The Hidden History of Women in Astronomy

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1 Introduction

What is the Sun made of? One hundred years ago our physical samples consisted of the Earth itself and a few meteorites, mostly rocky, with a few metallic and some rare 'carbonaceous' examples containing volatiles. Furthermore, spectral lines in the Sun's light showed that many of the chemical elements discovered on the Earth were indeed also present on the Sun. Why would you believe that its composition was radically different to the Earth's, especially when the prevailing theories of Solar System origin had them being formed from the same primeval stuff?

There was even a just about tenable theory that the steady accretion of meteorites onto the Solar surface might release enough gravitational energy to keep it hot. What alternative was there? Sir Arthur Eddington had indeed speculated that Einstein's $E = MC^2$ hinted at the possibility of extraordinary energy sources that might power the sun but we were still a decade away from understanding nuclear fusion reactions: Eddington's speculation may have been inspired but it was still vague speculation.

Spectral lines were, in fact, still something of a mystery. We did not know why some spectral lines were so much more prominent than others or why some expected lines failed to appear at all. Until Bohr's model of the atom in 1913 we had no understanding at all of the origin of spectral lines, and there were clearly many missing bits of the puzzle. If you do not fully understand why lines look the way they do line how can you be certain you understand the conditions in which they form and relatively abundances of the contributing chemical elements?

Atomic spectra began to be really understood only after 1925, when the new 'Quantum Mechanics', in the form of Schrödinger's equation, Heisenberg's Matrix Mechanics and Dirac's relativistic theory of the electron replaced the 'Old Quantum Theory'. Everything prior to that was just guesswork and pragmatic 'rules-of-thumb'; after that we really began to understand how atoms actually interacted with light, though much more is required in order to work from observed spectral lines to element abundances. We then had to understand how atoms ionise in the Solar atmosphere and distribute their electrons between different quantum states, and so predict how frequently they might jump between them, releasing or absorbing light in the process. We also needed a theory of the way temperature changes through Solar atmosphere because spectral lines only form when light from hot layers of the Sun pass through somewhat cooler layers. before there is any chance of relating the strength of a spectral line to a relative abundance of the element in the Solar composition. None of this is easy, even now, but in the early 1920s the bits of the puzzle were only just beginning to connect together.

Our current understanding that the visible universe is mostly hydrogen and helium is one of those really important discoveries are taught in every basic science course and the facts are part of 'what everyone knows'. They are now so much part of the astronomical furniture that to some extent we forget when and how the discovery was made and by who, especially if the story of the discovery is a little complicated, and especially, perhaps, if the discoverer was a women.

It was not until 1929 that the dominance of hydrogen and helium in the visible universe became widely accepted, after the publication of a definitive study by Henry Norris Russell (Russell 1929) (the same Russell of the Hertzsprung-Russell diagram). He was not, however, the first to make this deduction—and he fully acknowledged that in his own publications.

In 1925 Cecilia Payne (later Cecilia Payne-Gaposchkin) wrote a PhD thesis at Harvard University (Payne 1925) where Russell was also based, which was later described by the well known American astronomer Otto Struve[1] as *"the most brilliant ever written in astronomy"*. It was, in fact, also the first Havard PhD awarded to a women and the astronomy PhD in Harvard university. Yet Payne was British and had studied physics at Cambridge University, where she was, nevertheless, unable to formally take a degree because she was a woman[2]. After Cambridge she realised that the only career open to her in the UK would be as a teacher in a girls school, and asked for advice from Sir Arthur Eddington, the Plumian Professor of astronomy at Cambridge. He advised her that her career options would be better in the USA and provided a letter of introduction to Harlow Shap-

[1] Otto Struve was a forth generation descendent of the famous Struve family of distinguished European astronomers, and was a leading American astronomer from the 1920s through to the 1960s.

[2] Somewhat horrifyingly, Cambridge did not admit women to degrees until 1948—the last UK university to do so, a full 28 years after Oxford.

ley, the director of the Harvard Observatory, who he knew was interested in promoting opportunities for women in astronomy. With Eddington's recommendation Celia then received funding from a special fellowship established by Shapley and open only to women.

Payne had become enamoured of quantum theory and the new spectroscopic discoveries emerging from physics labs during her undergraduate years. The aim of her PhD was to apply the new understanding and new spectroscopic techniques in order to understand the physical conditions that led to the production of spectral lines in stellar atmospheres. It should then be possible to work back from the observational data towards estimates of temperatures, pressures and element abundances. Her eventual conclusion that stars were mostly hydrogen and helium was, however, very radical for its time and the senior astronomer who reviewed her thesis encouraged her to describe the result as 'probably spurious'. Shapley, her thesis advisor agreed. That reviewer was Henry Norris Russell, whose name was later most often connected with the discovery.

Is this a tale of heroes and villains? It would be easy to draw the wrong conclusions. We must remember that the methods she was using were new and only partly developed and in truth the science itself was new and only partly understood. (This was still prior to Quantum Mechanics.) Very few astronomers at that time would have been able to even follow the theory and fewer would have the skills to reproduce her truly cutting edge research. Until someone else confirmed the result it would be regarded with considerable scepticism.

We also forget these days that the business of making measurements of absolute light intensity using photographic emulsions is extremely delicate. Even in 1973, when I spent a summer working at the Royal Greenwich Observatory, as an assistant to a visiting American spectroscopist, calibrating photographic material was a long involved process. I spent weeks using micro-densitometers to measure the amounts of plate blackening across images of calibration stars, plotting many, many graphs to relate blackening to light intensity, and then transferring all this calibration to spectral scans. Photographic emulsions in the early 1920 were, of course, not nearly as good as they were in the mid 1970s, so the job was much harder then, and one would also have had to worry about the much greater variation of emulsion sensitivity across different light colours. This is all very tricky and easy to get wrong.

Shapley and Russell may have been a little too conservative, and perhaps they would have allowed a male student to publish a radical proposal, especially in a PhD thesis (which tend not to be widely read) but for a female student who would soon be looking for employment in a man's World, their advice was possibly pragmatic sensible.

Russell subsequently spent several years working on the same problem, using different methods. That is simply good science: attempt to reproduce the results using as far as possible independent techniques. He eventually came to the same conclusions and published the results in the widely read *Astrophysical Journal* (Russell 1929), fully acknowledging Payne's priority in his definitive work.

Henry Norris Russell did the essential confirmatory work required to support the radical proposals, and it is certainly true that he developed the techniques to a point where they stood the most rigorous examination by the reviewers of astronomy's most prestigious journal. (We should also remember that atomic theory was also advancing very rapidly after 1925—he had some advantages not available to Celia.) In the discussion section of his very substantial and impressive paper, Russell does indeed make the point more than once that his results confirm the earlier discovery of Celia Payne. Nevertheless, it was Russell's backing of the discovery with his own reputation and the wide dissemination of the conclusions in the *Astrophysical Journal* that marked the point when colleagues really started to believe in the new paradigm. That is why Russell is remembered.

This all may well have still occurred had Payne been a man: discoveries do sometimes tend to be associated with the names of the famous scientists who recognise their importance and bring them to wider attention. Where there are simultaneous and independent discoveries these are also often credited to the more famous or influential party[3]. Many women nevertheless think, probably correctly, that they suffer from this tendency rather more than men.

Perhaps one of the reasons the story is not commonly related is that the technique of deducing relative abundances from spectra lines is complex and delicate, and needs input from quantum theory, thermodynamics and the theory of stellar atmospheres: many professional astronomers outside the spectroscopy specialisation would find it hard to describe the process

[3] Darwin's name is, for example, more widely recognised than that of Alfred Russell Wallace in connection with evolution—though Wallace was in fact the first to submit the theory of natural selection for publication. Darwin, however, also provided a mass of supporting evidence well explained, whereas Wallace's short paper just described a theoretical mechanism. Darwin had also quite deliberately set out to establish an unimpeachable reputation for his more traditional work as a naturalist, keeping quiet about his radical theory. When he finally spoke he wanted to be listened to with respect. That is why Darwin is remembered.

even in outline. (My source is *The New Cosmos* (Unsöld 1977) by Albrech Unsöld's, who a few years later than Russell became the noted expert on stellar abundances. This book spends 50 pages just sketching out the theory, but you would also need considerable hands-on training to be able to actually do it.) All the more reason to respect Payne's work, which was remarkably sophisticated for a graduate student, but it is hard to turn it into an easily digested story.

Payne herself had a long and distinguished (though often forgotten) astronomical career. Nevertheless, it was many years before Harvard would give her any academic title'[4], and she was, indeed paid out of the 'equipment' budget of the observatory. As she was not formally an academic staff member the courses she taught were not recorded in the university catalogue until 1943. Eventually things changed and in 1956 she became the first women in their Faculty of Arts and Sciences to be appointed to a full professorship and also a little later the first woman to head a department at Harvard—a mere 31 years after making one of the most important discoveries in 20th Century astronomy.

2 The Harvard Computers

Cecilia Payne was not the first female astronomer at Harvard—there is quite a history.

You may well have heard of Henrietta Swan Leavitt who is remembered today for her discovery of the relationship between period and luminosity in Cepheid variable stars, which later because a vital step on the ladder to extra-galactic distance measurement. She was, however, like Payne employed for all of her working life as an 'assistant' astronomer paid only about 25 cents an hour—just half what a man in the same post would receive and less than they could have got working in the local mills. In addition, and unlike the men, she had little prospect of promotion, even though she eventually published under her own name, and became widely respected amongst other astronomers. Henrietta was also profoundly deaf, so had very limited alternative employment prospects, particularly in the traditional options for well-qualified women of teaching in girls' schools.

Henrietta was in fact just one of an entire band of female 'Computers'

[4] Harvard was perhaps, outside the astronomy department, even less welcoming to women than Cambridge. Until 1963 female students were awarded degrees from Radcliffe College—an 'associated' institution. Full integration with Harvard only took place in 1999.

at Harvard.

Way back in the 1880s the then director of the Harvard Observatory, Prof. Edward Pickering used to complain about his male assistants, and was once heard to exclaim *"My Scottish maid could do better!"*. His Scottish maid was Williamina Paton Fleming, who had been abandoned by her husband after the couple had emigrated to the USA and she had been forced to take up domestic work to support her child. Pickering's wife pointed out to the professor that her intelligence and organisational abilities were exceptional. So Pickering *did* indeed recruit her, first as a part-time administrative assistant. Later, he taught her how to interpret spectra and she then devised with Pickering the first scheme for classifying stellar spectra.

A lot of important astronomical discoveries rest on the rather unglamorous, somewhat routine and often forgotten work of compiling catalogues of what we can see in the sky. After the development of photographic spectroscopy at the end of the 19th Century it was clearly important to collect information from a large and complete sample of known stars and look for patterns. Edward Pickering (who was a master of begging) persuaded the widow of a wealth amateur astronomy, Henry Draper, to fund such a survey. He also developed the technique of using an 'objective prism' and photographic plates to record multiple spectra from a single observation. That is, you collect an image of the sky where at the position of each of the stars one could see the dot stretched out into a spectrum. These were ground breaking technical developments and made it feasible to collect large number of spectra, with enough resolution for classification relatively quickly, since each photographic plate might contain hundreds of spectra. However, as hundreds of plates began to accumulate, it became apparent that the real bottleneck was processing the data. So Pickering recruited an army of female 'computers' to analyse the spectra and compile the catalogue. (Women were much cheaper than men and were often more willing to take on this type of routine and frankly highly tedious work. They had fewer opportunities to move on: it was this or teaching basic science in girls' schools.) Fleming later took charge of Pickering's 'harem' as they became known to the misogynistic male staff at Harvard, and was officially the curator of Harvard's collection of astronomical photographs. She is also known as the discoverer of the Horse-head Nebula in Orion and was also the first to recognised a white dwarf from its spectra—the very first observation of a compact stellar object. (I shall end my talk with another tale of a women involved in the discovery of another type of compact object—neutron stars.)

The staff astronomers took the photographs themselves because women were not allowed to use the telescopes[5] (The claimed reason was that 'the observatory had no female toilets' but in fact giving women jobs that involved them being alone with men during the night was probably not something that anyone was prepared to contemplate.) Given the relative immunity of men against complaints of sexual harassment at the time, and what we might now regard as a somewhat aggressive sexual culture, this may have been wise[6].

At first the results were published under the names of the men, though lady 'assistants' actually performed all the analysis. Later, although they were still paid as assistants, it became very clear that they were doing rather more than just working under direction, and at least one valuable assistant (Antonia Maury) threatened to leave unless she was allowed to publish under her own name. The culture did change and some of the women became very well known by the contemporary astronomical community through their publications and attendance at conferences to present important material—though as is often the case, they were largely forgotten by subsequent generations of astronomers.

The spectral classification scheme of Fleming and Pickering was a noble first effort, but was entirely based purely on the apparent visual characteristics of of selected spectral lines with no attempt to relate it to any underlying theory. Hence the labelling sequence, A, B, C, D, etc. had no necessary connection to any variation in physical conditions on the stellar surfaces.

The initial funding from Draper's widow only covered the classification work on about 11,000 stars. When the catalogue was later to be extended to fainter stars there was some disagreement between Fleming, who wished to retain her simple scheme, and Antonia Maury, who, unlike Fleming, had a degree in physics. (Some even had masters degrees. They were very well qualified for 'assistants'.) Maury wished follow a more complex scheme which she could relate to the underlying physics. A compromise was nego-

[5] The distinguished British astronomer Margaret Burbidge found that she came up against similar restrictions in the 1950s at the Mount Palomar observatory—but her husband, Geoffrey, was also an astronomer at CalTech so he booked the telescope time in his own name (even though he was a theoretician) and for anyone who asked Margaret posed as *his* assistant. In fact she quietly got on with using the telescope with Geoffrey as *her* assistant.

[6] The US astronomical community was being embarrassed by a number of high-profile sexual harassment cases as late as 2016, and it seems likely that a predatory sexual culture has been a feature of US academic life for some considerable time. It is harder to say how common this is in the UK, because, claims the Guardian—26/08/16—of widespread use of non-disclosure agreements in universities to hide cases of sexual harassment.

tiated by Annie Jump Cannon (who was also a physicist). She introduced a relatively straightforward, but still physics-based classification which could be related directly to the stars' surface temperatures. This scheme (with some minor modifications) is still the basis of the 'Harvard Classification' that we use today. Cannon kept some of Flemings original letters but arranging them in a different order to denote the actual temperature sequence from hot to cold: O, B, A, F, G, K, M, R, N, S which my generation of astronomers recall with the non-PC but instantly memorable mnemonic '**O**h **B**e **A** **F**ine **G**irl. **K**iss **M**e **R**ight **N**ow, **S**mack!'. This was first taught to his students by Henry Norris Russell, but is said to have originated with Annie herself. (Modern students are often taught a variety of rather more politically correct mnemonics, and there are even competitions to find one that will stick, so far without real success.)

Cannon was the undoubted queen of spectral classification: she could do it more rapidly, accurately and reproducibly than anyone else, classifying three stars a minute, and continuing at this hour after hour and day after day. During her lifetime she personally classified about 350,000 stars, while also starting the famous Harvard bibliographic archive, personally populating it with about 200,000 entries, which allows astronomers interested in particular object to discover just about everything ever written about them. (This is still very much alive and now on the Web as the 'NASA/SAO Astrophysical Data System' and still extremely useful)

In her obituary, a colleague summed her up by saying *'She probably never had a speculative thought in her life. Observation was all.'* Cannon became well respected during her lifetime, becoming an honorary fellow of the UK's Royal Astronomical Society (women were not admitted as full-member until 1916), and in 1925 was the first women to receive an honorary doctorate from Oxford University, and some scientific awards for women were named after her. Like most of the other computers she did, however, fade from the conventional astronomical histories.

Cannon did not have the option of teaching in schools—generally the only other option available to educated women—because, like Leavitt, she was profoundly deaf.

Maury left the observatory after having a somewhat prickly relationship with Pickering who she believed failed to acknowledge the value of her work. (Some of it was published under his name.) The value of her classification scheme was, however, recognised by the Danish astronomer Hertzsprung which enabled him to distinguish between giant and dwarf stars, and is partly the reason why we now speak of the 'Hertzsprung-Russell' diagram rather than the 'Russell' diagram. (This diagram relates the colour of a star to its luminosity, and it is one of the most important discoveries in astrophysics, since it provided a key to the theoretical understanding of the internal structure of stars and their evolution through life.) Maury later returned to the observatory for a short period to finish her catalogue, on condition that her work was fully acknowledged, and in 1918 eventually became an 'adjunct professor' at Harvard, after teaching elsewhere for twenty years[7].

Although several of the 'computers' were, indeed, well known in their own day amongst their professional peers, they were quickly forgotten and their contributions relatively undervalued in astronomical histories. My own eyes were opened via the excellent history of astronomy in the 20th Century authored by Malcolm Longair (Longair 2006) and a series of articles published in the house journal of the Royal Astronomical Society in the course of 2016, the 100th anniversary of year in which they first admitted women as members.

It would be easy to consider these as exploited women, given their lowly status and pay in the university, but the Harvard astronomy department was in fact one of the very few places where women could gain employment in professional science. To some extent people such as Shapley and Russell were probably doing the best they could in a notably misogynistic establishment[8]

3 Annie Maunder (1868-1947)

The UK had its own army of 'computers' at the Royal Greenwich Observatory, who were mainly involved in calculating the contents of the annual astronomic ephemeris, an essential document for those attempting to navigate by the stars. (During the summer 1973, when I had a placement at the RGO, there were still staff who remembered the rooms full of women operating mechanical calculators, and their passing was mourned with obvious regret. An electronic computer replaced them in the 1960s.) Annie Maunder (born Annie Scott Dill Russell) was one such computer, having joined in 1891 after taking the maths tripos at Cambridge (but not of course actually receiving a degree). Many of you will be familiar with the 'Maunder diagram', first published in 1904, which shows the evolution of sunspot numbers over time and reveals the 11-year sunspot cycle. This was the joint work of

[7] In the UK we would call her a 'visiting professor'—invited to lecture in exchange for a certain amount of honour and status, but not much money and no power.

[8] Even as late as the 1950s there was a body of opinion favouring the complete separation of Radcliffe and Harvard.

both Annie Maunder and her husband Edward William Maunder (though published under his name) who also worked at the observatory, and who she married in 1895. As required by Civil Service rules at the time Annie Maunder had to resign her post on marriage[9] but she continued astronomical work with her husband.

In 1892, along with Elizabeth Brown, another experienced Solar observer, she was proposed for Fellowship of the Royal Astronomical Society, but the conservative and exclusively male membership in a secret vote once again refused to admit women to the Society. (The overt grounds were that the articles of the Society only included the work ”He” when speaking of members.) She was, however, one of the first to be elected in 1916 when the RAS finally opened its membership. In the mean time she (and Elizabeth Brown) became very active in the newly founded British Astronomical Association. which was far more welcoming to women than the RAS. Here they did a good deal of organising, and were active in a great deal of valuable research, particularly on topics such as variable stars and Solar observations.) Annie took part in a BAA expedition in 1896 to photograph a Solar eclipse in Norway—the first of five expeditions in which she participated. For the Indian eclipse of 1898 she designed her own equipment and is said to have achieved spectacular results, including the longest Solar streamer ever imaged at that time.)

Although the later work on what became known as the ‘Maunder Minimum’ (a period when the very few spots were visible) is exclusively attributed to her husband, the very close scientific partnership with her husband probably means that she also contributed to the work. She is remembered as a supreme observer.

See (Dalla & Fletcher 2016) for more information.

4 Mary Adela Blagg (1858-1944)

Mary Adela Blagg developed her interest in astronomy in middle age, after attending a course of university extension lectures in Cheadle, near Manchester, given by Joseph Hardcastle (who was a grandson of Sir John Hershel). She subsequently joined the BAA and eventually served as its secretary. We ought to remember her name, however, because of her work with S A Saunders (the BAA President) who had commenced a project to accurately measure the positions of lunar craters. Frustratingly, there was no standard nomenclature for lunar craters—some were known by different names

[9] This continued to be the rule in the UK Civil Service until the late 1940s.

in different countries so an Oxford professor, H H Turner, persuaded the Royal Society and the International Association of Academies to support the production of a standard list and received a mandate to arrange its creation. The job was given to Saunders, who recognised that he needed help and also that Mary Blagg had the required eye for detail. Their definitive *Collated List of Lunar Formations* was eventually published in 1913. (Blagg & Saunders 1913)

Mary also had an aptitude for mathematics which she taught herself from her brothers' schoolbooks. This proved useful in her later work on variable stars, where she applied harmonic analysis to variable star light curves. At that time much of the observational work on variable stars was conducted by amateur astronomers (and the BAA Variable Star Section is the oldest organisation in the World continuing this work). The work is relatively straightforward, can often be undertaken with telescopes within the reach of amateurs but is very time-consuming since the same star has to be observed many times of a considerable period. The small number of professional astronomers with their larger (but few in number) professional instruments were giving their attention to tasks that produced more scientific rewards for less investment in telescope time. These types of observations, of course, become useful to science only when they can be related to astrophysics, and in principle the variations in luminosity are indeed giving an insight into the internal processes of stars. This work has continued to be valuable over the years, though the modern generation of robotic telescopes with fully automated computerised analysis of observations is now relegating the amateur work to a minor contribution.

Mary Blagg pioneered the mathematical analysis with reduction of raw observations carried out over many years by Joseph Baxendell (1815-1887). The work was published under Prof HH Turners name in the RAS journal *Monthly Notices of the Royal Astronomical Society* in 1912. (Turner 1912) Prof. Turner noted, however, that *'I may perhaps be allowed to express my regret that Miss Blagg does not wish her name to appear as joint editor. The greater part of the work is due to her.'*

Mary was always shy and retiring and reluctant to leave Cheadle, but her work on selenography and variable stars eventually gained her an international reputation, and in 1925 she was invited to join the International Astronomical Union Commission on variable stars, and also the IUA Lunar Commission, where, in spite of her amateur status, she was fully accepted in a distinguished community including people such as Eddington and Hertzsprung.

See (Shears 2016) for further information.

5 Margaret Huggins (1848-1915)

Sir William Huggins was a renowned English amateur astronomer—and it is worth remembering that before the 20th Century much of the most significant English work in astronomy was being undertaken by serious amateurs, generally, of course, by people with private incomes, intellectual interests and time on their hands. Many of them were women.

In 1875 (at the age of 51), Sir William married the much younger Margaret Lindsay Murray (then 27) and according to his biographers 'derived great benefit from his wife's able assistance.' In fact, her contributions turn out to be much more substantial than 'being generally handy' as he described her. Margaret kept detailed notes of their joint work—much more comprehensive notes than William ever made—and from those it is clear that she made significant contributions to many aspects of his work. In particular her considerable expertise in photography may have been largely responsible for William's interest from then onwards in using photography to record astronomical observations.

William and Margaret worked together for 35 years though it was not until 1889 that her name appeared with his on a scientific paper. It was, however, a paper of great significance providing evidence that a spectral line visible in most astronomical nebulae did not correspond to the known spectral lines of any of the chemical elements found on Earth. They believed it was the signature of a new element, for which they proposed the name nebulum. (Their hypothesis was not at all unreasonable given what was then know about spectroscopy and the physics of atoms. It was, in fact, identified only in 1927 as a 'forbidden' transition of double ionised oxygen—a rare transition exceedlingly difficult to observe in Earth laboratories, because the metastable upper state is only able to survive long enough to decay at the very low number densities in interstellar space.) Their work, however, did effectively disproved the hypothesis of Norman Lockyer, who held that nebulae where incandescent clouds of colliding meteorites. In the context of the times this was of course not by any means an untenable hypothesis—meteorites were indeed the only astronomical substances of which we had direct knowledge. However, glowing clouds created by their collisions ought then to produce spectra of the elements known to be part of their composition.

Barbara Becker (Becker 2016) has forcefully argued that the narrative of Margaret as William's assistant (an able assistant, but nevertheless an assistant) has considerably undervalued her actual role. She deserves recognition as a scientist in her own right. It was, however, the romanticised view of her husband as an observer and innovator to which Margaret herself subscribed and assiduously promoted, even after her husband's death, because she wanted to preserve his scientific legacy. Margaret was a very intelligent woman, and while she may well have been strongly motivated by loyalty and the victorian tradition of the supportive but subordinate wife, I suspect that she may also have been afraid that a wider understanding of her own involvement may have diminished the value of the work in the eyes of the contemporary scientific establishment.

See (Becker 2016) for the source of this material.

6 Agnes Clerke

Agnes Clerke (1842-1907) came from an intellectual Irish family and in spite of having no formal education was a a talented pianist and fluent in several languages. She clearly had a formidable intellect and wrote a number of highly regarded books on astronomy aimed at both the public and professionals. She contributed numerous biographical articles to the *Encylcopedia Britannica* based on original research carried out while living in Italy.

Her reputation in the astronomical community rested largely on her comprehensive and unrivalled knowledge of the astronomical literature, and she was apparently frequently consulted by the better known men as a form of walking encyclopaedia, and they obviously respected her judgement in the selection of important problems in astronomy. Her *System of the Stars*, (Clerke 1890) is described as 'a general survey of knowledge regarding our sidereal surroundings' . A later book *Problems in Astrophysics* (Clerke 1903) is a 656 page 'survey of issues deserving the attention of professional astronomers'. She also wrote a successful *Popular History of Astronomy in the 19th Century* first published in 1885 (Clerke 1908). (These book are still in print: you can purchase them on Amazon today—though I advise you to download the copyright-free PDF versions from the Guttenberg archive. The *Popular History* is still regarded as an authoritative source on 19th Century astronomy.) Agnes was offered, but turned down, a chair of astronomy at Vassar College in the USA, on the grounds that she did not wish to leave her family in the UK.

The British Astronomical Association was a kind home to Agnes Clerke where she was elected to the council. However, in spite of a distinguished reputation, she had to wait until her sixties before she was elected to honorary membership of the RAS, one of a very small number of women honoured in this way, prior to the formal admission of women as fellows in

1916.

Agnes Clerke is commemorated in the name of a lunar crater, on the border of the Sea of Tranquility. (Picture available in RAS AandA article. (Russell 2016))

See (Russell 2016) for more information.

7 Mary Somerville (1780-1872)

I have drawn the following brief summary from an excellent article by the Oxford science historian, Dr Alan Chapman, in the RAS house journal, Astronomy and Geophysics (Chapman 2016).

In middle age Mary Somerville was sufficiently famous that an American admirer was able to address a letter simply as 'Mrs Mary Somerville, London'. She was another woman of little formal education but extraordinary intellect. Beautiful, charismatic and full of energy Mary enjoyed the social life open to a young lady of good family, but worried her parent by teaching herself mathematics, with the help of a brother who was at Edinburgh University.

Although she was married at 24 to a man with a low opinion of female intelligence, Mary was also widowed at 28, leaving her independent. Now she could throw herself into the study of astronomy, physics, maths and mineralogy Her substantial mathematical library was mostly in French, and included Laplace's *Mécanique Céleste*. (The French were the leading mathematicians of the age: teaching at Oxford and Cambridge was to a large extent stuck in the previous century and its mathematics incapable of dealing with the current problems in astronomy.) She had also taught herself Greek so she could read Xenophon in the original. (Her daughters thought it expedient, however, to cover up some of her other linguistic achievements, such as the ability, acquired from military relatives, to swear like a trooper.(Somerville 1873))

Four years later, in 1812 she married again, this time to Dr William Somerville FRS, who in contrast to her first husband did his utmost to promote her standing and reputation, and who spent many hours in the Royal Society library copying scientific papers out in long-hand for her use. Although she did not publish any scientific work until 1826, she became well known and highly respected through her letters to French *savants* such as Biot, Laplace and Lagrange, and when travelling on the Continent was welcomed everywhere by distinguished scientists.

Her status was such that she was asked to produce an English version of Laplace's *Mécanique Céleste*. Her *Mechanism of the Heavens* (Somerville 1831) turned out to be far more than a mere translation, and became a comprehensive and original exposition on all aspects of gravitational physics. At least one member of parliament was outraged that a women would dare to write such a book, but those who mattered recognised its worth and it was soon in use for teaching mathematics at Cambridge (probably the first textbook written by a women to be used at the university) Had Mary Somerville been born in the 20th Century I have no doubt that she would have established herself as an important theoretical physicist. Alan Chapman, the distinguished Oxford historian of Astronomy, gives his opinion that she would be an FRS in her own right. (Raynor-Evans & Chapman 2019)

It was soon followed by *On the Connexion of the Physical Sciences* (Somerville 1846) a wide-ranging explanation for a more general readership of the status of all the physical sciences, which she maintained up to date in many subsequent editions through to the end of her life. (It can still be purchased on Amazon, but you can now also get copyright-free PDFs from gutenberg.org.) Although it was a non technical work, she proudly related in her autobiography that John Couch Adams told her that it was her discussion of planetary perturbations that caused him to analyse the orbit of Uranus and compute the existence and position of Neptune.

As with other female astronomers of her time she was not permitted to become a fellow of the RAS (though they did give her an honorary fellowship, which entitled her to attend meetings).

In her 80's she was still writing major works, including *Microscopic and Molecular Sciences* (Somerville 1869), and having been invited to visit the battleship commanded by her nephew, she proceeded to give it a very detailed inspection, including all the engine rooms.

Somerville College in Oxford is, of course, now named for Mary Somerville, and honours her role in promoting the education of women, so we can hardly perhaps call her forgotten. Nevertheless, I was rather surprised to discover just how exceptional were her scientific achievements.

8 Caroline Hershel (1750-1848)

Caroline Hershel was considered un-marriageable by her family, and was therefore destined to the common fate of such women: look after your parents until they died and then, become a poor relative, dependent on whichever of your extended family would offer a home in exchange for unpaid service, or else, for those with sufficient education, take on the hardly less onerous role of a governess. In spite of Caroline's wish to learn, her mother ensured she would not become independent by training her only in household duties.

After her father's death, and against the opposition of her mother, her musician brothers William and Alexandre invited her to join them in England with the promise of a musical training so she could join the family enterprise. As well as running William's household, she did, indeed, eventually become an integral part of William's performances at small musical gatherings in Bath, and her voice was sufficiently good to take principle parts in oratorio concerts, including the Messiah. Her refusal, however, to sing for any conductor other than her brother meant that as he devoted more time to astronomy her promising singing career went into decline. Caroline was, however, again expected to support his astronomical work, at first unwillingly.

Their musical performances stopped entirely when William accepted the position of court astronomer to King George III, involving a move from the cultured city of Bath to a much more isolated house near Windsor Castle, where William would be on hand to entertain royal guests.

At first Caroline almost certainly resented the loss of her public career and the return to drudgery, and at one point wrote in her journal *'I did nothing for my brother but what a well-trained puppy dog would have done, that is to say, I did what he commanded me.'* Later on they developed an efficient method of joint working, and Caroline developed a strong interest in the work. Caroline also had her own independent research, that eventually lead to a number of discoveries of comets and nebulae.

When the crown began paying her as an assistant to her brother, she became the first woman to receive a salary for services to science. In 1828 she received the Gold Medal of the Royal Astronomical Society—the first woman to do so, and an honour that was not repeated until Vera Rubin in 1996.

In spite of the honours you cannot help feeling some sympathy for Caroline's lack of control over her own destiny. William Herschel, of course, was well remembered by history, as was his son, John Herschel, Caroline rather faded from the record until the modern reassessments of the contributions of women to science: her name was known but not many people would be able to say what she actually discovered.

9 Maria Mararetha Kirch: The First Women to Discover a Comet

Maria Margaretha Kirch was born in 1670 and was lucky to be be part a family that believe girls should receive the same education as boys. She was also fortunate that her interest in astronomy was encouraged by family connections through whom she met and later married Gottfried Kirch who became the Prussian court astronomer. Maria acted as her husband's unpaid assistant though it was widely recognised that the pair worked as a team, and she often published under her own name. During observations in 1702 she became the first woman to discover a comet. In the later part of her husband's tenure he became an invalid and in practice it was Margaretha who did all the work. Unfortunately, after her husband's death she was unable to take over his place as official astronomer and calendar-maker (a state monopoly and important source of income). Her petition was denied, undoubtedly because she was a women—even though she had demonstrated her eminent competence in the role. When her son eventually took over the official role and once again she acted as his 'assistant', members of the Prussian Academy of Sciences complained that she was *'too visible at the observatory when strangers visit'* and she was eventually forced to give up living in the tenured house in the observatory grounds.

Maria's case is one where men made a very deliberate attempt to hide her role in astronomy.

10 Maddalena and Teresa Manfredi

As we go further back in time, it gets more difficult to find the truth.

Gabriele Manfredi (1681-1761) was an Italian mathematician who is remembered for a seminal work on differential equations. His brother Eustachio Manfredi became a distinguished astronomer of the time. They came from a large family, including several other brothers who followed careers in science, medicine, the law and the church, and two sisters Maddalena (1673-1744) and Teresa (1679-1767) who received a basic schooling but then educated themselves (probably with some assistance from their academic brothers). The entire family lived in the palace of Count Luigi Ferdinando Marsili who wished to promote an intellectual atmosphere at his court.

We know little about them, other than they were reputed to have assisted their brother Eustachio in his work, and it is claimed (Bernardi 2016) that this support was significant. (The manuscript of Eustachio's 1715

Ephemerides of Celestial Motion notes that it was prepared with the assistance of his sisters - though it was published under his sole name.) The sisters also had a contemporary reputation as poets and translators, but rarely signed their work so historians have some difficulty in establishing which material was produced by the sisters and which by the brothers.

At that time, of course, it was of paramount importance to support the interests of the family which would mainly depend on promoting the status of its principle men-folk. One suspects that women took the pragmatic view that their own interests were better served by increasing the status of their men. That, at least, was what most of the men expected to happen.

11 The Ancient World

How far back can we go? The histories almost certainly under-represent the role of women—they were all written by men. Furthermore, there is no clear distinction between astronomy and astrology: so Aglaonice (of Thessaly) from about 4th Century BCE is mentioned by Plutarch as a mathematician and astronomer. Plato refers to the '*Thessalian enchantress who was able to bring the Moon down from heaven*' (which is now interpreted to mean that she was able to predict lunar eclipses). It seems that she used this ability to gain a reputation for herself as a sorcerer, along with a group of female astrologers who were known as the 'witches of Thessaly'.

Hypacia of Alexandria (born c.350-370 AD) was the daughter of a mathematician (Theon of Alexandria) and was renowned as a philosopher and teacher throughout the Mediterranean. It is not clear that she made any original contributions to mathematics or astronomy, but is now known to have edited Book III or Ptolomy's Almagest—by any standards she would have been considered to have expert knowledge. Hypacia was murdered in 415AD at the instigation of the local christian bishop, who disliked the fact that she was clever, a women, a pagan and a very astute advisor to his political opponent.

Queen Seondeok of Silla (b.610AD? - 647) was not herself an astronomer but her interest in the subject led to the construction of probably the first dedicated observatory in the Far East, at Cheomseongdae, which is probably also the World's oldest surviving observatory.

12 The Discovery of Pulsars

While I was a research student at Cambridge, two of my professors (Ryle and Hewish) received the 1974 Nobel Prize for Physics. (We lesser folk knew something was going on when just about every telephone in the Cavendish building seemed to start ringing at the same time!) Ryle's citation highlighted his development of the 'Earth Rotation Aperture Synthesis' technique, which had undoubtedly by that time led to a number of major discoveries, and later history has only confirmed its dominant importance. (For example, the recent images of a black hole from the Event Horizon Telescope were created by a development of this method.) That award was widely welcomed.

Similarly, few astronomers would deny that the discovery of pulsars was of sufficient importance that it was worthy of a Nobel Prize (some astrophysicists rank it as one of the most important astronomical discoveries of the 20th Century—it is certainly in the top ten). The mere fact that such highly compressed states of matter exist is of enormous importance to theoretical physics, and they are crucial for explaining other astronomical puzzles, including the abundances of heaviest elements and also as probes of general relativity.

The Nobel citation, however, credited Hewish with a *'decisive role in the discovery of pulsars'* and that became controversial almost immediately because his research student Jocelyn Bell (later Dame Jocelyn Bell Burnell) brought the initial observations to her supervisor's attention. (I was working at the Cambridge Institute of Astronomy later that same day and told another astronomer about the award—news travelled more slowly in pre-Internet days—and he immediately said 'Poor Jocelyn'.)

By this time, of course, the slightly romanticised story of the discovery had become widely known both within and outside the astronomical community, partly due some excellent BBC science programmes, and also because the popular press were very much taken with the story of a young female student making a major discovery. (Dame Jocelyn speaks cuttingly about some of the sexist attitudes of the journalists.) The award is now sometimes seen, however, as a prime example of a supervisor being credited with his student's work and especially likely and unfair because she was female. I recently, for example, read a web-biography of Dame Jocelyn which included the subtitle *'He had all the credit and she had none'*.

I am not sure that the story is quite as clear cut as the developing myth sometime makes out, so I am not going to take a definitive position of whether Dame Jocelyn should or should not have shared the Prize. All I want to explore here is background and to explain why the decision facing the Nobel committee was perhaps not all that easy, and had Dame Jocelyn been included that might also have raised some eyebrows. I have for a while believed that prizes such as the Nobel create inevitable unfairnesses, and it is easy to find many examples of surprising exclusions from the award. Where there is scope for unfairness, however, it often seems that women suffer rather more than men.

It is matter of plain fact that although Dame Jocelyn did not share in the Nobel Prize, she became widely known for her share in the discovery of pulsars and her subsequent distinguished career *before* the 1974 Prize announcement. The claim that she had *no* credit is simply wrong, and it is arguable that she had what really mattered: the respect of her professional peers who did give credit for her share in the discovery—with an informed understanding of the subtleties. She has also received a number of other important awards including the Breakthrough Prize. I suspect that she now rather more famous than Tony Hewish both inside and outside science, and it may now be that Tony's contribution to the pulsar discovery that is in danger of being undervalued. (On a number of occasions, when the topic of the undoubted disadvantages faced by women in science have been raised—not infrequently amongst scientist and engineers these days—and Dame Jocelyn's case almost inevitably raised, I have admitted to my former membership of the Cavendish Radio Astronomy Group, and found that there is little understanding of the real complexities of the case.)

The popular story holds that Jocelyn noticed the 'scruff' on the chart recorder trace which Tony Hewish then dismissed as merely interference, while she insisted on pursing the idea that it was a real radio source. I was not there at the time and later never talked to the principals about the issue—but I do know the way things were done in that group, where I started my own PhD just six years later, and I had my own share of operating radio telescopes and analysing raw data. I think that the story is almost certainly rather more nuanced that the public myth would suggest.

Firstly, you have to understand that new types of radio interference are relatively common—every new technology seems to cause new interference problems—while new types of radio source are extremely rare. If you see something unusual on a pen recorder trace it is very much odds-on that you are looking at interference. Dane Jocelyn certainly deserved credit for realising that the 'scruff' was not like anything they had seen before, and for drawing it to her supervisors attention. He would have been just as correct to downplay expectations by pointing out that it was highly likely to be interference: any experienced radio astronomer would have probably laid heavy bets on that outcome. There are, however, standard protocols to follow that can distinguish astronomical sources from Earthly sources.

Furthermore, one does not just dismiss radio interference from further consideration. It is a potentially serious problem for future observations, and where you see it once it is likely to pop up again and again. Producing unauthorised radio transmissions, especially in reserved frequency bands, is also illegal and if you can trace the source the law is on your side, and you may be able to prevent things getting worse.

In my time standard practice would have been to put in a certain amount of effort to track down the origin of interference and if possible eliminate it. I remember being told in my own briefing on dealing with interference that the Cambridge television factory of the PYE company was once on the receiving end of an injunction from the University after they failed to deal with some leaky signals which were causing problems at the observatory. Roll up to the observatory on any form of motor transport and you also were likely to find the head technician on site pointing a large aerial at your vehicle, just to check you were interference-clean. He had a 'carrot' and a 'stick: the first was an offer to fix any problem free of charge, the second was banning the vehicle from further access to the site.

If interference cannot be eliminated, they you have to characterise it so you can remove it from the observations. (At one time we had a lot of problem from meteorological balloons in Eastern Europe, which had particularly poor electronics: their transmitting frequencies were very sensitive to temperature, so by the time they had risen to high altitude and cooled to -30 degC, they were transmitting right in the middle of a radio astronomy reserved band. In the days of the Iron Curtain there was in fact little one could actually do to get them to fix the problem, so we just had to avoid pointing telescopes that way at certain times. One of our technicians used to mutter about going to East Germany with a shotgun.) Russell Hulse who shared another Nobel Prize for his discovery of a binary pulsar while he was a research student (see below) also talked about the difficulties of observing pulsars when there are many sources of confusing interference.

Nevertheless, I often wonder whether if I had been in the hot seat, whether I would have pursued the matter with the same tenacity. Would I have overlooked a major discovery? I am not by any means certain that I would have done as well. That is why Dame Jocelyn rightly deserves her share of credit as the discoverer of pulsars.

So, while Dame Jocelyn certainly made the initial observations, and did a good deal of the subsequent legwork, I suspect that Tony would also have been asking the type of questions that good supervisors pose to their students in order to ensure that they follow a good scientific method, particularly highlighting the necessity of eliminating all the alternative and perhaps more likely possibilities.

One of the obvious tests would have been to see whether the source moved with the sky (or whether it appeared at the same Solar time each day—which is sometimes the case with interference). Even that is not definitive: astronomers in radio 'noisy' cars may well need to turn up to observatories at times governed by the sidereal clock. Observatory equipment may get turned on or off according to sidereal time. (Hulse's pulsar observations suffered from interference from irregular electrical arcing from observatory lighting.)

Furthermore, radio telescopes are extraordinarily sensitive instruments, and you might be seeing terrestrial signals, such as radars, bouncing off the moon. The Arecibo observatory, for example, attempts to bounce powerful radar signals off asteroids: could we be seeing some stray reflections? One also has to worry about transmissions from satellites a good many of which are military and whose orbits, purposes and characteristics are therefore not publicised. So, it was necessary, for example, to make discrete inquiries at other observatories for potential sources of interference.

In fact, Dame Jocelyn was not alone in this endeavour. Tony organised and directed a small team that carried out a number of important experiments in order to clearly demonstrated that the radio source had to be at astronomical distances and, furthermore, was not associated with a planet orbiting a star. (No 'Little Green Men'!) Hence, the paper which announced the discovery actually had five authors (Hewish, Bell, Pilkington, Scott & Collins 1968), and the actual content of that paper—the official record of the discovery—makes no particular distinction of who-did-what.

The rules of the Nobel Foundation say that a Prize cannot be divided more that three ways, so this immediately creates an issue. Had the first pulsar paper been published as 'Bell and Hewish', she would, in my view, undoubtedly have shared in the prize, since the ordering would indicate that Bell deserved at least as much credit as Hewish. The same might have been true had the paper been submitted just as 'Hewish and Bell' which would indicate Hewish's leadership but acknowledge a substantial contribution from Bell. That would, of course, have ignored the work of the other three authors, and would certainly been unfair to them. Perhaps it would have been different if the report of the work had been split between two papers, discovery and subsequent investigations. The original pulsar paper, however, does make the better scientific story, and also makes clear that establishing the case for the discovery of a new type astronomical object required a good deal more that noticing a bit of 'scruff' on a chart recorder. And I doubt whether anyone was thinking about staking claims for Nobel prizes at the time.

The actual order of the authors on paper stakes Hewish's claim to be the lead author and, indeed, the directing mind behind the work, but makes clear the shared contribution from the rest of the team. Bell is the second author which in some circumstances indicates the team's consensus on the second-most important contribution, but we cannot draw that conclusion here since the normal convention would have been to place names in alphabetical order, unless there is a reason to signal a smaller or larger respective contribution, and Bell would be second anyway. (Collins appears last, out of alphabetical order, and this I would take to indicate team consensus that he had a less significant role.) This practice was unexceptional for the time, and in similar circumstance would still be considered normal. The Prize for observing the first gravitational wave events when to those who argued the scientific case for building the detectors and lead the very large team efforts. Before that, the Prize for the discovery that the expansion of the universe is accelerating also went to the leaders of the two (rather large) teams that contributed to the outcome[10].

It is, remember, also Hewish taking full responsibility for any fiasco if this major claim turns out to have been a mistake and such mistakes do sometimes occur. Pulsars would have been discovered much earlier if the observations were easy. They were, in fact, quite tricky since the Cambridge telescopes were not the optimum instruments for observing pulsars. Some embarrassing 'discoveries' have even reached the literature. Staff from the Parks Radio Observatory in Australia went public in 1998 with observations of signals they called 'perytons'—which however, could not be reproduced elsewhere and 17 years later were traced to a faulty microwave oven used by astronomers at the observatory. (Petroff, Keane, Barr, Reynolds, Sarkissian, Edwards, Stevens, Brem, Jameson, Burke-Spolaor et al. 2015) These things are not always easy.)

My own experience was that research students sometimes got rather more credit than they really deserved from the alphabetical-order convention, They certainly do all the tedious leg work, but most research is part of long commitment by the supervisors to particular research programmes, including a great deal of even more tedious effort arguing for the resources to

[10] It is interesting, nevertheless, that the 'Breakthrough' prize awarded for the same discovery included the entire research teams, after the three principles who were originally offered the award insisted that the rules were changed to include everybody.

support the work. A good supervisor also gives a lot of thought to defining PhD problems in such a way that the student both learns the important techniques in a field and ends up with something to say at the end of three years. They not infrequently give their students the topics most likely to be most productive on short timescales. If team leaders could not take some credit for the discoveries emerging after these unglamorous efforts there would be little incentive to undertake them.

My own publication list includes a paper based on data from the newly commissioned 5Km Telescope, then the World's most advanced instrument, which Sir Martin Ryle had spent years designing and building and which he then used to observe a very unusual sporadic radio source known as Cygnus X3—also involving a neutron star. It was dropped into my lap one day with instructions to write it up for publication. I certainly drafted the paper, but I am not certain my scientific contribution amounted to more than plotting graphs, even though it nevertheless eventually appeared as a fairly generous 'McEllin, communicated by Ryle' (McEllin & Ryle 1975). (Other students also got very plum observations on which to do solo write-ups.)

Dame Jocelyn herself is very gracious about the situation (Bell Burnell 1977) and believes that she has, in any case, done rather well out of the discovery including receiving a number of other prestigious awards. As she remarked to Jim Al Khalili (Al-Kalili 2011): *'Once you get the Nobel you don't get anything else!'.* No previous Nobel had been awarded for work in astronomy and it is possible that the Nobel committee at the time thought that they ought to be particularly uncontroversial. (Little did they know...) And of course, behind Hewish and Ryle, there is a long list of astronomers who were probably nominated and considered for inclusion in a Nobel prize, but missed out. Any astronomer could name several contenders and odd omissions. It is also possible that Dame Jocelyn was simply not nominated to the Nobel committee, which is quite plausible given that nominations tend to come from establishment figures (who probably lead their own scientific teams and therefore have sympathy with the interests of team leaders). Furthermore, the contemporary attitudes to the contributions of students (and perhaps particularly female students) were less enlightened than they are today. We shall not know until the Nobel archives for that year are opened to historians in 2025.

It is certainly true that two decades later another astronomy research student, Russell Hulse, shared the 1993 Physics Prize with his supervisor for the discovery of the binary pulsar. (Hulse & Taylor 1975). But note the order of the authors in this citation: 'Hulse and Taylor'. That is a clear signal to readers that in spite of the fact that Hulse is just a student and Taylor had spent many years arguing for telescope time for the surveying large numbers of pulsars and developing and perfecting the techniques that lead to this major discovery, the credit is being shared at least equally. Hulse's Nobel lecture describing the discovery indicates that he well-deserved his share of the credit, He was working in relative isolation at the Arecibo radio telescope thousands of miles from his home base (with no Internet, poor telephone connections and sometimes having to rely on chancy shortwave radio for communication with his supervisor). As a result he made the initial observations, hypothesised the correct interpretation and collected crucial confirmatory evidence before he even contacted his supervisor. Nevertheless, as with the initial discovery of pulsars, a good deal of later observational and theoretical work was required in cooperation with his supervisor in order to establish the scientific importance of the discovery at a level deserving a Nobel Prize.

Dame Jocelyn is perhaps wise to believe that it was all for the best. (Nobel Laureates have indeed complained that the sudden celebrity status can be difficult to handle and gets in the way of their research.) In 1973 Brian Josephson—who is just a few years older than Dame Jocelyn—received a share in the Nobel Physics Prize for his discovery, while a Cavendish research student in 1962, of what is now known as the 'Josephson Effect'. No one disputes that he fully deserved this award for an extraordinarily brilliant and completely unexpected prediction on a topic he was not even supposed to be researching and one which initially met widespread scepticism from the established experts [11]. Up until the award of the Prize Josephson had been very productive, and even without his eponymous discovery he had made his mark on physics, but some believe that his research career effectively came to an end at that point. Josephson used his new-found status to leave the physics mainstream and research topics that many regarded as on (or even beyond) the scientific fringe. He is still a somewhat isolated figure, now largely sidelined by his former colleagues.

There are, of course, wider issues about who really deserves credit for scientific discoveries. Should it be the leader of a research team, the principle enablers, who conceived a research program, argued for the money, recruited staff and supervised all the work? Or, should the rewards be shared by those who actually did the observations and whose intelligent handling of data—

[11] His idea was, in fact, summarily dismissed as nonsense by the double physics Nobel Laureate and the foremost authority on superconductivity, John Bardeen. Characteristically, after listening to Bardeen's conference talk, Josephson—still a student—then stood up and explained, with clarity and precision, why he thought Bardeen was wrong. Subsequent experiments quickly proved that Josephson was entirely right.

particularly the unexpected—leads the work in important and unanticipated directions? Should the spoils go to the person who make the last, and often one of the easiest steps to the final conclusion—or should it also recognise the many researchers who cleared the way?

My personal view is that such differing views are unlikely to be resolved particularly in the context of the modern scientific enterprise, which is often built around team efforts. I think that the rules of the Nobel are long overdue for revision and that the restriction to only three recipients will inevitably cause unfairness in a modern context.

(As an aside it is interesting that the *first* person to observe a compact stellar object—the identification of a white dwarf—was also a woman, Williamina Fleming. Dame Jocelyn's discovery was only the second example confirming observations of a compact stellar object.)

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