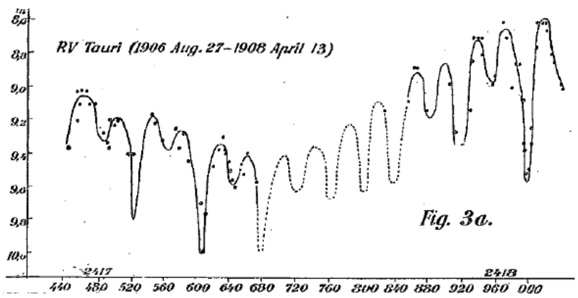

Figure 6: The light curve of RV Tau generated by Sigurd Enebo (after Enebo, 1908f).

outliers increased. A decade of observations did not allow Enebo to determine a definitive period. A letter from Ejnar Hertzsprung at Leiden Observatory dated 19 March 1929 informed Enebo that a reanalysis of his observations has led to P = 1.21 days. This was supported by further observations of SX Aur by P.T. Oosterhoff in Leiden, which revealed primary and secondary minima of different depth. When Enebo concluded his observations in 1936 he confirmed Hertzsprung's value. TT Aur initially revealed a period of 0.67 days but seven more years of observations led to twice that value. The initial observations of SV Tau suggested P = 2.167 days, but continued monitoring revealed minima of different depth and the period value was doubled. However, the star catalogues have retained the initial value pending clarification.

5 VARIABLE STAR SUMMARY

The main event contributing to the present centenary was the discovery of Nova Geminorum II = DN Gem at 20:32 MET on 12 March 1912 (Enebo, 1912a). It was an astrophysically-important discovery because the nova was detected before it reached maximum. This allowed the largest refractor in the world at Yerkes Observatory to obtain time-lapse spectra for the first time of the initial phases of nova developments. Immediately after the discovery the spectrum showed hydrogen lines in absorption, but on 15 March they appeared as strong emission lines. They increased in strength during the next few days as did the ultraviolet continuum (Parkhurst, 1912).3 The blue- shifted lines indicated approaching gas shells at several hundred km/s. Recent high resolution Hα imaging with the Nordic Optical Telescope has revealed remnants of several rings around the current 16th magnitude star (J.E. Solheim, private communication).

Enebo (1912b; 1912c; 1912d) was the first to refer to RV Tau type variables as a separate classification. This designation is still in use today, one hundred years later. Enebo had made a thorough study of RV Tau itself between 1906 and 1910 (Enebo, 1907b; 1908e; 1908f; 1910f), and one of his light curves is reproduced here in Figure 6. His interpretation at the time was governed by the observed β Lyr type light curve with a period of 78.8 days combined with the speculation that one of the components was also a slow variable in its own right with a period of 3 years. Based on light curves in the literature Enebo (1910f) remarked that R Sge and V Vul showed similar behavior.

By 1912 Enebo had recognized similar behavior in stars on his own observing program. TV And was his strongest candidate (Enebo, 1912b; 1912d). He also suggested RW Aur, TY Dra, RY Lac, UY Peg and RX UMa as possible members of the RV Tau class (Enebo, 1912b; 1912c; 1912d). This seems to have gone unnoticed for several years. In the review of variable stars in *Handbuch der Astrophysik*, Ludendorff (1928) referred to Sigurd Enebo as the originator of the term RV Tau type variables. This was pointed out in a note by Zsoldos (1993). Ludendorff's list of RV Tau stars included TV And from Enebo's proposal, but none of the others. It is remarkable that none of Enebo's candidates (except RV Tau itself) is included in the RV Tau class today, but R Sge and V Vul are.

Spectroscopy later revealed that RV Tau type stars are very luminous supergiants. Radial pulsations are thought to cause both volume and temperature fluctuations of the star. The location of RV Tau stars in the Hertzsprung-Russell Diagram suggests them to be in the short-lived post-AGB phase. Some (if not all) are binaries, and mass loss may have taken place during the giant phase of one component. The energy distributions show excesses at infrared and submillimeter wavelengths, indicating stable and extensive dust disks or shells.

During his career Enebo discovered a nova (DN Gem), a possible dwarf nova (SY Gem), seven long period/semiregular variables (SW Gem, SW Per, SY Per, TT Cas, AI Cyg, RY Lac, and AF Peg) and two Algols (RW Per and RW Lac). He determined periods for seven Cepheids, three RR Lyra stars, twenty-four Miras, thirty semi-regular variables, three RV Tau stars, and twenty-one eclipsing binaries. Throughout his career he monitored 125 different variable stars for which he collected 22,403 magnitude values. He had several comparison stars for each variable, so the total number of individual magnitude estimates was 49,783.

Two stars in Enebo's variable star program remain undecided. BD+43°1712 was bright enough to be observed twice in the *Bonner Durchmusterung* program in March 1857 at magnitude 9.5. Enebo reported it missing in 1905. This was confirmed by a visual search with a 19-cm Merz refractor at the University Observatory in Oslo and also by a photographic plate obtained at Harvard Observatory (Schroeter, 1905, Kreutz; 1905). Enebo also suspected a star in Cep to be variable. In his last paper (Einbu, 1944b) he noted that it had still not been designated a variable star, but he maintained that it may be variable.

6 CONCLUDING REMARKS

Enebo was appointed to honorary memberships of astronomical societies in France, Mexico, and Norway. He was elected a member of the Norwegian Academy of Sciences and in 1926 received its Fridtjof Nansen Award for excellent research. Five years later he received the Gunnerus Medal from the Royal Norwegian Society of Science. He became a member of IAU Commission 27 (Variable Stars) in 1933.

In Germany the Astronomische Gesellschaft awarded Enebo their Lindemann Award in 1906 for his discovery of the Algol variable RW Per. He received a reprinting of the complete *Bonner Durchmusterung* star catalogue and atlas. The Sociedad Astronomica de Mexico awarded Enebo the Atenogenes Silva Medal for his discovery of Nova Geminorum II in 1912.

In addition to his scientific efforts described in this paper, Sigurd Enebo was the leading popularizer of astronomy in Norway in the first half of the twentieth century. He contributed monthly articles on the night sky to numerous newspapers throughout the country for several decades. He authored several popular books on astronomy, some written with a particular focus on young readers.

The Enebo family changed their name to Einbu in 1926. This was motivated by their local dialect and also reflected the original name of the family homestead. Sigurd Einbu even changed the written orthography in his later astronomy books, from formal Norwegian to a form strongly reflecting his local dialect.

For several decades Enebo spent the summer months (which were too bright for observing) on lecturing tours throughout the country, presenting astronomy to the general public. Consequently, 'Sigurd Einbu' was a household name in Norway when he died on 10 May 1946, six months short of his 80th birthday. Two generations later he is largely forgotten. His remarkable career from self-taught astronomy studies to a unique, lifelong state stipend, to discoveries of variable stars, and his persistent, extended series of careful observations, should justify his place in the history of variable star astronomy.

7 NOTES

1. The GPS position of the observatory site is: 62° 04′ 22″ N; 09° 05′ 20″ E.
2. Enebo referred to RR Lyra stars as Antalgols. This term 'Antalgol' was first proposed by Hartwig for the short period Cepheids. The light curve is characterized by a constant phase during the minimum and a steep rise to maximum, i.e. the opposite of Algol eclipsing binaries.
3. An anonymous referee has pointed out the career similarities, from amateur to professional employment, for Enebo and Parkhurst, on the basis of careful observations and analyses.

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IAU HISTORIC RADIO ASTRONOMY WORKING GROUP: TRIENNIAL REPORT (2009-2011)

CHAIR **Kenneth Kellermann**
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COMMITTEE **Rod Davies**
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Masato Ishiguro
James Lequeux
Govind Swarup
Jasper Wall
Richard Wielebinski
Hugo van Woerden

1 INTRODUCTION

The IAU Working Group on Historical Radio Astronomy (WGHRA) was formed at the 2003 General Assembly of the IAU as a Joint Working Group of Commissions 40 (Radio Astronomy) and 41 (History of Astronomy), in order to: a) assemble a master list of surviving historically-significant radio telescopes and associated instrumentation found worldwide; b) document the technical specifications and scientific achievements of these instruments; c) maintain an on-going bibliography of publications on the history of radio astronomy; and d) monitor other developments relating to the history of radio astronomy (including the deaths of pioneering radio astronomers).

The HRA WG is now an Inter-Division (DX and DXII) Working Group.

2 WEB SITE

The IAU HRA WG maintains a web site at http://rahist.nrao.edu/ which includes past as well as current WG reports, brief biographical notes on Grote Reber Gold Medalists for Innovative Contributions to Radio Astronomy, photographs and memorial articles on recently-deceased radio astronomers, and links to various sources of material on the history of radio astronomy.

3 PRESERVATION OF HISTORICAL RADIO ASTONOMY SITES AND PAPERS

The WG noted with satisfaction that the reported deterioration of the Bell Labs horn reflector used by Penzias and Wilson to detect the CMB has been addressed by Lucent Technologies, and that the horn has been refurbished. However, the Bell Labs property where Karl Jansky made his pioneering discovery is being sold to a real estate developer. In 1998 Bell Labs erected a Karl Jansky Monument on the exact location of the original Jansky antenna. Regrettably this monument has fallen into disrepair, but efforts are underway to secure the preservation of the site and its public access.

In the Netherlands, the 25-meter Dwingeloo dish, inaugurated in 1956, and used for major research programs up to 1998, has been repaired and modernized by CAMRAS, a foundation run by radio amateurs, since 2006. The Dutch Ministry of Education, Culture and Science has granted a major subsidy for the full restoration of the radio telescope, which was started in June, 2012. The radio telescope will be made available for education and research projects by high-school students. The 60th anniversary of the first 21 cm mapping of the Milky Way with the 7.5 meter dish at Kootwijk was celebrated at the original site on 11 May 2011.

Ten of the original thirty two concrete piers which were part of Ron Bracewell's spectro-heliograph were shipped to the VLA site where they will form part of the Ron Bracewell Sundial designed by Woody Sullivan and funded by the Friends of the Bracewell Observatory. These piers contain the signatures of many radio astronomy pioneers, which were chiseled into the concrete at the time of their visits to see the radio heliograph.

The first telescope on Haleakala on Maui was Grote Reber's sea interferometer which he built in the early 1950s. Although most of Reber's antenna was destroyed in a storm, the base of the antenna, known as 'Reber's Ring', still remains, but will soon be transformed into a parking lot in support of the Advanced Technology Solar Telescope which is being constructed on a nearby site.

In 2003, the National Radio Astronomy Observatory initiated the first Archives devoted exclusively to radio astronomy. The NRAO Archives seeks out, collects, organizes, and preserves institutional records, personal papers, audio-visual materials, and oral histories of enduring value documenting NRAO's development, institutional history, instrument construction, and ongoing activities, including its participation in multi-institutional collaborations. As the national facility for radio astronomy in the USA, the Archives also includes an increasing collection of materials on the history and development of radio astronomy and the work of individual astronomers, especially in the United States. See http://www.nrao.edu/archives/.

The processed collection now extends to 435 linear feet and includes the institutional records of NRAO, Web resources on early radio astronomy courses and on Nan Dieter Conklin and Harold 'Doc' Ewen, as well as the personal papers of Don Backer, Ronald Bracewell, Bernard Burke, Marshall Cohen, John Findlay, Mark Gordon, David Heeschen, David Hogg, Kenneth Kellermann, John Kraus, Grote Reber, Arthur Shalloway, A. Richard Thompson, and Paul Vanden Bout. Processing of NRAO records is on-going, as material is transferred to the Archives from the Director's Office and from other NRAO sites.

Between 2010 and 2012, Woodruff Sullivan III donated research materials gathered over 30 years in writing his book, *Cosmic Noise: A History of Early Radio Astronomy* (Sullivan, 2009a), including 255 interviews with radio astronomers audio-taped between 1971 and 1988. His book covers the period up to 1953, but a significant portion of his interviews and his other materials illuminates post-1953 radio astronomy history. The 2011 Pollock Award from Dudley Observatory funded the digitization of the taped interviews and the preparation of detailed finding aids for the Sullivan collection. Work is currently in progress to transcribe previously un-transcribed interviews and to scan and correct existing transcripts. See http://www.nrao.edu/archives/sullivan/sullivan.shtml/.

Additional material on the history of radio astronomy can be found at: http://www.astro.Washington.edu/users/woody/hra.html.

We are very pleased to note that in recognition of his outstanding contribution to the history of astronomy, Sullivan was awarded the 2012 Doggett Prize of the AAS in recognition of his "leadership in the history of astronomy community".

4 NECROLOGY

We note with sadness the passing of the following friends and colleagues:

> David Axon, Don Backer, John Baldwin, Dipak Basu, Émile Blum, Geoffrey Burbidge, Tom Carr, Robin Conway, Dave DeYoung, Bill Ellis, Shinzo Enome, Istvan Fejes, Andrej Finkelstein, Georgij Gelfreikh, Vitali Ginzburg, Bill Gordon, Stan Gorgolewski, Albert Greve, David Heeschen, Yuri Ilyasov, Naum Kaidanovsky, Kinaki Kawabata, Masatoshi Kitamura, Bernard Krygier, Mukul Kundu, Arkadij Kuzmin, Norm Labrum, Thomas Legg, Jack Locke, Frank Low, Bernard Lovell, Bernie Mills, Jelena Miloggradov-Turin, Masaki Morimoto, Koh-Ichiro Morita, Vengataraman Radhakrishnan, Ernst Raimond, Jorma Riihimaa, Steve Rawlings, Bob Rood, Vagharshak Sanamian, Kevin Sheridan, Natalia Soboleva, Titus Spoelstra, Jaap Tinbergen, Keiya Takakubo, Atsushi Tsuchiya and Gisbert Winnewisser.

We are saddened by their loss but are grateful for having known them and for their contributions to science.

The Working Group web site maintains a list of deceased radio astronomers with brief career descriptions. Notification of future deaths should be brought to the attention of the Working Group Chair for posting on the web site.

5 CONFERENCES

Celebrations of the 50th anniversaries of the NRAO, Bridle et al. (2008) and Parkes in 2011 (see http://www.atnf.csiro.au/research/conferences/Parkes50th/program.html) and the 40th anniversary of Westerbork (http://www.astron.nl/wsrt40/) and Effelsberg (http://www.mpifr-bonn.mpg.de/div/effelsberg/40years/en/index.html) each contained historical reviews of the development of radio astronomy.

In November 2009, Kellermann and Ekers organized a session on *Discoveries in Astronomy* at the American Philosophical Society, with an emphasis on radio astronomy in presentations by Ekers and Kellermann (2011) on "Discoveries in astronomy," by Schmidt (2011) on the "Discovery of quasars," by Longair (2011) on "The discovery of pulsars and the aftermath" and by R.W. Wilson on "The discovery of the cosmic microwave background" (unpublished). All of the presentations can be viewed on-line at: http://www.amphilsoc.org/meetings/webcast/archive/y/2009/m/11.

At the 2011 General Assembly of URSI Commission J, Kellermann reviewed the careers of recently-deceased radio astronomers.

6 OTHER MAJOR PUBLICATIONS

Sullivan (2009b) has published an extensive history of radio telescopes covering the postwar period up to 1990. Wielebinski and Wilson (2010) have reviewed the history of radio astronomy instruments and their state of preservation. As part of her Master's thesis at West Virginia University, Kenwolf (2010) has discussed the personnel issues associated with the establishment and operation of the NRAO in Green Bank. Stewart (2009) has discussed the CSIRO Radiophysics field stations at Penrith and Dapto in his Ph.D. thesis with James Cook University (Australia). Tritton (2011) discusses the history of radio telescopes in Great Britain, while Strom (2008) reminds us of de Voogt's contributions as both an amateur and professional astronomer. Goss and McGee (2009) have published a biography of Ruby Payne-Scott which conveys her personal challenges in trying to do radio astronomy in post-war Australia. In 2012, a new edition of this book for a non-science audience, *Making Waves: The Story of Ruby Payne-Scott*, *Australian Pioneer Radio Astronomer*, will be published by Goss as part of the Springer *Astronomers' Universe* popular astronomy series.

Papers reviewing the history of radio astronomy in France have been published by Orchiston et al. (2009), Lequeux et al. (2009), Pick et al. (2011) and Encrenaz et al. (2011). The early history of radio astronomy in Germany has been published by Wolfschmidt (2008). Papers on the history of the Stockert Radio Telescope by Wielebinski (2010) and the Effelsberg Radio Telescope by Wielebinski et al. (2011) also document the development of radio astronomy in Germany. Kellermann (2012) has edited a translation by Denise Gabuzda of the 1986 book in Russian on *A Brief History of Radio Astronomy in the USSR*. Maarten Roos and Pieter-Rim de Kroon have produced a short film (see http://www.spiral-galaxy.nl/) "Spiral Galaxy - De Melkweg Ontrafeld" (in Dutch with English or German sub- titles) which discusses the development of our knowledge of the structure of our Galaxy, from Kapteyn (1886) up to the 21-cm mapping at Kootwijk and Sydney (1951-1958).

Orchiston and Mathewson (2009) have described the development of the Chris Cross at Fleurs, while Stewart et al. (2010) have described the Radiophysics field station at Penrith. Orchiston et al. (2011) have edited the publication *Highlighting the History of Astronomy in the Asia-Pacific Region* (Springer) which includes history of radio astronomy papers by Stewart et al. (2011a), Stewart et al. (2011b), Stewart et al. (2011c), Wendt (2011), Wendt et al. (2011a), Wendt et al. (2011b) and Wendt et al. (2011c). Orchiston has completed his project on early French radio astronomy and is working with Masato Ishiguro and other Japanese astronomers to document the early history of radio astronomy in Japan (e.g. see Ishiguro et al., 2012). Govind Swarup (2010) has written an important paper about the "Growth and development of radio astronomy in India".

Ken Kellermann

Chair,
Working Group on Historical Radio Astronomy

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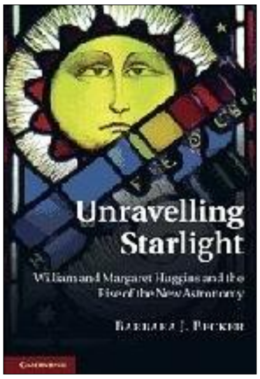
BOOK REVIEWS

Unravelling Starlight: William and Margaret Huggins and the Rise of the New Astronomy **by Barbara J. Becker (Cambridge, Cambridge University Press, 2011). Pp. xix + 380, ISBN 978-1-107-00229-6 (hardback), US$172.00.**



The rise of astrophysics has been the subject of considerable scholarship, ranging from the writings of David DeVorkin, Jack Meadows and others exemplified in the *General History of Astronomy*, to John Hearnshaw's detailed *Analysis of Starlight* (1986) and the special issue of this journal covering the first century of astronomical spectroscopy (Volume 13, July, 2012). Once considered an oddity among classical positional astronomers, in the twentieth century astrophysics came to dominate the field, revealing the nature of astronomical objects that the philosopher Auguste Comte famously declared would forever remain hidden to the human mind.

In the volume under review, Barbara Becker focuses on William Huggins, the man widely hailed as the founder of astronomical spectroscopy. It is a striking fact of history that Huggins (1824–1910) had no formal university education, and yet leapfrogged the professional astronomers of his time in expanding the theory and practice of astronomy to the new realm we now know as astrophysics. Becker examines how this happened in great detail, in the process providing a signal contribution to the history of astronomy.

Huggins could easily have remained in his father's business as a silk mercer and linen draper, never entering the field of astronomy. But instead, to the everlasting benefit of astronomy, he sold the business and pushed forward with his personal interests. Despite his lack of formal training, in 1856 Huggins built a rudimentary observatory in Tulse Hill, a suburb south of the Thames in London. He erected a new observatory at the end of 1862, which included an 8-inch Alvan Clark refractor he had acquired four years earlier. He began reporting startling results in 1864. What allowed him to obtain these novel results was spectroscopy, for Huggins's new observatory was "… the only work space of its kind in the world …" (page 58), with all manner of chemicals and chemical apparatus, batteries, Bunsen burners, and vacuum tubes spread around. With the help of his friend and neighbor William A. Miller (a Chemistry Professor at Kings College, who was skilled in laboratory spectroscopy), Huggins was able to set up not only an observatory, but an astronomical laboratory. This was the beginning of what a recent volume (David Aubin et al., *The Heavens on Earth*, 2010) dubs the "… observatory sciences …," analogous to broader laboratory sciences that historians have analyzed. Huggins and Miller proved to be an ideal team to bring spectroscopy into astronomy; and one of the themes of Becker's book is the necessity of crossing boundaries in creating a new discipline.

Chapters 5 and 7 detail Huggins' most famous discoveries: the gaseous nature of some nebulae, and stellar radial velocities. It is notable that both discoveries were made in the 1860s (1864 and 1868 respectively), very early in Huggins' investigations. Arguably, never again in his long career did Huggins match the fundamental nature of these discoveries, supporting the view (important even today for science policy makers) that new technology tends to yield its most fundamental discoveries early on. Becker's nuanced view of the discovery of nebulae shows that it was not as clear-cut as Huggins himself portrayed it more than three decades later in his personal retrospective on "The New Astronomy" (1897), often cited as the definitive description of his discovery. Becker sees Huggins' article as "… an alluring trap …" for the historian, and she looks beyond his description to argue that the discovery was likely much more complicated than pointing and seeing.

Huggins' discovery of stellar motion in the line of sight, today known as radial velocities, was perhaps even more fundamental than his determination of the gaseous nature of some nebulae, leading to a broad research program. In Huggins' time, however, the project was "… fraught with overwhelming mensurational and interpretive difficulties …" (page 104), a fact we tend to forget today when radial velocities are mass-produced. Becker uses observational notebooks to show how Huggins overcame these challenges, and how he had to persuade astronomers his measurements were real. For the star Sirius, for example, Huggins measured a velocity of 24 to 43 miles per second (the value today is about 6 miles per second). Much larger radial velocities of *galaxies* later became essential, especially with V. M. Slipher's work in the early 20th century, eventually leading to evidence for the expanding Universe. Stellar radial velocities continue to be essential to astronomical research, and have now been refined to such an extent they are one of the essential methods for detecting planets beyond our Solar System, as variations of stellar radial velocity due to perturbing planets are measured down to the meter-per-second level.

Throughout his long career Huggins occasionally followed up on his path-breaking work on nebulae and radial velocities, but more often he turned to other objects, including the Sun, planets, comets and novae, preferring to open new lines of research. In this he was aided by the Royal Society, which in 1871 equipped his observatory with a 15-inch refractor and an 18-inch reflector, with spectroscopic attachments. Huggins' relation with the Royal Society is another important theme of the book, illustrating how an amateur astronomer could break into the circle of the professionals.

In addition to the considerable published record (the *Scientific Paper*s were compiled by Huggins and his wife Margaret in 1909), Becker makes excellent use of archives around the world; indeed, it is the use of this unpublished material that makes her study so valuable. In particular, in addition to unpublished correspondence, the Hugginses' observatory notebooks covering the years 1856 to 1901, now located in the Wellesley College Special Collections in the USA, detail for the first time the important role of Margaret Huggins.

This points to another salutary feature of the book: it is important not only for the new historical details it reveals, but also for the broader themes it illuminates. True to her title, for example, in Chapters 10, 12 and 15 Becker demonstrates the essential role of Margaret Huggins as a working partner with her husband, a working relationship that seems even more substantial than Caroline Herschel's role with her brother William. Margaret was Huggins' junior by a quarter century; she was 27 and he was 51 when they married in 1875. Yet by all accounts it was a happy marriage, all the more because of Margaret's serious interest in astronomy. More than a partner, Becker argues that Margaret helped shape the research agenda of the Tulse Hill Observatory, in particular when it came to photographic spectra, since Margaret had photographic skills even before she met William Huggins. Together they pioneered the use of the dry-gelatin photographic plate as applied to spectroscopy.

Becker also draws attention to the largely-forgotten but recently-resurrected work of Ludwik Fleck on the changing boundaries of scientific disciplines, arguing that Huggins' work can best be seen in the context of his "thought collectives," circles of specialized and peripheral individuals that interact in complex ways. Huggins the outsider, she argues, gathered close associates, but in order to be successful also had to break into the larger collective of professional groups such as the Royal Society. She is attentive to social issues, including how an 'amateur' astronomer could make such fundamental discoveries and how he became accepted in the world of professional astronomy. While Becker does not characterize her book as a comprehensive definitive biography, it is something much more, a nuanced biography that illuminates broader themes in science. For this reason, it will be of interest not only to historians of astronomy and astrophysics, but also to historians and philosophers of science in general.

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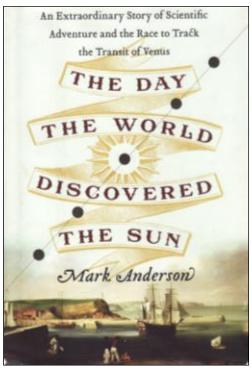
***The Day the World Discovered the Sun. An Extraordinary Story of Scientific Adventure and the Race to Track the Transit of Venus*, by Mark Anderson (Boston, Da Capo Press, 2012). Pp. [x] + 280, ISBN 978-0-82038-0 (hardback), US$26.00.**



The 2012 transit of Venus was the last chance for those of us alive at the time to see one of these rare astronomical events. While these transits now hold little scientific interest for most (but not all) professional astronomers, the eighteenth and nineteenth century transits played a crucial role in elucidating that fundamental yardstick of Solar System astronomy, the 'astronomical unit'. But the 1761, 1769, 1874 and 1882 transits were more than mere scientific endeavours, for attempts to observe them often involved international intrigue, tedious travel, debilitating diseases—even death—not to mention those cursed clouds at the very time of the transit.

That the historic transits of Venus were far more than just scientific events is brilliantly portrayed in Mark Anderson's new book, *The Day the World Discovered the Sun*, which deals only with the 1761 and 1769 transits. Since this book is primarily aimed at a scientifically-literate yet lay audience, it focuses on a small number of well-known characters: James Cook and Charles Green, Father Maximillian Hell and Joannes Sajnovics, Nevil Maskelyne and Robert Waddington, Charles Mason and Jeremiah Dixon, and one of my 'favourites', Jean-Baptiste Chappe d'Auteroche. Chapter by chapter, Anderson not only recounts the details of their respective transit expeditions, but also the associated background circumstances, and he does so in a charming and entertaining way, as evidenced, for example, by his account of the lead-up to Chappe's observation of the 1761 transit:

> By morning, however, the 4:30 sunrise had brought a dark veil. Clouds loitered. As the increasingly cloudy and sleepless night progressed, Chappe paced the observatory floor. His assistants, whom Chappe had woken earlier in the night, left their master alone—knowing they'd only be needed if clear skies returned …
>
> Soon after dawn, Chappe heard a commotion outside. Tobolsk's governor, the local archibishop, and some nobles had assembled at the new observatory to take in the heavenly spectacle. The first light of day shone upon the French visitor whose anxiety grew with each troubled glance at the clouded-over sky …
>
> As the dawn's blush gave way to early morning light, an easterly wind peeled back the top layers obscuring the sun. And with the increasing transparency, the mood both inside the observatory and in the nearby tent lightened. "The clouds began to exhibit a whitish colour, which grew brighter at every instant," Chappe wrote. "A pleasing satisfaction diffused itself through all my frame and inspired me with a new kind of life." (pages 46-47).

As the books nears its end, in Chapter 14 (titled 'Eclipse') Anderson briefly discusses the different values for the solar parallax that resulted from the various 1769 transit expeditions, before in his final chapter ('Epilogue') discussing other observations of the two eighteenth century transits and the resulting personal clashes as reputations were queried and egos bruised. He then ends by examining some of the consequences of these two transits and their associated astronomers, before discussing briefly the 2012 transit.

Between pages 231 and 240 is one of the most valuable features of this book, a 'Technical Appendix' where Anderson explains clearly and concisely the mathematics involved in converting contact observations of the 1769 transits into a value for the solar parallax and ultimately the astronomical unit.

Finally, Anderson provides 27 pages of notes and references, which readers will find very useful if they wish follow up on interesting areas of the book.

When I was asked to review this book, I thought "Not another book on historic transits of Venus." So you could say that I was less than enthusiastic! But Mark Anderson has produced a well-researched and beautifully-written book that was a pleasure to read.

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