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

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

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

1 INTRODUCTION

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

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

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


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

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

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

2 THE GREAT CORRELATION ERA

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

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

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

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

3 APPLYING SAHA'S THEORY

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

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

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

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

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

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

4 PAYNE'S THESIS AND RUSSELL'S ADVICE

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

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

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

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

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

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

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

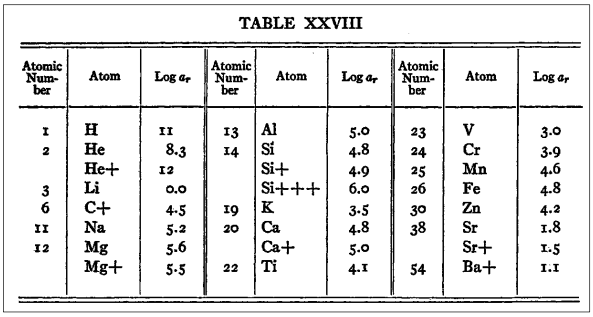

TABLE XXVIII

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

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

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

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

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

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

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

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

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

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

5 NOTES

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

6 REFERENCES

Abbreviations used for archival material:

CIT/GEH = California Institute of Technology, George Ellery Hale papers.
PUL/HNR = Princeton University Library, Henry Norris Russell Papers.
SLRC/MH = Schlesinger Library, Radcliffe College, Margaret Harwood papers.

Adams, W.S., n.d. Letter to A.S. Eddington. CIT/GEH

Adams, W.S., 1917. Letter to H.N. Russell, dated 22 January. PUL/HNR.

Aller, L.H., 1961. *The Abundances of the Elements*. New York, Interscience Publishers.

Contributions of 20th Century Women to Physics, 1995-2001. Payne-Gaposchkin, Cecilia Helena. http://cwp.library.ucla.edu/Phase2/Payne-Gaposchkin,CeciliaHelena@861234567.html.

DeVorkin, D.H., 1999. The discovery and exploitation of spectroscopic parallaxes revisited. In Philip, A.G.D., van Altena, W.F., and Upgren, A.R. (eds.). *Anni Mirabiles: A Symposium Celebrating The 90th Birthday of Dorrit Hoffleit*. Schenectady, L. Davis Press. Pp. 17-24.

DeVorkin, D.H., 2000. *Henry Norris Russell: Dean of American Astronomers.* Princeton, Princeton University Press.

DeVorkin, D.H., 1996. The development of Meghnad Saha's ionization equilibrium theory and its reception in the west. In Sinha, B. and Bhattacharya, M. (eds.). *From Astronomy to Astrophysics. Proceedings of a Symposium on Astronomy and Astrophysics*. Calcutta, Saha Institute of Nuclear Physics. Pp. 3-34.

DeVorkin, D.H., and Kenat, R., 1983a. Quantum physics and the stars (I): The establishment of a stellar temperature scale. *Journal for the History of Astronomy*, 14, 102-132.

DeVorkin, D.H., and Kenat, R., 1983b. Quantum physics and the stars (II): Henry Norris Russell and the abundances of the elements in the atmospheres of the sun and stars. *Journal for the History of Astronomy*, 14, 180-222.

Eddington, A.S., n.d. (circa 1920). Review commentary "Referee's Report" of Saha, "A physical theory of stellar spectra," (eventually published in *Proceedings of the Royal Society of London*, *A*99, 135-153). Royal Society Archives and Library (London).

Encyclopedia of World Biography, 2004. Cecilia Payne-Gaposchkin. http://www.encyclopedia.com/doc/1G2-3404705009.html.

Gingerich, O., 1982. Obituary: Cecilia Payne-Gaposchkin. *Quarterly Journal of the Royal Astronomical Society*, 23, 450-451.

Gingerich, O., "The Most Brilliant Ph.D. Thesis Ever Written in Astronomy," in *Cecilia Payne-Gaposchkin: Astronomer and Astrophysicist 1900-1980.* http://www.harvardsquarelibrary.org/unitarians/payne2.html.

Haramundanis, K., 2006. Cecilia Payne-Gaposchkin: A stellar pioneer (The Dorrit Hoffleit lecture), American Physical Society meeting, 22-26 April, 2006. Abstract at http://meetings.aps.org/link/BAPS.2006.APR.J5.2.

Kidwell, P.A., 1982. Cecilia Payne-Gaposchkin and stellar atmospheres. *Bulletin of the American Astronomical Society*, 14, 916.

Kidwell, P.A., 1984, An historical introduction to *The Dyer's Hand*. In Payne-Gaposchkin, 1984, Pp. 11-38.

Lankford, J., and Slavings, R., 1996. The industrialization of American astronomy, 1880-1940, *Physics Today*, 49(1), 34-40.

Milne, E.A., 1923. Recent work in stellar physics. *Proceedings of the Physics Society of London*, 36, 94-113.

Payne, C.H., 1925a. Letter to M. Harwood, dated 9 January. SLRC/MH. I thank Barbara Welther for alerting me to this correspondence in the Schlesinger Library of Radcliffe College.

Payne, C.H., 1925b. *Stellar Atmospheres: A Contribution to the Observational Study of High Temperature in the Reversing Layers of Stars.* Cambridge, MA, The Observatory.

Payne-Gaposchkin, C., 1984. *An Autobiography and Other Recollections*, edited by Katherine Haramundanis. New York, Cambridge University Press.

Russell, H.N., 1920. Letter to H. Shapley, dated 31 January. PUL/HNR.

Russell, H.N., 1925. Letter to C. Payne, dated 14 January. PUL/HNR, AIP Microfilm.

Russell, H.N., 1926. Letter to C. Payne, dated 12 May. PUL/HNR.

Russell, H.N., 1927. Letter to C. Payne, dated 7 March. PUL/HNR.

Russell, H. N., 1929. On the composition of the Sun's atmosphere. *Astrophysical Journal* 70, 11-82.

Russell, H.N., Dugan, R.S. and Stewart, J.Q., 1927. *Astronomy*, Volume II. Boston, Ginn and Company.

Smith, R.W., 1991. A national observatory transformed: Greenwich in the nineteenth century. *Journal for the History of Astronomy* 22, 5-20.

The Starry Universe: The Cecilia Payne-Gaposchkin Centenary, 2000. *American Association of Variable Star Observers Newsletter,* Number 25. http://www. aavso.org/ publications/newsletter/number25/cecilia.shtml.

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