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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. 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).

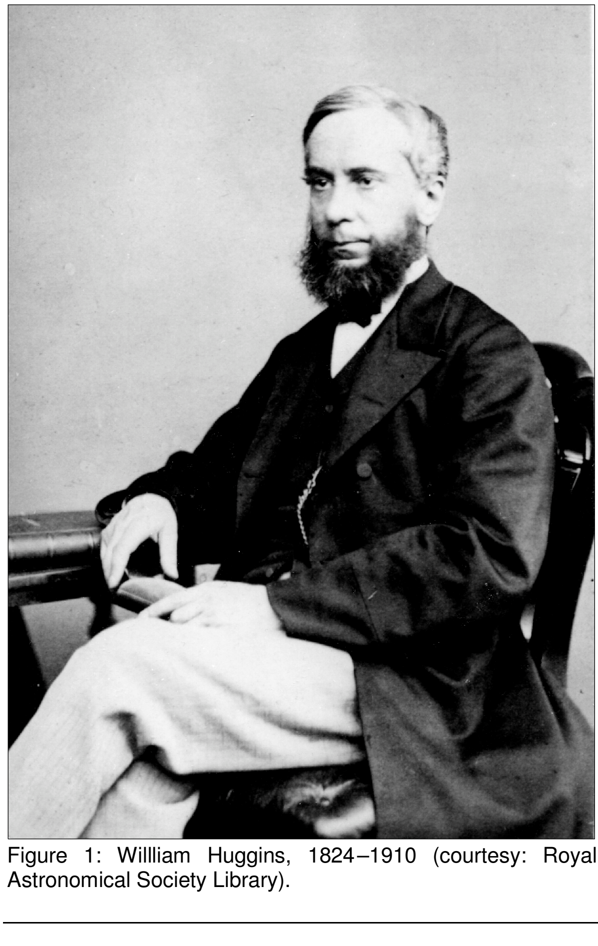

Figure 1: Willliam Huggins, 1824–1910 (courtesy: Royal Astronomical Society Library).

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", "soap-suds 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 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.

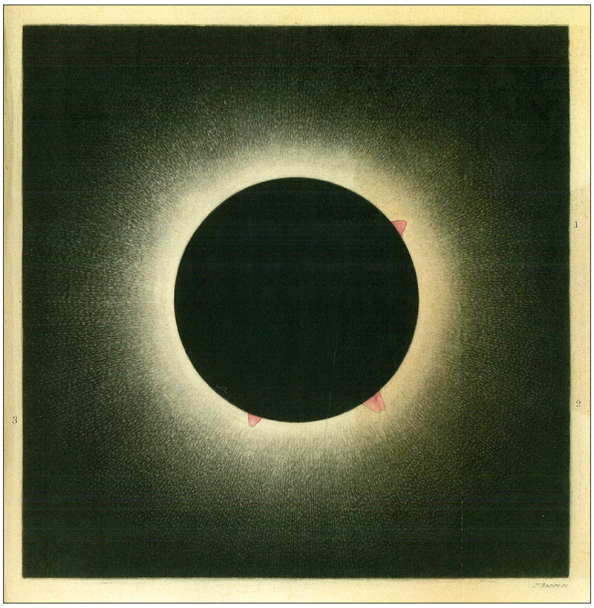

Figure 2: 'Protuberances' around the Sun (after Baily, 1842: facing 212).

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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