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COVER PHOTOGRAPH

Stained-glass window, one of three originally at the home and observatory of William and Margaret Huggins in England and now at Wellesley College's Whitin Observatory, in Wellesley (Massachusetts). The windows (along with log books and other memorabilia) were given to this Women's College by Lady Huggins in 1914, shortly before her death. It is believed that William Huggins designed this window, which features the Sun with prominences seen in the red light of hydrogen, a spiral nebula, and Halley's comet. Across the diagonal is the Fraunhofer spectrum of the Sun with the principal dark lines (to H!) labelled. Below the labels is Huggins' greatest discovery, the spectrum of a nebula, showing just three bright lines. This image is included in the paper by Jay M. Pasachoff and Terry-Ann Suer on pages 120-126 in this issue of the journal, and there is more on William Huggins in other papers in this issue as well. The cover image is reproduced by courtesy of the Whitin Observatory, Wellesley College.

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THE FIRST CENTURY OF ASTRONOMICAL SPECTROSCOPY

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Abstract: This is an introduction to the following seven papers, which are expanded versions of talks presented to the Historical Astronomy Division of the American Astronomical Society at its January 2010 meeting in Washington, D.C.

Keywords: astronomical spectroscopy

Spectroscopy speakers and session organizer in Washington. Front (left to right): Barbara Becker, Matthew Stanley, John Hearnshaw, Jay M. Pasachoff; Rear: Richard A. Jarrell, Barbara Welther, Joseph S. Tenn, Vera C. Rubin and David H. DeVorkin.

1 INTRODUCTION

For the meeting of the Historical Astronomy Division (HAD) of the American Astronomical Society in January 2010 I organized a special session of invited papers on the first century of astronomical spectroscopy. This was defined as starting with the breakthroughs of Gustav Kirchhoff and Robert Bunsen in 1859-1860 and commemorated the sesquicentennial of those discoveries.

All but one of the speakers in that session, plus the authors of a relevant contributed paper, have provided expanded versions of their papers for this issue.

In the first paper, John Hearnshaw (2010) gives us an overview of the history of stellar spectroscopy, with emphasis on what it took to learn stellar compositions in detail, thus proving Auguste Comte wrong in his famous use of stellar composition as the perfect example of unattainable knowledge.

Matthew Stanley (2010) provides a look at the reaction of leading astronomers and physicists to this new method of learning about stars.

In the late nineteenth century spectroscopy was dominated by amateurs, while the professional astronomers at national observatories and universities continued to measure and compute stellar positions and motions. Barbara Becker (2010) shows how one of the most successful amateurs, William Huggins (1824–1910), made the transition to insider and winner of medals within just a few years of starting work in spectroscopy.

An interesting sidelight is the development of nomenclature. Joseph Fraunhofer (1787–1826) had labeled the lines in alphabetical order some 45 years before Kirchhoff and Bunsen, but he did not get as far as the letter K. Jay Pasachoff and Terry-Ann Suer (2010) have tracked down the origin of the notation for the two lines of ionized calcium which dominate the spectrum of Sun-like stars.

One of the major goals of stellar spectroscopy in the early twentieth century was the determination of radial velocities, to obtain full three-dimensional motions and eventually the structure of the system of stars in which we live. This required international cooperation, and Richard Jarrell (2010) tells us how that grew with the 1910 solar conference in Pasadena and at Mt. Wilson Observatory.

Another major goal was the determination of stellar compositions. Breakthroughs in atomic physics and the application of statistical mechanics to stellar atmospheres by Meghnad Saha (1894–1956), Ralph H. Fowler (1889–1944), and E. Arthur Milne (1896–1950) made it possible for young Cecilia Payne to make the first quantitative determination of the composition of the solar atmosphere in her 1925 doctoral dissertation. The reception she received for this work from leading astrophysical theorist Henry Norris Russell (1877–1957) is the topic of David DeVorkin's (2010) fascinating paper.

Those who measured wavelengths of lines in stellar spectra required laboratory measurements in order to know which gases formed those lines in stellar atmospheres and, for radial velocities, their rest wavelengths. Later they needed multiplet tables to work out compositions. Much of the laboratory work was done to order, particularly at the United States National Bureau of Standards (now the National Institute of Standards and Technology), and gathered, organized, and published by Charlotte E. Moore Sitterly. We are fortunate to have a senior astronomer of today, Vera Rubin (2010), recount for us not only some of Sitterly's history, but her own interaction with Sitterly over half a century, starting when Rubin was a young graduate student.

We were unable, however, to cover all of the interesting developments in the first century of astronomical spectroscopy.

There is nothing in these papers on the spectroscopy of the objects now known as galaxies. The work of V.M. Slipher (1875–1969) in measuring the redshifts and blueshifts of what were then called spiral nebulae (Slipher, 1913; 1915), greatly expanded by Milton Humason (1891–1972) (1931) and a host of others, especially Allan Sandage (b. 1926) (see, for example, Humason, Mayall, and Sandage, 1956), was a major part of the first century of astronomical spectroscopy, as was the discovery of galaxy rotation by Slipher (1914; see also Brémond, 2009) and the 1939 measurement of the rotation curve of M31 by Horace W. Babcock (1912–2003).

Nor is there anything here about the spectroscopy of planets, where again Slipher was a major contributor (see Slipher, 1933), but we could also have included discussions of the work of James E. Keeler (1857–1900), who measured the rotation of Saturn's rings (1895); Walter S. Adams (1876–1956) and Theodore Dunham, Jr. (1897–1984), who found carbon dioxide on Venus (1932; also Dunham, 1933); and Gerard P. Kuiper (1905–1973), who discovered carbon dioxide and methane in the atmospheres of Mars (1952) and Titan (1944), respectively.

Also, a full discussion of the first century of astronomical spectroscopy would have to include not only more about the early rocket flights that extended solar and then stellar spectroscopy into the ultraviolet and infrared, but also what might be the single most important spectral line: the 21-cm radiation of atomic hydrogen, predicted by H.C. van de Hulst (1918–2000) in 1944 and detected in 1951 by Harold Ewen (b. 1922) and Edward Purcell (1912–1997).

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Joseph S. Tenn taught physics and astronomy at Sonoma State University from 1970 to 2009. He now serves as Secretary-Treasurer of the HAD and as an Associate Editor of this journal, and he maintains the Bruce Medalists website at http://phys-astro.sonoma.edu/brucemedalists/. He acted as Guest Editor for the papers in this section of the journal.

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AUGUSTE COMTE'S BLUNDER: AN ACCOUNT OF THE FIRST CENTURY OF STELLAR SPECTROSCOPY AND HOW IT TOOK ONE HUNDRED YEARS TO PROVE THAT COMTE WAS WRONG!

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Abstract: In 1835 the French philosopher Auguste Comte predicted that we would never know anything about the chemical composition of stars. This paper gives a broad overview of the development of stellar spectroscopy, especially from about 1860. Developments in stellar spectroscopy segregated quite clearly into three main fields of endeavour: spectral classification, radial velocities and spectral analysis. This paper concentrates mainly on spectral analysis, or how stellar spectroscopy one hundred years after Comte showed that quantitative information on the composition of stars was possible. The journey was quite arduous, as it required numerous developments in theoretical physics and in laboratory spectroscopy, which in turn allowed stellar spectral analysis successfully to be undertaken by the mid-twentieth century.

The key developments in physics that first had to be understood were in quantum and atomic theory, ionization theory, the concept of the Planck function, local thermodynamic equilibrium, the first stellar model atmospheres, line formation theory, turbulence, collisional broadening of spectral lines and the theory of radiative transfer and of the curve of growth. The close links between stellar spectroscopy and theoretical physics will be emphasized. In addition laboratory physics, to measure line wavelengths and oscillator strengths, was also an essential precursor to quantitative data on the chemical composition of stars.

Comte may have been an influential philosopher of science in his time. Perhaps his one small transgression was not to have read the works of Joseph Fraunhofer, which in the early nineteenth century already contained the first small clues that Comte's assertion might be wrong.

Keywords: Auguste Comte, spectroscopy, stellar composition, stellar abundances, Gustav Kirchhoff, Robert Bunsen, stellar evolution, curve of growth, radiative transfer.

1 INTRODUCTION

The French philosopher, Auguste Comte (Figure 1), in his *Cours de Philosophie Positive* in 1835 wrote in relation to the stars (all translations by the author):

Figure 1: Auguste Comte, 1798–1857.

> We understand the possibility of determining their shapes, their distances, their sizes and their movements; whereas we would never know how to study by any means their chemical composition, or their mineralogical structure, and, even more so, the nature of any organized beings that might live on their surface (Comte, 1835).

And further on:

> I persist in the opinion that every notion of the true mean temperatures of the stars will necessarily always be concealed from us.

This passage from Comte has been much quoted in the astronomical literature, especially by astronomers happy to ridicule Comte for his apparently misguided prophecy, yet ignorant of his enlightened epistemology concerning scientific method.

Nevertheless, Comte did make a mistake, and this paper analyses both why he got his prophecy wrong, and why it took essentially a century after Comte before quantitative data on stellar chemical composition became available.

The first astronomer seriously to study stellar spectra was Joseph Fraunhofer (Figure 2), who in 1814 and 1823 studied the line spectra of the Sun and of a few bright stars (Fraunhofer, 1817; 1823). In these papers, Fraunhofer labelled the most prominent lines in the solar spectrum with letters of the Roman alphabet (see Figure 1 in Pasachoff and Suer, 2010: 120), he measured some solar line wavelengths using an objective grating, and he showed that the spectra of Betelgeuse, Sirius and Venus differ among themselves, even though some lines (such as D, b) were common to all sources.

Evidently Comte had not read these seminal papers of Fraunhofer, which (in hindsight) already contained the first small clues that Comte's assertion might be wrong.

2 KIRCHHOFF AND BUNSEN, PIONEERS IN SOLAR SPECTRAL ANALYSIS

Following Fraunhofer's pioneering investigations in the early nineteenth century, many workers explored the spectra of sparks and flames in the laboratory, notable among them being William Henry Fox Talbot (1800–1877) and John Herschel (1792–1871). Their work culminated in the research by Gustav Kirchhoff (Figure 3) and Robert Bunsen (1811–1899) in Heidelberg, who in 1860 published a study of the emission line spectra of many chemical elements (Kirchhoff and Bunsen, 1860). Already, in 1859, Kirchhoff had deduced the presence of sodium in the Sun from the coincidence of the D lines in absorption with the bright lines observed in the laboratory, an observation made earlier by Léon Foucault (1819–1868) in 1849 (Foucault, 1849). Kirchhoff and Bunsen (1860) surmised that the way was now open for the qualitative analysis of the Sun and stars.

Kirchhoff then embarked on a large project to draw a detailed map of the solar spectrum showing hundreds of absorption lines (Figure 4). He used the map to deduce the presence of Fe, Ca, Mg, Na, Ni and Cr in the outer layers of the Sun, and the possible presence of Co, Ba, Cu and Zn (Kirchhoff, 1861; 1863). Less than three decades after Comte made his assertion, it was already shown to be incorrect, at least for the Sun. But the results for the solar spectrum were no more than qualitative and there was no theoretical understanding of how the absorption lines were formed.

3 THE REBIRTH OF STELLAR SPECTROSCOPY IN THE 1860s

It is at first surprising that nearly four decades after the pioneering work of Fraunhofer, suddenly at least five astronomers made observations of the visual spectra of the stars in the early 1860s. On the other hand, Kirchhoff and Bunsen had done the groundwork to make this next step—the application of spectroscopy to stars, in the same way as Kirchhoff had done for the Sun—a natural one to undertake.

The first person to embark on observing stellar spectra in the 1860s was Giovanni Donati (1826–1873) in Florence. He used a single prism spectroscope on his 41-cm refractor (Figure 5) and he described the spectra of 15 bright stars (Donati, 1862). But by far the most productive observers were William Huggins (1824–1910) in London (assisted at first by William Miller (1817–1870)) and Angelo Secchi (1818–1878) in Rome. Lewis Rutherfurd (1816–1892) in New York also undertook stellar spectroscopy from this time. All these observers commenced their work in late 1862. At the same time, the British Astronomer Royal, George Biddell Airy (1801–1892), initiated a spectroscopy programme at Greenwich.

Lewis Rutherfurd (1863) was the first astronomer to attempt to classify stellar spectra. He recognized three main classes. He was also the first after Donati to publish his results. His classification was soon eclipsed by that of Secchi, who initially defined two classes (Secchi, 1863), which were later extended to three classes (Secchi, 1866) and then four (Secchi, 1868). His classes were

I: white or blue stars, with spectra similar to Sirius;
II: solar-type spectra;
III: red stars with bands, such as α Orionis;
IV: carbon stars.

Figure 2: Joseph Fraunhofer, 1787–1826.

Figure 3: Gustav Kirchhoff, 1824–1887 (courtesy: Smithsonian Institution).

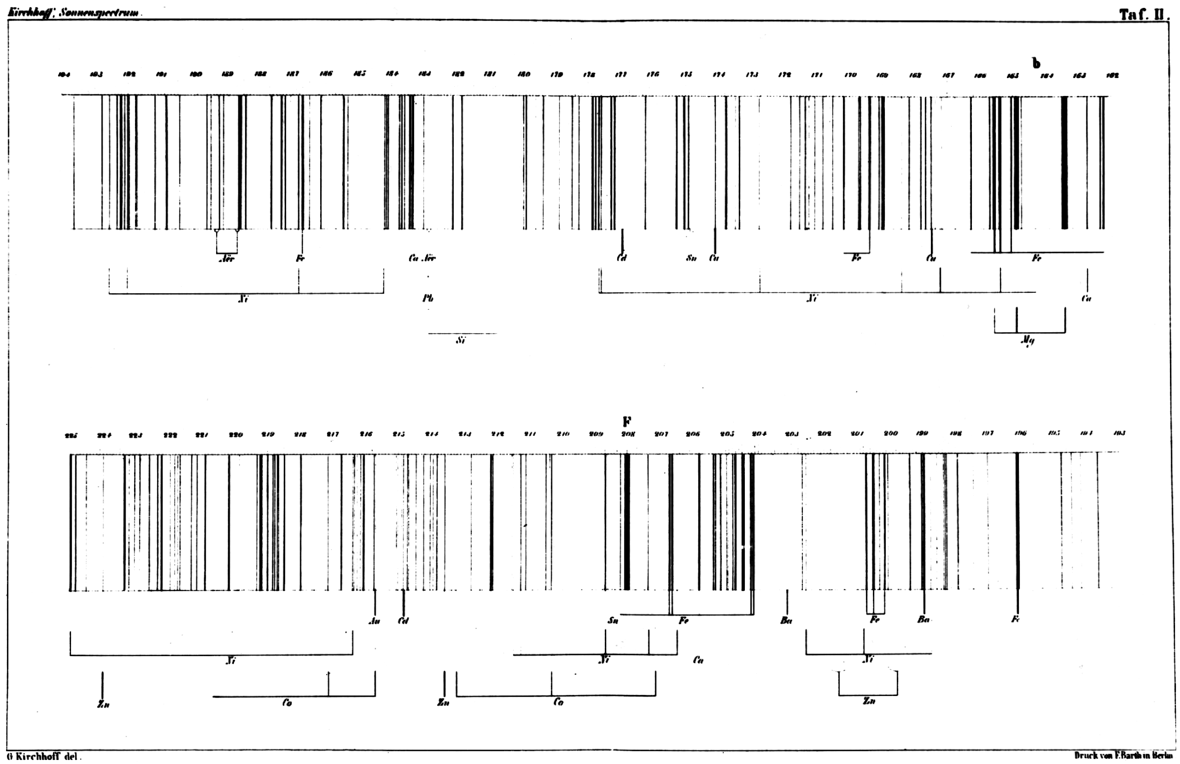

Figure 4: Part of Kirchhoff's drawing of the solar spectrum (after Kirchhoff, 1863).

The work of Huggins and Miller (1864a) focussed on line identifications in stellar spectra, using a two-prism spectroscope on the 8-inch refractor at Huggins' private observatory at Tulse Hill (see Becker, 2010). By 1864 they published descriptions of the spectra of some 50 stars, and compared the observed line positions with those recorded from laboratory sources. This work in qualitative spectral analysis thus continued the observations of Kirchhoff and Bunsen for the Sun, but now applied to the stars. For Aldeberan, 70 line positions were recorded and the presence of the elements sodium, magnesium, hydrogen, calcium, iron, bismuth, thallium, antimony and mercury in this star was reported, though evidently the last four of these were based on incorrect line identifications.

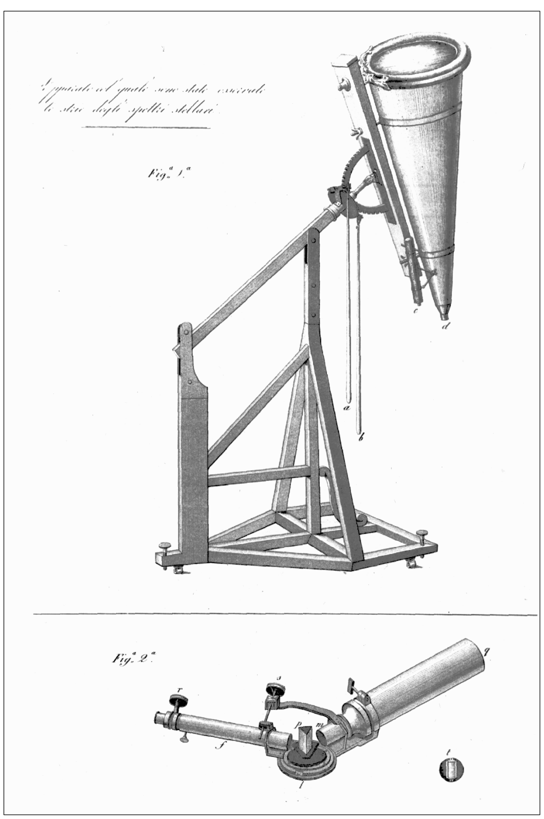

Figure 5: Donati's telescope and spectroscope (after Donati, 1862).

In 1864 Huggins and Miller (1864b) also turned their attention to the nebulae, and showed that objects such as the Great Nebula in Orion had an emission-line spectrum, quite dissimilar to the spectra of the stars. They suggested that these nebulae must consist of great clouds of hot nebulous gas which would never be resolvable into stars.

As for Airy, he directed the programme of observations carried out by members of his staff at Greenwich. Initially drawings of some stellar spectra were published (Airy, 1863). Later this developed into a programme that attempted visually to measure Doppler shifts, but without any significant success.

4 THREE BRANCHES OF STELLAR SPECTROSCOPY

From the 1860s and for almost the next century, research into stellar spectroscopy was rather clearly demarcated into three separate branches, namely spectral classification, stellar radial velocity determinations and spectral analysis. All three branches benefitted enormously from the introduction of the dry emulsion photographic plates into astronomy. The first successful experiments in stellar spectrum photography were by Henry Draper (1837–1882) at his Hastings-on-Hudson observatory in New York (Draper, 1879) in 1872, followed by William Huggins (1877) in 1876. In both cases, ultraviolet-transmitting spectrographs (quartz or Iceland Spar optics) and reflecting telescopes with silvered mirrors were used.

Stellar classification using objective prism spectrography advanced greatly with the work of Edward C. Pickering (1846–1919) at Harvard College Observatory from 1885. His initial trials were with an 8-inch astrograph known as the Bache telescope, which was equipped with a 13-degree objective prism. Huge classification programmes were undertaken at Harvard for the next 40 years, culminating in the *Henry Draper Catalogue* and its extension.

As for radial-velocity work, this became one of the most popular branches of the new astrophysics by the turn of the century. The early pioneers included Hermann Carl Vogel (1841–1907), the Director of the newly-established Potsdam Astrophysical Observatory, working with Julius Scheiner (1858–1913). From 1888 they perfected the techniques of photographic spectroscopy to determine Doppler shifts, by comparing the position of the Hγ absorption line in the spectra of the stars with the same line seen in emission from a hydrogen-filled Geissler discharge tube, which was mounted in the telescope. The plates were subsequently measured in a travelling microscope. A classical paper presenting the results for 51 stars followed in 1892 (Vogel, 1892). A light-weight two-prism slit spectrograph was used on the 30-cm Schröder refractor.

5 EARLY PROGRESS IN SPECTRAL ANALYSIS

Whereas spectral classification and radial-velocity determinations were both well established as important branches of stellar astrophysics by the turn of the century, the same cannot be said of spectral analysis. William Huggins had by 1864 shown that some of the common elements, whose spectra he observed in the laboratory, were also present in the stars and in gaseous nebulae. By 1867 the French astronomers Charles Wolf (1827–1918) and Georges Rayet (1839–1906) had discovered the emission line stars named after them (Wolf and Rayet, 1867), but the identification of the bright bands eluded them. Henry Draper and William Huggins had extended the observed spectra of bright stars into the near ultraviolet using photography, and they recorded the series limit of the Balmer lines. The element helium[1] was first found by J. Norman Lockyer (1836–1920) in the solar chromosphere in 1868, and by 1895 both Lockyer (1895) and Vogel (1895) had recorded this element in the spectra of blue stars of Vogel's class Ib (essentially B stars on the MK system). As early as 1868 Secchi had correctly identified carbon in his class IV stars, and other observers, including T.H.E.C. Espin (1858–1934), N.C. Dunér (1834–1914), C.F. Pechüle (1843–1914) and N. von Konkoly (1842–1916), greatly extended the number of these objects known. However, not all claims of element identifications in stellar spectra were reliable: thus Henry Draper (1877) photographed the ultraviolet solar spectrum and claimed to find oxygen lines in emission, a result soon discredited by Lockyer (1878).

This era in spectral analysis of the late nineteenth century was summed up by William Huggins in 1909, when he described his own early researches:

> One important object of this original spectroscopic investigation of the light of the stars and other celestial bodies, namely to discover whether the same chemical elements as those of our Earth are present throughout the universe, was most satisfactorily settled in the affirmative … A common chemistry, it was shown, exists throughout the universe. (Huggins and Huggins, 1909: 49).

In spite of these successes by Huggins and others, real progress in spectral analysis was stalled for about half a century from the rebirth of stellar spectroscopy in the 1860s. Ultimately the goal was to be able to interpret the lines observed in stellar spectra, including line strengths and even line profiles, so as to obtain quantitative information on the chemical composition and other physical parameters of stars. But progress required the theory of line formation, knowledge of stellar atmospheres, ionization theory, atomic theory, oscillator strengths and the ability to measure equivalent widths from high resolution spectra. To achieve all this and go on to make quantitative analyses of element abundances in stars took 100 years from Comte, and occurred about 75 years after Huggins' early work.

6 SOME NECESSARY DEVELOPMENTS IN THEORETICAL, LABORATORY AND INSTRUMENTAL PHYSICS

In hindsight, it is clear that developments had to occur in three separate areas before the deadlock that was holding up progress in spectral analysis could be resolved. These three areas were in theoretical physics, especially in atomic theory; in laboratory physics to measure such quantities as element wavelengths and oscillator strengths; and finally in astronomical instrumentation technology (see Stanley, 2010). The two big developments in the first few decades of the twentieth century were in high-resolution coudé spectrographs, especially those using blazed diffraction gratings and Schmidt cameras, and the recording microdensitometer, which converted a photographic plate of a stellar spectrum into a strip-chart record amenable for direct measurement of the lines.

Table 1 lists some of the essential developments within each of these three categories.

7 LOCKYER'S METEORITIC HYPOTHESIS

In 1887 Lockyer (1887; 1890) devised his so-called meteoritic hypothesis, in which star formation was presumed to occur from colliding streams of interstellar meteorites. The youngest stars were cool (such as Antares), but coalescence and contraction led to hotter stars such as Sirius or Vega. The next stage was the cooling of the stars after they reach a maximum temperature to the final stage of cool carbon stars, such as 19 Piscium (Figure 6).

Lockyer claimed to be able to distinguish between the ascending temperature branch and the descending branch spectroscopically, using selected lines he described as enhanced. The enhanced lines appeared stronger in the ascending branch of stars we now recognize as supergiants, and he theorized that they arose from the dissociation of elements into 'proto-elements' at high temperature and luminosity. In practice they are lines of ionized species which are stronger in stars of low gravity.

Table 1: Essential developments in physical theory, laboratory physics and astronomical instrumentation as precursors to progress in stellar spectral analysis.

Physical Theory	Laboratory physics	Instrumentation
Excitation theory (Boltzmann, 1871)	He I spectrum studied in lab (Ramsay, 1895)	Photographic dry plates in spectroscopy (Huggins, 1877; Draper, 1879)
Discovery of the electron (Thomson, 1897)	Line identifications: for the Sun: Rowland (1895-1897); Moore, Minnaert and Houtgast (1965); for stars: many workers, including Dunham (1929), Hacker (1935), and Davis (1939; 1947).	Blazed gratings (Wood, 1910; Adams and Dunham, 1938)
Planck's theory of black body radiation (Planck, 1900)	Study of atomic energy levels in the laboratory (Bowen, 1928)	Microdensitometer (Moll, 1920)
Radiative transfer (Schwarzschild, 1906)	Oscillator strengths (R. and A. King, 1935; Meggers, et al, 1961; Corliss and Bozman, 1962; Bashkin and Meinel, 1964)	Coudé spectrograph (Hamy, 1924; Adams, 1941; Dunham, 1934)
Line broadening theory (Voigt, 1912; Weisskopf, 1932; Lindholm, 1942)	Calibration of standard lamps for star colours (Kienle, et al., 1938)	Schmidt cameras (Schmidt, 1932; Dunham, 1934)
Ionization theory (Saha, 1920)		
Concept of LTE (Fowler and Milne, 1923)		
Model atmospheres (Eddington, 1926—grey atmosphere; McCrea, 1931; Strömgren, 1940)		
Curve of growth (Minnaert and Mulders, 1930; Schütz, 1930)		
H^- ion (Wildt, 1939)		

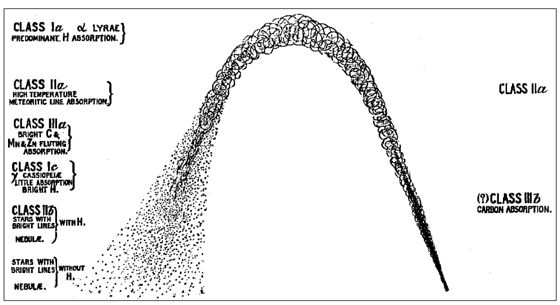

Figure 6: Lockyer's temperature curve for stellar spectral evolution (after Lockyer, 1887: 144).

Although Lockyer's ideas on stellar evolution never gained wide acceptance, his insights into the 'enhanced' lines predated the more rigorous ideas of ionization, based on the physics of an equilibrium reaction of

Figure 7: Meghnad Saha, 1894–1956 (after Sen, 1954).

Meghnad Saha (Figure 7). His ideas were also a forerunner to the sharp-lined c-stars defined by Antonia Maury (1864–1952) at Harvard (Maury and Pickering, 1897), which Ejnar Hertzsprung (1873–1967) showed to be stars of high luminosity (1909). Luminosity effects in stellar spectra were exploited by Walter S. Adams (1874–1956) and Arnold Kohlschütter (1883–1969) (but without reference to the work of Hertzsprung or Lockyer) to obtain spectroscopic parallaxes (Adams and Kohlschütter, 1914).

8 THE STELLAR TEMPERATURE SCALE AND SAHA'S IONIZATION THEORY

The first attempts to measure stellar temperatures came from the visual spectrophotometry of Johannes Wilsing (1856–1943) and Julius Scheiner at Potsdam (1909). They used five filter pass-bands to compare the brightness of stars with the energy distribution of a standard lamp, which was in turn calibrated using a black-body source. Hans Rosenberg (1879–1940) in Tübingen developed the technique of photographic spectrophotometry for comparing stellar energy distributions to those of black bodies (1914). A sequence of stellar temperatures thus resulted which placed stars in their correct order and showed a good correlation with spectral type, but the temperatures showed large systematic errors, because of the departures of the energy distributions of real stars from Planckian black-body curves.

Saha's (1920) ionization theory, when applied to the Harvard sequence of spectral types using the marginal appearance of different lines in stellar spectra, also gave a temperature sequence (Saha, 1921). The theory was developed further by Ralph H. Fowler (1889–1944) and E. Arthur Milne (1896–1950) (1923), explicitly taking into account the combined effects of ionization and excitation to deduce a more reliable temperature scale, and they also made the first determinations of electron pressures in stellar atmospheres. The electron pressures also clearly showed the difference between dwarfs and giants (Fowler and Milne, 1924; Milne, 1928). Thus, in the interval of less than a quarter of a century, the early work of Lockyer on enhanced lines and of Maury on c-type stellar spectra received a satisfactory explanation through Saha's ionization theory.

9 CECILIA PAYNE AND STELLAR CHEMICAL COMPOSITION

Ionization theory was the basis for a major study by Cecilia Payne (Figure 8) in her 1925 Ph.D. thesis at Harvard on stellar atmospheric element abundances using Harvard objective prism spectrograms. The results were published as a Harvard monograph titled *Stellar Atmospheres* (Payne, 1925), which was based on her thesis. According to Otto Struve, this was the most brilliant Ph.D. thesis yet written in astronomy (see Struve and Zebergs, 1962: 220).

In this work, Payne obtained ionization temperatures from the marginal appearance of selected spectral lines. In addition, she demonstrated low pressures for the Maury c-type stars (supergiants), as had been done earlier by Fowler and Milne. Her greatest achievement was to estimate the relative logarithmic abundances for 18 elements in the atmospheres of the stars studied. She demonstrated a general uniformity of chemical composition for stars of different spectral type, and also she demonstrated the preponderance of the light elements H and He. This latter result she herself doubted, possibly on the advice of Russell who reviewed her work (see DeVorkin, 2010).

Given the still rudimentary state of the understanding of atmospheric theory and the physics of line formation in the 1920s, it is remarkable that Payne made such striking progress in her interpretation of the Harvard spectra. The results nevertheless had much uncertainty, in part because the hydrogen abundance came from highly saturated Balmer lines, and not from the continuous opacity.

10 RUSSELL AND ADAMS AND STELLAR COMPOSITION

In the 1890s Henry Rowland (1848–1901) at Johns Hopkins University produced his much-used *Preliminary Table of Solar Spectrum Wavelengths* (Rowland, 1895-1897). In this catalogue of solar lines he gave wavelengths and identifications as well as so-called Rowland 'intensities' (R) on an arbitrary scale and based on visual estimates.

Russell, Adams and Charlotte E. Moore (1898–1990) calibrated the Rowland intensity scale in terms of the relative number of atoms in the Sun's reversing layer, using lines in the same atomic multiplet (with the same L and S quantum numbers in upper and lower states) (Russell, Adams and Moore, 1928). For example, a change in Rowland intensity from $R = -3$ to 40 implied $\Delta\log N \approx +6$. This calibration required invoking the new quantum mechanics of atomic structure, as developed by Ernest Rutherford (1871–1937) in 1911, Niels Bohr (1885–1962) in 1913, Arnold Sommerfeld (1868–1951) in 1919 and Erwin Schrödinger (1887–1961) in 1926.

Russell and Adams next used the calibration of the Rowland scale to analyse spectra of seven stars: α Ori, α Sco, α Boö, α Cyg, α Per, α CMi and α CMa. Spectral types ranged from A to M, and the first five evolved away from the main sequence. They studied 14 atoms or ions and obtained the relative abundances of line absorbers in reversing layers. The analysis was based on the reversing layer model of Arthur Schuster (1851–1934) (Schuster, 1902, 1905) and Karl Schwarzschild (1873–1916) (Schwarzschild, 1914), in which the line absorbers were concentrated in a thin layer above the photosphere. Large differences were found from star to star; thus the supergiants (α Ori, α Sco) had 100 times as much absorbing vapour in the reversing layer as the Sun, whereas Sirius had 100 times less. The electron pressures were 10^{-8} of the solar value for the supergiants but 200 times solar for the A-dwarf, Sirius, as deduced by the application of Saha's equation to elements that appeared in two ionization states (i.e. Fe, Sc and Ti).

This work represented the first analysis of stellar spectra using the stronger saturated lines. Cecilia Payne had relied entirely on the weakest lines of marginal appearance. Nevertheless, the analysis was limited to comparing the handful of lines within any given multiplet, so its scope was still quite restricted.

11 SOME INSTRUMENTAL DEVELOPMENTS: BLAZED GRATINGS, COUDÉ SPECTROGRAPHS AND MICRODENSITOMETERS

Important instrumental developments in the 1920s and 1930s permitted major advances in the ability to record the spectra of bright stars in great detail, with resolving powers of several times 10^4 and reciprocal dispersions in the range of about 2 to 8 Å/mm. The key developments were the construction of large stable coudé spectrographs, especially those with large format blazed-plane diffraction gratings and Schmidt camera optics (Schmidt, 1932), and the technology of producing a graphical strip chart record of a photographic spectrum, especially one with the non-linear photographic response taken into account so as to produce a record calibrated in intensity.

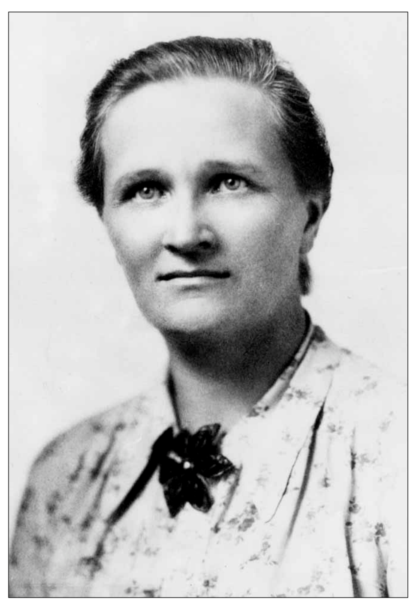

Figure 8: Cecilia Payne (later -Gaposchkin), 1900–1979 (AIP Emilio Segre Visual Archives, *Physics Today* Collection).

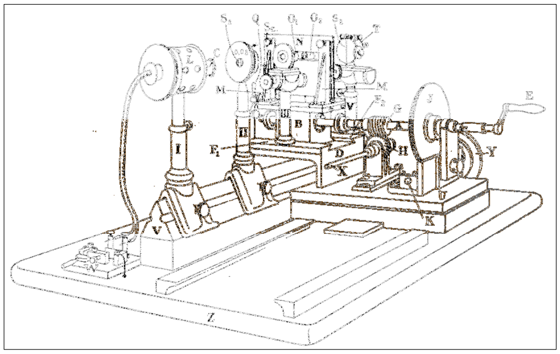

Figure 9: The first registering microdensitometer by W.J.H. Moll (after Moll, 1920).

The first coudé spectrograph was developed by Maurice Loewy (1833–1907) in 1907 at the Paris Observatory (see Hamy, 1924). But it was Walter Adams (1911; 1941) at Mt Wilson who built spectrographs with high resolving power and dispersion, first for the 60-inch telescope and later for the 100-inch. Together with Theodore Dunham, Jr (1897–1984) (1934; 1956), the key developments he introduced were the use of Wood blazed gratings instead of prisms and of Schmidt cameras instead of refracting systems that suffered from chromatic aberration.

The recording microdensitometer, developed by W.J.H. Moll (1876–1947) in 1920 at Utrecht (Figure 9), became the essential tool for producing graphical tracings from the photographic plates. A machine of this type was used by Marcel Minnaert (Figure 10) et al. to produce the *Utrecht Photometric Atlas of the Solar Spectrum* (1940), and many other instruments based on a similar principle were subsequently developed elsewhere, including by Robley C. Williams (1908–1995) and W. Albert Hiltner (1914–1991) (Williams and Hiltner, 1940) at Michigan, where they

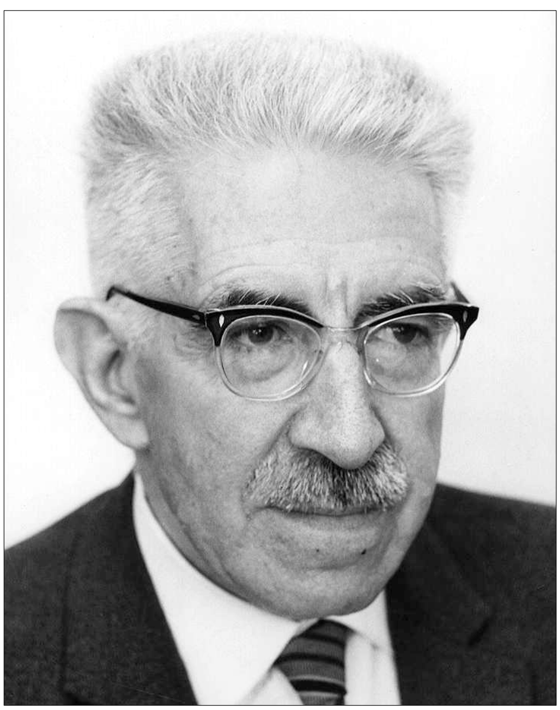

Figure 10: Marcel Minnaert, 1893–1970 (University of Utrecht).

produced a *Photometric Atlas of Stellar Spectra* (Hiltner and Williams, 1946). Such graphical tracings allowed the direct measurement of spectral lines so as to obtain their strengths or equivalent widths, instead of mere visual estimates of their strength as recorded hitherto by Rowland and others.

12 UNSÖLD AND RUSSELL ON THE COMPOSITION OF THE SUN

By the late 1920s the way was open for a more detailed analysis of the solar spectrum, based on the Schuster-Schwarzschild reversing layer model, Russell's calibration of the Rowland scale and the comparison of lines within a multiplet using quantum theory. In 1928, Albrecht Unsöld (1905–1995; Figure 11) in Kiel (Germany) made a new analysis of a solar atmosphere in radiative equilibrium and showed that the electron pressure, P_e, was about 10^{-6} atmospheres; he also deduced the relative reversing layer abundances of the elements Na, Al, Ca, Sr and Ba (Ünsold, 1928).

Also in 1928, Charles E. St John (1857–1935) and colleagues (St John, et al., 1928) at Mt Wilson produced a revision of Rowland's great catalogue, but still with the line strengths recorded on Rowland's arbitrary scale. This was, however, the basis for Russell's new solar spectrum analysis in 1929. He derived the relative abundances for 56 elements and 6 diatomic molecules. The very high abundance of hydrogen, first suggested by Payne, was now confirmed. Russell also discovered a peak in the abundance of iron in a plot of logarithmic abundance against atomic number. The iron peak had log $N_H/N_{Fe} = 4.3$. The irony is that Russell had discouraged Payne from publishing the high hydrogen abundance that she had found for stars in her doctoral thesis (see DeVorkin, 2010), and he suggested that she state that her result for hydrogen was "… almost certainly not real …"; now, just four years later, he proposed the same finding, and concluded that his results for the Sun were in reasonable accord with Payne's for giant stars.

13 SCHÜTZ AND MINNAERT AND THE CURVE OF GROWTH

In the 1920s, it was possible to compare the relative numbers of absorbers causing different lines in the same multiplet, but at this stage there was no way of comparing lines in different multiplets nor was there any knowledge of oscillator strengths, that would permit line opacities to be deduced. The next key step in the theory was the construction of the curve of growth, which linked the logarithm of line strengths (or equivalent widths) to the logarithm of the number density of absorbing atoms in a gas, such as in the reversing layer. This step was first taken theoretically by Wilhelm Schütz (1900–1972) in Munich (1930), and then followed up by Marcel Minnaert and his students in Utrecht, when they constructed an empirical curve of growth for the Sun based on equivalent width measurements instead of the old Rowland line intensity scale (Minnaert and Mulders, 1930). For lines in each multiplet, Minnaert found that a small segment of the curve was plotted; by shifting these segments horizontally, he in effect calibrated the relative strengths of the lines in different multiplets to produce a single curve of growth for the Sun.

The square root dependence of the strongest line strengths on abundance was demonstrated at this time, and the less sensitive dependence ($W \propto N^{0.31}$) of the intermediate strength saturated lines was also found. Unsöld, Otto Struve (1897–1963) and Christian T. Elvey (1899–1970), the latter two at Yerkes Observatory, applied the new theory to interstellar lines (Unsöld, et al, 1930), and Antonie Pannekoek (1873–1960) in Amsterdam applied the curve of growth to α Cyg, in order to study the damping constant for the strongest lines in this supergiant star (Pannekoek, 1931).

14 THE CONTINUOUS OPACITY PROBLEM AND THE ELUSIVE H^- ION DISCOVERED BY WILDT

The issue of the hydrogen-content of stars was crucial to understanding the source of continuous opacity in stellar atmospheres, which in turn was necessary to construct solar and stellar models and to interpret line strengths in the spectra of the Sun and stars. In 1934 Unsöld, believing the main solar opacity came from the photo-ionization of metals, rejected the H^--rich atmospheres found by Payne and Russell. He instead proposed a solar model in which the hydrogen-to-metals ratio was about 14:1. Unfortunately this gave a poor fit to the overall solar energy distribution, which was shown by G.F.W. Mulders (1936) to require an opacity source roughly constant with wavelength.

No opacity source was known in the mid-1930s which fulfilled these requirements, and this proved to be a major obstacle to any further progress in understanding the strengths of spectral lines. A breakthrough came in 1939, when Rupert Wildt (1905–1976) at Princeton proposed the H^- ion as the likely source of the missing opacity and that this ion was stable at solar temperatures. The existence of the ion with two bound electrons had been known for about a decade, but it had never hitherto been proposed as a source of opacity in the Sun and other late-type stars. With an ionization energy of just 0.74 eV and only one stable bound state, this was an ideal species for absorbing photons over a wide range in wavelength with $\lambda < 1.6$ μm, provided the high hydrogen abundance proposed by Payne and Russell was correct. Beyond that wavelength limit in the infrared, the H^- ion was still useful through free-free absorptions. S. Chandrasekhar (1910–1995) in 1944 calculated the photo-ionization cross-section of H^- as a function of wavelength, and the results gave a good fit to the missing opacity source for the solar photosphere. Moreover, they also explained the small Balmer discontinuity for solar-type stars, as H^- dominated over neutral atomic H opacity.

The H^- ion caused a complete revolution in our understanding of the spectra of late-type stars (types F5 to K). Unsöld's influential book, *Physik der Sternatmosphären*, which first appeared in 1938 just before the discovery of the H^- ion, had to be completely rewritten for its second edition of 1955 to take both H^- and the high hydrogen abundance into account.

15 THE FIRST MODEL ATMOSPHERES BY McCREA AND STRÖMGREN

In 1931 the problem of the continuous opacity in the solar photosphere was highlighted by William McCrea

Figure 11: Albrecht Unsöld, 1905–1995.

(1904–1999) when he was the first to calculate non-grey models for stellar atmospheres based upon atomic hydrogen photo-ionization and also free electron scattering as the sole opacity sources (Figure 12). The highly wavelength-dependent opacity of hydrogen atoms gave an extremely poor fit for the Sun's energy distribution, but the theoretical results were reasonable fits for the observed energy distributions of A- and B-type stars. In fact, grey atmospheres at 6000 K gave a much better agreement for the Sun than any of McCrea's models based on atomic hydrogen opacity.

This situation changed immediately after Wildt's discovery of the H^- opacity. In 1940 Bengt Strömgren (1908–1987) calculated a solar model atmosphere based on the H^- opacity (Strömgren, 1940). His analysis

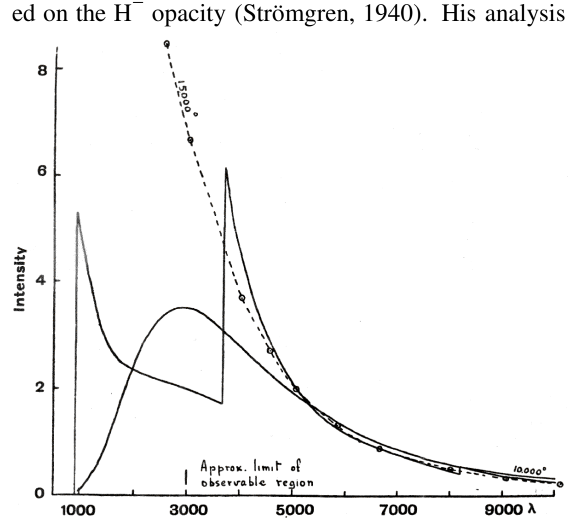

Figure 12: The first model atmosphere fluxes calculated by W.H. McCrea. The results for a pure hydrogen atmosphere at 10000 K are compared with black-body curves at 10000 K and 15000 K (after McCrea, 1931).

of the solar spectrum used T_{eff} = 5740 K, log g = 4.44 (cgs units) and a hydrogen-to-metals ratio ranging from 1000 to 16,000. Strömgren showed that the equivalent width of a weak line was proportional to the ratio of the line opacity to the continuous opacity, and hence to the ratio of the number density of metallic absorbers to hydrogen (given that the continuous opacity was dominated by hydrogen in one form or another). For the first time absolute solar curves of growth could be calculated, which allowed the conversion of equivalent widths into metal-to-hydrogen ratios.

In 1944 Strömgren 'computed' a grid of models for A5 to G0 stars using H and H^- for the opacity. These gave the first reliable flux gradients and Balmer jumps for the Sun and also for the later A- and F-type stars.

16 UNDERSTANDING THE VOIGT PROFILE AND MICROTURBULENCE

The Voigt profile for the line opacity function had been introduced by the German physicist Woldemar Voigt (1850–1919) as a convolution of Doppler and Lorentz profiles (Voigt, 1912). The Doppler component came from the thermal distribution of particle velocities in a gas, while the Lorentz component was shown to be the opacity function for a damped harmonic wave train as would be found by radiative damping. The Voigt function was therefore a hybrid profile for the line opacity that could be applied in the case of line absorptions in stellar atmospheres. It was this profile that enabled Schütz (1930) to calculate a theoretical curve of growth for spectral lines.

The Voigt profile for the line opacity function was applied by Struve and Elvey (1934) at Yerkes, when they analysed the spectra of six stars, including the supergiants 17 Lep, ε Aur and α Per. The line strengths depended on the Doppler velocity, but they found that much higher Doppler velocities for the supergiants were needed than predicted by thermal Doppler broadening alone. Thus they introduced the concept of turbulence to provide an additional Doppler broadening on a scale that was small compared to the photon mean free path (since described as microturbulence). High microturbulent velocities were found for the supergiants (e.g. 20 km/s for ε Aur). This has become a standard technique in stellar spectral analysis since that time; whatever the exact cause of microturbulence, it is generally required to explain the Doppler broadening of all stars, even of dwarfs, where microturbulent velocities of around 1 km/s are commonplace.

17 THE UTRECHT SOLAR ATLAS AND SOLAR CURVES OF GROWTH BY ALLEN AND WRIGHT

In 1940 Marcel Minnaert and his colleagues G.F.W. Mulders and Jacob Houtgast in Utrecht completed their major work, the *Photometric Atlas of the Solar Spectrum, from λ3612 to λ8771* … (Minnaert et al., 1940). The photographic plates had been recorded at Mt Wilson in 1936 by Mulders, with a resolving power of 1.5×10^5, and they were traced on the Moll microdensitometer at Utrecht, which Houtgast had modified to give direct intensity recordings. The atlas had a huge influence on solar and stellar high resolution spectroscopy after World War II. The second revision of Rowland's tables was produced by Moore, Minnaert and Houtgast in 1965. This volume for the first time recorded the equivalent widths of some 24,000 solar lines, 73% of them with identifications.

Soon after the publication of the Utrecht atlas, Kenneth Wright (1911–2002) at the Dominion Astrophysical Observatory used the atlas to measure the equivalent widths of about 700 lines in the solar spectrum (Wright, 1944). He constructed one of the first solar curves of growth for Fe I and Ti I, using oscillator strengths from the measurements of Robert B. King (1908–1995) and Arthur S. King (1876–1956) (1935; 1938). It was one of the best curves of growth produced at that time, and enabled Wright to measure an excitation temperature of $T_{ex}(\odot) = 4900 \pm 125$ K and a solar microturbulent velocity of $\xi = 0.9$ km/s from Fe I lines.

Wright's construction of the solar curve of growth superseded an earlier curve by Clabon W. Allen (1904–1987) in 1934. Allen had measured solar centre-of-disk equivalent widths from the solar spectrograph at Mt Stromlo in Australia and he constructed parts of the curve of growth for several elements, using the theoretical relative intensities of lines within a multiplet (Allen, 1934).

18 FOUR BASIC PREREQUISITES FOR STELLAR ABUNDANCE ANALYSIS

By the late 1940s, four basic requirements for good quantitative abundance analyses in the Sun and stars were now evident. First, good quality effective temperatures for stellar photospheres were essential. The great sensitivity of line strengths to a star's effective temperature emphasized the importance of a reliable temperature as a precursor to any abundance analysis. Thus a random error of ±100 K in a star's estimated effective temperature can produce an abundance error in (Fe/H) of about 50% by number.

The ability to measure good quality equivalent widths from calibrated intensity recordings generated from photographic spectrograms was the next essential requirement. Not only was the Utrecht *Photometric Atlas* highly influential, but also the atlas produced by Hiltner and Williams (1946) at Michigan for eight bright stars was another important step forward.

Thirdly, line lists for standard stars were very important so that as many lines as possible could be reliably identified. Several major spectral catalogues were produced. The *Second Revision of Rowland's Preliminary Table* by Moore, Minnaert and Houtgast (1965) provided the necessary data for the Sun. But other observers catalogued the lines in the spectra of stars of other spectral types. These included Dunham (1929) for the F5 supergiant, α Per; Struve and Dunham (1933) for the B0 dwarf, τ Sco; W.W. Morgan (1906–1994) for 13 A-type stars (1935); Sidney G. Hacker (1908–1983) for the K2 giant, Arcturus with 3883 lines (1935); John W. Swensson for Procyon (F5IV) with a catalogue of 3600 lines (1946); and Dorothy Davis (1913–1999) for Antares (M1Ib) (1939) and for β Peg (M2II-III) (Davis, 1947), the latter star having 10,000 lines in the catalogue.

A compendium that summarized much of the labortory data on the spectral lines of the different elements was Charlotte Moore's (1933) *Multiplet Table of Astrophysical Interest*, with editions in 1935 and, after revision, in 1945. This became a standard reference for line identifications when analysing new stellar spectra (see Rubin, 2010).

Finally, an urgent need for oscillator strengths was evident from the late 1940s, once the essential techniques for quantitative element abundance determinations had been worked out by many people over the preceding two decades. More than anything, the glaring lack of reliable oscillator strengths was a major obstacle to rapid progress. Robert and Arthur King (1935) at Mt Wilson began an extensive programme of oscillator strength determinations using an electric furnace. But an even larger programme was initiated by William Meggers (1888–1966) et al. (1961) at the US National Bureau of Standards in the 1930s. After some 25 years about 39,000 lines of 70 elements had been measured. Charles Corliss (1919–2002) and William R. Bozman calibrated some 25,000 of these line measurements so as to obtain absolute oscillator strengths (Corliss and Bozman, 1962). These are just a few of the many programmes directed at improving the data on oscillator strengths in the literature. For ionized lines, the work of Stanley Bashkin (1923–2007) and Aden B. Meinel (b. 1922) using the technique of beam foil spectroscopy was also notable (Bashkin and Meinel, 1964).

19 A PIONEERING CURVE OF GROWTH ANALYSIS OF R CrB BY LOUIS BERMAN IN 1935

A pioneering curve of growth analysis of a hot carbon star, R CrB (spectral type cF7p), by Louis Berman (1903–1997) in 1935 is especially remarkable, both because it was an especially early exposition of the curve of growth technique for a star other than the Sun, and also because Berman analysed a very peculiar star with highly non-solar abundances.

Berman's data mainly came from Lick prismatic spectra, and he obtained equivalent widths for over 600 lines after producing microdensitometer tracings. He obtained an excitation temperature of 5300 K from the Fe I curve of growth, and then used this to deduce abundances of 24 elements, expressed as column densities in the reversing layer. His results showed that the star contains 69% carbon and 27% hydrogen (by the number of atoms, for those elements studied).

20 FOUR PIONEERS OF STELLAR ABUNDANCE ANALYSIS IN THE 1940s

By the 1940s the long process of preparing the ground for abundance analyses of stellar photospheres had been completed and the way was now open for all the theory, laboratory data and instrumentation to come together for the first analyses that could give a full picture of the chemical composition of stars. The four astronomers who made the early progress were Albrecht Unsöld, Jesse Greenstein (1909–2002), Lawrence W. Aller (1913–2003) and Kenneth O. Wright.

Unsöld developed the coarse analysis method analysis or 'Grobanalyse' in which the structure of the photosphere was ignored and replaced by mean values of temperature and pressure. He analysed just one star, the B0 dwarf τ Sco (Unsöld, 1942). He used spectra from the newly-commissioned coudé spectrograph on the McDonald Observatory 82-inch telescope.

Although this was the only star Unsöld analysed, he was the pioneer in understanding the theory of line formation in stellar atmospheres, and hence his work was tremendously influential and set the standard for others to follow. The τ Sco analysis was the first curve of growth analysis of any star except for Berman's work on R CrB and Allen's work on the Sun. Abundances for nine light elements (H, He, C, N, O, Ne, Mg, Al, Si) were obtained and these were found to be solar, in agreement with the results of Russell (1929) and Strömgren (1940) for the Sun. As Strömgren had explicitly included the H^- in the Sun's continuous opacity, this agreement with a solar composition for τ Sco indirectly supported the role of H^- for the main source of solar opacity, as Unsöld explicitly noted.

At Caltech, Jesse Greenstein developed the idea of the differential abundance analysis. This technique obtained the composition of a star relative to a standard star, and in Greenstein's (1942) case, the analysis was of Canopus (F0Ib) relative to the Sun (Figure 13). The differential technique meant that oscillator strengths for each line were not required, and this therefore avoided one of the major uncertainties that plagued absolute abundance work for any star.

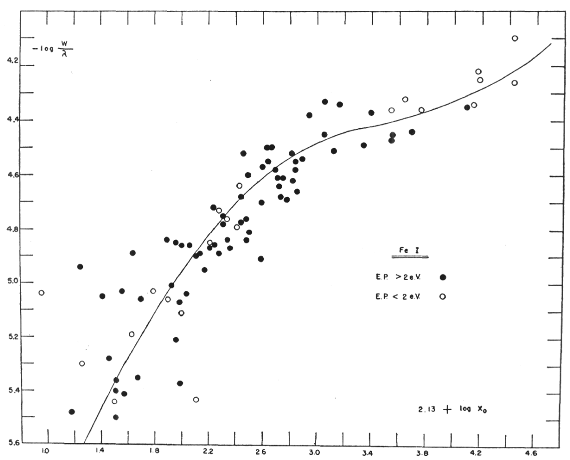

Figure 13: Greenstein's differential Fe I curve of growth for Canopus (after Greenstein, 1942: 186).

Although Greenstein's paper on Canopus did not take the analysis all the way to deducing differential element abundances, but only the degree of ionization, in 1948 he presented the results of the differential analysis of four further F-type stars (ρ Pup, θ UMa, Procyon and α Per), and also of the metallic-line star, τ UMa. Now the differential technique was developed to the logical conclusion of differential abundances (expressed as logarithmic values relative to the Sun (Greenstein, 1948).

Lawrence Aller was also developing spectral analysis techniques in the 1940s, and his main field of study was the A-type stars. His first paper in 1942 was on Sirius and γ Gem (Aller, 1942). This was a differential coarse analysis, similar to Greenstein's work on Canopus. He also analysed the O dwarf, 10 Lac (Aller, 1946) following the method of Unsöld for τ Sco. But his third paper, on γ Peg, introduced the method of fine analysis for a star, in which the temperature structure of the photosphere is taken into account (Aller, 1949). He used models whose temperature structure had been derived for grey opacities, following the procedure developed by Anne B. Underhill (1920–2003) in 1948. The absolute abundances of ten light elements (including C, N and O) were derived relative to hydrogen. The most abundant element of these three was oxygen, for which the fine analysis gave log (O/H) = –3.94.

The other notable spectral analysis pioneer of the 1940s was Kenneth Wright at the Dominion Astrophysical Observatory in Victoria, Canada. He analysed four stars: the F-type supergiants γ Cyg and α Per, the F-type dwarf Procyon, and the Sun. The observations came from the Cassegrain spectrograph on the DAO 72-inch reflector. The work was submitted for a Ph.D. thesis in 1940, but the full results were not published until 1947, being held up by the war. The initial work was an absolute coarse analysis based on the Schuster-Schwarzschild model. In 1946-1947, Wright (1948) reanalysed his data as differential analyses of three stars relative to the Sun. He derived logarithmic element-to-iron ratios relative to the Sun, but did not include hydrogen in the 21 elements surveyed.

Table 2: Some examples of stars with element abundance peculiarities from the mid-twentieth century.

^{12}C to ^{13}C ratio in evolved stars	McKellar (1947)
Discovery of weak lined stars	Roman (1950)
Analyses of halo stars with heavy element deficiencies	Schwarzschild & Schwarzschild (1950); Chamberlain and Aller (1951)
Technetium in red giant stars	Merrill (1952)
Rare earth abundances in Ap stars	Burbidge & Burbidge (1955)
Survey of lithium in stars	Bonsack (1959)
Abundance peculiarities in Am stars (high Fe-peak elements, low Ca, Sc)	van't Veer-Menneret (1963); Conti (1965)
^{3}He to ^{4}He ratio in 3 Cen A	Sargent and Jugaku (1961)
Holmium-rich star, HD101065	Przybylski (1963)
Barium stars	Warner (1965)
^{12}C to ^{13}C ratio in evolved stars	McKellar (1947)
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^{3}He to ^{4}He ratio in 3 Cen A	Sargent and Jugaku (1961)
Holmium-rich star, HD101065	Przybylski (1963)
Barium stars	Warner (1965)

Otto Struve (1950) summed up this very productive period in the 1940s when the foundations for spectral analysis were laid, roughly a century after Comte:

> Perhaps the most striking result … is the remarkable degree of uniformity that has been observed in the most widely different astronomical sources. The sun, the main sequence stars of type F and the He stars like tau Scorpii and even the O-type stars 10 Lacertae have all approximately the same composition … The first conclusion … is the establishment of a list of what we might call the normal abundances of the universe.

This statement is certainly reminiscent of that of Huggins (1909) when he described the early qualitative spectral analysis of the 1860s (see Section 5). In the intervening eight or nine decades, enormous advances in theoretical physics had been accomplished, as well as in instrumentation and laboratory physics. This had transformed spectral analysis from a descriptive science based on direct visual observation to a quantitative science based on photographic spectral recording and supported by the detailed physics of atomic quantum theory and of radiative transfer.

21 THE COSMIC ABUNDANCE DISTRIBUTION, BUT SOME STARS HAVE PECULIAR ABUNDANCES

The idea of a cosmic or normal abundance distribution of the elements to which Struve (1950) referred became the most notable achievement of stellar spectral analysis up until about 1950. But no sooner did a general principle appear to be emerging, than new data came to make the result seem less secure. Today we recognize many stars with unusual element abundances that do not follow the idea of a cosmic abundance distribution. Some of the most notable of these peculiar stars were found in the years of the mid-twentieth century. Table 2 lists some of the abundance peculiarities which were mainly discovered in the 1950s and 1960s.

22 BACK TO COMTE: SO WHY DID HE GET IT WRONG?

It is clear that Auguste Comte made a significant blunder in 1835, when he selected the chemical composition of stars as an example of knowledge we would never have. However, upon reflection, it seems like an excusable error. Evidently he believed the stars to be so far away (no observed parallax was published until 1838) that there appeared to be no possibility of ever going to the stars to carry out a chemical analysis or to measure their temperatures. Moreover, he did not conceive of making measurements at a distance using the message in the light.

Certainly Comte was not an astronomer, and perhaps he was just unlucky to pick an astronomical example to illustrate his argument in the *Philosophie Positive*, which was that "… every theory must be based upon observed facts … [and] facts cannot be observed without the guidance of some theories." So he was a pioneer of the scientific method based on empirical evidence.

Perhaps Comte's one small transgression was that evidently he was unaware of the papers of John Herschel, William Henry Fox Talbot and others on the spectral analysis of flames, and also of Fraunhofer on the spectra of the Sun and stars. In particular, Fraunhofer's papers contained the first small clues that just possibly Comte's assertion might be wrong.

23 NOTE

1. Although the D_3 line of helium at 5876 Å was observed by both Janssen (1869) and Lockyer

(1869) in the solar chromospheric spectrum, neither astronomer was at first convinced that this came from a new element. Lockyer suspected this, but did not publish this nor use the word 'helium' until after Ramsay's laboratory discovery of the gas in 1895. However, Lockyer and his colleague Frankland coined the word for their private use, as was confirmed later by Lockyer (1897). Lord Kelvin reported that Frankland and Lockyer had already proposed the name helium as early as 1871 when he wrote:

> Frankland and Lockyer find the yellow prominences to give a very decided bright line not far from D, but hitherto not identified with any terrestrial flame. It seems to indicate a new substance, which they propose to call Helium. (Thomson 1872).

A possible scenario is that Lockyer communicated this information to Lord Kelvin verbally, so it appeared in Kelvin's British Association Report for 1871. But they were reluctant to use the word in print until Ramsay had confirmed the D_3 line came from a new gas and popularized its name in 1895.

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SPECTROSCOPY—SO WHAT?

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Abstract: The development of astronomical spectroscopy allowed amazing achievements in investigating the composition and motion of celestial bodies. But even beyond specific measurements and results, the fruitfulness and practice of spectroscopy had important ramifications on a more abstract level. This paper will discuss ways in which spectroscopy inspired or boosted new theories of the atom, life, and the Universe; redrew the boundaries among scientific disciplines; demonstrated the unity of terrestrial and celestial physical laws; changed what counted as scientific knowledge; and even revealed divine mysteries. Scientists and science writers from the first half-century of astronomical spectroscopy will be discussed, including James Clerk Maxwell, William Crookes, John Tyndall, Agnes Clerke, William Huggins and Norman Lockyer.

Keywords: spectroscopy, history of astronomy

1 INTRODUCTION

Science is not driven by facts alone. When a new method such as astronomical spectroscopy is developed, it is tempting to look back and celebrate the new facts generated by the technique. But science is also driven by larger concerns and issues, and most major breakthroughs contribute to these larger concerns as much as they unravel specific technical puzzles. Thus we might ask: 'Spectroscopy? So what?' Why did scientists care about this new technique? How did it change the way they worked, and the way they thought about themselves and the Universe?

Thinking along these lines, the Victorians were stunned by Kirchhoff and Bunsen's achievement. There was widespread astonishment at what could be done with the spectroscope (Schuster, 1881). One commentator described the psychic impact of the discovery:

> In no science, perhaps, does the sober statement of the results which have been achieved appeal so strongly to the imagination and make so evident the almost boundless powers of the mind of man … [Spectroscopy] is worthy to be regarded as the scientific epic of the century. (Watts, 1904: v-vi).

The ability to peer inside incredibly distant bodies was something of a shock. What had been restricted to the laboratory and the workshop now extended across the Universe "… into almost unlimited space …" (Roscoe, 1873: 2). As Norman Lockyer (1873: 107) put it, "… we can take the very Sun itself to pieces." (cited by Schaffer, 1995: 283). These remarkable explorations came to be known as the 'New Astronomy'.

Part of the excitement about these techniques was that they seemed to do what had been forbidden explicitly by Auguste Comte, the widely-influential philosopher of science (see Hearnshaw, 2010) who in the 1830s notoriously declared that the composition of a star would be forever unattainable by science and indeed was the perfect example of unscientific knowledge (Comte, 2004: 130). But after 1859, it seemed that astronomers had dramatically shattered those philosophical boundaries:

> Before the discoveries of Bunsen and Kirchhoff no philosopher had ever ventured to think it possible that we should be able to analyse the sun and stars. (Watts, 1904: v).

Distance no longer seemed to matter, truly opening up the entire Universe to scientific investigation:

> The interest which the new discovery created in scientific and unscientific circles was due to the apparent victory over space which it implied. No matter whether a body was placed in our laboratory or a thousand miles away—at the distance of the sun or of the farthest star. (Schuster, 1881: 468).

This remarkable expansion of scientific possibility allowed spectroscopy to speak to a number of the most important debates of the century. These could easily fill a book, but here I will address three broad topics: the unity of natural laws and matter; the existence and structure of atoms; and the meaning of the Universe.

2 THE UNITY OF LAWS AND MATTER

A concern going back centuries regarded the question of whether the laws of nature that we can observe and experiment with here on Earth apply to the Universe as a whole. While the universality of laws is widely accepted in the twenty-first century, it was not always obvious that this is the case—the Aristotelian Universe rejected it completely. This principle of the unity of natural laws formed an important part of Newton's 'rules of reasoning' and was one of the major methodological contributions of his work (see Cohen, 1995: 116-118).

Unity became axiomatic for scientists, but it was difficult to know whether it was actually true. By the middle of the nineteenth century, the only law that astronomers were assured reached beyond our Solar System was that of Newtonian gravity—thanks to William Herschel's careful observations of double stars (Huggins and Miller, 1864). Spectrum analysis dramatically changed this by extending the laws of light, heat, and chemistry everywhere a telescope could be pointed (ibid.). Recognizable spectral lines linked our laboratories with the rest of the Universe (e.g. see Pasachoff and Suer, 2010).

Agnes Clerke (Figure 1), one of the great science writers of the nineteenth century, noted that this seemed to complete 'the unification of the cosmos' started by Newton:

> It means the establishment of a science of Nature whose conclusions are not only presumed by analogy, but are ascertained by observation, to be valid wherever light can travel and gravity is obeyed—a science by which

Figure 1: Agnes Mary Clerke, 1842–1907 (after Macpherson, 1905).

> the nature of the stars can be studied upon the earth, and the nature of the earth can be made better known by study of the stars—a science, in a word, which is, or aims at being, one and universal, even as Nature—the visible reflection of the invisible highest Unity—is one and universal. (Clerke, 1902: 141).

Newton's supposition, that had been so fruitful, finally had empirical evidence from the stars' spectral lines. This unity, or uniformity, of natural laws made it reasonable to talk about the temperature and constitution of objects that no human could handle or manipulate. Lockyer described the same achievement as Clerke, but somewhat more tersely, as "… nature, in the regions we cannot get at, works in the same way as she does in the regions which we can get at." (Lockyer, 1887: 265).

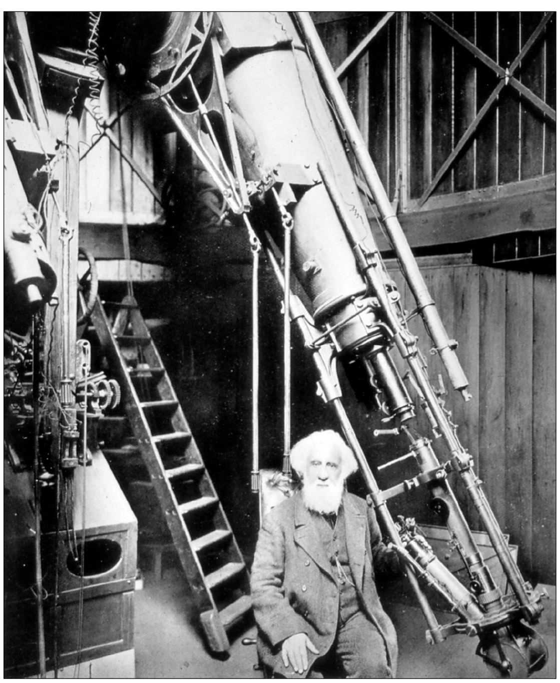

Figure 2: William Huggins, 1824–1910 (after Huggins and Huggins, 1909).

One of the consequences of the universal extension of natural laws was that different departments of knowledge suddenly overlapped. Without unity, the experiments of the chemist had no relevance for the calculations of the astronomer, and *vice versa*. Spectral analysis changed this, and what had seemed to be local, particular skills were now of cosmological significance. This is one of the reasons that many of the breakthroughs of early spectroscopy were conducted by physicist-chemist teams (such as Kirchhoff-Bunsen and Miller-Huggins). Again, Agnes Clerke described this transition in beautiful prose:

> … astronomy, while maintaining her strict union with mathematics, looked with indifference on the rest of the sciences; it was enough that she possessed the telescope and the calculus. Now the materials for her inductions are supplied by the chemist, the electrician, the inquirer into the most recondite mysteries of light and the molecular constitution of matter. She is concerned with what the geologist, the meteorologist, even the biologist, has to say; she can afford to close her ears to no new truth of the physical order. Her position of lofty isolation has been exchanged for one of community and mutual aid. The astronomer has become, in the highest sense of the term, a physicist; while the physicist is bound to be something of an astronomer. (Clerke, 1902: 142).

Clerke sung the philosophical praises of this new community of science, but in practice it was far from simple to suddenly move chemistry into the observatory. The era of astronomical practice as tranquil hours in the dark behind the eyepiece was over, replaced by an assault on the peace of all the senses. William Huggins (Figure 2) painted the picture vividly:

> Then it was that an astronomical observatory began, for the first time, to take on the appearance of a laboratory. Primary batteries, giving forth noxious gases, were arranged outside of one of the windows; a large induction coil stood mounted on a stand on wheels … together with a battery of several Leyden jars; shelves with Bunsen burners, vacuum tubes and bottles of chemicals, especially of specimens of pure metals, lined its walls. (Huggins, 1897: 8).

This was not the only collaboration between astronomers and chemists, as astronomical photography became widespread around the same time as the introduction of spectroscopy. The breakthroughs evolved together, showing how the techniques of one field could help advance another.

This need to draw on multiple fields certainly did not end rivalry among disciplines, however. William Crookes (Figure 3) cautioned that while interdisciplinary ventures were fine, one must still be wary:

> Inferences drawn from spectrum analysis per se are liable to grave doubt, unless at every step the spectroscopist goes hand in hand with the chemist. Spectroscopy may give valuable indications, but chemistry must after all be the court of final appeal. (D'Albe, 1924: 312).

The spectroscope may have brought uniformity to the laws of nature, but perhaps not so much to the competition among scientists.

The claim that all the Universe was governed by the same natural laws had a close cousin, that all the Universe was made of the same substances—the unity of matter. We are so accustomed to this idea now that it is hard to remember what a dramatic leap it was to claim that everything in existence was just like our little planet. But spectroscopy seemed to show that this was indeed the case. Lockyer noted the strangeness of gazing into distant reaches, only to find the familiar: "Where some, at all events, might have anticipated a new world of matter, we find likeness to the old." (Lockyer, 1887: 58).

The fact that spectral lines from distant stars could be matched up with material in terrestrial laboratories, down to mind-boggling levels of precision, seemed to dictate a complete uniformity of matter everywhere. James Clerk Maxwell (Figure 4) declared that the spectroscope had found hydrogen 'exactly identical' to our own far from Earth (Maxwell, 1890: 374), and Peter Guthrie Tait (Figure 5) stressed that these distant materials had all the same properties as terrestrial substances—there was no Arcturan carbon:

> … every atom of any one substance, wheresoever we find it, whether on the earth or in the sun, or in meteorites coming to us from cosmical spaces, or in the farthest stars or nebulae, possesses precisely the same physical properties. (Tait, 1885: 295).

To Maxwell, Lockyer, and most spectroscopists this was intuitively obvious upon seeing the spectral lines. The lines could not simply be a coincidence. A minority remained skeptical—could not other substances create similar looking spectral lines? Arthur Schuster (Figure 6) defended this position while expressing sympathy for the desire for unity:

> Most of us are convinced in our innermost hearts that matter is ultimately of one kind, whatever ideas we may have formed as to the nature of the primordial substance. That opinion is not under discussion. The question is not whether we believe in the unity of matter, but whether a direct proof of it can be derived from the spectroscopic evidence of stars. (Schuster, 1897: 212).

Proof or no, Maxwell, not Schuster, spoke for the majority. There seemed to be little room for doubt that the hydrogen in our drinking water was identical to that in the stars.

The uniformity of matter brought with it important consequences for cosmological theories, particularly the nebular theory. On this view, the Sun, Earth, and planets condensed from a single, self-gravitating primordial cloud. This suggested that the Sun and the Earth should be made of the same materials. This was a straightforward claim, but one impossible to test before the development of spectroscopy. The success of this prediction struck a strong blow for the nebular theory, and the Royal Institution's John Tyndall announced that "… in our day the [nebular] hypothesis of Kant and Laplace receives the independent countenance of spectrum analysis, which proves the same substances to be common to the earth and sun." (Tyndall, 1872: 32). Even more specifically, the structure of the dark lines suggested that the interior of the Sun was hotter than the exterior; again, a condition perfectly in line with Laplace's theory (Roscoe, 1873: 252).

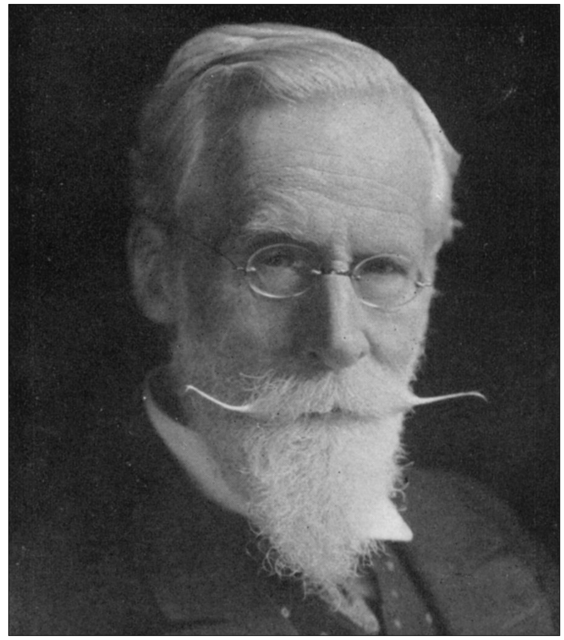

Figure 3: William Crookes, 1832–1919 (after D'Albe, 1924).

These realizations, combined with Huggins' observation that some nebulae were in fact completely gaseous, made plausible the leap that those clouds were our ancestors:

> The data furnished by spectrum analysis, too, favour the supposition of a common origin for sun and planets by showing their community of substance; while gaseous nebulae present examples of vast masses of tenuous vapour, such as our system may plausibly be conjectured to have primitively sprung from. (Clerke, 1902: 313).

The different types of stellar spectra were then inferred to be different stages in the evolution of stars from nebulae. All the pieces seemed to be in place for the

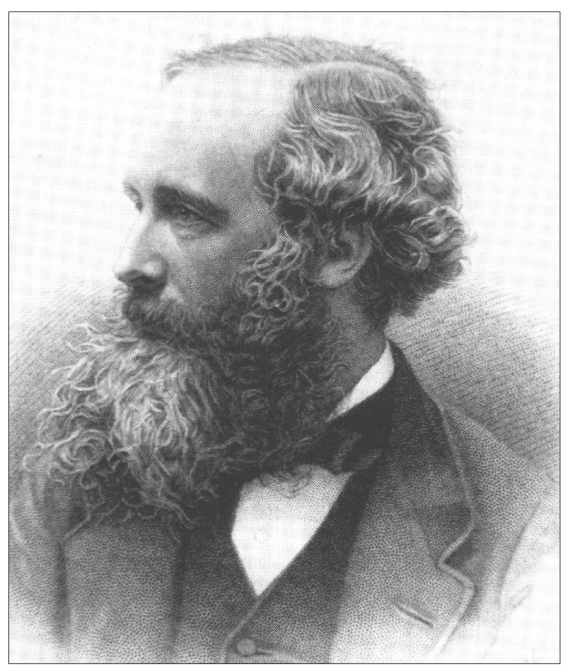

Figure 4: James Clerk Maxwell, 1831–1879 (after Campbell and Garnett, 1882).

nebular process—planets and stars sharing a common substrate, and a series of celestial bodies suggesting the collapse into a system. Huggins (1897: 107) was confident that there could no longer even be a question:

> There remained no room for doubt that the nebulae, which our telescopes reveal to us, are the early stages of long processions of cosmical events, which correspond broadly to those required by the nebular hypothesis in one or other of its forms.

One commentator assured his readers that the spectroscope's obvious support for the nebular hypothesis made it impossible to imagine any attack on the theory in the future (Clarke, 1873). Despite this, such attacks appeared quickly in the form of Lockyer's heterodox interpretation of astronomical spectra. Lockyer (1890) argued that the similarity among the spectra of stars, nebulae, comets, and terrestrial meteorites indicated that all those celestial bodies were nothing but clouds of small rocks, becoming bright through constant collisions. His theory was not widely accepted, but it did indicate the varying fortunes the nebular theory would have for decades to come. Regardless, its viability would rest for many years on the evidence of spectra.

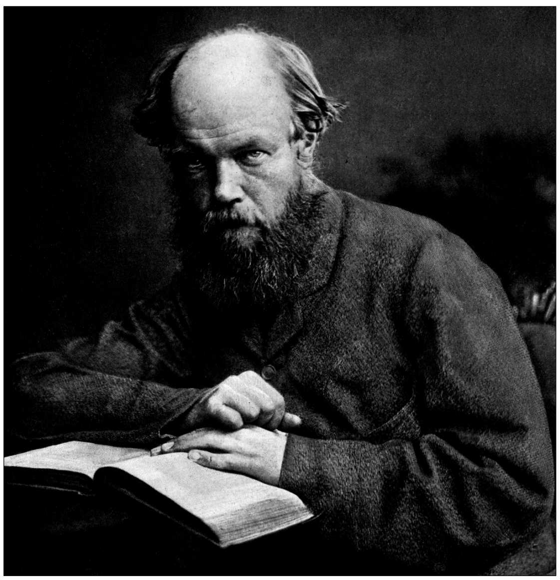

Figure 5: Peter Guthrie Tait, 1831–1901 (after Knott, 1911).

The uniformity of matter had further cosmic implications. Just as spectra linked the matter of our planet to distant stars, they seemed to also link us the same way. Lockyer (1900: 172) asked that since hydrogen, oxygen, nitrogen, etc., were "… common to the organic cell and the hottest stars … [so] is it possible that we have here a quite new bond between man and the stars?" Clerke expounded on the 'wonder' that our bodies were built from the dust of an ancient nebula:

> Custom can never blunt the wonder with which we must regard the achievement of compelling rays emanating from a source devoid of sensible magnitude through immeasurable distance, to reveal, by its distinctive qualities, the composition of that source … the application of prismatic analysis certified to the presence in the stars of familiar materials, no less of the earth we tread, than of the human bodies built up out of its dust and circumambient vapours. (Clerke, 1902: 372).

Not all the spectral revelations about life were positive, however. The high temperatures of the Sun indicated by its spectra finally destroyed William Herschel's proposal that it was inhabited by beings much like ourselves (Lockyer, 1887: 81).

3 ATOMIC THEORY

As much as spectroscopy spoke about the grandest scales of the Universe, it also revealed the smallest. It seemed to be the long-awaited window into the atom. Again, we are today so accustomed to thinking casually about atoms and their structure that it is important to recall how controversial such ideas were in the nineteenth century—it was not at all clear that atoms even existed. The spectroscope allowed a journey inward past the common appearances of the ordinary world into a deeper one: Crookes said it "… enables us to peer into the very heart of nature …" (Knight, 1967: 136), in particular, to see the atoms hidden beyond our vision.

For the first time scientists could experimentally investigate atomic phenomena, which otherwise drew criticism as matters only for speculation. One of the first implications of spectra was a perverse one—that atoms seemed to have an internal structure. This was opposed to the very concept of atoms (whose name literally meant that which cannot be cut) and many scientists at the time preferred to speak in a vague sense of 'molecules', meaning invisibly small but perhaps not indivisible particles.

The suggestion of substructure came from the multiplicity of sharp, discontinuous spectral lines associated with each element. Experimentalists concluded that those molecules must be compounds of some sort, with the different constituents each generating a different line (Schuster, 1881: 470-472). Maxwell pointed to the sharpness of the lines as the most important clue: "When the spectrum consists of a number of bright lines, the motion of the system must be compounded of a corresponding number of types of harmonic vibration." (Maxwell, 1890: 462). Numerous other investigators came to the same conclusion. If there were so many modes of vibration, there must be a number of different vibrators within the molecule (McGucken, 1969: 162-3).

This conclusion led to numerous attempts to calculate spectral patterns by mathematical manipulation of likely harmonic vibrations, notably by George Stoney (1871) and R.B. Clifton (1866). These attempts all failed completely, but strangely, physicists did not feel that was a problem. They did not need an exact description of the vibrations, just a broad assurance that it could be done in principle (Preston, 1880: 58). This is a common feature of 'so what' discoveries such as spectroscopy: much of their impact takes place in the realm of agenda-setting and imagination-firing, rather than solely contributing to discrete measurements.

Such implication of substructure for molecules was not particularly shocking, but the appearance of sharp lines even from materials that were apparently completely atomic (such as pure elements) proved unsettling. If atoms were, by definition, irreducible, how could they have a complex structure? Preston (1880: 56) wondered how "… to reconcile the proved indestructibility of the atom with its capacity for executing vibrations, as demonstrated by the spectroscope." Lockyer (1887: viii) argued that since we could observe spectral behavior in atoms that were associated with known compounds, perhaps it was time to change our notion of what an element was:

> … reasoning from the phenomena presented to us in the spectroscope when known compounds are decomposed, I had obtained strong evidence that the so-called elementary bodies are in reality compound ones.

Elementary atoms seemed not to be quite so elementary.

Lockyer (1887: 200-201) thought he could observe a process of elemental breakdown—which he called 'celestial dissociation'—in solar and stellar spectra. This was particularly visible, he said, in how spectra varied in different layers of the Sun, and he thought this hypothesis resolved various difficulties of interpreting spectra. He diligently quoted other physicists who were open to non-elementary elements (including Maxwell), and concluded that the formerly-inviolate atoms actually behaved "… like mixtures of organic compounds." (Lockyer, 1887: 301). This dissociation hypothesis became a major part of Lockyer's research agenda for the rest of his life, and much of his famous book *Chemistry of the Sun* (1887) is devoted to it.

Lockyer built on his dissociation hypothesis to form a vision of elemental evolution, where some primordial bits of matter change over time into the more complex elements we have on Earth. He explicitly drew on Darwin's ideas to justify his own, saying that chemical evolution "… derives its whole force from the fact that along many lines it runs parallel with the processes of development …" in the organic world (Lockyer, 1887: 262-263). He celebrated notions of evolution as "… the most profound revolution in modern thought which the world has seen." (Lockyer, 1900: 152).

Crookes followed a similar line of evolutionary reasoning, coining the term 'protyl' for the primordial material from which the elements were made. And, like Lockyer, he embraced terminology from organic evolution in an 1888 lecture:

> … elements owe their present stability in that they are the outcome of a struggle for existence, a Darwinian development by chemical evolution; that just as in the organic world we have "survival of the fittest," so here we have the "survival of the most stable" or possibly of the "most inert." (D'Albe, 1924: 324).

An alternative to non-elementary elements was the vortex atom, proposed by William Thomson (Lord Kelvin). These atoms were loops of ether that could vibrate in complicated ways, thus hopefully reproducing the spectral lines without discrete constituents (Preston, 1880; Silliman, 1963: 41). These vortices seemed to have the elasticity, complex behavior and indestructibility required of atoms, but again could not provide a quantitative explanation for the structure of spectra.

None of these atomic and evolutionary schemes came to any fruition, though they did form major research agendas for a number of late Victorian scientists, and no doubt made turn-of-the-century developments—such as the electron and radioactivity—much more sensible.

4 WHAT IT ALL MEANS

Finally, spectroscopy provided a launching pad for some grand philosophizing about the nature of things —why does the Universe exist? Why are we here? Lockyer waxed poetic about what spectra revealed about the relationship between man and nature:

> In this way, then, we have really been only continuing a train of thought, which has to do with Man's Place in Nature, in relation to the Sun's Place in Nature; and finding fresh grounds for thinking that the more different branches of science are studied and allowed to react on each other, the more the oneness of Nature impresses itself upon the mind. (Lockyer, 1900: 174).

This 'oneness' was a common theme of this category of reactions, no surprise given spectroscopy's implications for the unity of nature.

James Clerk Maxwell, in some articles for the *Encyclopedia Britannica* and in an address to the British Association for the Advancement of Science, drew attention to a different kind of oneness. He presented a complex argument beginning with the 'exactly identical' properties of molecular spectra from all over the Universe (Maxwell, 1890: 375). This identicality meant that molecules did not have the variation necessary for evolutionary processes to work, therefore

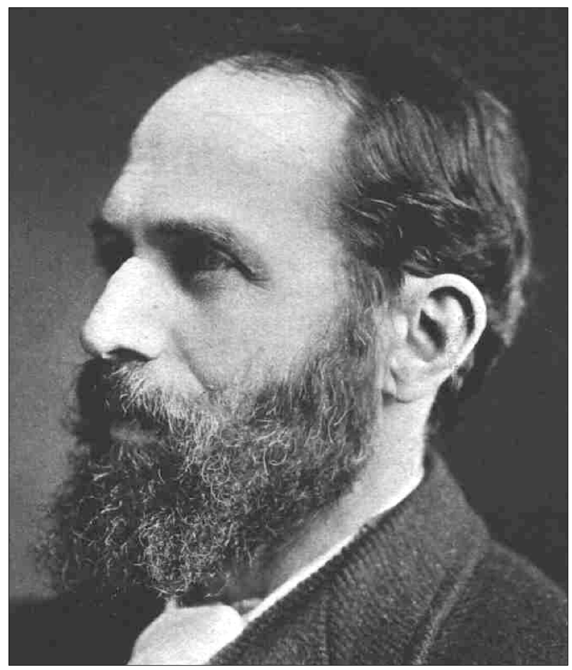

Figure 6: Arthur Schuster, 1851–1934 (courtesy: *Physical Laboratories*, 1906).

they cannot have changed over time due to natural processes. This meant that

> … we have strong reasons for believing that in a molecule … we have something which has existed either from eternity or at least from times anterior to the existing order of nature. (Maxwell, 1890: 482).

Celebrating the unity of matter through all the ages and all the reaches of the Universe, Maxwell expressed his wonder that every molecule of hydrogen remained the same despite the ravages of time and nature:

> They continue this day as they were created—perfect in number and measure and weight, and from the ineffaceable characters impressed on them we may learn that those aspirations after accuracy in measurement, truth in statement, and justice in action, which we reckon among our noblest attributes as men, are ours because they are essential constituents of the image of Him who in the

> beginning created, not only the heaven and the earth, but the materials of which heaven and earth consist. (Maxwell, 1890: 377).

Thus Maxwell drew a theological conclusion from the regularity of spectra: not only was divine manufacture required to make molecules, but their perfection was an echo of the attributes of the Creator himself. Such sentiments from a scientist were not at all unusual at the time. The vast majority of Victorian scientists were religious and were quite comfortable linking the discoveries of science to their faith.

Another theological tone was sounded in a crucial early paper on astronomical spectroscopy where Huggins and Miller (1864) reported on their stellar observations. They noted that there was an irregular distribution of elements in the sky—some stars have more magnesium, some more iron, etc. Similarly, on the Earth some elements are found in uneven clumps:

> Whatever may have been the physical causes which may have produced this separation, we see abundant evidence of the advantage of this distribution in their application to the purposes of man—smallness in relative amount being compensated for by the accumulation of the material in denser deposits, which allow of their comparatively easy extraction to supply the wants of mankind. (ibid.).

It was taken as given by the authors that this useful arrangement of minerals was due to God's plan. And if this was so for our humble planet, the analogous arrangement in the stars must mean something similar:

> If this arrangement be admitted as designed in the case of the earth, is it going beyond the limits of fair deduction to suppose that, were we acquainted with the economy of those distant globes, an equally obvious purpose might be assigned for the differences in composition which they exhibit? [Spectral analysis] seems to furnish a basis for some legitimate speculation in reference to the great plan of the visible universe, and to the special object and design of [stars]. (ibid.).

The distribution of the elements in space, then, must also be part of a plan. But for whose use? Huggins and Miller (1864) noted that stellar spectra indicated that the elements most widely diffused were those associated with living organisms. Further, these distant stars appeared to have everything needed for life here on Earth—heat, light, etc. The conclusion seemed inexorable:

> On the whole we believe that the foregoing spectrum observations on the stars contribute something towards an experimental basis on which a conclusion … may rest, viz. that at least the brighter stars are, like our sun, upholding and energizing centres of systems of worlds adapted to be the abode of living beings. (Huggins and Miller, 1864: 433).

To summarize their argument: spectroscopy shows us that everything needed for life and civilization fills the Universe, in the same way that those things cover our Earth. God arranged the Earth for our use, thus the rest of the Universe must be filled with life as well. Huggins and Miller creatively combined their observations, the unity of laws and matter, and religion to paint a picture of a Universe teeming with beings much like ourselves (cf. Becker, 2010).

5 CONCLUSION

So what? Why were scientists excited about spectroscopy? It was not only the measurements and the concrete results of investigation. It was also the sense that spectral analysis was a great leap forward in human ability, and had set science on a path to even greater discoveries:

> Who could have dreamt ten years ago that we should so soon attain such an insight into the processes of creation? And yet, great though the results of spectrum analysis already are, they are but a tithe of the numerous questions which this branch of discovery has opened up—questions of such number and magnitude, that many generations of men will pass away before they are all satisfactorily answered. (Roscoe, 1873: 358).

Spectroscopy provided a window into deep-seated puzzles about the nature of science—confirmation of the assumptions of unity of laws and matter; ancient hypotheses that seemed startlingly relevant—the behavior of atoms; and the big questions—the relationship among man, God and the Universe. Spectroscopy was not a destination; it was a road that promised to connect humanity's most powerful speculations to a future of dramatic empirical investigation.

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FROM DILETTANTE TO SERIOUS AMATEUR: WILLIAM HUGGINS' MOVE INTO THE INNER CIRCLE

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Abstract: Early in his career, like many other novice astronomers of his day, William Huggins (1824–1910) pursued a varied and opportunistic research programme. He devoted considerable time and serious attention to research problems generated by others, and to the exotic rather than the mundane. Free of the obligations and commitments that restricted his institution-bound contemporaries, he was driven by broad interests with an insatiable curiosity to explore the heavens in innovative and often technically-demanding ways. How did he maximize his exposure to opportunities for new discoveries without becoming identified as a speculative or impulsive dilettante? This paper will track his move into the inner circle of serious amateurs by following the steps he took to develop a reputation for his care in making observations and for his caution in suggesting explanations for the phenomena he observed.

Keywords: William Huggins, spectroscopy, amateur, 'willow leaves controversy', prominences.

1 INTRODUCTION

English amateur astronomer William Huggins (Figure 1) played a key role in introducing spectrum analysis into astronomical work (see Hearnshaw, 2010). He was the first to observe emission lines in the spectra of nebulae and the first to apply Doppler's principle to measure stellar radial motion. He served as President of the Royal Astronomical Society, the British Association for the Advancement of Science and the Royal Society. He reaped many awards and honors for his scientific contributions, including honorary degrees from Cambridge, Oxford, Edinburgh and Leyden. He received the Royal Society's Rumford, Royal and Copley Medals; the Académie des Sciences' Lalande Prize; and he is one of the few in the history of the RAS to be twice awarded its prestigious Gold Medal.

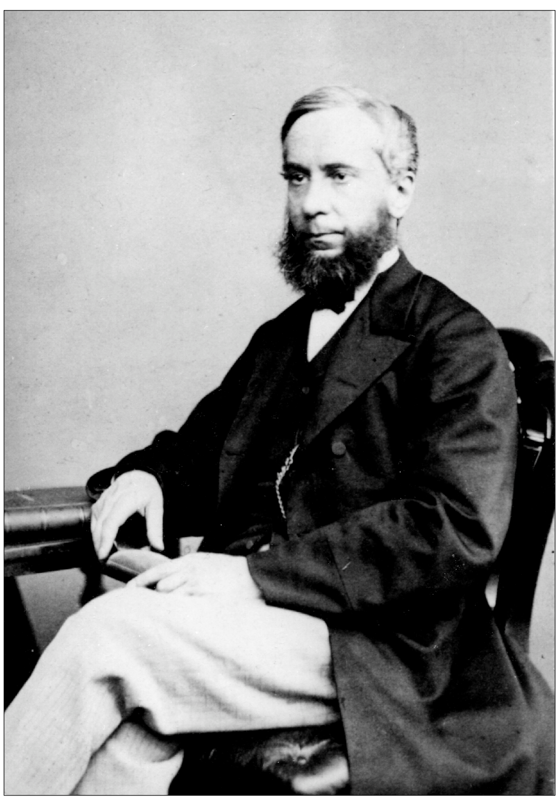

Figure 1: William Huggins, 1824–1910 (courtesy: Royal Astronomical Society Library).

The Emperor of Brazil bestowed on him the Order of the Rose, Queen Victoria created him Knight Commander of the Order of the Bath and King Edward VII awarded him the Order of Merit. He was the fifth recipient of the Astronomical Society of the Pacific's Bruce Medal. These are remarkable achievements for anyone, let alone a linen draper with little formal education and no professional or university training in science or mathematics (Becker, 1993; 2001; 2003; forthcoming).

At the age of thirty, Huggins sold his family's business and moved to a suburban location which afforded him both the leisure time and the darkened skies necessary to pursue his growing passion for astronomical observation. An amateur in the true sense of the word, he was free of the obligations and commitments that governed the observational choices of his institution-bound contemporaries (see Chapman, 1998: chapter 1). His research choices were controlled by other concerns: satisfying his own curiosity, and gaining the recognition and approval of his fellows. His challenge was to maximize his exposure to opportunities for new discoveries without becoming identified as a speculative or impulsive dilettante. It was a challenge his years as an entrepreneur had prepared him well to meet.

Huggins' early observatory notebook entries and published notices display an opportunistic work pattern that is common to most casual amateurs. But in time, his records took on an increasingly focused, albeit eclectic, character. He developed a reputation for his care in making observations and his caution in suggesting explanations for the phenomena he observed. The choices he made, as he moved from the periphery of scientific London toward its inner circle, expose the dynamic and often uncertain process by which the boundaries of acceptable research were redefined in astronomy during his lifetime (see Stanley, 2010).

Near the end of his life, he wrote a retrospective essay—"The New Astronomy"—intended as an eyewitness account of a select set of his pioneering research projects (Huggins, 1897). Readers of this popular and often-cited essay have come to view it as representing the sum of Huggins' life and work. But the unpublished record reveals that Huggins was involved in many lesser-known investigations as well. These overlooked observing choices, especially those made during the early phase of his long career, and exemplify the eclecticism and opportunism that served him so well. A closer look at a few examples shows how they shaped his observing practice. They help us identify the mentors from whom he acquired his technical and methodological expertise; the resources on which he relied to develop his methods, instruments and observational agenda; and the means by which he gained acceptance from his colleagues in scientific London as a serious amateur.

2 ACQUIRING TOOLS AND EXPERTISE

In 1854, Huggins, then an enthusiastic novice, needed guidance and encouragement in order to transcend his *ad hoc* observing pattern and learn how to operate within a research agenda. His election to Fellowship in the Royal Astronomical Society (RAS) provided him with much-needed support. At the Leeds meeting of the British Association for the Advancement of Science (BAAS) in September 1858 he came into direct contact with a wider circle of expert amateurs, most notably the Reverend William Rutter Dawes (1799–1868), the eagle-eyed binary star observer who had received the RAS's Gold Medal in 1855 for his catalogue of double stars and his discovery of Saturn's crepe ring.

Dawes's interest in double stars had led him to seek objective lenses with superior resolving power. He purchased several made by Alvan Clark of Cambridge, Massachusetts. In 1858, he sold one with an 8-inch aperture to Huggins for £200, a deal the two men likely negotiated at the Leeds meeting. Huggins soon had it mounted in an equatorial, clock-driven telescope built by Britain's premier telescope-maker, Thomas Cooke.

But acquiring this fine instrument cannot account entirely for the increasingly focused quality of Huggins' observations at this time. Clues to his metamorphosis from a true novice to a confident, self-directed amateur can be found in his cryptic notebook jottings and contributions to the *Monthly Notices of the Royal Astronomical Society*. They point to Dawes' influence on his selection of subjects to observe, his method of observation and his overall sense of purpose in making his astronomical observations.

Huggins' desire to follow his mentor's lead might explain why, despite the stunning apparition of Donati's Comet in 1858, he chose instead to focus on binary stars and changes in Jupiter's surface features. These were projects only observers with sufficiently fine instrumentation and experience could undertake. By comparison, the comet may have seemed like just one more fuzzy little object to him, something even the man on the street could see without instrumental assistance (Huggins, 15 October 1858, Notebook 1).

Between October 1858 and July 1860, with few exceptions, Huggins made at least one notebook entry every month, many of them indicative of his new interest in recording subtle changes in a single object over time. From 2 November 1858 through 10 February 1859, a period when Jupiter was favourably placed for viewing, he devoted his attention to observing variations in its surface, just as Dawes (1857) had done the previous year. Each of Huggins' thirteen recorded observations of Jupiter during this period is accompanied by a drawing, many of which he later excised from his notebook and presented to the RAS (Huggins, 1859).

Then, as Saturn moved into view in mid-February 1859, he kept close watch on that planet and its satellites. His observations of Jupiter and, especially, of Saturn continued with great regularity through May 1860. Indeed, these are the principal projects in which Huggins was engaged when reports out of Berlin reached English scientific circles concerning the claims of chemist Robert Bunsen and physicist Gustav Kirchhoff that the chemical and physical nature of the Sun could be discerned by analysing its spectrum (Kirchhoff, 1860; Kirchhoff and Bunsen, 1860; 1861). It would be nearly two years before this "… spring of water …", as Huggins (1897:911) later described the news, trickled down to the wider audience of which he was a part. It was only then that he famously embarked upon a new observational programme fraught with substantial risk and promise, namely the application of spectrum analysis to the light of celestial bodies.

He could have set himself the arduous task of systematically cataloguing the spectra of northern hemisphere stars, or examining the spectrum of every known nebular object. Instead, he continued to pursue a varied and opportunistic research programme like many other amateur astronomers of his day, devoting considerable time and serious attention to research problems generated by others, and to the exotic rather than the mundane.

3 THE 'WILLOW LEAVES' CONTROVERSY

In March 1861, James Nasmyth (1808–1890) had described the solar disk as having a filamentary structure that he likened to strewn willow leaves in appearance (Nasmyth, 1862). According to Nasmyth, these long and slender shapes, though fairly uniform in size, were layered in a helter-skelter fashion over the entire solar surface, but more clearly organized near the edges of sunspots. His claims were enthusiastically confirmed by other distinguished solar observers. There the matter rested until the fall of 1863.

Dawes also had considerable experience studying the Sun. He had remained silent on the subject of Nasmyth's 'willow leaves'. But he felt compelled to speak out after learning that John Herschel (1792–1871)—who was preparing a new edition of his authoritative *Outlines of Astronomy* (Herschel, 1849)—planned to amend his description of the Sun's visible surface as "… finely mottled with an appearance of minute, dark dots, or pores …" to reflect the promise of Nasmyth's "… remarkable discovery." The possibility that Herschel would give the 'willow leaves' his coveted seal of approval, roused Dawes from complacency (Herschel, 1864: 695-696).

With over a decade of experience as a solar observer using superior instruments, Dawes had seen these dynamic features, too. But he did not call them 'willow leaves'. Nor would he willingly grant Nasmyth, a relative novice at this sort of thing, credit for a new discovery. How an individual perceives and interprets the Sun's appearance in any given observation, Dawes argued, depends on the size of the telescope and the degree of magnification employed. Care must be taken to avoid the error of 'discovering' what has already been seen more clearly (and, hence, described differently) by others. Indeed, the mottling on the Sun's disk had been likened to "brain coral", "soapsuds in hard water" and "rice-grains" by some; others regarded the whole debate as a war of words. In Dawes' view, it would be better to liken the striated borders of sunspots to "… small bits of straw or thatching …" and the bright solar surface to "… minute fragments of porcelain." (Royal Astronomical Society …: 4-6 (1864a); Royal Astronomical Society …: 99-101 (1864b); Bartholomew, 1976: 263-289).

Huggins cast his lot with his friend Dawes. His own observations, he asserted confidently, led him to conclude that whatever they might be, the 'bright particles' lacked the uniformity of size and shape necessary to be classified as 'willow leaves' (Discussion on …, 1864). But he could hardly present himself as an expert on the matter, and the controversy continued to simmer without resolution for some time.

On the morning of 26 April 1866, Huggins spent two hours scrutinizing the solar surface. He took detailed notes, dividing his remarks into categories headed "distribution", "form", "size" and "brightness". Before preparing his report, he examined the solar disk at least three more times to confirm these observations (Huggins, 1866a).

He advertised his stance on the 'willow leaves' question by titling his paper "Results of some observations of bright granules on the surface of the Sun". Aware of the need to tread with great care through the field of egos that would hear and/or read his words, he couched his statements in neutral terms, offering constructive criticism and well-considered commentary to all who had voiced opinions on the issue. The name 'granule', he argued, is purely descriptive and free of any hypothesis as to the nature of the phenomenon.

Edward Stone (1870) expressed his great pleasure "… that observers were getting so close together on the subject of the solar photosphere." He personally preferred the term 'willow leaves', but he acknowledged the aptness of 'granule' to describe the elongation common to these features on the Sun's surface. Warren De la Rue (1815–1889) expressed his satisfaction that "… all observers were agreed on there being elongated forms …" regardless of what one called them. He congratulated Huggins and others on their efforts.

The controversy did not end so much as it faded away, thanks in large part to Huggins' astute presentation of his case. Rather than review the fractious past and reinforce the personal antagonisms that were blocking productive exchange between the pro and con 'willow leaves' camps, Huggins treated all views, including his own, as worthy but in need of improvement—improvement that could only come through working together. He used the generic term 'granule' to point the way toward a common middle ground where it was likely no one would be completely satisfied, but all would find enough agreement to move forward.

4 THE NOVA IN CORONA BOREALIS

In 1866, Huggins began a new notebook (Huggins, Notebook 2). His entries in it have a different character from those of the first notebook. For one thing, they are more complete, including more background information in each entry as to observing conditions and instrumentation employed. He even includes occasional interpretive remarks. Still, there are many gaps.

Judging from the form and variety of the observations Huggins recorded in his new notebook, he still eschewed a programme devoted to a single type of object or methodological approach in favour of one that left him free to explore whatever interested him. He seems to have had no regular observing schedule. Whenever he was notified of something new or unusual in the sky, he immediately subjected it to scrutiny.

His investigation of the recurrent nova T Corona Borealis is a case in point. On 12 May 1866, Irish amateur astronomer John Birmingham (1816–1884) was "… struck with the appearance of a new star in Corona Borealis …" as he walked home from a friend's house (Birmingham, 1866). Believing its spectrum worthy of analysis, he sent a note announcing his discovery to Huggins.

The sky was clear on the evening of 16 May 1866 when Huggins received Birmingham's news. He invited his neighbour and collaborator, William Allen Miller (1817–1870)—a well-respected chemist, pioneer spectroscopist and high-ranking official in the Royal Society—to join him in observing the new star spectroscopically. They found the nova's spectrum to be compound, that is, comprised of a series of bright lines superposed on a nearly continuous background. Huggins attributed the continuous spectrum broken by absorption lines to the body of the star (Huggins, 16 May 1866, Notebook 2). He believed the bright lines were produced by glowing hydrogen gas and drew attention to what appeared to him to be a nebulous region immediately surrounding the star. With a Royal Society meeting scheduled for the very next day, Huggins and Miller (1866) wasted no time preparing a brief paper describing their preliminary observations of the nova's spectrum.

Over the next week, Huggins observed the nova every evening, and as it grew fainter he developed his own theory of what had occurred. Based on the fact that its spectrum included both absorption and emission lines, he speculated the nova was a "… star on fire …", which, by virtue of some cataclysmic event, had let loose a large quantity of hydrogen gas into its immediate surroundings (Royal Astronomical Society …: 181 (1866)).

In his view, the intense heat of the star had ignited and consumed the gas in a short period of time—hence explaining the sudden rise and rapid decline in the nova's luminosity.

In publicizing his theory, he emphasized that he was only able to account for the nova's change in brightness because of his careful spectroscopic examination of the star's light (Huggins, 1866c). And, he posed the provocative question of what the star's spectrum might have looked like just before the outburst occurred. He wondered about the bright lines seen in other stars. Could such a feature portend a similar cataclysm in these stars sometime in the near future? Huggins believed that proper interpretation of the differences in stellar spectral signatures would lead to an understanding of the physical causes of variation in stellar luminosity. If a non-varying star with bright lines in its spectrum could be observed methodically over time, perhaps a longer chain of events could be formed linking nebulae, novae and stars together in some progressive scheme (Huggins, 1866b; 1866c; Huggins and Miller, 1866). Yet, as the star grew fainter, and the evening hours grew shorter, he returned to objects of routine interest including the solar surface.

5 THE RED FLAMES

While observing an annular eclipse in May 1836, Francis Baily (1774–1844) saw a "… row of lucid points, like a string of beads …" shine through the nooks and crannies of the trailing limb of the lunar disk at second contact (Baily, 1836). He presumed the beads to be a momentary divertissement with an annulus quickly forming once all the lunar mountains cleared the solar perimeter. Instead, much to his surprise, the beads not only persisted, they became elongated strands of liquid sunshine separated by pronounced parallel black lines. Seconds passed before the black lines dissolved and the familiar shimmering circle of sunlight appeared around the Moon. Was this something like the infamous 'black drop' effect that muddled the timing of transit events? If so, how could the attentive observer mark the true beginning and end of each phase in an eclipse? Baily urged colleagues to look carefully for signs of these remarkable phenomena and for clues to their cause during future eclipses.

A perfect opportunity arose six years later when a total eclipse crossed Europe in July 1842. Baily observed the eclipse from Pavia. He did report seeing the 'beads' again, but this eclipse introduced two other spectacles that absorbed his attention during totality: the "… corona, or kind of bright glory …" surrounding the black lunar disk, and "… three large protuberances … [resembling] Alpine mountains ... coloured by the rising or setting sun." (Figure 2). His vivid description and beautiful illustration of the latter in his report on the 1842 eclipse inspired new questions (Baily, 1842). Were the rose-coloured protuberances illusions brought on by eye fatigue or an over-active imagination? Were they some sort of dazzling atmospheric effect? Or, were they true solar phenomena? Being limited to momentary glimpses of these and other eclipse phenomena by the brevity of totality and the capriciousness of the attending weather made it difficult to obtain confirmatory observations (Royal Astronomical Society …: 264-265 (1868)).

A major breakthrough came in 1860 when De la Rue claimed success in photographing the near-solar atmosphere during totality. He interpreted the images he had obtained as showing the limb of the Moon sequentially occulting the flame-like protrusions, and thus convinced his fellow astronomers that the prominences were solar in origin rather than transient features in the terrestrial atmosphere or simply illusions brought on by the sharp contrast of dark and light (Smith, 1981).

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His photographs confirmed once and for all the flames' reality and solar origin (De la Rue, 1862; Rothermel, 1993; Smith, 1981). They also conjured multiple new mysteries that left solar specialists on tenterhooks until they could view the flames again. The total phase of the next total eclipse, in December 1861, was expected to be barely two minutes long. In April 1865, another promised over five minutes of totality, but to observe it required travel to South America or Portuguese West Africa (Angola). A third eclipse, with almost three minutes of predicted totality, was expected in August 1867, but it was even less inviting as an expedition prospect. Its centre line was due to cross Argentina and then plunge southeast over the Atlantic before terminating near the Antarctic circle. By the latter part of that decade, interest in the nature of solar prominences was again on the rise.

J. Norman Lockyer (1836–1920), for example, turned his attention to the Sun in the mid-1860s in part because of the excitement generated by the 'willow leaves' controversy (Meadows, 1972). In March 1866, he began a spectroscopic study of sunspots using a clever method of his own design. He projected the Sun's image onto a screen that had a small slit. The screen could be moved to position the slit across a sunspot. In this way a linear segment of the sunspot as

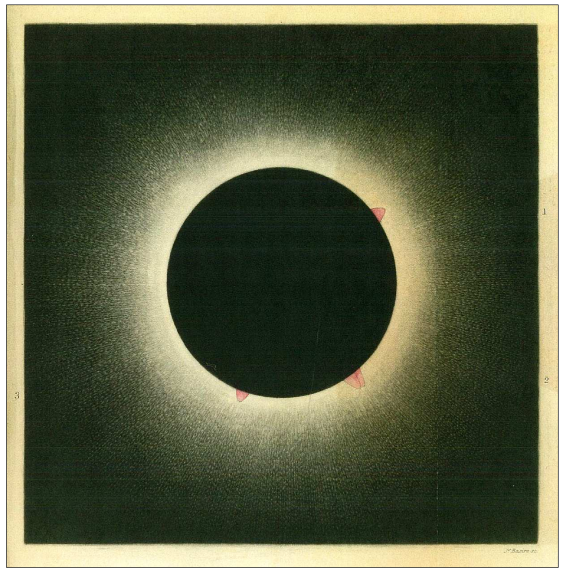

Figure 2: 'Protuberances' around the Sun (after Baily, 1842: facing 212).

well as a portion of the adjoining photosphere were thrown into the attached spectroscope allowing a comparison to be made of the two contiguous spectra.

An analysis of these observations formed the basis of his first paper to appear in the *Proceedings of the Royal Society* (Lockyer, 1866). Titled "Spectroscopic observations of the Sun", the paper was communicated on Lockyer's behalf by the Society's Secretary, physiologist William Sharpey, on 10 October 1866, and read at the 15 November meeting. The dates are significant in terms of the dynamics of Lockyer's increasingly competitive relationship with Huggins. Indeed, the ensuing commotion over who first conceived, developed and executed a successful plan to observe solar prominences without an eclipse diverted the attention of the paper's readers from Lockyer's sunspot findings to his suggestions for possible future applications of the spectroscope to solar research. In particular, his query, "… and may not the spectroscope afford us evidence of the existence of the 'red flames' which total eclipses have revealed to us in the sun's atmosphere; although they escape all other methods of observation at other times?" became a central point of contention (Lockyer, 1866: 258). At the time he submitted the paper, Lockyer noted that his spectroscope possessed insufficient dispersing power to render the prominence spectral lines visible without an eclipse.

Meanwhile, on 10 November 1866, one day after the regular monthly meeting of the RAS, and not quite a week before the Royal Society meeting at which Lockyer's paper on solar spectroscopy was to be read, Huggins wrote in his observatory notebook, "I tried a new method of endeavouring to see the red-flames …" by a method that "… had appeared to me probable (for some weeks)." (Huggins, 10 Nov 1866, Notebook 2).

Huggins' method was not spectroscopically based. If, as reported, the prominences were red in colour, he reasoned it should be possible to filter out most other regions of the solar spectrum using a stack of differently coloured pieces of glass held together with Canada balsam. Did he fail to recognize the potential of prismatic analysis as a practical means by which the solar prominences might be rendered visible? If he had been contemplating viewing them by filtering the Sun's light "for some weeks" already, what motivated him to test his method at this particular time? Did Lockyer reveal something of his own intentions in informal conversation at the RAS meeting the night before? For the moment, at least, it did not matter. Lockyer's inadequate apparatus prevented him from executing his clever plan, while Huggins could not be coaxed to perform as he had hoped. Besides, searching for prominences was just one of many irons Huggins had in the fire at the time: he was also busy measuring the heat of celestial bodies, observing changes in the lunar crater Linné and preparing his assault on the problem of measuring stellar motion in the line of sight (Huggins, 31 May, 2 Nov, 8 Nov, 27 Nov, 5 Dec 1867, 6 Feb, 15 Apr, 19 Dec 1868, Notebook 2). Aside from a few notes on sunspots, he recorded very little relating to solar investigation during this period. A view of the red flames without an eclipse would have to wait.

6 FIREWORKS AND SHOOTING STARS

In November 1866, RAS president Charles Pritchard called members' attention to the meteor shower expected to occur on the 13th or 14th of the month. This meteor shower was a much anticipated event—the first to have been predicted in advance (Newton, 1864a; 1864b). Pritchard (1866) warned the assembly, "If any man went to bed on either of those nights, he was not worthy to be called an astronomer."

Huggins had already been preparing himself by viewing the spectra of sudden flashes of flaming metallic substances produced by fireworks displays in September and October at the Crystal Palace, not far from his home. He used an instrument he called a meteor-spectroscope (Huggins, 1868). The hand-held instrument was a small direct-vision spectroscope with three contiguous prisms, one of flint glass inverted and sandwiched between two of crown glass. The records of his fireworks observations indicate that he had no difficulty spotting transient events and felt confident that he could detect spectral characteristics in the light produced (Huggins, 13 Sep and 29 Oct 1866, Notebook 2).

He made an effort to view the meteors between 1:45 and 3:15 a.m. on 14 November, and reported seeing many small meteors during the first hour of his vigil, but very few afterwards. Only one bright meteor appeared, but it was behind a cloud. "Saw one or two faint ones through prism, but nothing satisfactory. The display at this time, a very poor one." (Huggins, 14 Nov 1866, Notebook 2). Meanwhile, other observers reported the display as being especially fine. It had come just as predicted and did not disappoint most of those who reported their observations. Accounts of observations made under excellent weather conditions filled almost half a page in *The Times* the next day. They variously described the shower as surpassing "… anything that the present generation has witnessed …", "… like sparks flying from an incandescent mass of iron under the blows of a Titanic hammer …", "… bursting globes of fire …" and a "… magnificent spectacle." (*London Times*, 15 Nov 1866: 10b-d).

The shower generated considerable discussion at the RAS meeting of 11 January 1867. If Huggins participated, his comments were not reported in the *Astronomical Register* (see Royal Astronomical Society …, 1867). In fact, it appears likely he did not attend the meeting, for he recorded in his notebook that on that very evening a Mr. Leaf and his sons called to have a look at Mars through the telescope (Huggins, 11 Jan 1867, Notebook 2).

Huggins' only published comment on his meteor observations did not appear until some time later in a brief paper on the hand-held spectroscope. In it he succinctly and unapologetically stated "Unfortunately, I was prevented from making the use of the instrument which I had intended at the display of meteors in November 1866." (Huggins, 1868b: 242). What pressed him to make this unfounded claim? Did his need to guard his reputation as a careful and capable observer intensify his ever-present worry that colleagues would respond unfavourably to news that, despite his advanced preparation and expertise, he had, in fact, failed to observe many meteors, or their spectra?

7 THE CRATER LINNÉ

In the 1820s, cartographer Wilhelm Gotthelf Lohrmann (1796–1840) and astronomer Johann Heinrich Mädler (1794–1874) described the lunar feature Linné—named in honour of Swedish taxonomist Carl von Linné—as a deep crater with a diameter of some five to six miles, a size that made it the third largest crater in an otherwise smooth and barren plain. Located near the western edge of Mare Serenitatis, it had been noted simply as a round white spot with no mention of any crater-like features by German astronomer Johann Hieronymous Schröter (1745–1816) as early as 1788 (Clerke, 1885: 315). When in 1830 Mädler teamed up with Berlin banker Wilhelm Beer (1797–1850) to produce their renowned lunar map, crater Linné was clearly depicted.

In October 1866, however, the German-born Director of the Athens Observatory, Johann Friedrich Julius Schmidt (1825–1884) announced that the crater had suddenly and inexplicably vanished. He had seen Linné in the early 1840s looking as it had been mapped by Mädler and Beer (Clerke, 1885: 315-316; Schmidt, 1867). But now, observing it again nearly a quarter century later, Schmidt concluded that a real and significant change had recently taken place on the lunar surface. He communicated his observation by letter to the avid English lunar observer, William Radcliff Birt (1804–1881), who immediately set to the task of corroborating the finding and alerted fellow observers (Birt, 1867; Key, 1867; Knott, 1867).

The news broke at a time when interest in the study of lunar features was increasing among British astronomers, and it stimulated a great deal of speculation. Some saw it as evidence of recent volcanic activity on the Moon, while others thought the crater may have been erased by a disturbance in the lunar atmosphere. Agnes Clerke (1885: 313-314) wrote:

> A change always seems to the inquisitive intellect of man like a breach in the defences of Nature's secrets, through which it may hope to make its way to the citadel.

Huggins first examined Linné in December 1866 and monitored it sporadically until December 1873. Although he had shown no interest in lunar surface features before 1866, he had searched for evidence of an atmosphere on the Moon two years earlier by observing, through a spectroscope, the extinction of the light from a star during a lunar occultation. He interpreted the negative results of this effort as probable, though not conclusive, evidence against a lunar atmosphere (Huggins, 1865). Renewed speculation that changes in lunar features might be caused by the weathering effects of an atmosphere drew him to examine the crater.

In his notebook entries on Linné, Huggins referred to the region ascribed to the crater as a " … white hazy patch … [and] less defined …" than other areas on the lunar surface (Huggins, 14 Dec 1866 and 14 Feb 1867, Notebook 2). On 8 May 1867, he suggested that the crater Hercules also presented what he called a 'twilight' appearance. He claimed this twilight effect was absent in other more sharply defined craters, but did not view this as evidence of a lunar atmosphere. Instead he attributed Linné's "… cloudy appearance …" to a "… peculiar, partly reflective property of the material of which Linné consists." (Huggins, 1867: 296).

In January 1874, he submitted to *Monthly Notices* a summary of six years of observations of Linné including selected extracts from his notebook records of the appearance of the crater under different degrees of illumination. From these records he concluded that changes in the crater were, in fact, illusions caused by variations in the direction of the light hitting the Moon's surface in that region (Huggins, 1874).

8 THERMOMETRIC RESEARCH

In 1867, a new and completely different type of observation captured Huggins' attention, namely measurement of heat reaching the Earth from the Moon and brighter stars. He made no public announcement of these efforts, however, until February 1869 when he described what he had done both in his yearly Observatory Report in the *Monthly Notices of the Royal Astronomical Society* and in a brief paper submitted to the *Proceedings of the Royal Society* (Huggins, 1869a; 1869b).

Huggins' thermometric research has been ignored by his biographers and by historians of astronomy. Laurence Parsons, the 4th Earl of Rosse (1840–1908), and Edward Stone (1831–1897) are the individuals normally associated with thermometric observations of celestial bodies during this period. Both of these men, however, began their work ignorant of Huggins' earlier efforts and long after he had given it up (Parsons, 1869; 1870b; 1873; Stone, 1870).

In the decades preceding Huggins' stellar heat measures, a number of individuals developed ingenious methods of adapting the thermopile to the telescope to measure the quantity of radiant heat that reached the Earth from celestial bodies (Brashear, n.d.: 1-12). But if his previous performance is any clue, Huggins did not derive his research questions from the existing literature. His venture into celestial thermometry at this particular time, a task which involved the acquisition and mastery of an entirely new kind of instrumentation and investigative method, presents something of a puzzle.

One clue may be found in the minutes of the RAS meeting on 10 January 1867, just one month before Huggins recorded his first thermometric observation. At that meeting, James Park Harrison read a paper on the radiation of heat from the Moon (Harrison, 1868). Harrison, an active member of the Royal Meteorological Society, had analysed long-term records kept at the world's major observatories to show that terrestrial temperatures were directly related to lunar phase. Sunlight reflected from the Moon's surface, he claimed, had the capacity to evaporate cloud cover on the Earth. What Harrison was arguing was not new. Nearly twenty years earlier, John Herschel had presented nearly identical views in the first edition of his classic *Outlines of Astronomy* (Herschel, 1849: 253-254).

Pressed on whether he could "… measure the heat from the Moon by a thermo-electric apparatus?" Harrison replied he was convinced that the heat was "… used up in the atmosphere …" leaving little or nothing to measure. The subsequent discussion was lively if inconclusive (On the radiation …, 1868), and the subject was never again discussed at the RAS.

Huggins had no interest in accounting for terrestrial temperature fluctuations, but it is intriguing to ponder the influence Harrison's presentation may have had on him at that time. Because no human sense can directly receive the information being examined and measured, thermometric work required instrumental intervention. Thus, Huggins may have been encouraged to try to measure the heat of the Moon and stars from an interest in the instrumentation and the gadgetry rather than any theoretical concerns.

He worked hard to cajole consistent results from his apparatus. He drew diagrams of his equipment, gauged the accuracy of his measures on the basis of the consistency of the data he collected, suggested possible sources of error and described modifications which he felt would reduce those errors. In early 1869 he even provided advice to others on techniques of carrying out such research (Stokes, AddMS 7656. TR77; AddMS7656.TR79; AddMS 76 56.TR81). In the end, however, his disappointment over the unreliability of his results, coupled with his difficulty in converting deflections of the galvanometer's needle into an equivalent quantity of heat, persuaded him to abandon thermometrics in favour of other projects.

9 ACHIEVING "A MARK OF APPROVAL AND CONFIDENCE"

In November 1866, Huggins was awarded the Royal

Society's Royal Medal for his work on the spectra of both terrestrial chemicals and celestial bodies (Sabine, 1866: 280-282). In January 1867, he and Miller were jointly named to receive the Royal Astronomical Society's coveted Gold Medal for their researches on nebular spectra (Royal Astronomical Society …: 31 (1867)). In the Council's view, their investigations had laid the foundation for the eventual resolution of the decades-old problem of the nature of nebulae. In his Presidential address on the occasion of the Medal's award (Pritchard, 1867), Charles Pritchard (1808–1893) nested his tribute to Huggins' and Miller's nebular spectra work in the midst of his congratulatory remarks on Huggins' observations with Miller of T Coronae and his innovative use of the air spectrum as a standard against which to compare celestial spectra.

10 CONCLUSION

Contrary to what might be expected given the acclaim he received following his spectroscopic analysis of nebulae, William Huggins continued to pursue an independent and often eclectic observing programme from the time he was elected into Fellowship in the Royal Society in June 1865 until he was officially awarded responsibility for the Grubb telescope paid for by the Royal Society in November 1869. He cultivated working relationships with valued mentors. At times, as in the case of the nova in Corona Borealis, the objects of his study were opportunistic responses to reports of others' findings. Or, as in the case of his thermometric studies, he was completely original, albeit unsuccessful, in developing a new method of acquiring useful information about the physical nature of celestial bodies. Although he was not the first to observe solar prominences without an eclipse, Huggins noisily claimed priority for suggesting that they could be observed in the first place. He thus intruded upon the claims of his chief competitor, Lockyer. In the process, he gained a healthy respect for the constructive potential of establishing priority.

Driven by broad interests and an insatiable curiosity, Huggins explored a number of different subjects in innovative and often technically-demanding ways. In all of these efforts, he betrayed his skill, energy, ambition and enterprise as he continually sought new ways to make contributions to astronomy worthy of recognition. His successes led to more opportunities for success, and he became identified as a valued resource by a small but influential circle of experienced observers who actively sought his advice on how to make their instrumentation capable of collecting spectroscopic information on celestial bodies.

An individual's incremental career choices may not determine the shape and direction of a developing research agenda, but as concrete instances of personal effort to establish a foothold in the community at large they make visible otherwise tacit acts of negotiation and maneuvering strategies. When a novice like Huggins succeeds despite his lack of access to the proper channels, the historian may find in the unpublished record the tell-tale signs of the tunnel that was dug to undermine the walls.

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THE ORIGIN AND DIFFUSION OF THE H AND K NOTATION

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Abstract: Though many or most astronomers and astronomy students may think that H and K, as in the Ca II 'H and K lines', were named by Fraunhofer, actually only the H line was in Fraunhofer's original notation. He also used 'I' to indicate the end of the spectrum in his widely-reproduced 1814 drawing, of which an engraved version was published in 1817. We have searched references from nineteenth-century books and journals to find the first use of 'K' to indicate the ionized-calcium spectral line at 393.3 nm and located the probable first use and eventually the reuse of the notation.

Key words: spectroscopy, history, Sun, spectrum, Fraunhofer lines

1 INTRODUCTION

The H and K absorption lines of ionized calcium are the strongest in the solar spectrum, and are key for understanding a host of astronomical phenomena. Though today they are referred to as separate lines, K was originally considered and denoted as the twin companion of the H line. Through a survey of major nineteenth century publications on the spectrum we have been able to track down the evolution of the identity of the K line and its diffusion into general use.

The labeling convention for the major absorption lines is attributed to the German optician and glass manufacturer, Joseph Fraunhofer (1787–1826). Though he was not the first to notice divisions of the solar spectrum, he was the first to accurately measure their indices of refraction. He used the line positions to evaluate the quality of quartz prisms that he made. The 1814 measurements along with sketches of his observations were published in 1817 (Figure 1).

The absorption lines in this watercolor illustration are labeled alphabetically starting with 'A' at the red end of the spectrum, 'H' to designate the pair of dark lines near the violet end and 'I' at the violet end. 'K' was never used as a label in his work. According to the excerpts of a lecture delivered at Munich University (Hearnshaw, 1986: 27), Fraunhofer thought the two stripes at H to be "… the most extraordinary …" and pointed out that "… they are both almost completely the same." Details of Fraunhofer's drawing and etching appear in Hentschel (2002a), which also discusses other nineteenth-century images of the spectrum.

A dozen years before Fraunhofer's work, William Wollaston (1766 – 1828) had noted color zones, and separated them by letter, using both upper-case and lower-case letters (Figure 2). He used upper-case letters for the color borders, including D and E for the two limits of violet. His D is presumably a blend (at his resolution) of what today we know as the H and K lines.

Notations and vertical lines above and below Fraunhofer's spectrum, including vertical divisions of what we now know as a black-body curve, show that Fraunhofer still used capital letters to divide parts of the spectrum, while using lower-case letters for additional spectral lines.

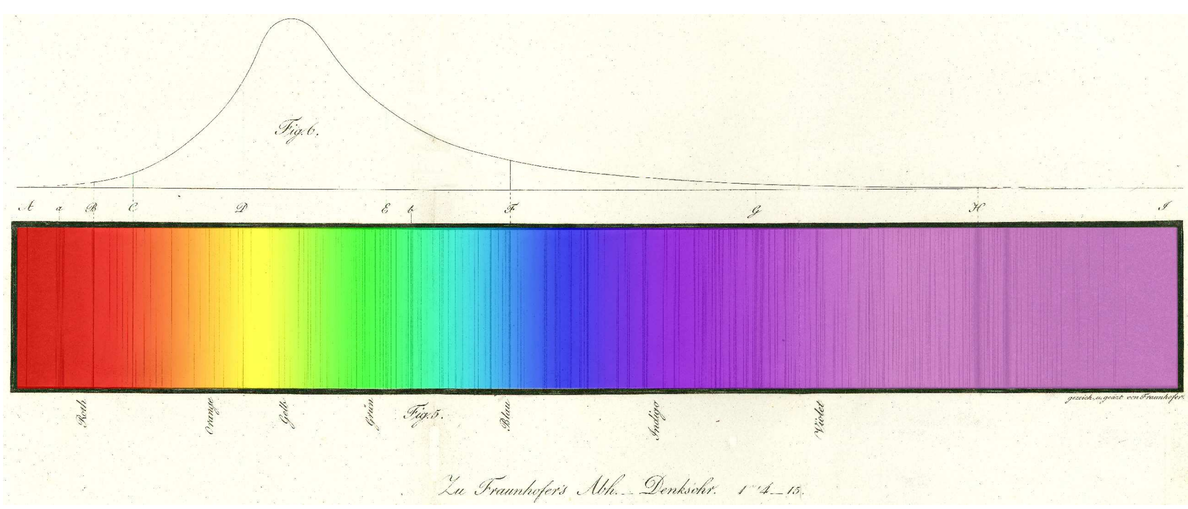

Figure 1: The 1814 map of the solar spectrum by Fraunhofer, with color added (after Fraunhofer, 1817).

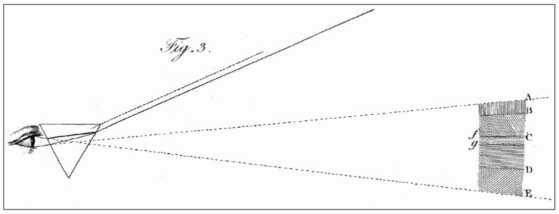

Figure 2: Drawing of the solar spectrum by Wollaston, showing his use of capital and lower-case letters, a scheme carried over by Fraunhofer (after Wollaston, 1802; courtesy: the Royal Society).

2 POST-FRAUNHOFER NOTATIONS

In 1842, Alexandre-Edmond Becquerel (1820–1891) of the Musée de l'Histoire Naturelle in Paris reproduced Fraunhofer's observations of the dark lines and saw further into the violet by using a flint glass prism, since flint glass has greater transmission than crown glass in the violet range. The light was focused onto a screen where he mounted a silver-nitride-coated Daguerreotype plate to capture the observations. This was the first attempt to study the spectrum photographically and he was the first to show that the Fraunhofer lines were zones of chemical inactivity (Hentschel, 2002a: 194). The resulting publication of the Fraunhofer lines was made from a copper engraving of the observation. The two calcium lines were once again labeled as H, with the letter H placed over today's H line.

When J. Norman Lockyer (1836–1920) reproduced Becquerel's map of 1842 on a reduced scale, he retained the nomenclature. Later in his article, he showed his own photograph of the spectrum, with the lines labeled H_1 and H_2 (1874).

This trend of using H to annotate the double calcium lines was broken for the first time in 1843 by John William Draper (1811–1882), a Professor of Chemistry at the University of the City of New York (now New York University), who labeled the lines as H and k (lower case) in order of decreasing wavelength (Figure 3). He experimented with different techniques of applying photography to spectroscopy and found several new lines beyond both ends of the then-known spectrum. It is not obvious why he chose to distinguish between the two lines, and the notation did not catch on for several decades. For further discussion of Draper's work, see Hentschel (2002b).

As observational techniques were invented and refined, many different maps of the solar spectrum appeared in print (see Hearnshaw, 2010). The wavelength range was extended, resolution of lines improved, and measurements of line positions made more precisely. There were even discussions on possible physical interpretations of the lines (Stanley, 2010). In the absence of standard notations there was some inconsistency in the labeling used in the resulting publications.

In 1852, George Stokes (1819–1903), then Lucasian Professor of Physics at Cambridge, extended the original spectrum further into the violet. He utilized the refractive properties of quartz in ultraviolet light as well as the ability of some materials to give off ultraviolet radiation under certain conditions. Unsure of how the many lines in this part of the spectrum related to the lines in the visible, he decided not to use the capital letters employed by Draper in 1843 but used lowercase letters from k to p on his metal relief engraving of the spectrum. Stokes' k corresponds to k on Draper's photographic spectrum (Stokes, 1852).

Though Stokes had some insights about the origins of the dark lines in the spectrum, Gustav Kirchhoff (1824–1887) and Robert Bunsen (1811–1899) at Heidelberg were the first to link the lines in the spectrum to chemistry. They demonstrated the existence of emission lines in the electric spark spectra of several known compounds (1860). Later Kirchhoff (1861; 1862) published these laboratory spectra along with a solar spectrum for comparison. The lines are labeled with the abbreviations of the elements, calcium being denoted 'Ca', for example.

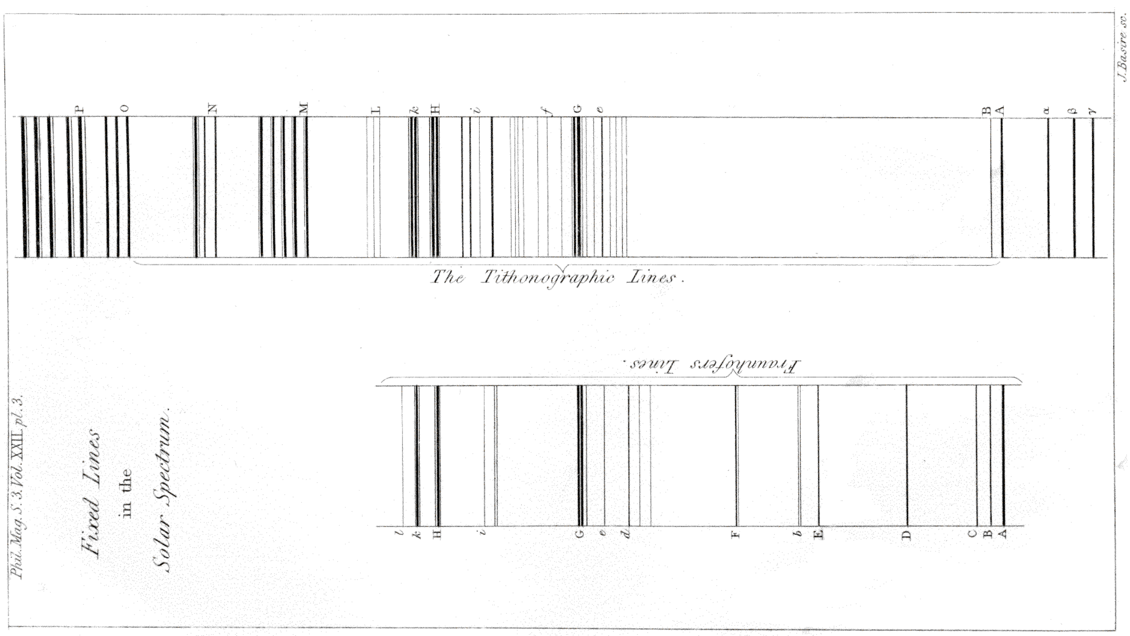

Figure 3: Map of the spectrum using k (after Draper, 1843: Plate III).

In 1864, Éleuthère Mascart (1837–1908) from the École Normale Supérieure in Paris published his atlas of the ultraviolet region of the spectrum. He replaced the glass optics of his spectroscope with prisms and lenses made of quartz or Iceland spar, which both have better transmission in ultraviolet light. His map contained nearly ten times as many spectral lines as its forerunners (Figure 4). He was the first person to use capital K to designate the second H line. (Coding showing that Mascart defined some notations, including K, appeared in Appendix 1 in Stratton, 1925: 183, which was included in Hearnshaw, 1986: 493-494.)

Not long after Mascart's map, high quality diffraction gratings replaced prisms as the dispersive element in spectrum analysis. Anders Ångström (1814–1874) and his colleague, T. Robert Thalén (1827–1905), were among the first to publish a normal spectrum (Ångström and Thalén, 1866). They used H_1 and H_2 along with the abbreviation Ca to denote the calcium lines in their drawings (Figure 5). In 1868, Ångström published another map of the normal solar spectrum including detailed measurements of more than 1000 spectral lines. There and in a table (Figure 6) he used H_1, H_{11}, and Ca to label the calcium lines, with H· and H" labeling the atlas chart, adding yet more variety to the notation.

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Raies	Sixième spectre		Cinquième spectre		Quatrième spectre		Valeur moyenne de Longueur d'onde	Différence
	m_6	λ	m_5	λ	m_4	λ		
	—	—	918,0	4016,53	708,0	4016,94	4016,73	20
	—	—	1047,0	4004,62	810,0	04,80	04,71	9
	—	—	—	—	839,0	4001,36	4001,36	—
	—	—	—	—	869,0	3997,78	3997,78	—
H_1	—	—	1446,0	3967,76	1119,0	3968,00	67,88	12
	—	—	*[1778,0	37,04]	—	—	—	—
H_{11}	—	—	1823,0	3932,82	—	—	3932,82	—

Figure 6: A table showing H_1 and H_{11} (after Ångström, 1868: Plate X of Tableaux section).

It was not until 1872 that K reappeared in the work of Marie Alfred Cornu (1841–1902), a colleague of Mascart. He set out to map the ultraviolet region of the spectrum using Ångström's normal spectrum as a template (Figure 7). In this publication he quoted one of Mascart's earlier maps, and this probably explains his use of K. Hermann Wilhelm Vogel (1834–1898), known for developing emulsions and sensitizers in photography, also quoted Mascart's maps and showed a diagram with H centered between H and K. However, he used the notation H and H' in the text, while discussing Fraunhofer's lettering (Vogel, 1875).

From the mid-1870s and onwards, the two calcium lines took on greater importance in the astronomical

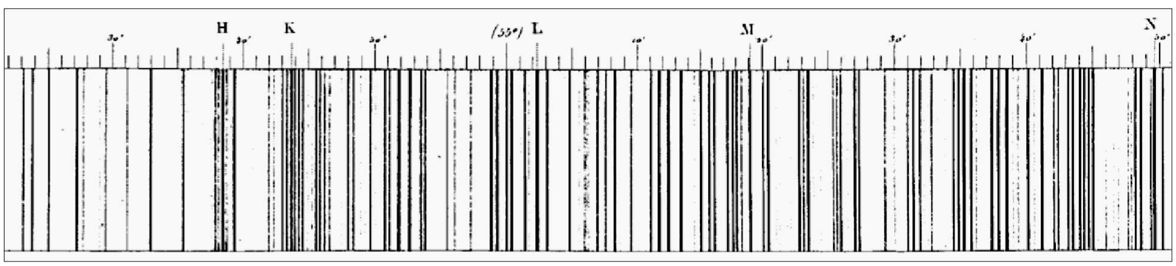


Figure 4: Small portion of the spectrum showing H and K (after Mascart, 1864: Figure 1 in Planche IV).

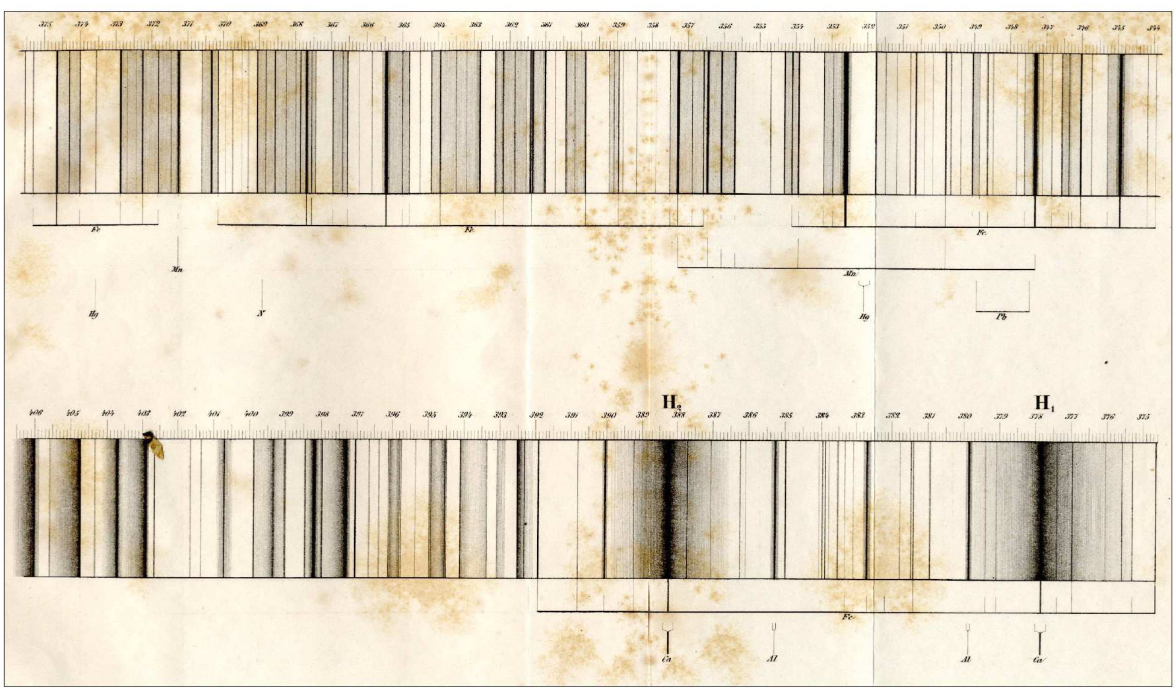


Figure 5: A spectrum including H_1 and H_2 (after Ångström and Thalén, 1866: Plate II).

community. While this was due to improvements in instrumentation and observing techniques it was also because of the interesting behavior that the two calcium lines displayed. In 1872, during the Mount Sherman Astronomical Expedition, Professor Charles A. Young (1834–1908), then of Dartmouth College, observed reversals of the Fraunhofer lines in spectra of the solar chromosphere and prominences and also found 170 new lines. He stated:

> The only lines of much importance are the two Hs at the extreme violet end of the spectrum. These were found to be constantly reversed … and I am pretty confident always reversed in the spectrum of sunspots … This reversal of the H lines does not involve at all the disappearance of the dark shade, but a bright streak rather than a line makes its appearance in the center of the shade … (Young, 1872).

Demonstrating the inconsistency of the time, in his book *Die Sonne* (1872), Fr A. Secchi refers to H and H' as labels in an image credited to Lewis Rutherfurd (p. 232), H and H' again in an image credited to Van der Willigen (p. 241), and H_1 and H_2 in a table (p. 247).

3 CURRENT NOTATION IS STANDARDIZED

In 1874 Norman Lockyer was still using the notation H_1 and H_2 (Figure 8). In 1878, he discussed the physical causes of the Fraunhofer lines and compared the solar spectrum and stellar spectra with laboratory results of dissociating calcium chloride. His drawing

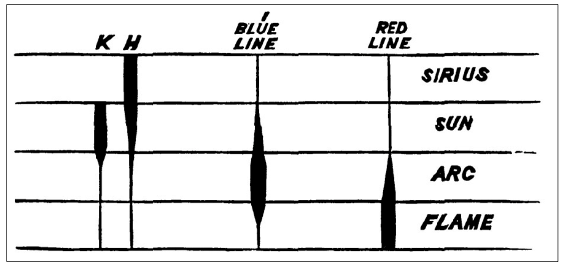


Figure 9: The K line in spectra of various objects (after Lockyer, 1878: 171).

(Figure 9) demonstrated how the widths of the H and K lines vary in the spectra of the Sun, Sirius and sodium chloride. He also attempted to link the line widths to the temperature variations in these bodies

The spectrum of Sirius used in Lockyer's work was obtained by the British amateur astronomer, William Huggins (Becker, 2010), and communicated privately before the latter published this material (1880). These observations by Huggins were crucial to establishing K as a line with unique behavior.

Astronomer George Ellery Hale (1868–1938) best summarized this work:

> Dr. Huggins arranged the stars observed in a series, in which the principal criterion of the position was the character of the K line. In Arcturus this line is broad and more diffuse than in the sun, in Sirius it has

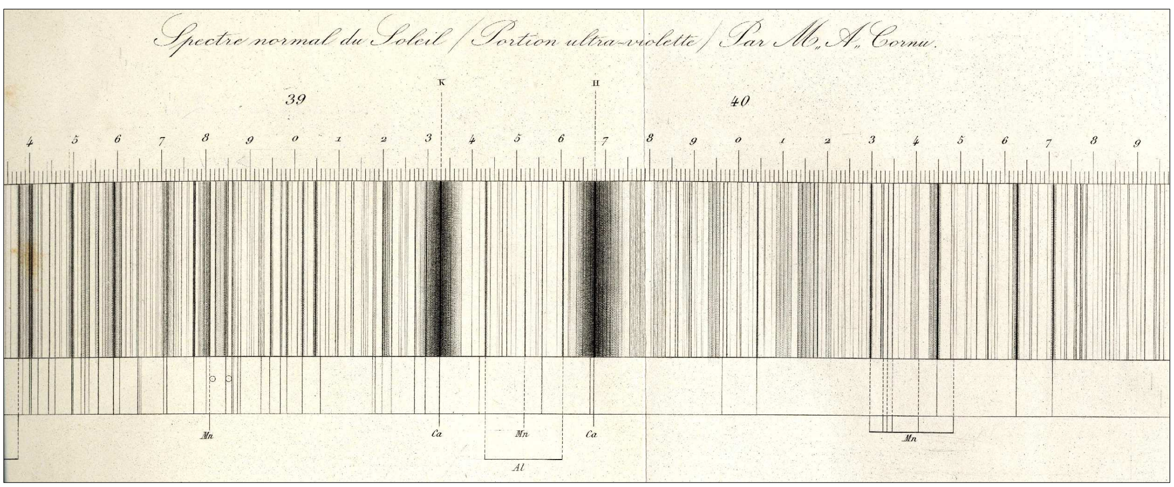


Figure 7: Portion of Cornu's map of the spectrum, with H and K notation (after Cornu, 1880).

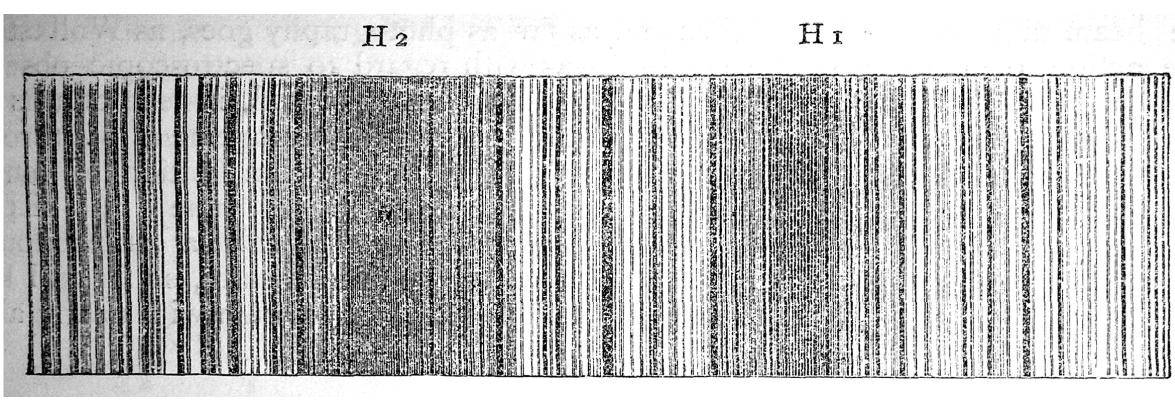


Figure 8: The H-lines in the blue end of the solar spectrum (after Lockyer, 1874: 110).

> narrowed down to a fine, sharp line. Other stars give intermediate breadths and in some it has entirely disappeared. In the case of H the question is complicated by the nearby Hydrogen line, so it is best to consider only K. (Hale, 1892).

In the same work Hale went on to discuss the causes of the line reversals:

> From the variations of this line it will be seen, apart from the interesting subject of stellar evolution so evidently suggested that the narrow dark line at the center is very possibly produced by the same substance which, vibrating under different conditions, causing by its absorption the broad dark band.

He referred to the lines as H^1 and K, to distinguish the first calcium line from hydrogen. He also compared the spectra of these stars to the solar spectrum map of Alfred Cornu who, as discussed earlier, quoted Mascart, the originator of the K notation.

To emphasize the varied use of notation during this period, we turn to the notebook of Margaret Huggins, the wife and collaborator of William. She used H_2 in 1877 to refer to the calcium line and on 23 October 1878, she referred to "… half the width of the second H line." (Figure 10). On 2 January 1880, she wrote: "The spectrum of Sirius is beautiful, and in this photograph one sees clearly the line H_2 (or K) faint and thin ..." (Figure 11). But by 30 June 1881, it was clear that K had entered into her standard notation. Referring to faint lines at that end of the spectrum between H and h and G and g, she wrote: "… the faint lines used in shading between and beyond the intensely [?] lines beyond K are also a guess." See also Figure 12, a stained-glass window from the Huggins Observatory (given to Wellesley College's Whitin Observatory by Lady Huggins), which notably extends only to Fraunhofer's original H.

This variation in nomenclature continued to occur on both sides of the Atlantic. J. Norman Lockyer (1887) used H^1 and H^2 in his figures of the solar spectrum. Astronomy handbook author George F. Chambers (1890) crediting E.W. Maunder for the spectroscopic chapter, wrote that "H is a pair of bands near the limit of vision in the extreme violet." (p. 302), but later referred to H and K (pp. 324 and 334). In 1892, Henri Deslandres (1853–1948) in Paris discovered the weak emission cores in the centre of the H and K lines which he labeled H_2 and K_2, and H_3 and K_3. C.A. Young used H and K in a 1904 article. It was not until the 1920s that K became the standard notation.

As it turns out, several misnomers and ambivalent notations were straightened out at the first International Astronomical Union meeting, held in Rome in 1922 (Hearnshaw, 1986: 256). There was a Spectral Classification Committee headed by Walter S. Adams (1876–1956)—the Adams Committee—whose job was to examine the notation for individual spectral lines. The Committee accepted that only some of the mostly century-old Fraunhofer symbols should be retained, only A, a, α, D, b, G, H and K were to be preserved because they are "… so well established that it does not seem desirable to abandon them… [while] for the hydrogen lines, the notation Hα, and Hβ, etc. should be adopted."

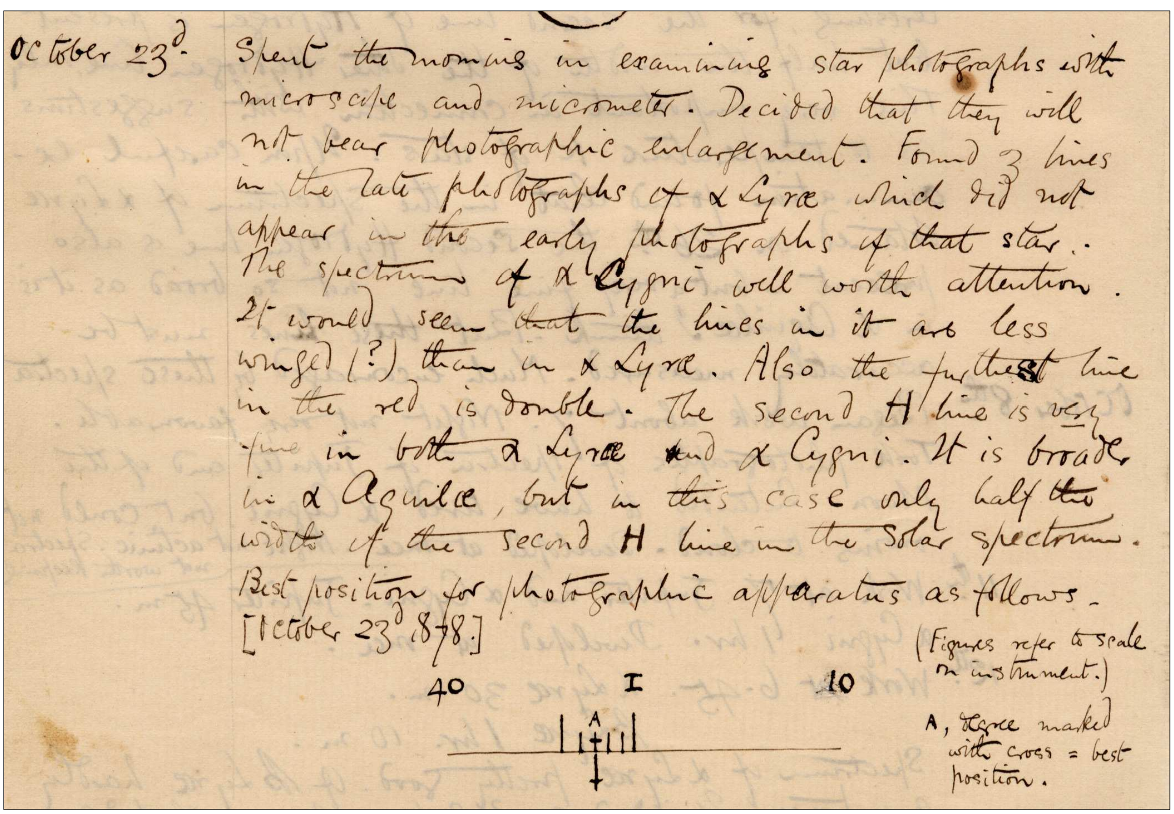
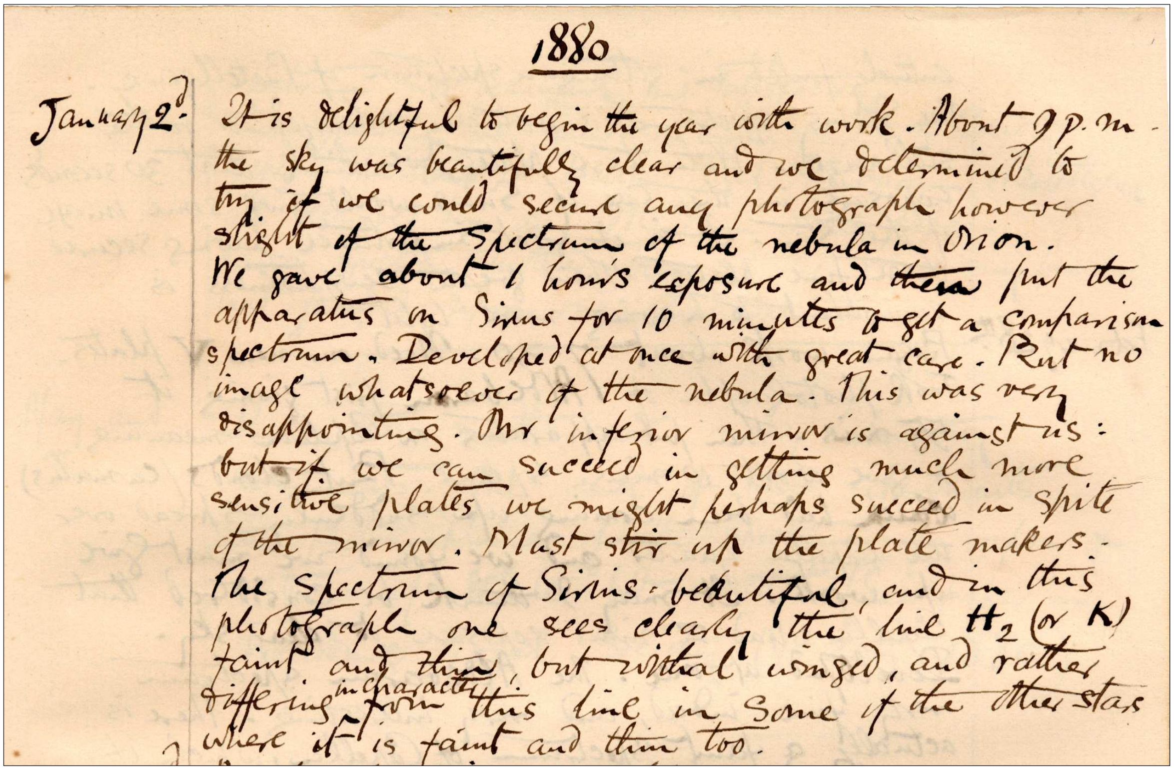

October 23d. Spent the morning in examining star photographs with microscope and micrometer. Decided that they will not bear photographic enlargement. Found 3 lines in the late photographs of α Lyrae which did not appear in the early photographs of that star. The spectrum of α Cygni well worth attention. It would seem that the lines in it are less winged (?) than in α Lyrae. Also the furthest line in the red is double. The second H line is very fine in both α Lyrae and α Cygni. It is broader in α Aquilae, but in this case only half the width of the second H line in the Solar spectrum. Best position for photographic apparatus as follows.
[October 23d 1878.]
(Figures refer to scale on instrument.)
40 I 10
A
A, degree marked with cross = best position.

Figure 10: Margaret Huggins' notebook page from 23 October 1878, with a reference to "… the second H line …" (after Huggins, W. and M., 1856-1870).


1880

January 2d. It is delightful to begin the year with work. About 9 p.m. the sky was beautifully clear and we determined to try if we could secure any photograph however slight of the spectrum of the nebula in Orion. We gave about 1 hour's exposure and then put the apparatus on Sirius for 10 minutes to get a comparison spectrum. Developed at once with great care. But no image whatsoever of the nebula. This was very disappointing. Our inferior mirror is against us: but if we can succeed in getting much more sensitive plates we might perhaps succeed in spite of the mirror. Must stir up the plate makers. The spectrum of Sirius beautiful, and in this photograph one sees clearly the line H_2 (or K) faint and thin, but withal winged, and rather differing in character from this line in some of the other stars where it is faint and thin too.

Figure 11: Margaret Huggins' notebook page from 2 January 1880, with a reference to "… the line H_2 (or K) …" (Huggins, W. and M., 1856-1870; both images by permission of Wellesley College, Margaret Clapp Library, Special Collections).

4 CONCLUSION

Fraunhofer never originally labeled the K line. It was considered and labeled by many as the twin of H. Several labels, including H_2, k, H′, H_{II}, were assigned to the line for over a century. Once its unique behavior was observed, the K identity was established.

It was in stellar spectroscopy, incidentally, that this identity came out fully.

5 NOTES

1. Becquerel was later to become the father of Antoine-Henri Becquerel (1852–1908), who discovered radioactivity and shared the third Nobel Prize in Physics with Pierre (1859–1906) and Marie Curie (1867–1934).
2. A French version of the book is available online as *Le Soleil* (1875).

6 ACKNOWLEDGMENTS

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Figure 12: Stained-glass window, one of three originally at the Huggins home and observatory and now at Wellesley College's Whitin Observatory, Wellesley, MA, USA. (courtesy: Whitin Observatory, Wellesley College).

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THE 1910 SOLAR CONFERENCE AND COOPERATION IN STELLAR SPECTROSCOPY

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Abstract: In the early twentieth century, cooperative astronomical programmes were not new: the Carte du Ciel project involved nearly twenty observatories. G.E. Hale's International Union for Cooperation in Solar Research, forerunner of the IAU, was organized in 1904. At the 1910 meeting of the American Astronomical Society, W.W. Campbell proposed to create a committee to foster cooperation in radial-velocity measurements. At the Pasadena meeting of the IUCSR, a scheme to pursue measurements of fainter stars emerged. Few observatories had telescopes sufficiently powerful for the work, the new 60-inch reflector at Mount Wilson being one of the exceptions. J.S. Plaskett, of the Dominion Observatory in Ottawa, brought into this group, determined that Canada would contribute. He was central to the eventual cooperative work in the 1920s and it was his 72-inch reflector at Victoria that became the template for a number of similar telescopes which would make significant contributions to stellar spectroscopy over the next forty years.

Keywords: spectroscopy, radial velocities, J.S. Plaskett, Lick Observatory, Mount Wilson Observatory, Dominion Astrophysical Observatory.

1 INTRODUCTION

While inter-institutional and international cooperation are commonplace in astronomy today, they were less important in the first third of the twentieth century. One of the most significant examples of cooperation was the joint programme of stellar radial-velocity measurements made by the Mount Wilson, Lick and Dominion Astrophysical Observatories in the 1920s. The origins of this venture lay in two meetings in the summer of 1910, the Harvard meeting of the Astronomical and Astrophysical Society of America and the conference of the Solar Union in Pasadena and Mount Wilson, California. While there were no immediate results from the discussions, cooperation to secure radial velocities of fainter stars was well underway by 1920. In the longer-term, this cooperative venture helped to set in motion the building of a generation of large, two-metre class reflectors designed for spectrographic research. Thanks to these instruments, radial velocity work continued into the 1960s. The figure linking the radial-velocity scheme and the eventual expansion of the instrumentation for stellar astronomy was the very junior participant of the former, John Stanley Plaskett (1865–1941). After brief discussions of the state of international cooperation in astronomy and radial-velocity research before 1910, I will turn to the meetings, their participants and the factors that worked against their vision. Next, Plaskett's role in these events and his participation in radial-velocity work on the 1920s and 1930s will be described, followed by remarks on where this programme led.

2 INTERNATIONAL COOPERATION IN ASTRONOMY

Although astronomers have been in contact with one another for more than two thousand years, one could argue that the first serious collaboration was implemented by Tycho Brahe (1546–1601), who maintained an impressive correspondence network and astronomical research institute. National networks, most notably centred on the Paris Observatory in the eighteenth and nineteenth centuries, linked observers for specific projects. Truly international gatherings date from a 1798 meeting organized by Baron F.X. von Zach (1754–1832) in Seeburg. Soon afterward, he and associates created the Vereinigte Astronomische Gesellschaft, partly to coordinate a search for the planet assumed to exist between the orbits of Mars and Jupiter. The modern Astronomische Gesellschaft was formed in Heidelberg in 1863 and by the end of that century was the most internationally-oriented astronomical society.

Organized cooperation in astronomical research emerged with the Carte du Ciel and Astrographic Catalogue project. Realizing the potential of photography for mapping stars, Admiral E.A.B. Mouchez (1821–1892), Director of the Paris Observatory, hosted an international meeting—the first Astrographic Conference—in Paris in April 1887. In this project, he had the firm support of David Gill (1843–1914), Director of the Cape Observatory. A key element of the Carte du Ciel was the idea of standardized photographic objectives. Of the twenty observatories—later twenty-two—willing to cooperate, European observatories mostly obtained their lenses from the Henry brothers in Paris, while the Empire observatories employed lenses designed by Gill and manufactured by Howard Grubb (1844–1931). An important contributor was the Royal Greenwich Observatory, where H.H. Turner (1861–1930) provided technical innovations for the project. Turner, who moved to Oxford in 1893, managed to complete about one-quarter of the Astrographic Catalogue. While still moving forward before World War I (Turner, 1912), the project was never completed: the Carte du Ciel never appeared but the catalogue of more than 4.6 million star positions later provided a base of comparison for the Hipparcos Catalogue of the 1990s.

In the early twentieth century, George Ellery Hale (Figure 1) took the next step in internationalization of astronomy with the formation of the International Union for Cooperation in Solar Research. As recently-named chairman of the Committee on Solar Research of the National Academy of Sciences, he had circularized colleagues around the world in 1904 to invite them to St. Louis to discuss cooperation. Delegates who attended agreed to form an international union for solar research with Hale at its head (Origins of the

Figure 1: George Ellery Hale (1868 –1938) in 1916 (University of Chicago Yerkes Observatory, courtesy: AIP Emilio Segrè Visual Archives).

Union, 1906). The second meeting was held at Oxford in September 1905, with a third meeting at Meudon in May 1907.

By the time of the Pasadena meeting in 1910 (Figure 2), the Solar Union had existed for only six years and there was no international organization that could encompass the rising interest in stellar astrophysics. It is not surprising that the idea to extend the Union's work to a broader field would be raised. There must have been backroom discussion of the idea; in the event, it was Karl Schwarzschild (Figure 3) who rose at the conference to make the motion: "Ich möchte dann beantragen 'Die Union erweitert ihr Arbeitsgebiet über die Sonnenphysik hinaus auf Astrophysik im allgemeinen'" ["I would like to then move that 'The Union broaden its sphere of work beyond solar physics to astrophysics in general'"] (*Proceedings of the Conference*, 1911: 111). Alfred Fowler (1868–1940), Heinrich Kayser (1853–1940) and Turner all spoke in favour of the idea and it passed without demur. This helped to legitimize the discussions of the Radial Velocity Committee within a framework of wider international cooperation. Schwarzschild's motion was also, in effect, a major step towards the transformation of the Solar Union into the International Astronomical Union.

A third international cooperative scheme was launched in 1906 to obtain stellar statistics: this was Jacobus Kapteyn's Plan of Selected Areas (Lynds, 1963). Recognizing that obtaining observational data for all stars in the Galaxy was impossible, Kapteyn (Figure 4) designated 206 (with later additions) representative and distributed areas for observation. Eventually more than forty observatories participated, with Harvard, Yerkes and Mount Wilson as major American contributors (van Rhijn, 1930). Of the three cooperative projects underway by 1910, only the work of the Solar Union was truly devoted to astrophysics.

3 RADIAL VELOCITIES IN STELLAR SPECTROSCOPY

It was Edmund Halley (1656–1742) who recognized in 1718 that stellar proper motions exist, but not until accurate stellar parallaxes were available in the nineteenth century could tangential velocities be measured. For a true three-dimensional sense of stellar motion, line-of-sight velocities were necessary. A detailed account of the development of radial-velocity work can be found in Hearnshaw (1986). Following Angelo Secchi's (1818–1878) suggestion, William Huggins (1824–1910) made the first attempts to measure stellar radial velocities in the late 1860s (Huggins, 1868). With a 15-inch refractor at Tulse Hill, he was able to measure radial velocities of thirty stars, which he published in 1872 (Huggins, 1872). These were visual observations. In the same year, Henry Draper (1837–1882) obtained the first spectrogram in the USA but his contemporaries, notably E.W. Maunder (1851–1928) at Greenwich and James E. Keeler (1857–1900), working with the Lick Observatory 36-inch refractor in 1890-1891, observed radial velocities visually.

Much enhanced accuracy came with the application of photography to recording the spectra by Hermann C. Vogel (1841–1907) and Julius Scheiner (1858–1913) at Potsdam. Vogel, following Huggins' lead, had made visual observations of radial velocities. Photographic work in collaboration with Scheiner commenced at the new Potsdam Astrophysical Observatory with the 30-cm refractor in 1887. By 1892, Vogel was able to publish the radial velocities of fifty-one stars (Vogel, 1892). During the 1890s, a number

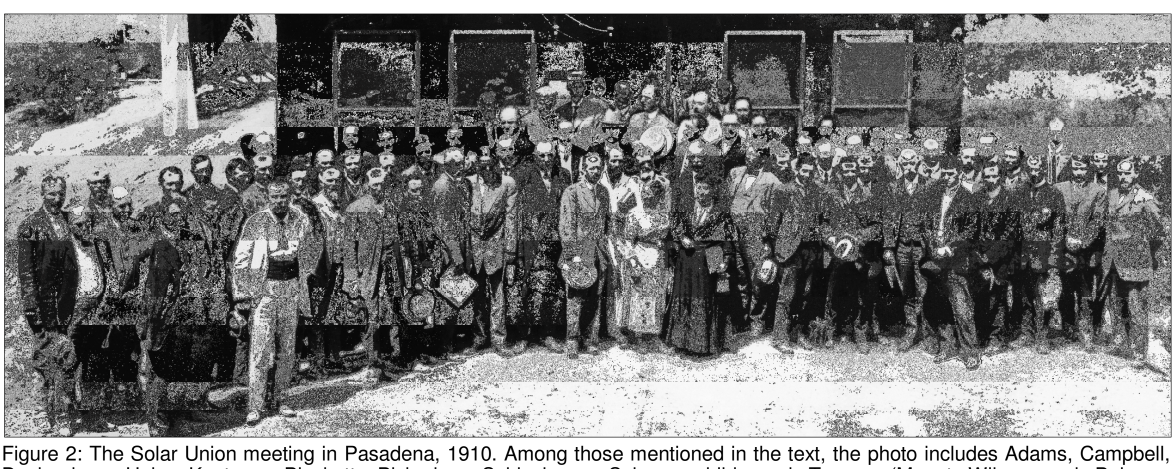

Figure 2: The Solar Union meeting in Pasadena, 1910. Among those mentioned in the text, the photo includes Adams, Campbell, Deslandres, Hale, Kapteyn, Plaskett, Pickering, Schlesinger, Schwarzschild, and Turner. (Mount Wilson and Palomar Observatories, courtesy AIP Emilio Segrè Visual Archives).

of other workers became active, including A.A. Belopolsky (1854–1934) at Pulkovo, Keeler at the Allegheny Observatory, H.-A. Deslandres (1853–1948) at Paris and Hugh F. Newall (1857–1944) at Cambridge. Even with large refractors, spectrograms were not easily obtained: Belopolsky, working with the 76-cm refractor with a spectrograph based upon Vogel's design, could obtain a spectrogram of a fourth magnitude star in an hour's exposure. Few telescopes in the world were larger than Pulkovo's; thus, unless much larger telescopes were constructed or much more efficient spectrographs and photographic emulsions became available, radial velocities for fainter stars would be a long time in coming.

By the late 1890s, the undisputed master of stellar spectroscopy was W.W. Campbell (Figure 5) at Lick Observatory. After Keeler's departure for Allegheny, Campbell experimented with Keeler's visual spectroscope and then had a new spectrograph built. Thanks to a donation from banker D.O. Mills (1825–1910), Campbell was able to design and have constructed a new spectrograph with Brashear optics. Ready in 1896, the Mills Spectrograph (Figure 6) utilized three prisms and included the iron arc comparison method pioneered by Vogel and Belopolsky. Campbell (1898) reported that he could obtain a satisfactory spectrum for a magnitude 5.0 star in about an hour. Attached to the world's second largest telescope, the Mills Spectrograph was a formidable instrument. Working with William H. Wright (1871–1959) from 1897, Campbell commenced an observing programme to obtain radial velocities of stars brighter than magnitude 5.51 within reach of Lick. Flexure and loss of light were serious problems with the Mills Spectrograph, leading Campbell to design a much improved model in 1902. After Campbell succeeded Keeler as Lick Director in 1901, he tapped Mills again for funds to create a southern station in Chile. With a 93-cm Cassegrain reflector, Wright (1911), who had been despatched to direct the D.O. Mills Expedition, was able to obtain the radial velocities of 150 southern stars by 1906.

But why amass radial-velocity data in the first place? The initial urge seems to have been to obtain a more complete idea about stellar motions. The first discoveries of spectroscopic binary stars by Vogel and by Edward C. Pickering (1846–1919) in 1889 launched an important facet of radial-velocity research as it was soon realized that spectroscopic binary stars could yield stellar masses (Batten, 1988). The Lick Observatory would become a major player in this arena. A more focused interest in stellar dynamics emerged with the announcement, at the International Congress of Arts and Science at the St. Louis Exposition in 1904 by Kapteyn, of the discovery of two star streams. While Kapteyn's data came from proper motions, it was immediately clear that radial velocities would provide valuable information on the structure of the Milky Way.

4 PLASKETT BUILDS HIS LINKS

J.S. Plaskett (Figure 7) backed into astronomy. A skilled mechanic, he had worked in industrial shops before being hired as mechanical assistant in the Physics Department at the University of Toronto in the 1890s. Already married with a family, he entered the University as a student and took his BA in physics, at the age

Figure 3: Karl Schwarzschild, 1873–1916 (courtesy: Springer-Verlag).

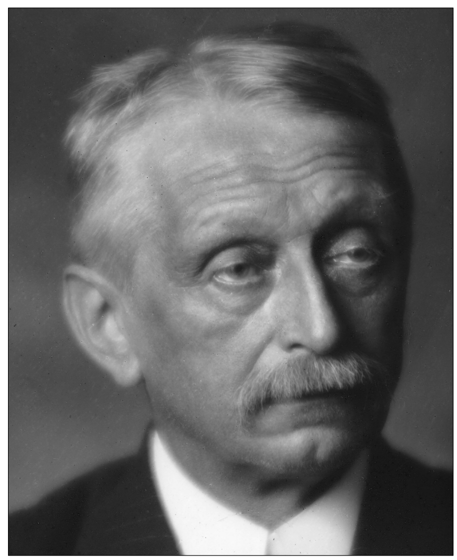

Figure 4: Jacobus Cornelius Kapteyn, 1851–1922 (courtesy: Adriaan Blaauw, University of Groningen).

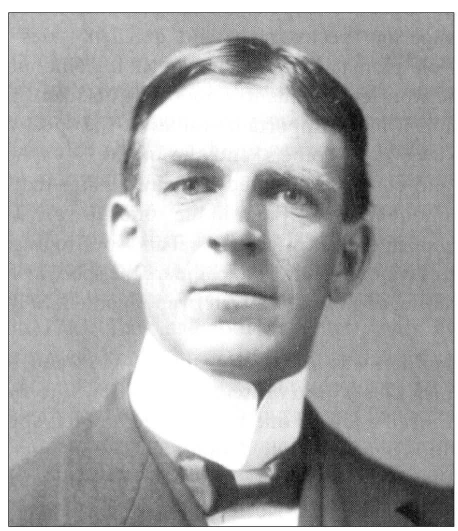

Figure 5: William Wallace Campbell, 1862–1938 (after Macpherson, 1905, facing p. 240).

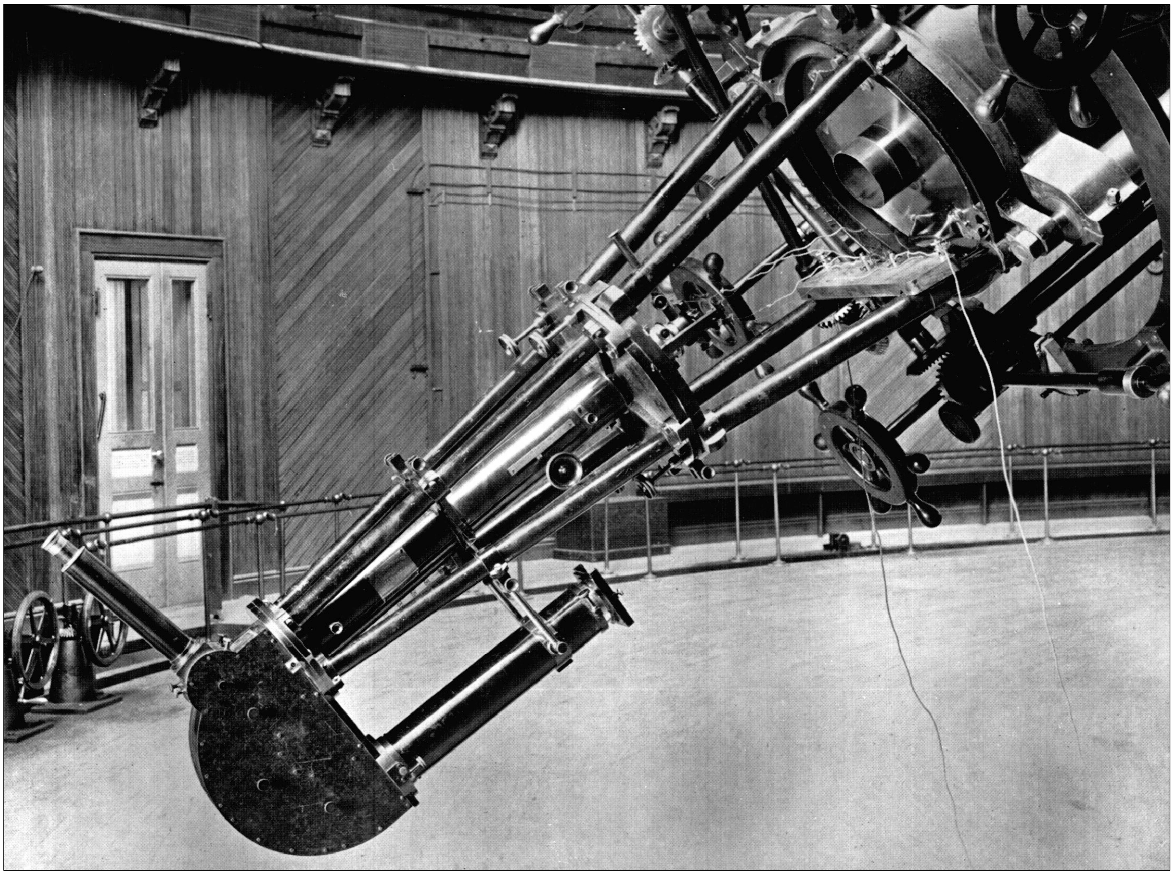

Figure 6: The Mills Spectrograph mounted on the Lick 36-inch refractor (after Campbell, 1928).

of thirty-three, in 1899. There was as yet no astronomy programme at the University and there is no indication of Plaskett's interest in the subject. Continuing

Figure 7: John S. Plaskett (courtesy: National Research Council of Canada, Herzberg Institute of Astrophysics, Dominion Astrophysical Observatory).

in the employ of the University, he turned his attention to experiments in colour photography. In 1902, upon hearing that the Dominion Observatory, then being erected in Ottawa, was seeking employees, Plaskett applied and was hired the following year as Mechanical Superintendent.

When the initial staff was assembled in 1903, there were only two permanent employees—Dr William F. King (1854–1916) and Otto J. Klotz (1852–1923)—both of whom were veterans of the survey of western Canada and neither of whom had a direct interest in astrophysics. Nonetheless, they ensured the Observatory was equipped with a Warner and Swasey 15-inch equatorial refractor with Brashear optics. With the telescope came an off-the-shelf spectrograph (Figure 8). None of the staff had any experience with such equipment, so Plaskett was placed in charge of the instrument. An ambitious man, he immediately began thinking about research projects for the Observatory and wrote to key figures in American astronomy. Early in 1906, with King's blessing, he undertook a 'grand tour' of American observatories, including Lick, Yerkes, Lowell, Mount Wilson, Allegheny, Flower, Harvard and the US Naval Observatory. This brought him into direct contact with later correspondents such as Campbell, Schlesinger and Hale. From a mechanic's point of view, the highlight was having Campbell show him the details of the rebuilt Mills Spectrograph at Lick.

From what he saw and heard, radial-velocity work appealed the most to him and he wrote to Edwin B.

Frost (1866–1935) at Yerkes for advice. Frost replied on 19 June 1906 that he believed the Dominion Observatory's telescope would be capable of dealing with brighter stars and spectroscopic binaries. As he noted, K.F. Küstner (1856–1936) in Bonn had been successful with a somewhat smaller instrument (Frost, 1906). At that point, Frost who, like Belopolsky, had trained under Vogel, was a key American radial-velocity researcher. Plaskett also approached Hale with the idea of entering solar research; during the decade, with much encouragement from Hale, Plaskett built a horizontal solar telescope and began working on solar rotation. It was this work that brought him into contact with Walter S. Adams (1876–1956) (Figure 9) at Mount Wilson and linked the Dominion Observatory to the Solar Union.

With the help of newly-hired junior assistants, Plaskett inaugurated work on radial velocities and spectroscopic binaries. He was open to cooperation, and Frost was known publicly as a proponent of cooperative work: in 1902, Frost published an article, "Co-operation in observing radial velocities of selected stars", in the *Astrophysical Journal*. In that year, he had distributed two circular letters to the key radial velocities workers—Belopolsky, Campbell, Deslandres, Gill, Newall, Vogel and Henry C. Lord (1866–1926) of the McMillin Observatory of the Ohio State University—to ask for their cooperation to observe twenty 'fundamental velocity stars' and to compare their measurements. Frost's proposal was received with approval. Once his own work was up and running, Plaskett turned to Frost for advice on what to do with his data. Frost (1907a) replied that

> In the present state of research on the radial velocities of stars, so much remains to be done that it seems important to avoid duplication of work in certain directions, and to secure instead cooperation where feasible.

He suggested that the *Astrophysical Journal* might be the vehicle to let people know what the Dominion Observatory planned. If others already have plates, they might share them. He invited Plaskett to announce his intentions in the pages of the Journal, of which he had been Editor since 1902. Plaskett did send a list of stars he intended to target; in reply, Frost (1907b) suggested that it might be time for the establishment of a Standing Committee on Stellar Spectroscopy in the Astronomical and Astrophysical Society of America.

5 THE COMMITTEE ON RADIAL VELOCITIES

From 17 to 19 August 1910 members of the Astronomical and Astrophysical Society of America gathered in Cambridge, Massachusetts (Figure 10). Their host was Edward C. Pickering, Director of the Harvard College Observatory and an internationally-recognized authority on stellar classification; he was also, like Hale, an apostle of international cooperation. Despite its name, the Society had a modest international flavour, with a handful of Canadian members and the occasional visitor from overseas. The 1910 meeting was noteworthy thanks to a contingent of European and British astronomers *en route* to Hale's Solar Union conference to be held in Pasadena a week later. Two proposals which would invoke international cooperation were mooted in Cambridge during the meeting. First was a proposal by Pickering that a committee be struck to find consensus on a stellar spectral-classification scheme. North American astronomers were familiar with Pickering's Henry Draper Catalog classification scheme but it was not uncontested: both Norman Lockyer (1836–1920) and Vogel had competing systems (see Hearnshaw, 2010).

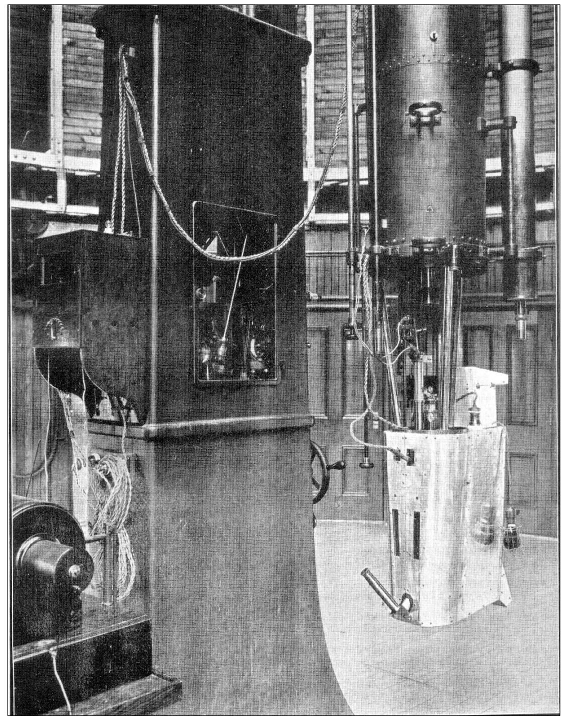

Figure 8: The spectrograph of the Dominion Observatory 15-inch refractor (*Report of the Chief Astronomer*, 1907).

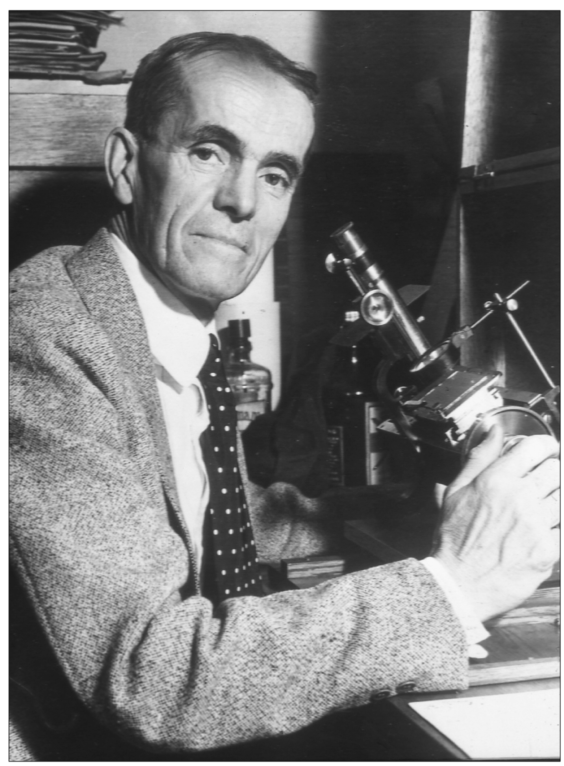

Figure 9: Walter S. Adams (courtesy: Yerkes Observatory, University of Chicago).

Figure 10: August 1910 meeting of the Astronomical and Astrophysical Society of America (after Plaskett, 1910).

The Committee on Classification was cooperative in the sense that Pickering was hoping to obtain wide consensus on a system which could be adopted as an international standard (DeVorkin, 1981). This form of cooperation—which combined intense Committee discussion, circulation of a questionnaire and commentaries from experts from a number of nations—was really a high-level form of housekeeping.

Pickering's team at Harvard had worked assiduously for years obtaining and classifying spectra. He was not asking for astronomers at other observatories to undertake a commitment for further, extensive observational and classificatory work. Detailed Committee discussion mostly occurred on the train west from Boston to California (Plotkin, 1978).

Somewhat overshadowed by the Classification Committee was a second Committee devoted to international cooperation in radial-velocity determinations. This was Campbell's dream, which he proposed in a letter of 9 August 1910 to William J. Hussey (1863–1926), Secretary of the Society. In it he suggested a Committee be struck "… to study and report upon the subject of cooperation on the part of observatories engaged in the measurement of stellar radial velocities." (Campbell, 1912b). Campbell's idea was a programme to observe stars fainter than fifth magnitude. There was no need for a cooperative effort for brighter stars, as Lick and its Chilean station had systematically worked through these—the programme largely finished in 1909—except for a few spectroscopic binaries. At the Cambridge meeting it was Campbell who proposed striking the Committee. It is not clear whether Campbell proposed names for the Committee, whether they volunteered or were proposed by third parties. All the members selected were to travel by train to California and to re-convene for further discussions during the Solar Union meeting. Those selected were a veritable 'Who's Who' of stellar spectroscopy (Figure 11): besides Campbell there was Newall from the Cambridge Solar Observatory; Frost of Yerkes; Johannes Hartmann (1865–1936) of Göttingen University; Karl Schwarzschild of the Potsdam Astrophysical Observatory; Frank Schlesinger (1871–1943) of the Allegheny Observatory and Plaskett of the Dominion Observatory. Hartmann and Percival Lowell (1855–1916) were added when the Committee came together at Mount Wilson (Plaskett, 1910), but the latter seems not to have participated in any meaningful way.

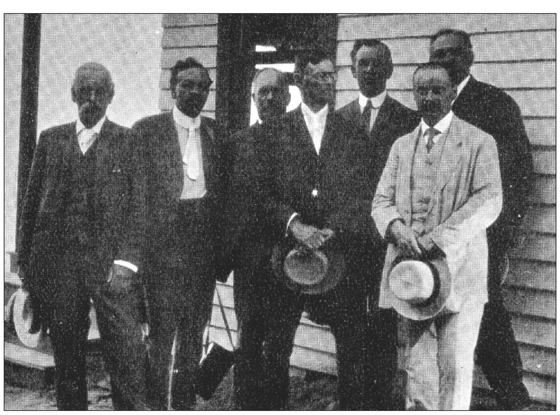

Figure 11: The Radial Velocity Committee at Mount Wilson. Left to right: Newall, Plaskett, Hartmann, Frost, Campbell, Schwarzschild, Schlesinger (Plaskett, 1910).

Curiously, two key workers in the field, Belopolsky and Deslandres, were not named for the Committee, although both were at Pasadena. They may have been sounded out but declined to join. Neither were they nor Lockyer named for the Classification Committee. There was considerable overlap of Pickering's Classification Committee and Campbell's Radial Velocity Committee, with Campbell, Frost, Newall, Plaskett, Schlesinger and Schwarzschild serving on both. Plaskett reported that the Committee had one meeting at Mount Wilson on 1 September "… where the question was discussed in a general way, with especial reference to means of overcoming the enormous loss of light in all modern spectrographs." (Plaskett, 1910:

376). By way of a preliminary report (Plaskett, 1911), Campbell wrote to Hussey after the meeting noting that the Committee felt that there was not the requisite instrumentation to tackle fainter stars and suggested that observatories focus on spectroscopic binaries. Committee members did not know in August 1910 whether the Solar Observatory at Mount Wilson would participate in their work; its two-year-old 60-inch telescope (Figure 12) was the most powerful instrument in the world. A few months after the meeting, Adams consulted with Campbell on future work and agreed to undertake radial-velocity observations of stars fainter than magnitude 5.5. Campbell was disappointed that so few could carry on the work. He noted that the spectrograph on the 36-inch refractor, when in the Littrow form, could obtain spectrograms of stars down to magnitude 7.0 with reasonable accuracy and exposure times. Plaskett's response to Campbell's preliminary report was to reaffirm his desire to improve spectrograph efficiency, given that he had no immediate prospect of a larger telescope.

6 THE AFTERMATH OF THE 1910 MEETINGS

Campbell had intended to provide a report of the Committee's deliberations at the 1911 Ottawa meeting of the Astronomical and Astrophysical Society of America. At the time of the meeting, he found himself in a Munich hospital recovering from typhoid fever; he sent his regrets to King, which were read out at the meeting (Chant, 1911). No publication or official report ever appeared. The energetic Campbell and his colleagues continued their work unabated at Lick. Systematic stellar radial-velocity work terminated with the publication in 1913 of a catalogue of 915 stars (Campbell, 1913a). During the decade from 1910 to 1919, Campbell, Wright and Joseph Moore (1878–1949) concentrated on nebular spectroscopy.

Most of the other Committee members abandoned radial-velocity work in the following decade. Newall had published a number of radial-velocity measurements in 1903 and 1905 and designed and built spectrographs. His site and instrument—he was effectively restricted to fourth-magnitude stars—were factors limiting further work although, in the event, a serious bout of illness led Newall to abandon stellar work and shift to solar research after 1904 (Hutchins, 2008: 301). He published nothing on radial velocities after the Solar Union meeting. Hartmann, although an accomplished spectroscopist, also published nothing further on the subject. Having lost the Directorship of the Potsdam Observatory to Schwarzschild, he had moved to Göttingen in 1909 where he had no access to modern equipment. Schwarzschild himself had never published on radial velocities. He joined the German army in 1914 and was dead from illness two years later. Schlesinger, at Allegheny Observatory, had the 30-inch Thaw refractor available from 1914 but his interest lay in stellar parallaxes, although his assistants, Zaccheus Daniel (1874–1964) and Frank Jordan (1865–1941), published a few papers on stellar radial velocities and binary star orbits. Parallaxes continued to be central for Schlesinger after his move to Yale in 1920. Frost had made a promising start at Yerkes but with the move of his collaborator, Adams, to California and a heavy administrative load, he essentially dropped out of active research. The increasing loss of his eyesight from 1915 spelled the end of observational work. That left only Plaskett, but he, having realized the value of the large reflector when he saw G.W. Ritchey's 60-inch telescope at Mount Wilson, put his energies into obtaining a large reflector for the Dominion Observatory. While some radial-velocity work continued at Ottawa, it was performed by his assistants. Thus, apart from Campbell and Plaskett, the personal commitment to radial-velocity work was lacking in the Committee members.

Campbell and Plaskett remained in touch. In 1911, the former described Lick's efforts in Chile and noted that the only observatories prosecuting radial-velocity work were Lick, Allegheny, Lowell, Ottawa and Yerkes in North America; Bonn, Potsdam and Pulkovo in Europe; and, in the southern hemisphere, only the Cape and the Lick station. Mount Wilson had just commenced work, and Michigan would soon join in with Ralph Curtiss' (1880–1929) new 37.5-inch reflector at the Detroit Observatory (Campbell, 1911b).

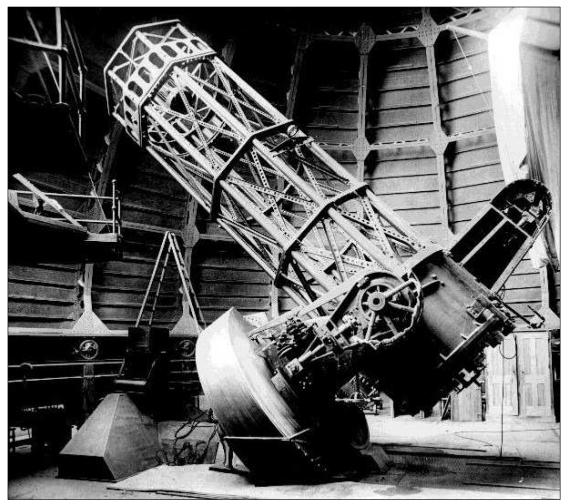

Figure 12: The 60-inch reflector at Mount Wilson with the three-prism spectrograph (after Adams, 1912: Plate 10).

Plaskett recognized he could not continue to participate in an international spectroscopic enterprise with the limited equipment in Ottawa. His quest to obtain a large reflector for the Observatory was focused upon keeping Canada (and himself) 'in the game', and he called upon his contacts for support at critical moments in his lobbying efforts. The Astronomical and Astrophysical Society of America met at the Dominion Observatory in 1911, the first meeting of the AASA outside the United States.[1] Plaskett had worked with King to lure the Society north, partly to obtain its sanction for the telescope project. When he heard that the Society had endorsed the scheme, Campbell (1911a) wrote to King to add his approbation:

> My investigations on the radial velocities of stars have led me to take a special interest in the Dominion Observatory's researches in the same field, by Dr. Plaskett. Considering the size of the telescope at Dr. Plaskett's command, his results have certainly been all that the most hopeful could have wished.

Campbell added that the work needed to be pressed to stars of fainter magnitude, so a large telescope was essential.

For further support, Plaskett called upon other members of the Committee on Radial Velocities—Schlesinger, Newall and Schwarzschild—along with Frank Dyson (1868–1939) at Greenwich and Pickering at Harvard. Schwarzschild (1912), in writing to King, lauded Plaskett's publications which

> … belong to the class which are awaited by us with eagerness for the reason that they are of the best in the province of radial velocity determinations.

For Schwarzschild, it had been a friendly rivalry but now astronomers needed to reach sixth-magnitude stars. He reminded King that the Committee formed in 1910 had hoped to partition the work for northern hemisphere observatories but only Lick and Mount Wilson were capable of reaching stars between magnitude 5 and 6.5. At the same time, in writing to

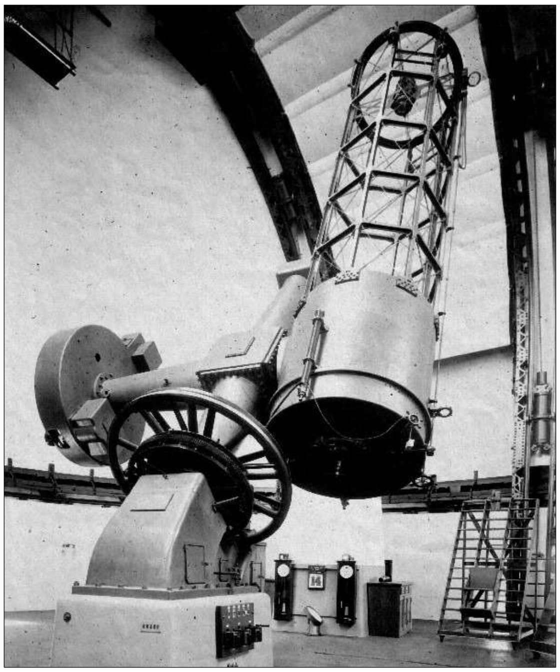

Figure 13: The 72-inch reflector of the Dominion Astrophysical Observatory, now rightly named the John S. Plaskett Telescope (courtesy: National Research Council of Canada, Herzberg Institute of Astrophysics, Dominion Astrophysical Observatory).

King, Campbell (1912a) reiterated the need for cooperation:

> There ought to be a coöperative organization of observatories possessing large telescopes to the end that the sky would be divided amongst these observatories for the spectrographic observations referred to. There is so much to be done that even half a dozen large institutions in each of the hemispheres cannot complete the work within a generation.

The project obtained the blessing of the Canadian Government but observations would not commence in Victoria until the spring of 1918.

It is clear that the Committee on Radial Velocities, despite its hope of international cooperation, could not follow through on its promise. First, there was the limitation of instruments capable of such work. Only two refractors, those at Lick and at Yerkes, were large enough to permit radial-velocity work on fainter stars. Campbell could obtain usable spectrograms of sixth-magnitude stars, but those required 2.5-hour exposures. While the Yerkes refractor was marginally larger, its site was inferior and its staff uninterested. Most of the other large refractors in America and Europe were in the 70-80 cm range; they were simply not capable of pushing the limits to fainter stars. Only one reflector of large size, the Mount Wilson 60-inch, was up to the challenge; larger reflectors at Mount Wilson and Victoria were almost a decade away. No large telescope projects were underway in Europe.

A second limitation came with the outbreak of war in 1914, which sundered international scientific cooperation. In October 1914 Adams, in remarking upon cooperation in solar research, admitted to Plaskett that

> … it seems questionable whether it would be desirable for us to inaugurate work which would require a considerable amount of cooperation during the present extraordinary state of international affair. (Adams, 1914).

This would have applied equally to stellar spectroscopy. German astronomers, so prominent in the rise of astronomical spectroscopy, were soon seen as pariahs by American, British and French astronomers. We need only recall that the British heard about Einstein's relativity through the conduit of de Sitter in the neutral Netherlands. Near war's end, Kapteyn and others argued to salvage international cooperation but to no avail (Kevles, 1971). When the International Astronomical Union came together in 1919, Hale, who had been a key scientific adviser to the American government, and his associates ensured that Germany was not part of the IAU.

7 A COOPERATIVE PROGRAMME FINALLY LAUNCHED

The hopes of the original Committee were quickly deflated. Then came the war. Only during the last year of the war did a gleam of hope for a revival of a cooperative venture appear. In this Plaskett was to become a pivotal actor. As early as 1913, when the Canadian Government agreed to order the 72-inch telescope, Plaskett had signaled that radial-velocity work would be its primary purpose, which Campbell (1913b) was pleased to hear. Hale had been kept in the loop and in November 1916 welcomed Plaskett's plan to come down to Pasadena to discuss the future observing programme for Victoria. By the next summer, work was sufficiently advanced for Plaskett to initiate planning. As he mentioned to Adams, with three large telescopes soon to be in operation on the west coast, cooperation would be needed; he had written Hale for advice but as he was away from Pasadena much of the time mostly in Washington for war work, Adams became the key contact and Plaskett (1917) planned to travel south for detailed discussions with him.

The 72-inch (1.83-m) telescope (Figure 13) began operations in May 1918. Plaskett had written to Kapteyn in late 1917 asking for advice. Kapteyn had suggested tackling stars in Boss' Preliminary General Catalogue, which had been published in 1910. When Campbell visited Victoria at this time, he committed Lick to a cooperative venture, offering to take brighter stars from declination –5º to the pole if Mount Wilson and the DAO would take the rest. But, as Plaskett reminded Campbell, there were only himself and R.K. Young (1886–1977) to do all the work; they could tackle 1,000-1,200 stars but no more (Plaskett, 1918a;

1918b). Hale was willing to cooperate and was sure that Adams would share in the work. Observing Boss stars would not work, however; Adams was well advanced on working through the list and expected to complete the work within eighteen months. As Plaskett told Campbell, there was no point in tackling Boss stars, although Hale had suggested observing all stars brighter than a certain limit in selected areas. Plaskett felt that the chance of Mount Wilson co-operating was now small. In the meantime, the DAO could tackle stars north of the celestial equator brighter than magnitude 6.5 or about 1,500 stars (Hale, 1918; Plaskett, 1918c). What would Campbell counsel? Campbell's reply (1918) suggests that he was never sanguine about the prospects:

> I read your first suggestions as to the cooperative plans and likewise the comments in your last letter. I think I made it clear, on your first mentioning the subject, that my expectation of the outcome is in good agreement with what the sequel developed. There would seem to be nothing gained by pursuing the subject further.

By the end of July 1918, however, Adams agreed to cooperate and to divvy up the remaining Boss stars with Lick and the DAO, while hoping that spectroscopic parallax observations would overlap to some extent. Plaskett knew that Mount Wilson had concentrated on later spectral types (F to M) and offered to begin with the O-type stars, which turned out later to have been fortuitous (Adams, 1918; Plaskett, 1918e; 1918f). The work was a grind: at Victoria, Plaskett and Young did all the observing—an entire night each—plus the measuring of plates and the computations with no assistance from staff like at Mount Wilson. Nonetheless, as he reported to Hale, it was routine work that a national institution should undertake and a contribution to the good of science (Plaskett, 1918d). By 1922, Adams and Alfred Joy (1882–1973) assembled a catalogue of 1,013 stars whose radial velocities had been measured from plates from both the 60-inch and 100-inch reflectors (Adams and Joy, 1922). In Victoria, Plaskett and Young had been joined by W.E. Harper (1878–1940) from the Ottawa staff and by Plaskett's son, Harry (1893–1980), allowing for a quickening of pace; their first fruits appeared in 1921 (Plaskett et al., 1921). Over time, staff would push the instrument to stars as faint as magnitude 9, depending upon the spectral type of the star and dispersion of the spectrograph (Batten, pers. comm.., 2010).

Radial-velocity work did continue at other observatories, but few could reach any but the brighter stars. Despite the interest in this field of research, the number of workers remained small. Struve and Zebergs (1962) estimated the number of professional astronomers in the world in 1920 at about 1,000. Of these, a search of the Harvard ADS for the period 1920-1925 shows approximately fifty people publishing on radial velocities. This was a rather small subset of astronomy, and the number of workers involved in systematic observation was considerably smaller. Up to this point, cooperation had been worked out on a personal basis; from the mid-1920s, the organizational focus for radial-velocity work was in the International Astronomical Union (IAU).

8 RADIAL-VELOCITY WORK IN A LARGER CONTEXT

Plaskett had maintained his contact with Hale after the 1910 meeting, although most of his correspondence with the Mount Wilson staff was with Adams. Hale was pleased to hear of the progress of the Victoria telescope, but international cooperation was still on his mind. Adams had shown him Plaskett's letter on radial-velocity cooperation; Hale (1918), as President of the National Research Council, had a special duty to foster such ventures. A few months later, the International Council of Scientific Unions, another pet project of Hale's, met in Paris. The delegates at the November 1918 meeting, wanting to expand the cooperative efforts already in progress with the Carte du Ciel and the Solar Union, voted to create the IAU.

Hale, in another of his roles as a member of the National Academy of Sciences, established the American section of the proposed Union in Washington, DC, in March 1919. At that time, eighteen research areas were identified for committees, one of which was radial velocities. One of the delegates was Schlesinger, Plaskett's long-time friend. They had spent time together at the 1913 Bonn meeting of the Solar Union and, although their research interests diverged, had maintained contact. Schlesinger (1919) put down Plaskett's name for the Radial Velocity Committee, although Plaskett was not an American. Joel Stebbins (1878–1966) confirmed that he would join Adams and Campbell on the American Committee. The three were asked to prepare a report which would be delivered to the organizational meeting, probably in July (Stebbins, 1919).

This must have been awkward for Plaskett. He reported to Hale on 3 July 1919 that he had recently attended a meeting of the Royal Society of Canada but no mention was made about Canadian participation in the new Union (Plaskett, 1919). Plaskett certainly desired Canadian membership in the Union and asked Hale's advice on how to proceed, considering the former was a member of an American Committee. Hale was supportive: "Be assured that it will be a pleasure for us to cooperate with you in every possible way." (Hale, 1919). Canada became an adhering nation in 1920 but when the first regular meeting of the IAU opened in Rome in 1922, Plaskett found that Dominion Astronomer Otto Klotz would be the Canadian delegate, with R.K. Young as second choice. This decision, he reported to Hale petulantly, had probably been 'engineered' by Ottawa as his success had never been well received (Plaskett, 1922a). In fact, Klotz and Plaskett had had cool relations since the latter's move to British Columbia. However, a year later Klotz was dead and Plaskett had much more freedom of action in Victoria. He was also soon in the thick of IAU activities. With the establishment of commissions at the Rome meeting, Plaskett was elected to Commission 27 (Variable Stars), Commission 29 (Stellar Classification) and Commission 30 (Radial Velocities). The west-coast group dominated Commission 30 for decades: Campbell was President for 1922-1932; followed by Plaskett for 1932-1935; by Adams for 1935-1948; and by Joseph A. Pearce (1893–1988), later a Director of the DAO, for 1948-1952. Over the years, five of the nineteen Commission Presidents have been Canadians.

Thanks to the on-going work on radial velocities of individual stars and published orbits of spectroscopic binary stars, accumulating data called for the production of regular catalogues. From the early 1920s, except for its southern station, Lick was largely out of the picture for radial-velocity work, although Moore contributed a few papers and Peter van de Kamp (1901 –1995) undertook a more detailed study of solar motion. Campbell's election to the Presidencies of the IAU and the American Astronomical Society in 1922 and his appointment as President of the University of California in 1922 largely removed him from active research. Lick's contribution in the 1920s and 1930s was to compile updates to Campbell's catalogue. The staff at Mount Wilson, on the other hand, with two large reflectors, continued to be active. Adams and Joy employed the 100-inch (2.54-m) reflector from 1918, continuing to concentrate upon the late-type stars. Two catalogues, one in 1923 and a second in 1929, provided the radial velocities of an additional 1,754 stars (Adams and Joy, 1923; Adams et al., 1929).

Both Adams and Plaskett had other interests in stellar spectroscopy. The former and his colleagues developed the method of spectroscopic parallax and pursued absolute magnitude observations, while the latter and his colleagues carried on their work on spectroscopic binaries. With Plaskett's appointment to the American Radial Velocity Committee, this opened another possibility for cooperation on spectroscopic binaries. In 1920, he suggested to Adams that the new Committee deal with the issue of spectroscopic binary observations piling up faster than anyone could deal with them; a meeting of the west-coast members of the Committee might be useful (Plaskett, 1920). With no immediate action, Plaskett (1922b) wrote to Campbell in April 1922 to suggest a meeting of the Mount Wilson, Lick and DAO staff to talk about further cooperation. What Campbell had in mind was a revision of his 1910 catalogue of spectroscopic binaries (Campbell, 1910). Young and Harper had hoped to undertake a new catalogue at Victoria, but evidently Campbell's wish prevailed. R.G. Aitken (1864–1951), another of Plaskett's long-time friends, was relieved to hear that the DAO would not pursue their project, which would please Campbell. Plaskett thought that Lick had abandoned the idea years earlier but agreed to provide his unpublished data; in fact, he had already written to Mount Wilson, Yerkes, Allegheny, Ann Arbor, the Cape, Vienna, Potsdam, Pulkovo and Yale to solicit material (Aitken, 1924; Plaskett, 1924). Adams (1924) was not pleased with the outcome:

> I am sorry that you are not going to compile the catalogue of spectroscopic binaries at Victoria. I feel that you would do it very much more satisfactorily than Dr. Campbell could in view of the great amount of work which he has on his hands. I suppose, however, that you are rather helpless in the matter so long as he presses his intentions to carry out this work.

Adams turned out to be partly right: it was not Campbell who undertook the third catalogue but Joseph Moore (1924). Moore also produced the fourth and fifth catalogues in 1936 and 1948, respectively. After Moore's death, the torch was passed to the DAO with the intention that Pearce and Petrie produce a new edition, but for various reasons neither could do so (Batten, pers. comm., 2010) and eventually Alan Batten (1967) produced the sixth catalogue in 1967.

By the late 1920s, the increasing mountain of spectroscopic binary star data called for a catalogue with weighted values. After discussion in IAU Commission 30, Plaskett (who was then the President) and its members delegated the task to Moore, who produced a catalogue of the radial velocities of more than 6,700 stars (Moore, 1932). The Lick Observatory monopoly on cataloguing was eventually broken: Ralph Wilson (1886–1960) of Mount Wilson, produced a much larger catalogue, of some 15,107 stars, in the early 1950s (Wilson, 1953). Three quarters of the radial-velocity determinations had come from Mount Wilson, Lick and the DAO.

The original impulse to undertake large-scale measurements of stellar radial velocities had come in the wake of Kapteyn's work on stellar motions. Harlow Shapley's (1885–1972) publication in 1922 suggesting that the centre of the Milky Way was as much as 20 kpc from the Solar System was a clue for Bertil Lindblad (1895–1965) to suggest our Galaxy is a disk with differential rotation (Shapley, 1922; Lindblad, 1925). Given the mass of radial-velocity data available by the late 1920s, Jan Oort (1927) was able to argue persuasively for the idea. Not surprisingly, some of his data came from publications from Lick, Mount Wilson and Victoria. As Plaskett and Pearce had accumulated data on O-type stars which, thanks to their intrinsic brightnesses, allowed for a probe to greater depths, plus a number of B-stype stars, they were able to confirm Oort's model almost immediately (see Plaskett, 1928; Plaskett and Pearce, 1934). In fact, Plaskett had visited Oort in Leiden in 1927 and was likely already attuned to Lindblad's and Oort's ideas. This was a satisfactory result of a decade's work.

9 CONCLUSION

Plaskett retired in 1935 but left a long-reaching legacy. Radial-velocity work continued at Victoria in the hands of Pearce and later Robert M. Petrie (1906–1966) and their colleagues, who turned to a more ambitious survey of O- and B-type stars with magnitudes down to 9.0. Others on the Victoria staff undertook more limited surveys of the radial velocities of A- and K-type stars. Young, one of Plaskett's early assistants at both Ottawa and Victoria, had moved to the Astronomy Department at the University of Toronto in 1924. Toronto had no large telescope but thanks to the unflagging efforts of the Department's head, Clarence Augustus Chant (1865–1956), the University opened the David Dunlap Observatory in 1935, which was equipped with a 74-inch (1.88-m) reflector patterned on the Victoria instrument. Young had supplied the technical expertise for the telescope's design. As this was an instrument that was built primarily for spectroscopy, radial-velocity work also continued at Toronto under Young and his successors as Director, Frank Hogg (1904–1951) and Jack Heard (1907–1976). Even as late as the mid-1950s, Henry King (1955: 312) could write in reference to Campbell's discovery that corrected radial velocities of stars change steadily with Harvard spectral class:

> This significant relationship together with work in radial velocities in general, has received and continues to receive the close attention of the astronomers at Mt. Wilson, at Victoria, B.C., and at Toronto.

The other long-term effect was, as I have argued elsewhere (Jarrell, 1999), the construction of a generation of two-metre reflectors based upon Plaskett's design. The work of most of these telescopes was spectroscopic as none was as powerful as the 100-inch and later 200-inch telescopes for extragalactic research. Hearnshaw (1986) notes a number of important stellar radial-velocity workers. Otto Struve (1897–1963) and his co-workers used the 82-inch reflector at the McDonald Observatory from 1939, reviving the Yerkes tradition. In the late 1940s, the 74-inch Radcliffe reflector in Pretoria was brought into play to re-inaugurate southern hemisphere radial-velocity work, particularly by A.D. Thackeray (1910–1978). A similar instrument at Mount Stromlo in the mid-1950s allowed for the expansion of Australian efforts in this work. While this important line of research lost its lustre by the 1960s, an enormous mass of data was available, much of it procured by the astronomers and instruments of the cooperative ventures that had their origins in 1910.

10 NOTES

1. The Society would take its current name, the American Astronomical Society, three years later.

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RG48 (followed by the volume number) = Dominion Astrophysical Observatory records
LAC = Library and Archives Canada.

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EXTRAORDINARY CLAIMS REQUIRE EXTRAORDINARY EVIDENCE: C.H. PAYNE, H.N. RUSSELL AND STANDARDS OF EVIDENCE IN EARLY QUANTITATIVE STELLAR SPECTROSCOPY

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Abstract: The ionization equilibrium theory of Meghnad Saha was hardly four years old, and still far from general acceptance, when a graduate student at Harvard University, Cecilia H. Payne, applied it to calibrate the Harvard spectral sequence as a temperature sequence. Payne indeed utilized Saha's relation not in its original form, but in its more acceptable form based upon a statistical mechanical re-derivation by E.A. Milne and R.H. Fowler. Her temperature calibration was, therefore, not at issue for her mentors at Harvard, such as Harlow Shapley, and her external reviewer for her Ph.D., Shapley's former teacher, the influential Princeton astronomer, Henry Norris Russell. Other conclusions she drew from her analysis, moreover, went beyond the evidence, they felt, and so she had to moderate her most provocative finding: that hydrogen dominated the atmospheres of the stars. She did so, however, in a manner that was designed to record for posterity that she was the first to make this observation, right or wrong. In so doing, Payne can be credited with profound political acumen, a quality that deserves more attention in the history of twentieth century astronomy.

Keywords: Cecilia Payne, Payne-Gaposchkin, Henry Norris Russell, Meghnad Saha, spectroscopy, solar composition, abundances of elements

1 INTRODUCTION

Much has been written of Cecilia Payne-Gaposchkin's (Figure 1) experiences at Harvard and how she arrived there to extend and refine E. Arthur Milne (1896–1950) and Ralph H. Fowler's (1889–1944) rederivation of Megnad Saha's (1894–1956) theory; how this led her to realize that the light elements hydrogen and helium dominated in the atmospheres of the Sun and stars; and how she was counseled to reject this conclusion in her thesis by her external advisor, Henry Norris Russell (1877–1957) of Princeton, in January 1925.[1] The issue at hand for many students and writers of astronomical lore in the past has been that in subsequent years, certainly well into the 1960s and 1970s, astronomers cited a 1929 paper by Russell as establishing the fact of hydrogen's dominance and typically failed to credit Payne. (For example, Aller, 1961: 118) This has been expressed here and there as a matter of concern and even an example of gender discrimination.[2] My purpose in this paper then is not to recapitulate the story, which is readily available in references noted here. Rather, I will explore the importance of considering the context of Russell's advice to Payne in terms of standard practice in that day.

In a 1984 appreciation of Cecilia Payne-Gaposchkin's contributions to astronomy, for instance, Katherine Haramundanis noted that in her mother's thesis "… conclusions were suppressed by her advisor, H.N. Russell, but she wisely published her data with a disclaimer." Since an immediately following sentence claims that she faced "… overt gender discrimination throughout her career …" (Haramundanis, 2006), naturally one would include Russell's action in this fact of her life. My purpose here is to suggest strongly that, in the case of Russell's actions in this singular instance, one must look beyond superficial impressions for what Russell was really trying to do, and what he accomplished. In no way do I want to minimize the fact that Payne did face considerable discrimination in her professional life, and her story certainly bears telling. Nor do I claim that Russell was particularly progressive in his views regarding gender inequalities. But I do feel that a deeper understanding of why Russell advised her to be doubtful of her conclusions about hydrogen and helium helps to illuminate standards of practice in astronomy in that day, standards that applied, in Russell's mind, to everyone.

For any astronomer, let alone a graduate student, even at Harvard, to demonstrate that the Universe is profoundly different than previously supposed, assumed or even determined to be, would be extraordinary. Astronomers and physicists from Henry Rowland (1848–1901), to Arthur Stanley Eddington (1882–1944), to Russell were very comfortable with the standard picture that the relative abundances of the elements in the Universe mimicked those found in the Earth's crust. By the early 1920s, using an abundance profile similar to the Earth's crust, Eddington had built up a mathematical model describing stars as gas spheres in radiative equilibrium that was very successful in describing their observed characteristics. When his model was able to recapitulate the observed

Figure 1 (left to right): Cecilia Payne (-Gaposchkin), 1900–1979, with Annie Jump Cannon, 1863–1941 (courtesy: *Sky & Telescope*).

mass-luminosity relation for both giants and dwarfs, it was a true watershed for theoretical astrophysics.

The larger historical significance of this episode, however, lies well beyond the fact that it marked the period when the truly modern notion of hydrogen's dominance emerged, reversing many decades of assumptions. This complete paradigm shift did not happen quickly, nor was it obvious to most astronomers during the transition, or in the aftermath. But it set the stage for modern views of how stars derive their energy, how they produced the elemental composition of the Universe observed today, and finally, how the Universe itself came into existence and changed through time. But as an historical moment, it also reveals a fundamental shift in what constituted acceptable practice in astronomy. It marked the end of what can be called the 'The Great Correlation Era.' Although there are no well-defined dates one can muster to identify when this era began or ended, it marks the period in early astrophysics before solid links were made between the physics of the atom, how that physics governs the light that all heated matter exhibits, and what that physics reveals through direct analysis of the spectra exhibited by celestial objects about their physical state.

2 THE GREAT CORRELATION ERA

The Great Correlation Era began in the 1860s when astronomers first began to describe the stars in terms of their spectra, correlating these with all available data about stars: their color, apparent brightness, absolute magnitude, motion, and eventually distribution in space. The discovery of spectral differences themselves drove astronomers to classify and reclassify them, adopting both linear and non-linear schemes in the hope of deducing fundamental knowledge about the nature of the visible Universe, thought then to be a single vast system of stars. Attempts were made continually to derive from these classes information on the properties of the stars—their masses, radii, composition, even ages. Schemes of stellar evolution were both derived from these systems and influenced them as well. Other correlations related spectral class to motion and position in the Galaxy, and to relative age. The intrinsic brightness of the stars seemed also to correlate closely with spectra, and there were details within stellar spectra that revealed relative luminosity. These correlations led to the general recognition that stars existed in luminosity classes as well as spectral classes. Photometric correlations starting with the period-luminosity relation also provided critical new clues and new techniques, ranging from vastly extended powers of determining the distances to objects in space, to exploring the nature of stellar structure, the conditions required for stability, and instability.

Other purely empirical correlations emerged within this era, lasting into the 1920s with vestiges and resurgences lasting into the modern era (the direct consequence of new technologies and new ways of perceiving the Universe revealing new phenomena). The Great Correlation Era, however, merged into the still present 'Correlation Era' that continues today, where some form of physical theory, deriving from atomic, quantum, nuclear or particle physics has either been applied, or in fact has stimulated astrophysical knowledge.

These empirical correlations were highly regarded as steppingstones to new knowledge. But by the second decade of the twentieth century as correlation upon correlation emerged and as one built upon another, thoughtful astronomers knew that astrophysical correlations lacked a rational physical framework. For these astronomers, astronomical knowledge required some form of relationship to a rational framework. After all, for well over a century and a half, astronomy had been extraordinarily successful interpreting and reducing its observations of position and motion of comets, planets and even stars using the rational framework called Newtonian physics. Physical measurements of brightness and spectra and correlations between them, however, emerged without a universally-accepted interpretive framework.

As one example, the spectroscopic parallax technique was a great discovery, but like other empirical relationships in spectroscopic astronomy, no one had a clue as to why this one existed. This bothered the astronomer most credited with its discovery, Walter Sydney Adams (1876–1956) of Mount Wilson. Why would certain line intensity ratios be an indicator of vast differences in luminosity? In 1916 Adams asked Eddington if he had any ideas, wishing that "… we had more physical knowledge regarding the interpretations of stellar spectra." (Adams, n.d.). He also confided his doubts to Russell in 1917, concerned that the laboratory evidence he and the physicist Henry Gale (1874–1942) had collected was not an explanation. Why did reduced pressure favor the strengths of some lines and not others? (Adams, 1917). Neither Russell nor Eddington could shed any useful light on the subject at the time, but they all knew that spectra harbored clues to varying conditions of temperature, density and pressure in stellar atmospheres (DeVorkin, 1999).

3 APPLYING SAHA'S THEORY

Even though Meghnad Saha's theory was the first to demonstrate that one could analyze stellar spectra by the relative strength of lines of elements in differing stages of ionization, and from these assess temperature and pressure in the stellar atmosphere, thus creating the first solid link between the laboratory, the physical theory of atoms, and the stars, it was not universally accepted in the original form presented by Saha. Saha had not derived his equation and its consequences using rigorous physical theory. Rather, he made many assumptions about the physical state of the stellar atmosphere, and also simplified his derivation assuming that the stellar atmosphere was completely homogeneous and consisted of one element only. His theory was an ingenious pastiche of chemical thermodynamics, Bohr theory and equilibrium theory. He was considered at best a marginal figure, based as he was at Calcutta University, and so working on the periphery, whose revelations required rederivation and refinement using more acceptable means. His theory was regarded as an important breakthrough, but not something one could use to fundamentally change the way astronomers thought about the Universe.

Although British theorists like E. Arthur Milne (1896–1950) keenly recognized that Saha had closed a "… gap in the logical argument …" rationalizing a "… definite relation between effective temperature and type of spectrum …" (Milne, 1923: 95), they also knew that the methods Saha had employed would not lead to reliable quantitative knowledge of the physics of the stars. Therefore Milne and R.H. Fowler (1889–1944) set about rederiving Saha's relationship, based upon the systematic application of statistical mechanics. By 1923 they also directed one of their promising young students, Cecilia Payne, to explore in greater detail just how well the actual spectral sequence exhibited by the stars agreed with their revised theory.

The edifice Payne built upon was, therefore, far from rock solid. Eddington originally reviewed Saha's papers feeling that he was on the right track, but the details "… must be rather shaky." (Eddington, 1920). Saha also painfully knew that he had made many assumptions about the physical state of the stellar atmosphere, and that as yet the amount of observational data available to him to test his theory was inadequate. Saha also knew well that these data resided in the United States in Massachusetts and in California, and he tried unsuccessfully to obtain support from George Ellery Hale to visit Mount Wilson.

Payne's arrival at Harvard must be appreciated in terms of the fact that it was just then that the Great Correlation Era was on the wane, merging into the normative correlation era that benefitted from an emerging interpretive framework based upon applicable physical theory. At least it was a time when, finally, physical theory had developed to the point where it could be applied to the stars, or, more to the point, provide a new independent perspective from which observed correlations might be rationalized. The problem was that although astronomers were willing to utilize physical theory *post hoc* to rationalize correlative phenomena, they were neither equipped nor willing to exploit this new and potentially revolutionary tool as a central and defining element for designing their research programming. Russell (as well as George Ellery Hale—1868–1938) was among the very few Americans who advocated this latter approach, which was becoming more acceptable practice in Europe. Russell in particular was a leader in this charge.

Russell (1920) also viewed the Harvard College Observatory as a "… land of settled habits." It was a place where the data had been gathered in over the past forty years that formed much of the evidentiary basis for the Great Correlation Era underlying the period-luminosity relation and Russell's version of the HR diagram. But it was being increasingly challenged by Hale's Mount Wilson staff and others more attuned to what Russell saw was the most effective path to new knowledge. "If I had to run the place," Russell advised Harlow Shapley (1885–1972) in January 1920, "I think that I would plan to draw in sharply on the large routine jobs …" Echoing his philosophy expressed in an essay on "Some problems in sidereal astronomy" for the National Research Council the previous year, Russell (1920) would turn the staff to "… investigations on specific problems,—large problems, not in extent, but in content."

When Shapley (Figure 2) assumed the Directorship at Harvard later that year, Russell had every expectation that he would follow this philosophy. Shapley, of course, encouraged this expectation by asking Russell to be an external advisor to the Harvard staff, making frequent visits to Cambridge to consult, lecture, and interact with staff at all levels. Russell enjoyed this responsibility, since it also put him into contact with the data he so much desired. Shapley, however, did not adhere at all to Russell's view of what a modern observatory needed to do to be competitive. In fact, Shapley extended the so-called factory system that Pickering had so deliberately created.[3]

4 PAYNE'S THESIS AND RUSSELL'S ADVICE

Russell started questioning Shapley's priorities and his oversight after he sent his newest graduate student, Donald Menzel (1901–1976), to Harvard to work in the plate stacks to answer the same questions Cecilia Payne was asking. Russell scolded Shapley; if Shapley had told him about Payne's parallel interests, "I should have set Menzel at something else." As a result, Russell followed Payne's progress, and took special care to advise her at various points in her work, visiting the Observatory between October and November 1924 when she was deeply involved in determining relative abundances. Russell was especially attentive to her needs, agreeing with her plan to utilize Saha's marginal appearance technique and suggesting that she confine her attention to giant stars (DeVorkin, 2000: 201-204).

Figure 2: H.N. Russell and H. Shapley during the 1938 IAU meeting in Stockholm. (Photo by Dorothy Davis Locanthi; courtesy: E. Segrè Visual Archives, American Institute of Physics).

After meeting and working with Russell on these occasions, at Harvard and also at AAAS meetings in Washington in January 1925, seeing him in action, Russell's power and authority were, to Payne, very real. Yet she found that he could be charming as long as they avoided astrophysics. As she confided to Margaret Harwood (1885–1979), she could not allow herself to fear such a clever man (Payne, 1925a), but she was astute enough to sense that

> His power in the astronomical world is another matter, and I shall fear that to my dying day, as the fate of such as I could be sealed by him with a word.

She had been sending Russell drafts by then, but it took Russell some time to get to them. When he did have a chance to fully absorb her work, and her estimates of relative abundances, Russell (1925) advised caution:

> It seems evident to me that one further step which will be necessary before we can fully utilize thermodynamic principles for abundance calculation is to have at least an approximate theory concerning the relative number of atoms in a given state which will absorb various lines originating in this state … I believe that this question of intensities, that is, of probabilities of quantum jumps, is the next big problem in spectroscopy; but even now we may make approximate allowances for it.

For Russell, Saha's methods were only an interim step, useful for their heuristic value. Russell's colleague, John Q. Stewart (1894–1972), was then beginning to explore abundance effects throughout an inhomogeneous atmospheric layer, looking especially for line-broadening due to differential pressure and scattering, and he hoped his work would clarify the matter. Russell was keenly aware of the many unknowns and carefully coached Payne as to what tone to take in her reports. All this was happening at the same time he strongly recommended her for a National Research Fellowship, which he was delighted to see come through during this time. He also recommended her for a major observatory position in Canada, as she was "… quite the best of the young folks …" in astrophysics at Harvard (Kidwell, 1984: 25).

TABLE XXVIII

Atomic Number	Atom	Log a_r	Atomic Number	Atom	Log a_r	Atomic Number	Atom	Log a_r
1	H	11	13	Al	5.0	23	V	3.0
2	He	8.3	14	Si	4.8	24	Cr	3.9
	He+	12		Si+	4.9	25	Mn	4.6
3	Li	0.0		Si+++	6.0	26	Fe	4.8
6	C+	4.5	19	K	3.5	30	Zn	4.2
11	Na	5.2	20	Ca	4.8	38	Sr	1.8
12	Mg	5.6		Ca+	5.0		Sr+	1.5
	Mg+	5.5	22	Ti	4.1	54	Ba+	1.1

Figure 3: Table xxviii from Payne's thesis identifies hydrogen and helium as hugely abundant. (Payne 1925b: 186).

From the passage quoted above, however, it is clear that even though he knew Payne was using Saha's thermodynamic methods, and encouraged it, Russell keenly sensed their limitations. By then she had utilized the theory to calibrate the temperature sequence of Harvard spectra, and very creatively used line ratios in the spectra over varying classes to estimate ionization potentials. All of this was acceptable to Russell because it did not revolutionize received views, only refined and extended knowledge. The hydrogen anomaly was quite another thing, he felt. "It is clearly impossible that hydrogen should be a million times more abundant than the metals …" he wrote her in January, "… there seems to be a real tendency for lines, for which both the ionization and excitation potentials are large, to be much stronger than the elementary theory would indicate." (Russell, 1925; cf. DeVorkin, 2000: 204; Kidwell, 1984: 19-20).

Russell provided detailed and reasoned arguments. Payne followed them, of course, and in her thesis presented her conclusions for hydrogen and helium as a direct outcome of her methodology, and claimed, echoing and citing Russell as source, that the results were "… almost certainly not real." Far from suppressing her results, Russell in fact approved the thesis in its entirety in April 1925, including the summary table of her results (Figure 3). Table XXVIII from Payne's thesis, where a_r is the relative abundance of the element, identifies hydrogen and helium as hugely abundant. (The third column is log a_r.) Payne however, noted in conclusion: "Although hydrogen and helium are manifestly very abundant in stellar atmospheres, the actual values derived from the estimates of marginal appearance are regarded as spurious." (Payne 1926: 186). Note, too, that the relative abundance of hydrogen and helium is reversed from today's values. (DeVorkin, 2000: 204; Kidwell, 1984: 22).

Proper practice for Russell, then, at that time, was to present results but moderate confidence in them by the strength of the techniques and processes employed to achieve those results. In his own scientific career, Russell knew rejection from his seniors when he overstated his case. Early on, his mathematical method of hypothetical parallax determination, based upon his analysis of double star orbits, while in and of themselves not in question, were considered inappropriate by leading senior astronomers like S.W. Burnham, given the poor quality of data available at the time (DeVorkin, 2000: 82-83). He carried this experience a bit painfully through his life as a cautionary tale, but continued to squeeze as much knowledge as he could from data that were at hand. By the time he had gained position and prominence in the 1920s, and legendary status in the 1930s, he openly promoted the heuristic value of weaving new knowledge from "… a tissue of approximations." (DeVorkin, 2000: 273-274; 366). By that time he well knew that he could get away with arguments that less well-placed colleagues could not. He never suggested that any of his graduate students, or anyone of less stature than a mature colleague, take such risks.

If she wasn't already, Payne soon became aware of this fact of professional life as Russell saw it. With her thesis finished, Shapley piled all sorts of tasks on her desk. One was to be the internal editor for manuscripts by other staff (Payne-Gaposchkin, 1984: Chapter 14). In May 1926, Shapley sent Russell a manuscript by a Harvard graduate student named Davidovitch and asked him to respond to Payne who was responsible for putting it into publishable form. Davidovitch had written on Nova Pictoris, and Russell felt it contained some useful material that was worth publishing. However, Russell felt that the author had "… seriously over-discussed his material." He found some of his applications "… rather amusing …" and made editorial suggestions as well (Russell, 1926), feeling the paper should be "… toned down a little, introducing some judicial weasel words to make the statements less positive." This exchange, repeated more than once, shows that Russell acted consistently, no matter the gender of the author, counseling humility and avoiding, at all costs, over-confidence. This was acceptable practice in that day.

After 1925, Russell also started to end his letters to Payne with personal admissions of his own inability to set personal limits on his time and energy. He would do this only with his colleagues, those he respected as members of his circle. In 1926, he admitted to her that he had exhausted himself completing his textbook, and soon started asking her to help out with professional tasks such as writing reviews for core journals of seminal books by authors as eminent as Eddington.[4]

Although Russell cautioned Payne to qualify her results on hydrogen and helium, he apparently never doubted that her derivation was internally consistent and that the results indicated that the light elements appeared to be anomalously strong in the atmospheres of giant stars. In Volume II of his influential 1927 textbook Russell cited Payne as authority for confirming, finally, that "… the uniformity of composition of stellar atmospheres appears to be an established fact." He also singled out her result that hydrogen and helium appear anomalously abundant, and 'puzzling', because hydrogen itself appears in virtually all spectral classes from the coolest M-stars through the hottest O-stars (Russell, Dugan and Stewart, 1927: 869). Here though, Russell cited Svein Rosseland's (1894–1985) speculation that hydrogen was highly concentrated in stellar atmospheres, having been rejected from the interior. Nevertheless, even at the time of writing the textbook, in 1925–1926, Russell was puzzled by hydrogen's high visibility throughout the spectral ranges of the stars, given that the excitation potential for its Balmer series was so high that "… only a very small fraction of all the hydrogen atoms in the atmosphere, even of an A-star, should be in a condition to absorb these lines." And he concluded (ibid.: 869-870):

> Unless some unrecognized influence is at work, it is not easy to see how so small a proportion of excited atoms can produce the strong lines which are observed.

Russell would eventually use just these arguments in his 1929 paper to push for hydrogen's dominance. By then, as is well known, Russell had confirmed the results of Payne's 1925 thesis using independently-derived arguments centered on the physics of the hydrogen atom. Indeed, he prominently cited Payne's thesis there (Russell, 1929: 64) as "The most important previous determination of the abundance of the elements by astrophysical means …" As I argue in greater detail elsewhere (DeVorkin, 2000, Chapter 14), it was through this form of persuasion, employing the most basic knowledge of the physics of the atom as his primary argument, that Russell felt that such a revolutionary reversal of commonly held opinion would be accepted. Evidently he was right.

5 NOTES

1. On Payne's contributions, see Payne-Gaposchkin, (1984) and Kidwell (1984). See, also, DeVorkin, (2000: Chapter 14). Detailed background on Saha, Milne, Fowler, Russell and Payne's contributions to the hydrogen abundance problem, and why it was a problem, can be found in DeVorkin and Kenat, (1983a; 1983b) and DeVorkin (1996).
2. See, for instance, Contributions of 20th Century Women to Physics, 1995-2001. This singular accomplishment is raised frequently in recalling Payne-Gaposchkin's life. See also *The Starry Universe: The Cecilia Payne-Gaposchkin Centenary*, 2000. Published sources include Gingerich (1982) and Kidwell (1982).
3. On the factory system see Lankford and Slavings (1996) and Smith (1991).
4. Russell (1927). In this instance as he was already reviewing Eddington's *Internal Constitution of the Stars* for the *Astrophysical Journal*, he recommended that Payne review it for the *Physical Review*, since she was the "… best person in the country …" for the task.

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PUL/HNR = Princeton University Library, Henry Norris Russell Papers.
SLRC/MH = Schlesinger Library, Radcliffe College, Margaret Harwood papers.

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CHARLOTTE MOORE SITTERLY

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Abstract: Charlotte Moore Sitterly was a scientist in an era when it was rare for a woman to have the opportunity to devote her life to forefront science. Following her graduation from Swarthmore College in 1920, she accepted a position at Princeton University as an assistant to Henry Norris Russell. In 1925 she started a study of the solar spectrum. She could then not know that she would devote much of her scientific career to gathering basic atomic data that are invaluable to the scientific community, even today. In 1931 she obtained a Ph.D. degree at the University of California, Berkeley, and returned to Princeton as a staff member of the Princeton University Observatory. In 1945 Moore moved to the National Bureau of Standards (NBS), to supervise preparation of the widely-used tables of atomic energy levels. Following the successful launching (1946) of a V2 rocket to obtain the ultraviolet spectrum of the Sun, she started working also with Richard Tousey and his group at the Naval Research Laboratory (NRL). Ultimately, they extended the solar spectrum down to 2200 angstroms. She continued her affiliations with both the NBS and the NRL until her death in 1990. Charlotte Moore was a rare scientist who devoted her career to obtaining accurate numbers, thus enabling the scientific community to open her tables and know that the data are reliable.

Keywords: Sitterly, Moore, multiplet tables.

1 INTRODUCTION

Charlotte Moore (Figure 1) was born in 1898 in Ercildoun, a country village in Chester County, Pennsylvania. This was an auspicious time to be born for one who would spend her adult life insuring that spectroscopists would have access to accurate wavelengths. One year before her birth, Henry A. Rowland, Johns Hopkins University's first Professor of Physics, completed publication of 20,000 lines in the solar spectrum. These would become part of Charlotte Moore's library, and today astronomers still use Rowland-style gratings. One year after her birth, Lewis E. Jewell of Johns Hopkins defended his (Jewell's) measurements of Hδ in the solar spectrum. It was Jewell who measured and calculated the values published in Rowland's tables. In that same year, 1899, Julius Scheiner, at the Potsdam Observatory, published the first-known spectrum of the Andromeda Galaxy, and announced that it was composed of stars like the Sun. Charlotte was two years old when Samuel P. Langley extended the solar spectrum still farther into the infrared (see Figure 2). Charlotte was 16 years old when Bohr advanced his model of the hydrogen atom.

Charlotte's parents had no college degrees, but they were teachers who taught in an 'old style' (her term) academy and later became interested in public school education. They worked in the education system of Chester County for many years, and her father became Superintendent of Public Schools. Her parents were strict about learning, especially grammar. Charlotte was the youngest in a family of three daughters and one son. They all attended public schools.

Because of their Friends (Quaker) background, her parents chose Swarthmore College for Charlotte, who apparently had little choice in the decision. When interviewed by David DeVorkin in 1978 (Sitterly, 1978), Moore said:

> We were not well off financially. I had to do a lot of extra work to get through college. I used to do substitute teaching because it paid well. Substitute teaching and tutoring were the two fields in which a woman could get some money toward working her way through college; almost everything else favored the men. I taught every grade from first grade up to senior high school.

Moore's attitude toward college was to learn as much as possible in various fields. Ultimately she majored in mathematics, and took an astronomy course using Forest Ray Moulton's general astronomy text. She graduated in 1920, but failed to get the graduate fellowship that she applied for. Her advisor, John A. Miller, surely recognized her as an unusual student, when he suggested that she consider taking a job with

Figure 1: Charlotte Emma Moore Sitterly, 1898–1990 (courtesy: National Institute of Standards and Technology).

Henry Norris Russell at Princeton. She neither made an application nor met Russell in advance, but the job was hers. Her brilliance was evident even then.

In those years, Russell was devoting his time to problems of spectroscopy, chemical abundances, and stellar evolution. When Charlotte joined him at Princeton, she was his only assistant, although the work he needed done would have taxed several. Her office was located in an old back room over the furnace, from which she breathed coal fumes all the time. As Russell's 'assistant' she supplied Russell with whatever materials he needed, and he usually needed them in a hurry. She got to know Russell's graduate students, Donald Menzel, Theodore Dunham, and Bancroft Sitterly, among others. She would marry Bancroft over a decade later.

Nancy Roman (1991: 1492) has written:

> Russell was a quick and brilliant thinker. Although he did tell Charlotte how to do the various things he wanted done, his explanations were quick, leaving her to fill in the details and all of the background.

Her first job was to determine an accurate position of the Moon from photographic plates, a procedure that Charlotte described as 'terribly involved'. Shortly after, she was working on double stars and a "… wealth of spectroscopic material." (Sitterley, 1978) She attended some of Russell's evening graduate school lectures, and within five years she was the first author on a Moore-Russell publication (Moore and Russell, 1926).

She was also exhausted. So in 1925 she asked Russell for leave to work at Mt. Wilson, to help with the revision of the *Rowland Astronomical Tables* (St. John, et al, 1928). She worked mostly with Charles St. John, and was called a computer, as were all the women working there. She considered this an insult. She remained in touch with Russell, and saw him each summer when he spent one month at Mt. Wilson. Charlotte returned to Princeton in 1928, continuing the spectroscopic work. In 1931 she earned her Ph.D. from the University of California at Berkeley, after two years of graduate study while Russell was away in Europe. The first year she took courses at Berkeley, but she spent the second year at Mt. Wilson Observatory offices in Pasadena analyzing sunspot spectra. Princeton's graduate school would open to special women only in 1961.

Although she could have accepted other positions, Charlotte returned to Princeton in 1931, and Russell offered her a Research Assistantship. In 1936, she became a Research Associate. In 1937, she married Bancroft Sitterly, the graduate student she had known since their Princeton days in the early 1920s. She continued to gather data for Russell, and he continued to ask her for "…his colossal jobs. He never asked for small ones. He thought I over stressed (accuracy), but I would never give him a list that I wouldn't stand in back of." (Sitterly, 1978). Perhaps due to her Quaker upbringing and to her parents' examples, Charlotte may have recognized that her major role in science could be to make accurate wavelength values available to the scientific community. But regardless of her intentions, she is remembered best for the several editions of her monumental work, *A Multiplet Table of Astrophysical Interest* (Moore, 1933, 1945, reprints in 1959 and 1972; Figure 3). Even today, these publications identify her as an important scientist.

In 1945, Russell was failing, and Charlotte moved to the US National Bureau of Standards. She continued some work with Russell, but she intended to complete her work on the infrared solar spectrum with Harold Babcock. Edward Condon, the Bureau Director, wanted a complete project on atomic energy levels, which was an enormous undertaking.

However, for Charlotte, there were thrills yet to come. In 1946, the Naval Research Laboratory (NRL) launched an XUV rocket and obtained its first successful rocket solar spectrum. Charlotte was so excited that she called Richard Tousey, whom she did not know, to congratulate him on the success. She recalled a day long ago in Princeton, when she and Russell and others were discussing what the ultraviolet solar spectrum would look like. They had fun guessing, but concluded that they would never live to see it, because no one could build a spectrograph stable enough.

At the Bureau of Standards, the Atomic Energy Levels Program was Charlotte's full time work, and the XUV program had to be fitted in when possible. But it did become a large part of her interest. In later years she divided her time between the Bureau of Standards and the NRL. She continued gathering data and or-ganizing tables of atomic spectra long after retirement age, publishing her last collection (Moore, 1985) shortly before her 87th birthday.

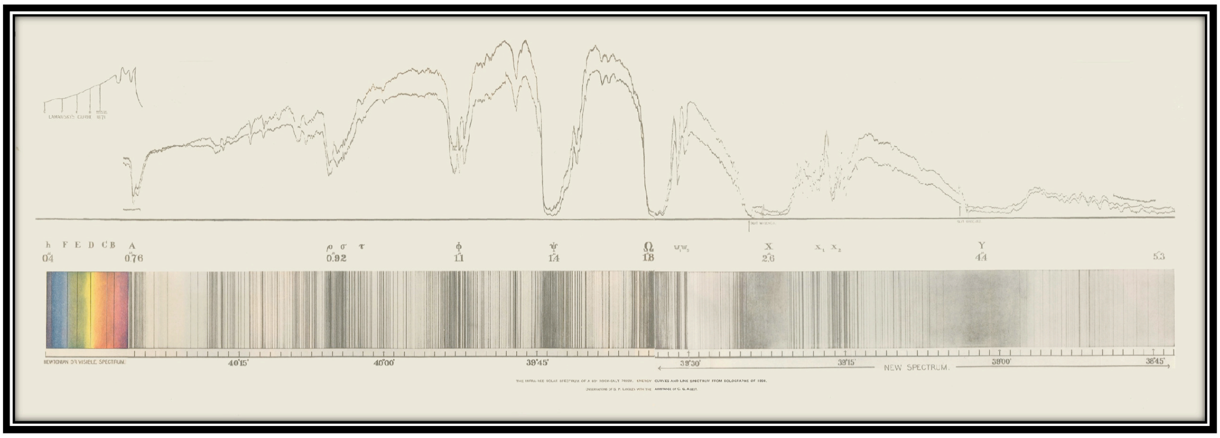
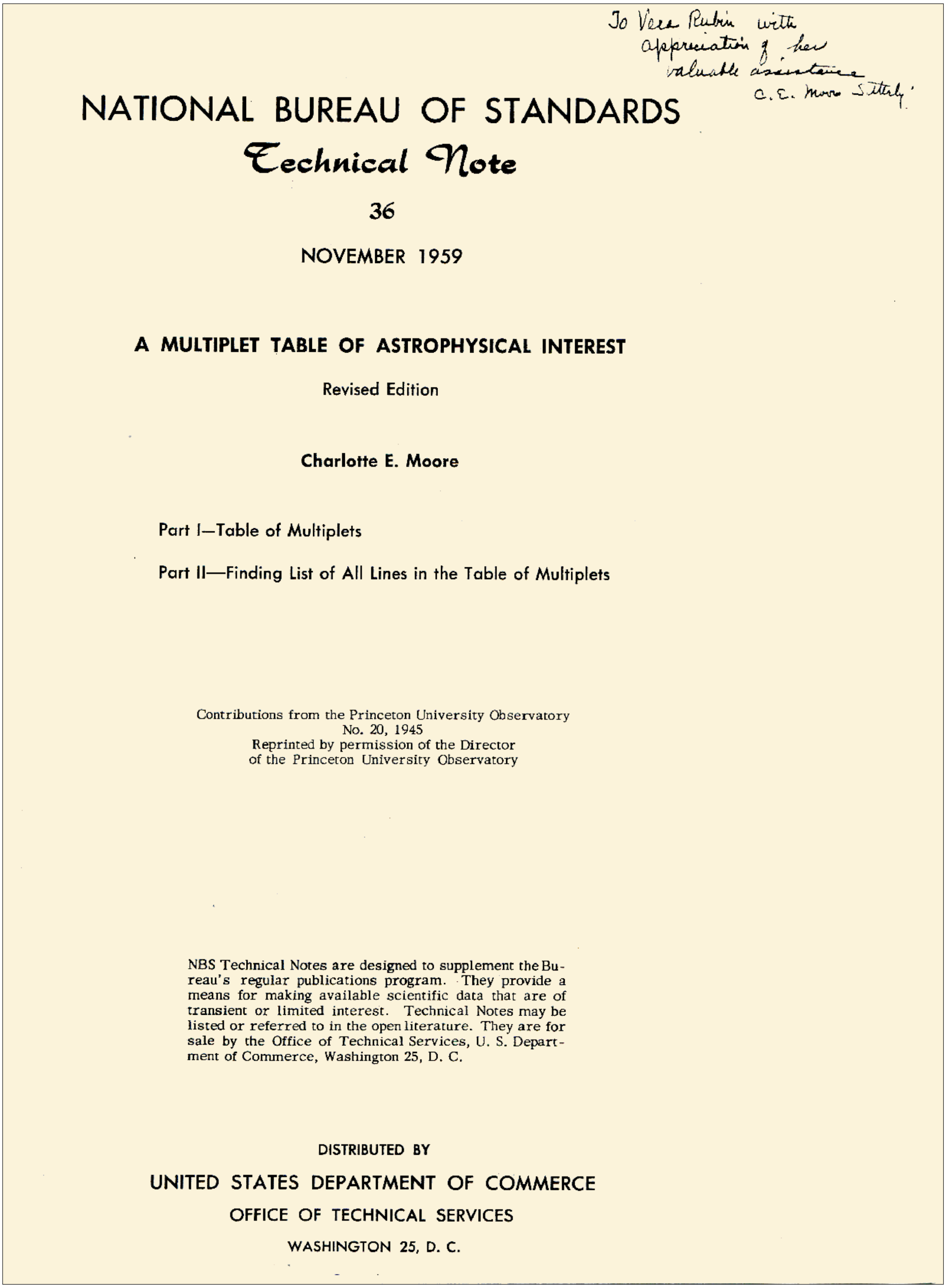


Figure 2: The infra-red solar spectrum of a 60 deg rock-salt prism. From bolographic observations of 1897-1898 (after Langley, 1900: Plate 20).


To Vera Rubin with appreciation of her valuable assistance
C. E. Moore Sitterly

NATIONAL BUREAU OF STANDARDS

Technical Note

36

NOVEMBER 1959

A MULTIPLET TABLE OF ASTROPHYSICAL INTEREST

Revised Edition

Charlotte E. Moore

Part I—Table of Multiplets

Part II—Finding List of All Lines in the Table of Multiplets

Contributions from the Princeton University Observatory
No. 20, 1945
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Figure 3: The title page of the 1959 revised edition of the *Multiplet Table* (Moore, 1959).

The scientific community recognized that her work was a gift to all, and Moore was awarded numerous prizes. In 1937 she received the second Annie Jump Cannon Award of the American Astronomical Society, an award that is reserved for women. But her highest honors came much later: the William F. Meggers Award of the Optical Society of America, for outstanding work in spectroscopy, in 1972, and the Catherine Wolfe Bruce Gold Medal of the Astronomical Society of the Pacific, for lifetime achievement, in

1990. She was notified of the latter award, but died before it could be formally presented to her.

For more details of Charlotte's life, I highly recommend the extensive and interesting interview that David DeVorkin recorded in 1978 (Sitterly, 1978). It is clear from Charlotte's first words that she is a 'take charge' person. In the interview, she said: "My own library was not very extensive until my husband supplemented it with his good books." To this, David, who surely intended to start from the beginning, replied, "Yes, we'll talk about that later. Dr. Sitterly, I know that you were born in 1898 in Ercildoun", and they went on from there. Of the many articles following her death in 1990, the one written by Nancy Roman (1991) for the *Bulletin of the American Astronomical Society* is especially sensitive and interesting.

2 PERSONAL CONTACT

I will conclude this brief account of Charlotte Moore Sitterly's life with a few personal comments. I no longer remember when and where I met Dr Sitterly. In her conversation with DeVorkin, she describes phoning Dr Tousey at the Naval Research Lab after the first successful rocket (X1JV) ultraviolet spectrum of the Sun had been safely returned in 1946 (Baum, et al, 1946). I worked in Dr Tousey's division as a summer student in 1947 and 1948, and my first project was to determine the optical properties of the spherical glass bead that was the detector for the rocket flight. I do not remember whether I met Charlotte during that time. I do remember that I periodically went with other scientists (all male) to the Naval Station in southern Maryland where the rockets were launched. I also remember that there was no ladies room on the base, so I had to use the Captain's bathroom. I suppose that Charlotte did also.

In 1952 I returned to Washington with a husband, an infant, and a Master's degree in astronomy, having studied physics under Richard Feynman, Philip Morrison, and Hans Bethe at Cornell University while my husband, Bob, was completing his Ph.D. there. I entered the Georgetown University graduate school in astronomy, the only astronomy graduate program in Washington, and took a wonderful course in spectroscopy with Carl C. Kiess, of the National Bureau of Standards. He tried to interest me in looking for and classifying the weak lines in the solar spectrum for my doctoral thesis, but I was interested in galaxies. I chose to write my thesis under George Gamow, who was then in the George Washington University Physics Department, but my Ph.D. degree is from Georgetown University, and it is in astronomy.

In my graduate student years there were few women astronomers in Washington. I took advantage of Charlotte's friendliness and her willingness to discuss astronomy and spectroscopy with a young student. Periodically I would visit her at the Bureau of Standards (now NIST) to discuss spectroscopy and astronomy. I started measuring spectra for her, using a measuring machine at Georgetown University that could accommodate her enormously-large solar plates from Mt. Wilson. They were too large for the Bureau of Standards measuring machinery. I continued visiting her over many years, talking spectroscopy, and being introduced to many of the Bureau of Standards spectroscopists and visitors. Occasionally Bob and I would drive her to lectures of interest. She was a brilliant scientist, a lovely lady, and a delight to know.

3 ACKNOWLEDGMENTS

This paper is based on a talk given in a special session on "The First Century of Astronomical Spectroscopy" at a meeting of the Historical Astronomy Division of the American Astronomical Society on 3 January 2010. The session was organized by Joseph S. Tenn of Sonoma State University.

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ST. HELENA, EDMOND HALLEY, THE DISCOVERY OF STELLAR PROPER MOTION, AND THE MYSTERY OF ALDEBARAN

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Abstract: St. Helena was the location of Halley's observatory in 1677-1678. The site has been identified and I report on a visit in November 2006. The principal use of the observatory was to accurately map the stars of the southern sky. In the summary of his work, the *Catalogus Stellarum Australium*, Halley noted evidence for the "… mutability of the fixed Stars." He would not return to this subject until much later in his career. Halley later compared contemporary positions of Arcturus, Sirius, and Aldebaran with the ancient positions recorded in the *Almagest*. He found that these stars had apparently moved southward by >30′ and concluded that they had their own particular motions. Modern proper motion measurements are consistent with this conclusion for Arcturus and Sirius, but are not even close for Aldebaran. While some authors are aware of the problem, it generally is not mentioned in books on the history of astronomy or in biographical works on Halley. Errors in the *Almagest* positions can be ruled out; an error of 30′ in the early eighteenth century position is highly unlikely; a misidentification of the star is implausible; and, we are left with the conclusion that there is most likely an error in Halley's calculations.

Keywords: St. Helena, Edmond Halley, proper motion, Aldebaran.

1 INTRODUCTION

Our modern view of the heavens holds that the stars are independent, luminous bodies. The stars, including the Sun, have their own particular motions and share a general motion around the center of the Milky Way Galaxy. Because of these motions, the positions of stars show small changes as seen from the Earth, called proper motions. This modern view did not reign three centuries ago when the stars were thought to be just points of light embedded in the Starry Firmament. The road to overturning the old view and establishing our modern view began on a small, remote island, St. Helena. Edmond Halley would note concerns about the mutability of the fixed stars in his catalogue reporting the positions of the southern stars. He would return to the subject decades later and discover stellar proper motions. Despite the fundamental importance of this discovery, it would be one of the least appreciated facets of Halley's illustrious career.

2 ST. HELENA, EDMOND HALLEY AND HIS OBSERVATORY

2.1 The Island

The island of St. Helena sits in splendid isolation in the South Atlantic Ocean, approximately 2,000 km (1,200 miles) from continental Africa and even further from South America (see Figure 1). I was fortunate to visit the island on 14 November 2006 during a call by Holland America's *Prinsendam*. The weather was mild—a tropical, marine weather tempered by trade winds. Clouds and light rain were frequent. The island was discovered circa AD 1500, and since 1673 Great Britain has had continuous control. The capital is Jamestown on the north coast. The population is approximately 5,000.

St. Helena's supreme isolation was utilized in the second exile of Napoleon Bonaparte. The sites associated with his exile are the principal tourist attractions on the island. In addition to the remarkable historical significance of the Napoleon-related sites, St. Helena for an astronomer is fascinating because of an expedition by Edmond Halley in 1677-1678. Observations at this site began the investigations that would lead Halley to one of his most important and least appreciated discoveries.

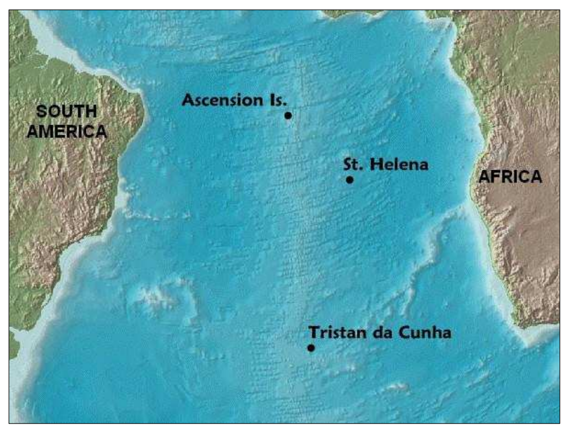

Figure 1. The South Atlantic Ocean and islands of interest. Modified from a map on web pages maintained by Barry Weaver (2009a), University of Oklahoma.

2.2 The Observatory Site

A substantial rise along the northward extension of the Diana's Peak-Mt. Actaeon ridge (Tatham and Harwood, 1974: 493) has been known as Halley's Mount since the time of Halley's visit or shortly thereafter; see Figure 2. However, in 1968, Tatham and Harwood (1974) found that there was no consensus on the location of the observatory. The prime candidate was the site identified by astronomer David Gill (1877). He stopped at St. Helena in 1877 on his way to Ascension Island to determine an accurate value of the astronomical unit. As reported by Mrs Gill (1878), Mr.

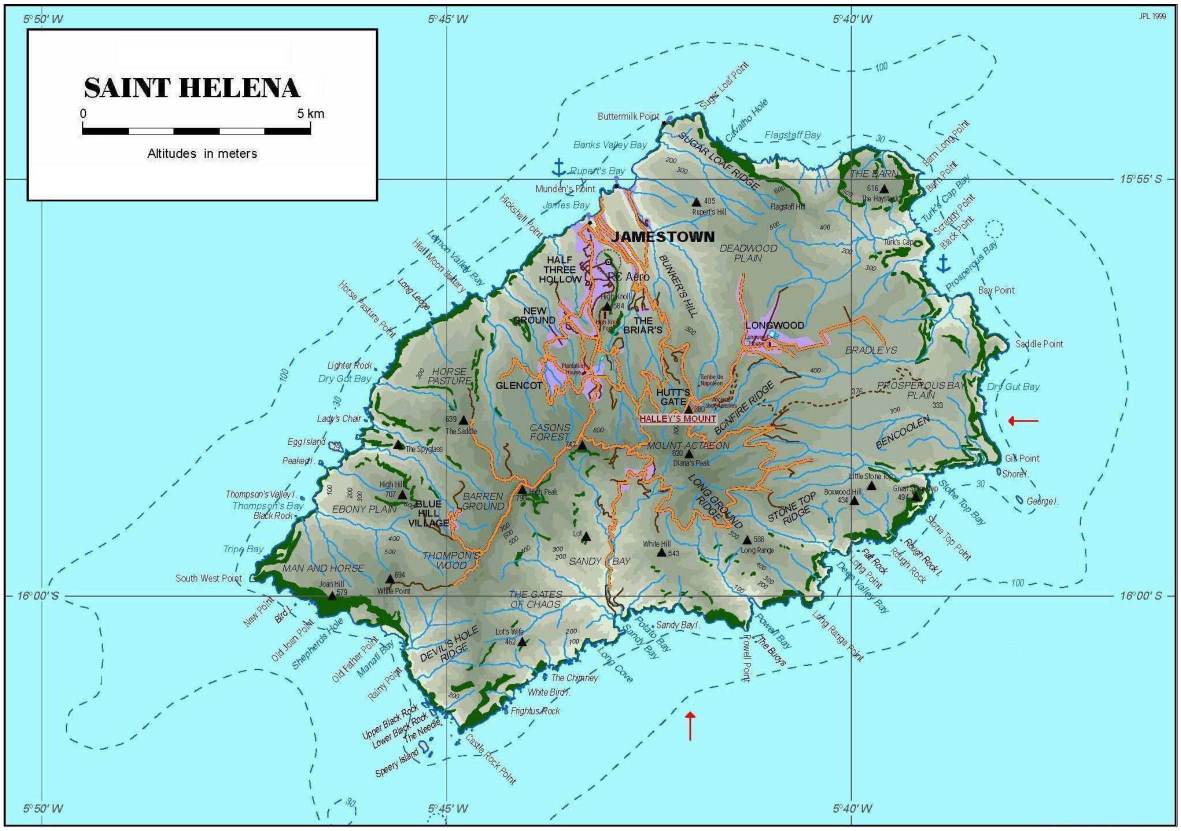

Figure 2. The Island of St. Helena. Halley's Mount, close to the center of the island, is labeled in red inside a gray box, and marked by two red arrows outside of the island. Adapted from a map on web pages maintained by Barry Weaver (2009b), University of Oklahoma.

Gill felt that "… this had been the Observatory, without doubt." The site was convenient to Jamestown; the only level one in the area; was suitable for astronomical observations; and was sheltered from the trade winds by a little knoll to the south that formed a windbreak. Still, there were conflicting views, and Tatham and Harwood (1974) undertook to rediscover the site. Their paper reviews the evidence for the observatory site. They found the site discovered by Gill. With the assistance of volunteers, the site was excavated and they uncovered "… four almost complete walls of dressed stone." The walls were found to be closely aligned to north/south and east/west. They also found a small quarry nearby which had also been noted by Gill. The walls had dimensions of about 12 by 14 feet, dimensions appropriate for the base of a structure sheltering Halley's instruments. Finally, the area, where not too precipitous, was searched by a slowly-moving line of volunteers, and no other plausible sites were found.

Cook (1998: 69-72) provides a description of Halley's principal instrument on St. Helena, the one used for his catalogue of the southern stars. He used a 1.7-meter radius sextant fitted with telescopic sights. Two stars could be observed simultaneously. Generally, one star would be one of Tycho's bright stars and the other a southern hemisphere star. The position of the southern star can be determined by spherical trigonometry when the arcs from two stars of known position are measured. Halley's large sextant and his other instruments presumably were housed in the building at the observatory site.

A significant problem is the excellent condition of the walls and the likelihood that the site was used as a signal station in the early nineteenth century. Because so much evidence points to this site for Halley's use as an astronomical observatory, Tatham and Harwood (1974) conclude that the lower stones in the walls were laid in 1677 and that the upper stones were part of the probable later use of the site as a signal station. Specifically, the view of A.W. Mawson (Ashbrook, 1970) that there was no observatory and that the site was used for an alarm cannon cannot be supported.

The investigation by Tatham and Harwood and the volunteers on St. Helena was clearly a labor of love. Tatham was honorary archivist to the Government of St. Helena, and Harwood was an English ophthalmic optician who made regular visits to the island to examine and prescribe for the eyes of the residents. Their words summarize the conclusion well (Tatham and Harwood, 1974: 501-502):

> Absolute proof concerning this site is not likely. But it seems certain that the early use of the name 'Halley's Mount' indicates that Halley operated somewhere on this hill … This spot was almost certainly Halley's Observatory, and should be marked and preserved as such for ever, by some enclosure and monument.

A commemorative plaque (see below) has been placed at the site.

Other locations on the island have astronomical interest. Nevil Maskelyne observed the transit of Venus in 1761 from a site near Halley's observatory (Tatham and Harwood, 1974; Warner, 1982). Later, Manuel Johnson built an observatory on Ladder Hill just to the west of Jamestown and some 700 feet above it. From this observatory, Johnson produced his *Catalogue of 606 Principal Fixed Stars in the Southern Hemisphere* published in 1835 (Tatham and Harwood, 1974; Warner, 1982).

2.3 Edmond Halley and Reasons for the St. Helena Expedition

Edmond Halley (1656–1742) is one of the most famous astronomers of all time; see Figure 3 for a portrait of Halley done some ten years after his visit to St. Helena. Some major works on Halley are Armitage (1966), British Astronomical Association (1956), Cook (1998), De Morgan (1847), Lancaster-Brown (1985), MacPike (1932), Ronan (1969) and Thrower (1990). A diagram by Hughes (1990: 327) nicely illustrates the breadth of Halley's scientific interests.

Halley wanted to produce a catalogue of the southern stars and selected the island of St. Helena because it was under British control and because it was reputed to have good weather. The latter information was faulty and the bad observing conditions were a major obstacle to his observing program. In a letter from Halley to Sir J. Moore (a patron of Halley's expedition) dated 22 November 1677, written on St. Helena, we find:

> But such hath been my ill fortune, that the Horizon of this Island is almost always covered with a Cloud, which sometimes for some weeks together hath hid the Stars from us, and when it is clear, is of so small continuance, that we cannot take any number of Observations at once; so that now, when I expected to be returning, I have not finished above half my intended work; and almost despair to accomplish what you ought to expect from me. (MacPike, 1932: 39-41).

While the latitude was only 16º S, it was a major improvement from London's latitude of 51.5º N.

2.4 My Visit: 14 November 2006

Curiously, in November 2006 the path to Halley's observing site was not marked on the paved road. The route is described by Mathieson and Carter (1993, Walk No. 22). At first, the route follows a steep, wide, grassy path up Halley's Mount. A short distance up the path, a sign marks a narrow path that leads to the site. Although the observatory building is long gone, what appear to be foundation walls remain (Figure 4).

There is also a gray plaque (Figure 5) that shows considerable deterioration, but the text is legible and reads: "The Site of the Observatory of Edmond Halley. He came to Catalogue the Stars of the Southern Hemisphere 1677-1678". While I was visiting the site, fog with a light mist moved in. I could not help imagining a young Edmond Halley in similar conditions over three centuries ago hoping for the skies to clear.

3 *CATALOGUS STELLARUM AUSTRALIUM* AND THE DISCOVERY OF STELLAR PROPER MOTION

Halley made observations of some 350 stars not included in Tycho Brahe's catalogue. Upon his return to England, the observations were expeditiously reduced and the catalogue—as was the custom of the day—was published in Latin. The catalogue was hailed as a great achievement and Halley was considered to be the 'Southern Tycho'. The *Catalogus Stellarum Australium* is now available electronically, and a summary in English was published in the *Philosophical Transactions of the Royal Society* for 1678 (actually printed in 1679). Despite being in the third person, the summary was probably written by Halley himself.

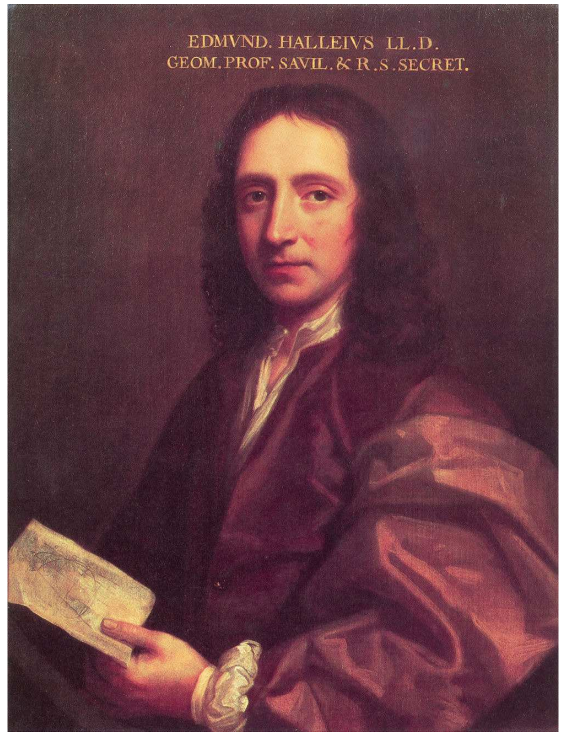

Figure 3. Portrait of Halley painted around 1687 by Thomas Murray (Royal Society, London). Public domain.

In the summary (Halley, 1678: 1033), we find the first inklings of the discovery of stellar proper motions. Halley's new observations were compared to some old catalogues and this statement follows:

> It might evidently appear how very much the Ancient Globes do almost every where differ from the Heavens. From these Observations, as he proceeds, he also proposeth some conjectures of the corruptibility, or at least the mutability of the fixed Stars.

Halley did not return to the question of stellar proper

Figure 4. The Observatory site showing the walls (see text for discussion) and the commemorative plaque. Photograph by the author.

Figure 5: Close-up view of the plaque. Image processing was used to enhance legibility. Photograph by the author.

motions for decades (Aitken, 1942). He maintained his lifelong interest in stellar positions, but this was primarily for the stellar side of a method to determine time or geographic longitude by lunar occultations.

In 1717, Halley published an extraordinary three-page paper in the *Philosophical Transactions of the Royal Society*. The paper's title is "Considerations of the Change of the Latitudes of some of the principal fixt Stars", and a full electronic copy is available through JSTOR. It is also reprinted in Aitken (1942), which is available through ADS. During an investigation into the value of the obliquity of the ecliptic and the precession of the equinox, Halley found some large changes in latitudes or declinations for some stars. Halley's paper contains the usage of his time of simply Latitude and Longitude, but Declination is used interchangeably with Latitude a few times in the paper. Precession is a well-understood phenomenon; its discovery is generally credited to Hipparchus (Dreyer, 1953; Pannekoek, 1961; Sarton, 1959), who compared his observations to those made by Timocharis approximately 140 years earlier. For technical details on precession see Kovalevsky and Seidelmann (2004), Smart (1949) and van de Kamp (1967).

Halley compared contemporary positions for the bright stars Sirius, Arcturus, and Aldebaran with the positions given by Ptolemy, Timocharis, and Hipparchus. Note that Aldebaran is called 'Palilicium' or 'the Bulls Eye' in Halley's paper. Halley took the time span between the old observations and his to be 1,800 years. He noted problems with the latitudes (or declinations) of these stars and wrote:

> All these three Stars are found to be above half a degree more *Southerly* at this time than the Antients reckoned them … What shall we say then? It is scarce credible that the Antients could be deceived in so plain a matter, three Observers confirming each other. Again, these Stars being the most conspicuous in Heaven, are in all probability the nearest to the Earth, and if they have any particular Motion of their own, it is most likely to be perceived in them, which in so long a time as 1800 Years may shew itself by the alteration of their places, though it be utterly imperceptible in the space of a single Century of Years. (Halley, 1717: 737).

Near the end of his paper, Halley (1717: 738) wrote: "This Argument seems not unworthy of the *Royal Society's* Consideration, to whom I humbly offer the plain Facts as I find it, and would be glad to have their Opinion."

This was the discovery of stellar proper motions—that stars move. The astronomers and the educated public of the time were still digesting the Copernican view of the Solar System and searching for conclusive proof (see the fascinating treatises by Johnson (1937) and Van Helden (1985)). Recall that two conclusive measurements—stellar aberration (Bradley, 1728) and stellar parallaxes (Bessel, 1838; 1839; Henderson, 1839; Struve, 1840)—had not yet been accomplished. Of course, some 'perturbations' in the heavens were known. The supernovae of 1572 (Tycho's) and 1604 (Kepler's) had been observed, and the brightness variations of Mira were well known. Also, there were speculations on the size and distance of the stars. Still, almost everyone at the time, including luminaries, believed that the stars were fixed in space, points of light embedded in the starry firmament. Clerke (1908: 9) summarized the situation:

> Until nearly a hundred years ago the stars were regarded by practical astronomers mainly as a number of convenient fixed points by which the motions of the various members of the solar system could be determined and compared. Their recognized function, in fact, was that of milestones on the great celestial highway traversed by the planets, as well as the byeways of space occasionally pursued by comets.

The changes in star latitudes from antiquity to the beginning of modern astronomy were, in fact, discovered by Tycho Brahe (1648). This has been described by Moesgaard (1989) and by Evans (1998). Tycho used the same data from the *Almagest* that Halley would use over one hundred years later. Because Tycho believed that the relative positions of the fixed stars did not change, he concluded that the changes were due to a decrease in the obliquity of the ecliptic. This result required Tycho to adjust some ancient positions that he believed to be in error.

Halley's discovery started a changed view of the heavens. The basis of his argument was accurate contemporary positions and positions determined by ancient astronomers. The difference of more than half a degree is greater than the angular diameter of the Moon, and indeed the ancient observers were unlikely to have made an error this large.

4 THE ALDEBARAN PROBLEM

However, there is a significant complication (Brandt, 2008). Modern proper motion measurements are readily available (Allen, 1955; Urban et al., 2004). Over a time span of 1,800 years, Sirius, Arcturus and Aldebaran have moved 36.7′, 60.0′ and 5.7′ southward respectively (see Table 1). Thus, the consistency check is fine for Sirius and Arcturus, but the proper motion value is not even close for Aldebaran. This problem is worth pursuing because, although it is known to some writers (e.g., Evans, 1998; Fomenko et al., 1993;) it generally is not mentioned in encyclopedias or history of astronomy books (a link to a paper with the list of references is given in Section 7), or in the biographical works on Halley cited in Section 2.3. Also, the problem is not mentioned in that venerable astronomy text of the nineteenth century, *Outlines of Astronomy*, by J. Herschel (1871), nor in a modern astrometry text that covers classical astronomy (Kovalevsky and Seidelmann, 2004). An additional curiosity is that Delambre (1827) provides almost no coverage of stellar proper motion in his history of eighteenth century astronomy. Halley's discovery is not mentioned, and the confirmation for Arcturus by Cassini (1738), discussed in Section 5, merits only a single sentence.

Certainly, there is indirect evidence of some additional knowledge of the problem. Sometimes there is no mention of Aldebaran, but Procyon is listed as the third star along with Arcturus and Sirius (Abetti, 1952; British Astronomical Association, 1956; Ronan, 1969). This substitution might indicate knowledge of the problem, yet Procyon is not mentioned in Halley's (1717) paper. Some textbooks (e.g. Russell et al., 1927; Young, 1895; 1904; 1912) mention only Arcturus and Sirius. Because these texts quote correct values for the proper motions of Arcturus and Sirius, it is likely that the Princeton University astronomers were aware that Aldebaran's proper motion was much too small.

Halley (1717) thought that he had three independent measurements. These were taken from Ptolemy's *Almagest* and the observers were thought to be Timocharis (ca. 270 BC), Hipparchus (ca. 130 BC) and Ptolemy (ca. AD 140). The translation of the *Almagest* I have consulted for this paper is that by Toomer (1998).

Before the discussion can continue, the nature and status of the *Almagest* must be examined. Historical evidence, long available, strongly suggests that Ptolemy's star catalogue in the *Almagest*, often called the Ancient Star Catalogue (ASC), was not completely original. Dreyer (1953: 202) would succinctly write:

> The great work of Ptolemy also contains a catalogue of stars, which, however, is nothing but the catalogue of Hipparchus brought down to his own time with an erroneous value of the constant of precession.

Table 1: Proper motion summary.

Star	Proper Motion* (″/year)	Motion in 1,800 Years (′)
Sirius	1.223	36.7
Arcturus	1.999	60.0
Aldebaran	0.189	5.7

* All motion southward.

The literature on this subject is extensive and opinions have evolved with time. Evans (1998: 264-274) has given a concise history of the varying views on Ptolemy's reliability. For a recent view, see, for example, the papers on the ASC in the September 2002 issue of *DIO: The International Journal of Scientific History*. The papers by Duke (2002a; 2002b) and Pickering (2002a; 2002b; 2002c) convey the flavor and intensity of the debate. Duke (2002b) presents a short summary of the current consensus view of the ASC:

- Someone, perhaps Hipparchus, measured a fairly complete catalogue of star in *equatorial* coordinates.
- That catalog was the basis for the results presented in Hipparchus' *Commentary to Aratus.*
- Analog computation was used to convert most of the catalog to *ecliptic* coordinates.
- It is this converted catalog, with longitudes shifted by 2º 40′, that we have received through Ptolemy and the *Almagest*.

The *Almagest* catalogue is given in Books VII and VIII and occupies pages 341-399 of Toomer (1998). The catalogue gives a single ecliptic latitude for each star. The ASC is unlikely to be the source of positions believed to be based on three independent observers. However, three positions are given for a short list of stars (pages 331-332) as part of the discussions with page headings "Comparative declinations of stars…" and "Constancy of latitudes deduced from declinations." On page 331, three declinations are listed for Sirius ("The Bright star in the mouth of Canis Major"), credited to Timocharis, Hipparchus, and Ptolemy ("… as found by us."). The values are 16⅓º, 16º, and 15¾º, South, respectively. On page 332, three declinations are listed for Arcturus credited to the same observers. The values are 31½º, 31º and $29^{5}/_{6}$º, North, respectively. On page 331, three declinations are listed for Aldebaran ("The bright star in the Hyades") credited to the same observers. The values are 8¾º, 9¾º, and 11º, North, respectively. The systematic progression of these positions with time is the result of precession, as noted by Ptolemy. Independent of any details, there was clearly a source for three positions.

I have used a Precession Routine (2009) to calculate the ancient positions of all three stars from modern positions, both B1900.0 (Allen, 1955) and J2000.0 (Urban et al., 2004). Precession and proper motion are included, and the expected accuracy is about 0.1″. The results show remarkable agreement between the modern positions precessed with proper motion and the positions as reported for Timocharis, Hipparchus, and Ptolemy. The average difference in declination for all nine comparisons is 0.23º. However, one difference (for Timocharis' measurement of Arcturus) is 1.94 times higher than the next highest difference. If this one is removed, the average difference is 0.17º, or ~10′ to 11′. There is little significant difference between observers. This means that the original positions as recorded in the *Almagest* (Toomer, 1998) appear to be accurate.

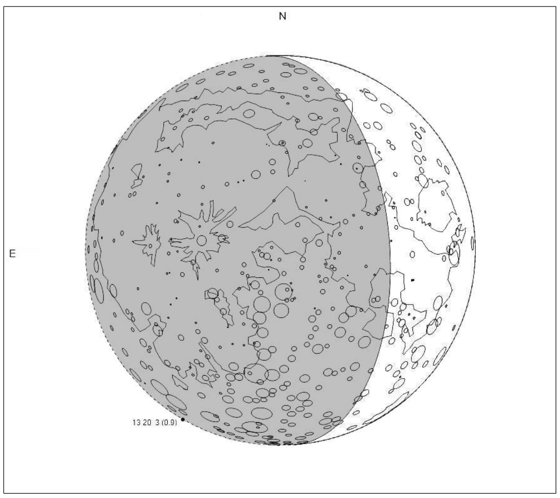

Figure 6. The lunar occultation of Aldebaran on 11 March 509. The calculation was run using the Occultv4 software authored by David Herald, Canberra (2009), and available at the International Occultation Timing Association (IOTA) web site. The shading has been added.

The net result is the remarkable agreement of the positions to within 0.17º. Thus, Halley's results for Sirius and Arcturus are sound and the discovery of stellar proper motions is secure. Still, there is a major problem with Aldebaran. Given the history—probable change of coordinate system, possible transcription and translation problems, and attempts to 'correct' the observations—an erroneous value should not be surprising. Note that some of the steps just cited may not have occurred for declinations that are given in the text and are not part of the ASC. Possible errors through history may explain the single discordant value for Arcturus but does not reasonably explain three ancient positions for Aldebaran that are systematically in error by 0.5º or more in declination. The plausible explanations are that the observed ancient positions for Aldebaran used by Halley were in error or systematically altered at some time through the years and were not the ones in the current version of the *Almagest*, or that Halley's calculations were in error, or that the star was misidentified. It seems inconceivable that the position of Aldebaran could be in error by some 30′ in the early eighteenth century, although Fomenko et al. (1993) suggest that an erroneous position may have come from a preliminary version of Flamsteed's catalogue.

There has been some confusion about the third star, Aldebaran, cited by Halley. Recall that Halley gave the star as Palilicium or the Bull's Eye. Clerke (1908: 10) adds Betelgeux to the list of stars. Betelgeux has a small proper motion and is highly unlikely to be a candidate. Abetti (1952: 143) and Ronan (1969: 201) list Halley's three stars as Arcturus, Procyon and Sirius. Procyon is a viable candidate for the third star in the sense that it is bright and has a large proper motion. But Aldebaran and Procyon are separated by about 3 hours in right ascension, and confusing the two seems unlikely. I have been unable to find any convincing evidence that the star cited by Halley as Palilicium, or the Bull's Eye, is not Aldebaran.

Halley (1717) cited a lunar occultation of Aldebaran on AD 11 March 509 to bolster his case for the southward motion. The text, in part, reads:

> But a further and more evident proof of this change is drawn from the Observation of the application of the Moon to *Palilicium* … when in the beginning of the Night the Moon was seen to follow that Star very near, and seemed to have Eclipsed it … Now from the undoubted principles of Astronomy, it was impossible for this to be true at *Athens* or near it, unless the Latitude of *Palilicium* were much less than we at this time find it.

To verify this statement, Peter Zimmer, University of New Mexico, kindly ran the program Occultv4 available through the International Occultation Timing Association (IOTA) (Herald, 2009). The results (Figure 6) confirm the occultation close to the Moon's *south* limb.

Unfortunately, there are significant problems with the records of the occultation and the occultation itself. As noted on Figure 6, the occultation occurred in daylight at 13:20 UT. The local time would have been approximately 14:55 or 2:55 pm, i.e., mid-afternoon. Thus, it was probably not observed directly at Athens. The occultation may have been inferred from the location of the Moon after dark (Neugebauer, 1975: 1038, 1041) or the occultation was observed from another location. Alexandria has been suggested, but Aldebaran was not occulted by the Moon as seen from Alexandria. The difficulties with this occultation were known in the seventeenth century (Riccioli, 1665: 154). Thus, a fair conclusion may be that the cited record of the occultation is unreliable. But the report is probably too much to ascribe to coincidence, and thus an inferred occultation is probably the best explanation.

A more important question for the problem with Aldebaran is how things would have appeared to Halley. The calculation should have been difficult and Halley may or may not have been aware that the occultation took place in daylight. If he did notice this problem, he may not have regarded an error of a few hours in time as significant. Or, he may have used the idea that the occultation could be inferred from the Moon's location after dark. In any event, Halley's paper makes clear that his results for the occultation supported his large proper motion for Aldebaran.

Comparison of Figure 6 with Halley's text leaves us in the unenviable position of trying to determine Halley's meaning when he wrote that Aldebaran's latitude needed to be "… much less than we at this time find it." Aldebaran's position for 509 in Figure 6 is based on the modern position and small proper motion. Halley wanted 30′ or more of proper motion in 1,800 years. On this assumption, Aldebaran would have moved southward by 10′ or more from ancient times to 509, or it would be roughly at most 20′ north of the calculated 509 position. It would have undergone occultation at this position, but not if it were at the original ancient position that Halley believed. The star would have needed to move southward by roughly 10′ to have the 509 occultation occur close to the Moon's *north* limb.

Thus, the modern calculations confirm the 509 occultation, but also show that it is not evidence confirming a large proper motion for Aldebaran. The results seem to imply that Halley's ancient positions for Aldebaran were some 30′ north of the correct position. The possibility that the version of the *Almagest* available to Halley contained erroneous values for Aldebaran's declinations (Evans, 1998) should be considered. All in all, the putative occultation of Aldebaran by the Moon as seen from Athens in 509 has no value in the discussion. It is simply a major red herring.

Several editions of the *Almagest* could have been available to Halley, and we should remember that the same source may have been used by Tycho when he discovered the changes in star latitudes as he observed them as compared with latitudes recorded in antiquity (Evans, 1998; Moesgaard, 1989). An easy edition to check is the English translation in the series *Great Books of the Western World*. Taliaferro (1952: 229) gives the same triad of values: 8¾º, 9¾º and 11º.

Pedersen (1974: 11-25) has summarized the history of the *Almagest* through the ages, and specific editions are listed. The first complete, printed Latin translation from an Arabic version appeared in 1515 (Ptolemy, 1515). A Latin translation from a Greek version was published in 1528 (Ptolemy, 1528). Owen Gingerich (Harvard-Smithsonian Center for Astrophysics) has these two Latin editions in his personal library and when he checked them he found the same triad of declinations.[1] Pedersen (1974: 21) notes that these Latin sixteenth century versions were "… the only translations for centuries to come."

A printed version of the original Greek text (Ptolemy, 1538) was prepared from a manuscript (now lost) at Nuremberg that was formerly in the possession of Regiomontanus (Pedersen, 1974). This edition may have been consulted by Halley (see below).

In the seventeenth century, astronomy texts would contain the ancient positions of interest. In Tycho's *Progymnasmata* (Brahe, 1648: 169), the declinations for Aldebaran are listed. Again, they are the same triad and presumably came from the *Almagest*. In addition, in his monumental work, Riccioli (1651: 442) clearly lists the same triad of positions for Aldebaran along with Tycho's declination of 15º 38′. Certainly, the presumed correct positions for Aldebaran were available in the seventeenth century.

The books in Halley's personal library may provide a clue to the origin of the ancient positions that he used. His books were offered for sale after his death and, fortunately, the sale catalogue has been preserved (Feisenberger, 1975), but with a complication. Halley's books were commingled with those of an anonymous "… late eminent Serjeant at Laws." However, it is safe to assume that astronomy books generally and books by Ptolemy specifically belonged to Halley. The list appears to show five copies of Ptolemy's *Geographica*, his other famous writing; these are items numbered 813-816 and 2027 (Feisenberger, 1975). No copy of the *Almagest* is listed. Even though there is evidence (Cook, 1998: 344) that Halley planned to produce an edition of Ptolemy's *Geographica*, the possibility that Halley owned five copies of *Geographica* and no copies of the *Almagest* seems unlikely. A closer examination of the list indicates that some of the listings may be in error. Specifically, item No. 814 is given as an edition of *Geographica* in Greek published at Basel in 1538. Eames (1886) has published a listing of the editions of *Geographica* for the years 1475-1730 and no edition is listed for 1538. But the language (Greek), the place of publication (Basel), and the year (1538) match the first Greek text printed version of the *Almagest*. The entry was probably mistitled in the listing. Halley read Greek and apparently had this edition in his personal library. Thus, the 1538 Greek edition emerges as the prime candidate for Halley's ancient declinations.

I have consulted a scan of the 1538 Greek-language edition (Ptolemy, 1538), Book VII, Chapter 3, kindly provided by the University of Minnesota. Again, the positions given in this edition are exactly the same as those found in Toomer (1998). For completeness, I have also checked the Greek text for the *Almagest* published by Heiberg (1903). Note that Heiberg uses a symbol for ½ that is different from the classical Greek number system (Greek Numbers and Arithmetic, 2009) and that does not have an MS Unicode (Re: Greek fractions, 2010). Again, the declinations are exactly the same.

While we cannot be absolutely certain, the evidence against Halley using erroneous ancient declinations is quite strong. Many editions of the *Almagest* from the sixteenth century on, including the edition likely owned by Halley, all give exactly the same positions. The solution must be elsewhere. Note that erroneous positions could have entered Halley's calculations by a misreading of the *Almagest* or by a conscious adjustment.

Unfortunately, Halley did not provide details of his calculations. Still, definitive resolution of this problem might be possible if Halley's original calculations can be located. Halley's papers were presented to the Royal Society in 1765 by his daughter Catherine Price. They were originally deposited at the Royal Greenwich Observatory and now reside at Cambridge University. An extensive, detailed description of Halley's papers at the Royal Greenwich Observatory Archives (2009) is available on-line, and there is no entry identifiable as relating to his proper motion calculations. In addition, there is no relevant material in MacPike's (1932) compendium of Halley's correspondence and papers. Lacking additional input, the question of Aldebaran's role in the discovery of proper motion is likely to remain a mystery, but an error in Halley's calculations is the prime candidate.

5 CONFIRMATION OF PROPER MOTION

Observing techniques improved rapidly in the eighteenth century, and Halley's discovery of stellar proper motion was confirmed by J. Cassini (1738) using Arcturus. He compared his observations with observations taken in 1690 by Flamsteed, in 1672 by Richter at Cayenne (French Guiana) and in 1586 by Tycho Brahe. Cassini (1738: 338) concluded that the extensive evidence constituted irrefutable proof (*preuve incontestable*) for the proper motion of Arcturus. Recall that Arcturus has a proper motion close to 2.0″ per year southward. Thus, this relatively short time span was sufficient to clearly show the proper motion. Halley's health began to decline in 1738, and I have found no mention in his biographies to indicate that he was aware before his death in 1742 of Cassini's confirmation. Hornsby (1773-1774: 93) states that the proper motion result for Arcturus "… cannot possibly be attributed to the uncertainty of observation …" and quotes a value close to modern results. The discovery could be considered well established when W. Herschel (1783: 247) would succinctly write: "That several of the fixed stars have a proper motion is now already so well confirmed, that it will admit of no further doubt."

6 DISCUSSION AND CONCLUSION

The discovery of proper motion constituted one of the most fundamental discoveries in astronomy and led to a completely changed view of the Universe beyond the Solar System. As obvious as the significance of the discovery is in retrospect, it was not universally appreciated in the eighteenth century. In his biography of Halley, Ronan (1969: 202) notes that this discovery was not mentioned "… in at least three of the obituary notices prepared after his death."

Neglect of this important discovery persists. On 13 November 1986, a memorial to Edmond Halley was unveiled in the Cloisters of Westminster Abbey (Henbest, 1987; Laufer, 1986). Although Laufer (1986) reports that Halley's discovery of stellar proper motion was noted by the dean of Westminster Abbey and it was also mentioned in the ceremonial booklet, the memorial summarizing Halley's achievements did not include it.

The problem with the results for Aldebaran is a mystery. Perhaps further historical research can definitively determine the source of the error. In the meantime, the regular return of Halley's Comet serves as a reminder of this great astronomer. Let us remember that his genius extended well beyond his work on comets and included the proper motion of stars.

7 ON-LINE RESOURCE

The on-line paper is an expanded version of this paper. The additional material includes more coverage of St. Helena, Halley's career, his observatory site, and Halley-related sites in London, in addition to the references mentioned in Section 4. See http://panda.unm.edu/jbrandt/Halley.pdf or contact the author at jcbrandt@unm.edu.

8 NOTES

1. Another Latin edition of the *Almagest* was published in 1541, but is not mentioned by Pedersen (1974). The WorldCat description suggests that it is based on earlier translations. The declinations listed for Aldebaran, verified by Owen Gingerich from a copy in his personal library, are the same. The abbreviated reference is*: Claudii Ptolemaei Pelusiensis Alexandrini omni, quae extant, opera.* Basel, Henricus Petrus, 1541.

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THE FIRST ASTRONOMICAL HYPOTHESIS BASED ON CINEMATOGRAPHICAL OBSERVATIONS: COSTA LOBO'S 1912 EVIDENCE FOR POLAR FLATTENING OF THE MOON

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Abstract: Acceptance by the scientific community of results obtained with new technology can be a complex process. A particularly good example is provided by the unexpected hypothesis raised by Francisco Miranda da Costa Lobo upon examination of the cinematographic film obtained during the solar eclipse of 17 April 1912. Contrary to contemporary practice this eclipse was eagerly awaited in view of its astrometrical rather than astrophysical scientific interest. The observation of this hybrid eclipse provided, in theory, a good opportunity to improve several astrometric parameters, and in particular the Moon's apparent diameter. Observations were performed from Portugal to Russia and, for the first time, movie cameras were widely deployed to register astronomical phenomena. Upon analysing the film obtained at Ovar (Portugal), Costa Lobo realised that during totality Baily's Beads were not symmetrically distributed around the Moon. As an explanation and opposing current belief he proposed a lunar flattening in the range 1/1156 to 1/380. Initially other eclipse observers supported Costa Lobo's claim. In particular, Father Willaert obtained a flattening value of 1/2050 from his cinematographic film taken at Namur (Belgium). However, these results were quickly disregarded by the international astronomical community which favoured an explanation based upon the irregularities of the lunar profile.

In this paper we recall the characteristics of the 17 April 1912 eclipse and the cinematographic observations, and review the results obtained. We conclude that the lack of attention paid by the astronomical community to the new cinematographical results and Camille Flammarion's superficial analysis of the data were instrumental in the rejection of Costa Lobo's hypothesis.

Keywords: Astronomical cinematography, lunar flattening, shape of the Moon, 17 April 1912 solar eclipse, Francisco Miranda da Costa Lobo

1 A 'RARE' ECLIPSE

From the 1840's onwards solar studies were a 'hot' research topic. Observation of the 8 July 1842 eclipse, the discovery of the sunspot cycle and its correlation with the Earth's magnetic field, and Kirchoff's spectral laws all contributed to an increasing interest in the Sun (Bonifácio et al., 2007; Meadows, 1970). Following observations of the corona and prominences in 1842 (see Becker, 2010: 115), solar eclipse expeditions were sent to the far 'corners' of the Earth in order to study these features. Better transport systems and local logistics provided by host countries or colonial entities helped to make the nineteenth-century eclipse expedition a standard astronomical endeavour (Hingley, 2001; Pang, 2002; Ruiz-Castell, 2008). While solar eclipses had previously been used mainly to confirm solar and/or lunar ephemerides or—assuming these to be correct—as a tool to determine the longitude of the observing station, these applications declined during the nineteenth century. New techniques (i.e. electric telegraphy) were available for longitude determinations and the precision of astrometric predictions improved to the point where it was well above the data one could obtain from the majority of solar eclipses. So from the 1860s onwards what we refer to today as solar physics became the main scientific rationale behind solar eclipse expeditions. There was, nevertheless, one exception: eclipses of very short duration could in principle still be employed to better define several astrometric parameters, but particularly the Moon's position and its apparent diameter.

According to the eclipse predictions of Fred Espenak (2010), between 1800 and 1912 only five annular and four hybrid solar eclipses had maximum durations of ≤7 seconds (see Table 1). The 7 seconds cut-off

Table 1: Nineteenth-century hybrid (H) and annular (A) solar eclipses with durations of less than or equal to 7 seconds

Date	Type	Maximum duration (seconds)	Geographic location (*)
11 February 1804	H	0	Algerian desert
21 February 1822	A	2	USA and Canada
4 March 1840	A	3	India, China & Russia
27 June 1843	H	7	Pacific Ocean
30 October 1845	H	2	Antarctica
15 March1858	A	2	UK, Sweden, Finland & Russia
25 March 1876	A	1	Canada & Greenland
6 June 1891	A	6	Russia
6 April 1894	H	1	China

* Note that the last column is only a crude indication of the geographical location of the eclipse path. For instance, if the totality could be observed from Russia this only means in some part of that country.

Table 2: Elements for the 17 April 1912 solar eclipse according to different publications: NA = *Nautical Almanac*; EC = *Efemérides de Coimbra*; CT = *Connaissance des Temps*; AE = *American Ephemeris*; SF = *Almanaque Nautico de San Fernando*; M = Madrid Observatory paper. The last column lists the difference between the maximum and minimum values.

Body	Element	NA & EC			CT	AE	SF	M	Difference
		h	m	s	s	s	s	s	s
Conjunction in R.A. civil GMT time		2	3	45.2	35	27.1	45.2	45.18	18.1
Sun and Moon R.A.		1	40	36.54	36.53	36.5	36.55	36.53	0.05
		°	′	″	″	″	″	″	″
Sun	Hourly Motion in R.A.	0	2	19.1	19.1	19.05	19.1	19.1	0.05
	Declination	10	26	51.2	51.0	51.2	51.2	51.209	0.209
	Hourly Motion in Declination			52.8	52.8	52.8	52.8	52.8	0.00
	Equatorial horizontal parallax			8.76	8.8	8.8	8.77	8.76	0.04
	True Semidiameter		15	55.51	55.5	55.5	55.51	55.51	0.01
Moon	Hourly Motion in R.A.	0	30	51.5	51.4	51.45	51.4	51.5	0.1
	Declination	11	0	52.4	49.7	47.9	52.4	52.30	4.5
	Hourly Motion in Declination		15	0.7	0.8	0.8	0.8	0.7	0.1
	Equatorial horizontal parallax		57	40.98	41.0	41.4	40.98	40.98	0.42
	True Semidiameter		15	42.24	43.3	42.3	42.05	43.39	1.34
Moon eclipse semidiameter			15	31.65	32.71	31.88	31.89	32.83	1.18
			15		31.53			31.53	

chosen was arbitrary but relates to the maximum duration predicted for the 17 April 1912 eclipse. Note that Herald (1983) believes that the eclipses listed in Table 1 cannot be categorized as either Type A or H since Baily's Beads were still visible at the maximum phase of each eclipse owing to irregularities of the lunar limb profile. As far as we can ascertain, only the 15 March 1858 eclipse was observed inside the Moon's shadow, an expected outcome due to the unfavourable geographical locations intersected by the Moon's shadow in the case of the other Table 1 eclipses.

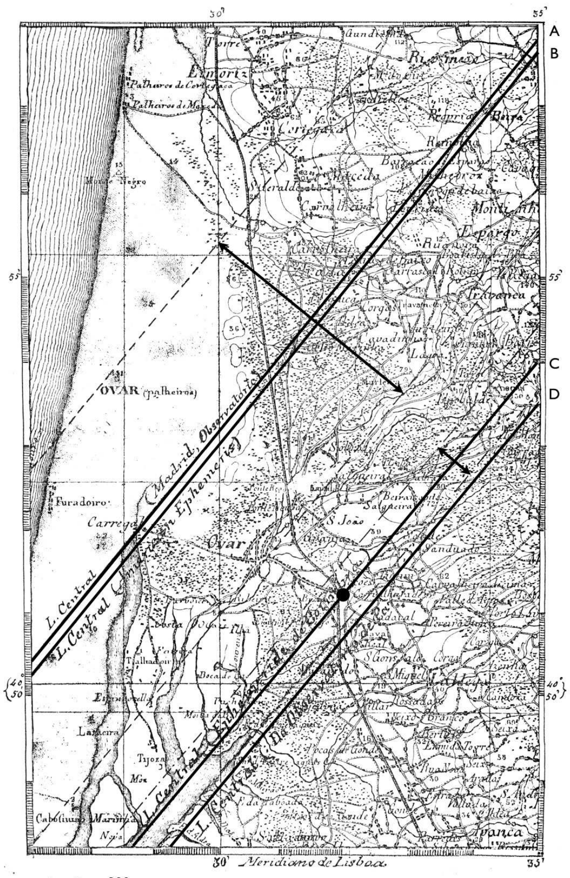

Figure 1: Central eclipse trajectories in the vicinity of Ovar are indicated by straight lines. The upper line (A) was calculated by the Madrid Observatory. Costa Lobo's calculated lines obtained using the eclipse elements from the *American Ephemeris* (B), *Efemérides de Coimbra* (C) and Paris Observatory (D) follow from top to bottom. Shadow band widths according to the Madrid Observatory and the *Efemérides de Coimbra* are indicated by the double arrows. The circle marks the approximate location of Costa Lobo's main eclipse observing station (after Costa Lobo 1912a:190).

The first twentieth-century solar eclipse with a duration of ≤7 seconds occurred on 17 April 1912, and this particular eclipse generated considerable interest for the following reasons:

1) The rareness of the eclipse was known at the time (Bigourdan, 1912b). To put it in context one should realise that during the five millennia from 2000 BC to AD 3000 analysed by Espenak and Meeus there were only 195 annular, total or hybrid eclipses with durations of ≤7 seconds. These represent just 2.5% of all solar eclipses that occurring during this interval.
2) The eclipse shadow path crossed several European countries (Costa Lobo, 1912a).
3) Prediction uncertainties allowed for the occurrence of either a hybrid or an annular eclipse.
4) The observed circumstances of this particular eclipse would provide a test for several astrometric parameters.

In the beginning of the twentieth century the major ephemerides were calculated with slightly different parameters. Table 2 summarises the parameters used in predicting the 17 April 1912 solar eclipse. The ephemerides mainly differed in their listing of the Moon's declination and semidiameter (Oom, 1912).

2 THE OVAR LOCATION

Different lunar eclipse elements implied an uncertainty in the location and width of the lunar shadow cone at the Earth's surface. The effect of the different eclipse parameters may be better appreciated by looking at the different predictions in the vicinity of Ovar, a town located on the west coast of Portugal close to the point where the Moon's shadow cone would first intersect the European continent.

It is obvious from Figure 1 that the eclipse shadow bands calculated using elements from the Madrid Observatory and the *Efemérides de Coimbra* do not overlap, the shadow path half-widths being smaller than the 4900m separation between the two predicted central line trajectories (Costa Lobo, 1912a). This meant that an observer located at the central line as defined by the Madrid Observatory would only see a partial eclipse according to the *Efemérides de Coimbra*, and *vice versa*.

The eclipse would start as annular in Venezuela and terminate as annular in Russia. If it was a hybrid then totality would start over the Atlantic Ocean and terminate either in the Gulf of Biscay, as predicted by the *American Ephemeris* (see Figure 2), or in Belgium, according to the *Connaissance des Temps* (Anonymous, 1912a; Moreux, 1912b). Modern calculations by Espenak (2010) confirm that the April 1912 eclipse was a hybrid, with a maximum totality duration of 2 seconds. The eclipse transitions from annular to total and back occurred over the Atlantic Ocean and in the Bay of Biscay (ibid.).

The best locality for a total eclipse observation was the Iberian peninsula, and expeditions from the Imperial Academy of Sciences of Saint Petersburg, Paris Observatory and the South Kensington Solar Physics Observatory were located in and around Ovar, even though the predicted duration of the eclipse was at best just a few seconds. Madrid Observatory predicted the longest duration, which was just 6.7 seconds (Costa Lobo, 1912a; 1912c).

Being aware of the eclipse characteristics, Francisco Miranda da Costa Lobo (Figure 3), Professor of Astronomy in the Faculty of Sciences at Coimbra University and an astronomer at the University Observatory, decided to establish several observing stations in a line perpendicular to the shadow path predicted by the Coimbra ephemeris in order to better define the true one. This technique had been tried before, for instance by Airy during the annular solar eclipses of 1847 and 1858 and in Algeria during the 30 August 1905 eclipse (Airy, 1896; Fouché, 1912). It was also applied elsewhere in 1912: for example, between Trappes and Neauphle students from the Paris Polytechnic School were located at observing sites 100m apart (Carvallo, 1912). In Ovar, eleven different observing stations approximately 500m apart were spread along a 6km line. Nine stations were maintained by Coimbra University, and one each by the French and Russian expeditions. Not surprisingly, the majority of the Coimbra University expedition equipment was located near the central line calculated by Costa Lobo (Figure 1). Besides the expected astronomical equipment, this station boasted a novel instrument: a 'modest' film camera (Costa Lobo, 1912c).

3 ASTRONOMICAL CINEMATOGRAPHY DURING THE 1912 ECLIPSE

The film camera featured a horizontally-placed 0.07m aperture and 1.14m focal length lens. A heliostat tracked the Sun and fed the solar radiation to the lens (Figure 4).

In the camera focus plane the solar image had a diameter of 10.6mm. A film of the partial and total phases was obtained (Anonymous, 1912b). During totality the camera recorded 560 images per minute, that is approximately 9.3 images per second (Costa Lobo, 1912b). The total length of the film is unknown. To our knowledge this was the first Portuguese scientific film ever made and one of the earliest astronomical films worldwide (see Matos-Cruz, 1989).

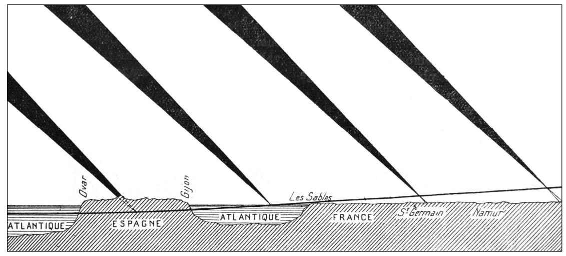

Figure 2: Path of the Moon's shadow cone vertice (after Moreux, 1912a).

The beginning of time lapse photography to capture astronomical events occurred when Janssen attempted to record the 1874 transit of Venus contacts with his 'photographic revolver', the first of all the cinema precursors (see Launay and Hingley, 2005). The film camera's adoption by astronomical observatories did not happen quickly, due—we believe—to the lack of

Figure 3: Francisco Miranda da Costa Lobo, 1864–1945 (after Amorim, 1955).

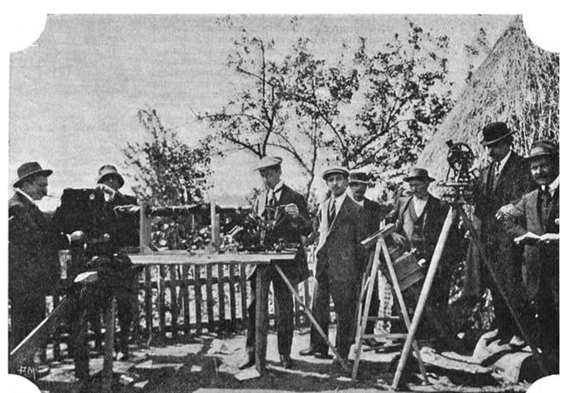

Figure 4: The main Portuguese eclipse station. The film camera can be seen on the left facing the heliostat approximately located at the figure center. Costa Lobo is behind the theodolite wearing a bowler hat (photograph by Ricardo Ribeiro, after Costa Lobo 1912c: 190).

Table 3: A list of films obtained during the 17 April 1912 solar eclipse.[1]

Location	Observer	Nº	D_{Sun} (mm)	Frames /s
Ovar, Portugal	Francisco Costa Lobo	1	10.6	9.3
Barco de Valdeorras, Spain	José Comas Solá[2]	1	-	-
Cacabelos, Spain	Fred Vlès; Jacques Carvallo[3]	1	4.6	?
Between Trappes and Neauphle, France	Emmanuel Carvallo	?	?	?
Saint-Germain-en-Laye, France	Aymar Baume-Pluvinel	1	14	13-14
Grand-Croix, France	Léon Gaumont	1	?	?
Lyon Observatory, France[4]	Perrigot	1	?	10
Namur, Belgium	Father Willaert	1	8.5	14
Hagenow, Germany	Kasimir Graff; Lippert	1	5.4	7.5

Notes:
1. The following sources were used in assembling this table: André, 1912; Carvallo, 1912; Costa Lobo, 1912b; de la Baume Pluvinel, 1912; Flammarion, 1912a; 1912c; Schorr, 1912; and Vlès and Carvallo, 1912.
2. Solá used two prisms in front of the film camera and successfully recorded the variation of the solar spectra during the eclipse.
3. Vlès and Carvallo had two film cameras but one stopped working. Emmanuel Carvallo simply referred in passing to the use of a few film cameras.
4. At Lyon Observatory a partial eclipse was observed. The film camera recorded the eclipse projected onto a screen.

convenient observable subjects, celestial objects usually being rather faint. Solar eclipses offered a notable exception, where a permanent record of these short-duration event was desirable. Although film cameras had been deployed during solar eclipses prior to 1912, this year marks the first time that widespread use was made of this technology (Deslandres, 1900; Solá, 1905). Table 3 summarises the various attempts that were made to photograph the 1912 solar eclipse using film cameras.

Figure 5: Seven frames from Costa Lobo's film published in the *Comptes Rendus* (Lobo, 1912b: 1398). The non-uniform distribution of Baily's Beads is apparent.

The first reports of cinematographical observations were usually succinct. They referred to the use of film cameras, provided a short description of the apparatus used—but not always, as an inspection of Table 3 shows—and gave a brief account of the images obtained. Scientific results obtained from the film images were not discussed. For example, at the 22 April 1912 session of the *Académie des Sciences de Paris* the cinematographic results of the Paris Polytechnic School effort led by Emmanuel Carvallo were still unknown (Carvallo, 1912), and we could not find a later reference to this expedition. However, at the next session of the Academy, on 29 April, the cinematographical observations and apparatus used by Aymar Baume-Pluvinel in France and Fred Vlès and Jacques Carvallo in Spain were described. De la Baume Pluvinel (1912) used the apparent intensity of the three Baily's Beads to estimate the eclipse central phase (cf. Carvallo, 1912). On 12 May Richard Schorr wrote a paper that was later published in *Astronomische Nachrichten* where he described the observations made at Hagenow in Germany. According to Schorr (1912), the film confirmed the observers' visual impression that their station was located to the north of the central eclipse line. Several film strips were reproduced in Schorr's paper, but no comment about them was provided.

Table 4: Costa Lobo considered the Moon's radius and velocity relative to the Sun as equal to 1,736.66km and 1km/s, respectively.

Lunar	Lower limit	Upper limit
$D_{eq} - D_{pol}$ (km)	4	12
Flatness	1/1800	1/600

4 COSTA LOBO'S ECLIPSE FILM ANALYSIS

A substantially different approach was presented by Costa Lobo in a note read at the 20 May session of the Paris Academy and published in the 28 May issue of *Comptes Rendus de l'Académie des Sciences* (Costa Lobo, 1912b). In it he described the Ovar cinematographical apparatus, analysed the film obtained and proposed an unexpected hypothesis. The Ovar film recorded the eclipse totality at a rate of approximately 9.3 images per second (ibid.). The second and third contacts were registered, and 158 images showed Baily's Beads. Costa Lobo realised that the images revealed a non-uniform distribution of Baily's Beads around the lunar limb (see Figure 5).

In particular, forty-four images after the appearance of the first Baily's Beads they disappeared for 4.4 seconds (40 images), in the approximate direction of the Moon's movement while staying visible at the Moon's north and south limbs. The unavailability of Costa Lobo's complete totality film, which is presumably lost, prevents a confirmation of these times. Costa Lobo concluded that the eclipse was total in the direction of the Moon's movement and annular in the perpendicular direction. Further, assuming that the observed asymmetry arose from polar flattening (defined as equatorial radius — polar radius/equatorial radius) he proceeded to estimate it in two limiting situations. Firstly, Costa Lobo considered that the lunar valleys grazed the solar disk in the direction perpendicular to the Moon's movement. This implied a difference between the lunar equatorial and polar diameters equal to the space travelled by Moon during the 4.4 seconds when the Baily's Beads were invisible. This provided a lower limit for the lunar flattening parameter. Secondly, he assumed that the highest lunar mountain tops rose 8km above the lunar valley floor and, somewhat arbitrarily, that Baily's Beads became visible when the mountain was half inside the solar disk. The first and last Baily's Beads appeared approximately in the Moon's east and west directions, respectively. By assuming that the highest north and south lunar mountains were at most half inside the solar disk Costa Lobo obtained an upper limit to lunar flattening. In this situation, the difference between the equatorial and polar diameters was equal to twice the half height of a lunar mountain (4km) plus the space travelled by the Moon during the Baily's Beads' 4.4 seconds of invisibility. Taking the lunar radius and velocity relative to the Sun as equal to 1,736.66km and 1km/s respectively, he obtained the values shown in Table 4.

We were unable to reproduce Costa Lobo flattening results. Our calculation approximately doubles Costa Lobo's flattening values for both lower and upper limits. This points to a trivial mistake, which coupled with the crudely-approximated lunar velocity used in the calculations led us to believe the note presented at Académie des Sciences de Paris was hastily written.

Later that year Costa Lobo wrote a longer more detailed paper for the new Coimbra University journal *Revista da Universidade de Coimbra*. This paper also included two illustrations of 42 positive and 80 negative film frames (see Figure 6). According to Costa Lobo, the maximum eclipse phase was shown in both.

In this new paper Costa Lobo improved his film analysis. For the lunar flattening lower limit, he maintained the rationale presented in May but changed the Moon's relative velocity to 692.66m/s, which was its then-accepted value. He then proceeded to estimate a different upper limit based upon a new assumption. An interval of 13.2 seconds elapsed between the appearance of Baily's Beads before the second contact and their disappearance after the third contact. This implied a lunar travel distance of 9.143km. Assuming the worst case scenario, i.e. that the Beads appear firstly in the east-west direction and at most the top of the north and south mountains graze the solar disk, the distance travelled corresponds to the diameter difference between the two directions everything else being considered equal. The results obtained are presented in Table 5.

In this paper he also determined whether the libration of an ellipsoidal Moon, with the longest axis in the Moon-Earth direction as predicted by celestial mechanics models, could explain the observations. He concluded that the effect was far too small. Costa Lobo believed the Moon was slightly flattened, although he had reservations about the upper limit in the absence of more data, remarking that "It is evident that other observations are necessary so that definitive values may be established." (Costa Lobo, 1912c: 571; our translation).

We believe that Costa Lobo's second paper (1912a), despite being written in French, was not available to a wide audience. For instance, the journal *Revista da Universidade de Coimbra* does not appear in the library catalogues of either the Royal Astronomical Society or Paris Observatory, although an offprint exists in Paris. Unfortunately it was not possible to establish Costa Lobo's offprint distribution network. Importantly, for the discussion that will follow, we do not know if Camille Flammarion had access to this paper and its larger number of film images.

5 DISCUSSING THE ECLIPSE RESULTS

Costa Lobo's papers not only break with usual astronomical movie film analysis methodology but more importantly propose a totally unexpected hypothesis—evidence of lunar flattening. At the time it was known that fluid dynamics implied a non-spherical Moon. An ellipsoid with the longest axis directed towards the Earth was the model commonly employed. Some authors considered the lunar shape was better described as a spheroid (i.e. with equal polar and equatorial radii), while others claimed a small difference—less than 20m—between these two radii (Saunder, 1905; Puiseux, 1908).

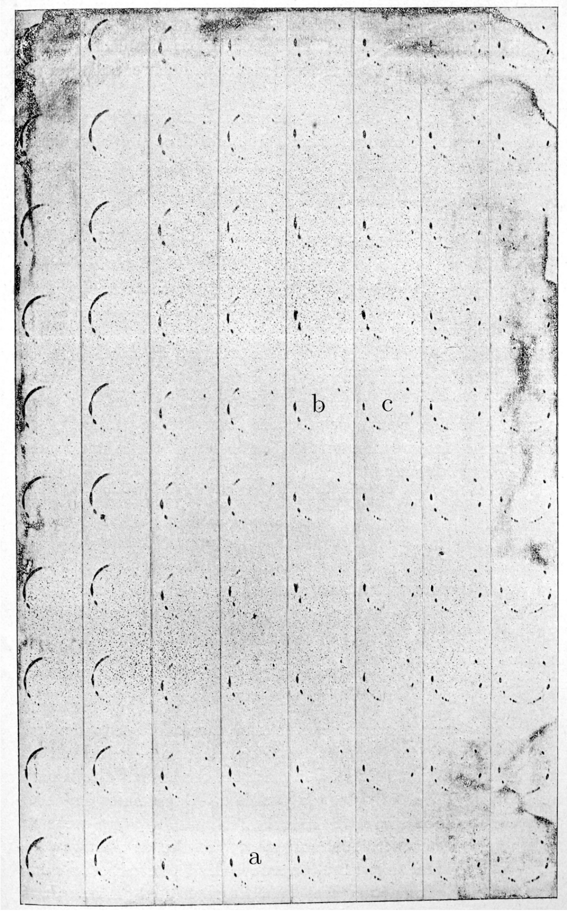

Figure 6: Eighty non-consecutive negative film frames including the maximum eclipse phase. The seven frames published in the *Comptes Rendues* note (Figure 4) are similar to that indicated by letter *c*. The images should be read vertically from the top left image to the bottom right one for correct eclipse evolution. After reaching the bottom of a column the reader should continue at the top of the following column to the right. The letters allude to frames that will be analysed later in Figures 6 and 8 (after Costa Lobo, 1912c: 583).

The noted French astronomer, Camille Flammarion (Figure 7) reprinted Costa Lobo's "... especially interesting ...." 28 May *Comptes Rendus* note in the July 1912 issue of the *Bulletin de la Société Astronomique de France* (Flammarion, 1912c). In his communication, entitled "Lunar shape deduced from cinematographical observation" (our translation), Flammarion mentioned that Léon Gaumont's film obtained at Grand-Croix also showed a larger lunar axis in the direction of lunar movement than in the perpendicular one. The paper also briefly referred to another observer claiming a similar conclusion (Flammarion, 1912a). Nonetheless, the best support for Costa Lobo's hypothesis was provided by Fernand Willaert (1877–1953) in a paper he and D. Lucas published in the 20 July 1912 issue of the journal *Revue des Questions Scientifiques*. At Namur (in Belgium) the eclipse was

Table 5: The values assume a 1,736.66km Moon radius and a Moon-Sun relative velocity of 0.69266 km/s.

Lunar	Lower limit	Upper limit
$D_{eq} - D_{pol}$ (km)	3.004	9.143
Flatness	1/1156	1/380

Figure 7: Camille Flammarion, 1842–1925.

annular, and when the film was first inspected by Willaert he was

> … struck by the fact that not only the north-ernmost part of the ring was thicker than the southern part which indicated a station located to the north of central line, but that the ring's southern part was thicker than in the equatorial region. (Lucas and Willaert, 1912; or translation),

Analysing the Moon's movement registered on the film and assuming a circular Sun, Willaert obtained a difference between the two lunar radii that was smaller than Costa Lobo's and a corresponding lunar flattening of 1/2050, and he claimed that this result contradicted the previous finding by Bessel and Wichmann of no lunar flattening. From his paper, one realises that Willaert was aware of the other 1912 eclipse cinematographical attempts and, in particular, of Costa Lobo's *Comptes Rendus* note.

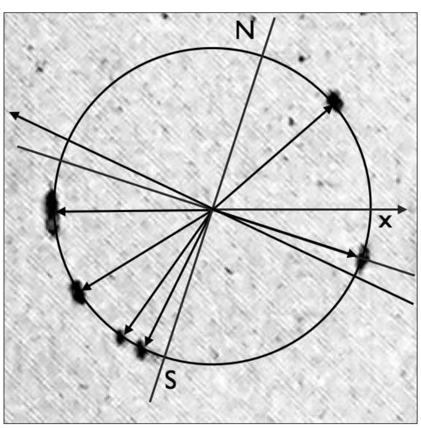

Figure 8: Eclipse frame from Ovar during the central phase. This is the frame signalled by letter *c* in Figure 5. Letters *N* and *S* indicate lunar north and south poles, respectively. The longest arrow defines the approximate direction of lunar motion. The arbitrarily-chosen x-axis (see the text) is also represented.

In a communication read on 16 September 1912 at the Paris Academy, Fred Vlès presented his Cacabelos film analysis and results. He measured the size of the chords joining the Moon tips and their direction as a function of the angle between the chord and the horizon. He then compared the results with those obtained by passing several geometrical figures (circles and ellipses) in front of each other. Several combinations were compatible with the film data although all implied the need for "… at least one of the celestial bodies to possess a non-circular shape." (Vlès, 1912.; our translation). More importantly, Vlès claimed that an ellipse with a major axis in the Moon's movement direction and a circular Sun, as proposed by Costa Lobo, did not agree with his film results. Any possible consequences of Vlès' location approximately 4km away from the eclipse centre line and outside the lunar shadow path were not discussed (ibid.; Vlès and Carvallo, 1912).

By November Flammarion had changed his mind. While still analysing more than 250 eclipse reports he received or that were sent to the Société Astronomique de France, he was already dismissing Costa Lobo's interpretation, remarking

> … that the Moon was not elongated as we believed in the east-west direction. The observed difference is due to the mountains. (Flammarion, 1912b; our translation).

At the time it was known that Baily's Beads were a consequence of the lunar profile irregularities. The specific conditions of the 1912 eclipse led to *a priori* predictions and to *a posteriori* determinations of the lunar profile (Graff, 1912a, 1912b; Hayn, 1912; Simonin, 1914). In particular, Graff (1912a, 1912c) determined a profile from micrometrical measurements performed upon an eclipse photograph obtained at Becklingen (Germany) that was published in *Astronomische Nachrichten* and later in the December issue of the *Bulletin de la Société Astronomique de France*.

In the *Annuaire Astronomique et Météorologique pour 1913*, Flammarion summarised all the relevant eclipse observations relating to possible lunar flattening, including his previously referred to analysis of Gaumont's film. He concluded (Flammarion, 1913; our translation) that Costa Lobo's "… explanation does not seem likely. Instead, we must attribute the [observed] irregularity …" to the Moon's limb profile. Flammarion (1912a) believed Graff's lunar profile showed a lunar movement direction almost parallel to the highest lunar mountains and perpendicular to the deepest valleys.

To test Flammarion's explanation we selected a frame from Costa Lobo's film strip (Figure 6) similar to those published in the Costa Lobo (1912b). This frame is shown in Figure 8, where six Baily's Beads are visible. We qualitatively estimated the Moon's centre by passing a circumference through all of the Bailey's Beads. Next we considered an arbitrary cartesian coordinate system with x- and y-axes parallel to the image sides. In this system we measured the angle between each Bailey's Bead direction (indicated by the arrow) and the x-axis. If the Bailey's Beads were located at the deepest lunar depressions by adding or subtracting a constant from our measured angles we would be able to match Graff's profile results. Despite position uncertainties resulting from the shape of the Baily's Beads and the Moon's center estimation there

Table 6: The deepest lunar valleys and the locations of Baily's Beads.

PA (°)	10 x valley depth (arcsec)	Baily's Bead location (°)
108	–25	108
140	–15	139
162	–9	162
172	–15	171
266	–21	269
328	–17	328

Note: The first two columns show the position angles (PA) of the lunar valleys measured from the lunar north pole in a counter-clockwise direction and ten times their depth referred to a mean circular Moon as determined by Graff.

is a good qualitative agreement between the two data sets (Table 6). This also enabled us to approximately mark in Figure 8 the Moon's polar and equatorial directions (Graff, 1912a; Flammarion, 1912a).

However Flammarion's objection to Costa Lobo's hypothesis referred not to lunar positions but to diameters. Using Graff's data, we therefore proceeded to calculate the lunar diameters as a function of position angle (see Table 7). The polar and equatorial diameters correspond to angular positions of 0° and 90°, respectively.

Analysis of Figure 6 shows that following the disappearance of the upper left Baily's Bead, five Baily's Beads are still visible (Figure 9a), four in the lower left quadrant and one in the upper right one (Figure 9b), before a new Baily's Bead appears at the lunar equator (Figure 9c). This seems to contradict Graff's small lunar diameter in the east-west direction (Table 7) and to indicate—as was originally claimed by Costa Lobo —a larger lunar diameter in the direction of the Moon's movement than in the perpendicular direction. The problem is, however, more complex since Graff's published values do not include any uncertainties.

The Paris Observatory astronomer Martial Simonin reviewed all of the 1912 eclipse information that he could locate, and in his 1914 memoir he presented a mean lunar profile for the eclipse day calculated from micrometrical measurements of photographs obtained by Graff, Hayn, Senouque, Croze and Solomos (Simonin, 1914). In Figure 10 we have plotted Simonin's mean profile and corresponding sample standard deviations where available as a function of position angle.

Two conclusions are immediate. Firstly, the data have large uncertainties (Table 8). Secondly, four out of Graff's six lunar depressions correspond to the deepest and better-defined (higher signal-to-noise ratio) lunar valleys (see Tables 6 and 8), the other two valleys also being compatible with Simonin's data. Consequently, the good agreement previously mentioned between the observed Baily's Bead locations and lunar profile depressions is maintained, despite two poorly-defined (low signal-to-noise) valleys not showing Baily's Beads (Table 8). This result lends support to the suggestion that the irregular lunar profile is the origin of the appearance and evolution of the observed Baily's Beads.

Table 7: The six smallest lunar diameters according to Graff and the observed locations of Baily's Beads.

PA (°)	10 x diameter variation (arcsec)	Baily's Bead location (°)
56	–13	
90	–11	89
108	–30	108
140	–20	139
146	–16	148
172	–15	171

Note: The first two columns show the lunar diameter position angle (PA) measured from the polar direction in a counter-clockwise direction and ten times the corresponding variation from a mean circular Moon. The third column presents the Baily's Bead location as explained in the text.

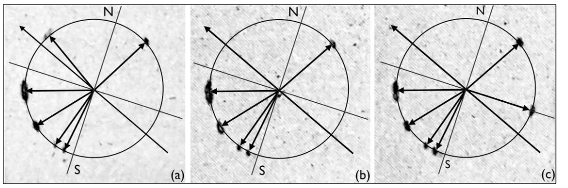

Figure 9: Three eclipse instants. Time increases from (a) to (c) (see corresponding letters in Figure 6). Arrows from the frame centers indicate Bailey's Bead directions. The Moon's polar, equatorial and approximate movement directions were superimposed on the film frames.

On the other hand, the lunar diameter variation from the mean value was poorly defined in the north-south and east-west directions. For example, from Simonin's data one obtains for position angles of 0.6 and 89.6 values of –0.40 ± 0.64 and 0.20 ± 1.2 arcsec respectively.

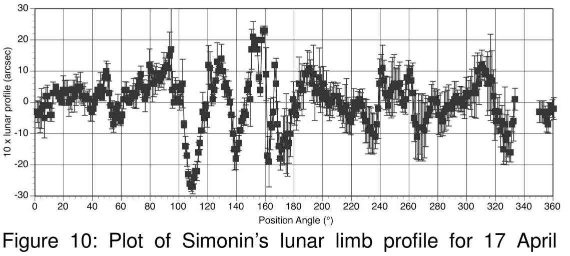

Figure 10: Plot of Simonin's lunar limb profile for 17 April 1912. Uncertainties are the sample standard deviation. Position angle (PA) is the angle measured from the lunar North Pole in a counter-clockwise direction. The Moon's west and east correspond to PA angles of 90° and 270°, respectively. The gap indicates a region for which only one set of measurements was available.

Due to all the uncertainties, we are not surprised by the cautious response provided by Paul Stroobant (1914; our translation), a future Director of the Royal Belgian Observatory:

> The agreement between Costa Lobo and Fr Willaert's film results is remarkable, especially since direct measurements have never shown this difference, which must be verified by other methods.

Table 8: The locations and depths (<1 arcsec) of the deepest lunar valleys according to Simonin (1914).

PA (°)	10 x Valley depth (arcsec)	SNR	Baily's Bead locations
108.6	–26.7 ± 1.5	18	108
139.6	–18.2 ± 3.5	5.1	139
162.6	–19.3 ± 8.1	2.3	162
170.6	–17.8 ± 3.7	4.9	171
175.6	–14.3 ± 9.7	1.4	
236.6	–11.1 ± 3.9	2.8	
265.6	–10.7 ± 7.5	1.5	269
325.6	–14.5 ± 4.8	3.1	328
330.1	–15.6 ± 3.1	5.2	

Note: PA is the angle measured from the lunar North Pole in a counter-clockwise direction. Valley depth uncertainty is given by the sample standard deviation. SNR is the signal to noise ratio. PA angles 325.6 and 330.1 correspond to the two deepest points in a wide depression.

In reality, and despite the fact that no definitive conclusion could be obtained without more data (see Table 9), a 'consensus' was quickly established within the international astronomical community and Costa Lobo's and Willaert's observations were soon forgotten, as the following examples show.

In his report for the *Annuaire du Bureau des Longitudes pour 1913*, Bigourdan (1912a; our translation) remarked that

> It seems premature to conclude that there is Moon flattening since everyone agrees that the serrated edge of the Moon is more pronounced in the north and south than in the east and west.

Meanwhile, the "Council note on solar research in 1912" delivered at the annual meeting of the Royal Astronomical Society on 14 February 1913 merely mentioned that "Kinematograph records were obtained by some of the French observers." (Anonymous, 1913), and the Eclipse Commission report presented on 4 August 1913 at the Fifth International Union for Co-operation in Solar Research conference in Bonn did not even mention any 17 April 1912 eclipse films (Anonymous, 1914: 147).

Table 9: Costa Lobo's and Willaert's differences between the lunar equatorial (R_{eq}) and polar (R_{pol}) radii, and flattening. For comparison, this table also includes the median, mean and range of the data sample standard deviations calculated from Simonin's memoir.

Author	$R_{eq} - R_{pol}$ (arcsec)	$R_{eq} - R_{pol}$ (km)	Flattening
Lobo (1912b)	0.82	1.5	1/1156
Lucas & Willaert	0.47	0.83	1/2050
Profile uncertainties			
	Std dev'n (arcsec)	Std dev'n (km)	
Median	0.40	0.74	
Mean	0.44	0.81	
Range	< 1.4	< 2.6	

6 CONCLUSIONS

In contrast to late nineteenth and early twentieth century common practise, the main scientific rationale behind the 17 April 1912 eclipse observation was astrometric rather than astrophysical. This was also the first astronomical observation where movie cameras were widely used to record a solar eclipse. Films of this eclipse were obtained in Portugal, Spain, France, Belgium and Germany. Analysing the Ovar film, Portuguese astronomer Costa Lobo hypothesised a lunar flattening to explain the observed asymmetrical distribution of the Baily's Beads. Costa Lobo estimated a lunar flattening in the range of 1/1156 to 1/380. Father Fernand Willaert analysed a film recorded at Namur where the eclipse was annular and he also concluded that the Moon was slightly flattened, albeit with a lower value of 1/2050. Initial international support for this viewpoint soon waned due to a lack of supporting data, and the international community preferred to attribute the film observations to lunar limb profile irregularities. Soon Costa Lobo's lunar flattening hypothesis was disregarded and the cinematographical observations were forgotten. In our opinion, several facts contributed to this outcome:

1) Costa Lobo and Willaert were not well known within the international astronomical community. In particular, the 20 May note read at the *Académie des Sciences de Paris* was not only Costa Lobo's first international astronomical paper but we believe that it was also hastily written. A more thorough follow-up paper written by Costa Lobo in French was published in *Revista da Universidade de Coimbra*, but this new journal was not available to most astronomers.

2) Costa Lobo's results were obtained with a recent and still unproved technique, namely cinematography. We also need to point out that when compared with photographs, film images were small and difficult to disseminate.

3) Three different interpretations of the cinematographical data were put forward to explain the observations: lunar and/or solar flattening, or a lunar limb profile irregularity effect. This last-mentioned option not only maintained the accepted view of a mean circular Moon in the line of sight but was also supported by influential astronomers like Flammarion.

4) Finally, confirming the 1912 cinematographical results would require similar observations but unfortunately short duration eclipses of this type were rare. In fact, astronomers had to wait until 1927 for the occurrence of a suitable eclipse, so a speedy confirmation of the 1912 eclipse observation was not possible.

This paper reviews the first astronomical hypothesis based upon cinematographical observations and its impact. Costa Lobo's lunar flattening hypothesis caught the scientific community by surprise. Despite the fact that the available data were inconclusive—in particular, the lunar polar and equatorial radius differences estimated by Costa Lobo and Willaert were comparable with known lunar profile uncertainties—the scientific community quickly arrived at an agreed-upon interpretation. The observations could best be explained by irregularities of the lunar profile and the cinematographical results were quickly forgotten. Notwithstanding this decision, it is time, we believe, to highlight this important milestone in the history of astronomy.

Finally, we should note that Costa Lobo's proposed lunar flattening interval of 1/1156 to 1/380 accommodates the modern value of 1/581.899 which was obtained recently by the Kaguya (Selene) satellite mission (see Araki et al., 2009).

7 ACKNOWLEDGEMENTS

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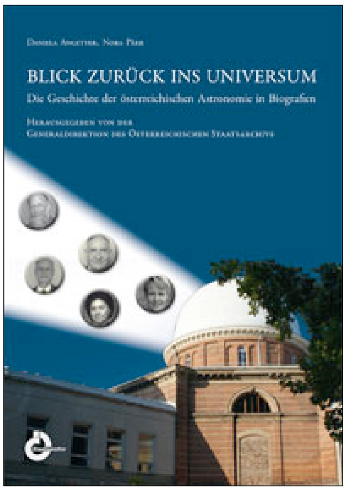
BOOK REVIEWS

Blick zurück ins Universum. Die Geschichte der österreichischen Astronomie in Biografien, **by Daniela Angetter and Nora Pärr (edited by the Generaldirektion des Österreichischen Staatsarchivs, Vienna 2009), pp. 321. ISBN 978-3-902575-27-2 (paperback), €22.00, 240 x 170 mm.**



Looking Back into the Universe is the somewhat unusual title of this publication of the General Administration of the Austrian State Archive which contains biographies of about 125 astronomers associated with Austria who lived between the fourteenth century and the present day. While Nora Pärr covered almost 30 astronomers who lived in the fourteenth to eighteenth centuries, Daniela Angetter wrote biographies of the more recent ones—about 100 astronomers of the nineteenth and twentieth centuries. Of the 24 living astronomers included in the book, 13 provided their own biographies.

The term 'Austrian' is taken in a broad sense; thus the book includes astronomers like Johannes Regiomontanus, who was born in Germany and died in Italy, but spent some of his years at Vienna University; Johannes Kepler, who spent many years as a teacher and mathematician in Graz and Linz; and Rugjer Josip Bošković, who was born in 1711 near Dubrovnik (which only in the nineteenth century was part of the Austrian-Hungarian Empire). Among twentieth century scientists, both Austrians working abroad (like the well-known emigrant, Thomas Gold) and immigrants (like the Vienna Observatory Director in Nazi times, Bruno Thüring) can be found. On the other hand, Joseph Johann von Littrow (1781–1840), the most famous Director of the previous Vienna Observatory, and author of the famous popular book *Wunder des Himmels*, is inexplicably absent from the book—only his son, Karl Lugwig, is included.

Besides dates and places of birth and death, and (in most cases) a portrait of intermediate quality, a curriculum vitae of from one to five pages is given, which is supplemented by a selection of references and biographical information.

The entries have about the same length and quality as those in Hockey et al.'s *Biographical Encyclopedia of Astronomers*, but a cursory comparison shows that only about 25% of the deceased astronomers in the Austrian book are included in Hockey's two volumes, indicating that Angetter and Pärr were able to include quite a number of 'stars of fainter magnitude'.

This book can be recommended to anyone interested in the history of European astronomy, and is available from the Austrian State Archive (see http://www.austria.gv.at/site/5075/default.aspx).

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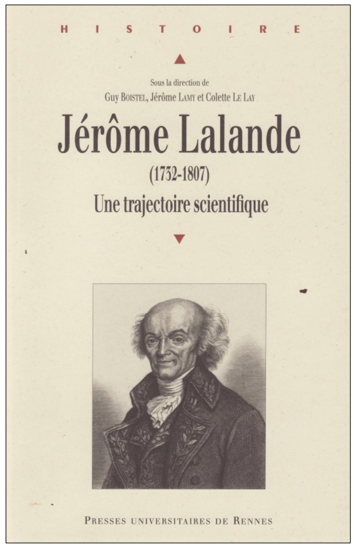
Jérôme Lalande (1732-1807). Une Trajectoire Scientifique**, edited by Guy Boistel, Jérôme Lamy and Colette Le Lay, (Rennes, Presses Universitaires de Rennes, 2010), pp. 234, ISBN 978-2-7535-0991-7 (paperback), €18.0, 240 x 155 mm.**



Jérôme Lalande (1732–1807) is one of the most interesting figures of eighteenth century astronomy. He is mainly known for his *Histoire Céleste* which contains the positions of 50,000 stars, but his other activities were considerable and very varied. While centring on astronomy, his work extended to several other scientific domains and even to tourism. He was also a very good popular writer. An excellent biography was recently published by Simone Dumont (*Un Astronome des Lumières, Jérôme Lalande,* 2007, Paris, Vuibert and Paris Observatory), and should be read before this new book, which gives many details on Lalande's activities but assumes some previous knowledge of his multi-faceted personality.

The present book was written by twelve historians with complementary competences on the variety of topics addressed by Lalande. The two first chapters show how Lalande—who had no official position in an observatory and in particular at the Paris Observatory, which was the exclusive domain of the Cassinis—still managed to assemble a network of pupils and friends in order to gather large quantities of astronomical data and to reduce observations. The next chapter describes how his talents for organization reached their climax with the founding of the *Bureau des Longitudes* in 1795, during the French Revolution (Lalande was active in the Revolution and was consequently powerful amongst the scientists of the time). This gave him the opportunity to make official and develop even further his network of collaborators, and the Bureau essentially took control of all French astronomy.

Lalande had many connections with the Navy, as described in the following chapter. He was for many years the editor of the official ephemerides of the Academie des Sciences, the *Connaissance des Temps,* and managed to transform it into a nautical almanac, with lunar tables for the determination of longitudes borrowed from Maskelyne's tables in the English *Nautical Almanac*. In 1793 Lalande also published an *Abrégé de Navigation* which contained examples of astronomical calculations useful for sailors.

The second part of the book is devoted to the many close relations that Lalande maintained with foreign scientists. The first chapter recalls that he was sent to Berlin in 1751, aged only 19, to make observations complementary to those of La Caille at the Cape of Good Hope in order to obtain parallaxes of Mars, Venus and the Moon. He was so successful that he was elected to the Academy of Berlin in 1753, together with Le Gentil. In Berlin, Lalande met Euler, with whom he later had an active exchange of correspondence and some minor disputes. The next chapter tells us that Lalande visited England twice, in 1763 and 1788, when he met and became a friend of Nevil Maskelyne. Many letters between the two men are preserved (and two letters from Lalande are reproduced in facsimile). Their relations were not interrupted by the wars between France and England. Another chapter is devoted to the tumultuous relations between Lalande and the Austrian Jesuit, Maximilian Hell. The last chapter of this part of the book is concerned with the numerous exchanges of letters between an aged Lalande and the amateur astronomer, Honoré Flauguergues, and also with the famous Baron Franz Xaver von Zach (during the blockade of France by England the letters between Lalande and Zach transited through Switzerland). Some letters are also reproduced in these two chapters.

The last section of the book describes some non-astronomical activities of Lalande. He was a remarkable successor of the Encyclopaedists (he actually wrote many articles for the encyclopaedia of Panckoucke). In 1778 he published a large book with splendid illustrations devoted to ship canals, in particular the famous *Canal du Midi* which connects the Atlantic Ocean with the Mediterranean Sea (first chapter of this section). Lalande also wrote an *Art du Papier* (The Art of Paper-making) and an *Art du Tanneur* (The Art of the Tanner), but these are only mentioned in the conclusion without further details. The next chapter tells us that Lalande travelled through Italy in 1765 and, once back in Paris, produced an extensive guide that became 'the bible' for many travellers—including Chateaubriand and Stendhal—well into the nineteenth century. Of course, this guide includes scientific matters, and for this reason is a precious resource for historians of science. The final chapter recalls that, as with several other prominent scientists and artists of his time, Lalande was an atheist and a very active freemason.

Overall, this book is a mine of information, not only on Lalande but also on the astronomical life of his time, during which a 'Republic of Sciences' developed in parallel with the better-known 'Republic of Letters'. It was during this period that science, and in particular astronomy, became truly international with considerable collaborative enterprises like the expeditions to observe the transits of Venus. The editors are to be commended for producing a relatively homogeneous book in spite of the large number of authors. However, a good knowledge of the French language is required to take full advantage of it. Actually, most of the literature concerning Lalande is in French, as can be seen in the bibliography at the end of the book. This book, and the earlier one by Simone Dumont, certainly deserve to be translated into English.

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CORRIGENDUM

Re Thompson, A.R., 2010. The Harvard Radio Astronomy Station at Fort Davis, Texas. *Journal of Astronomical History and Heritage*, 13(1), 17-27.

The name Tom Wilson that occurs in Section 11 and as T. Wilson in the caption of Figure 11 (on page 25) should in fact be Tom Clarke.

I thank D. Downs and J.M. Moran for pointing out this correction.

A. Richard Thompson

CONTENTS