Chapter 8
Henry Norris Russell's Campaign to Make Physics the Core of Astrophysics

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1 For Wayne

I am delighted to take part in Wayne's 80th birthday celebration. Recognizing his tireless efforts to establish a global perspective on the history of modern astronomy, including its broader social and cultural aspects, and his devotion to expanding our exposure to the rise of radio astronomy in the southern hemisphere, my contribution seems painfully narrow. But in the spirit of his festschrift, I share how, like Wayne, I was converted to history through the question posed by the title to this paper. So, it is offered here as a summation of my early efforts (see DeVorkin, 2000a), and in the spirit of Wayne's attention to the growth of astrophysics beyond Europe and America (Nakamura & Orchiston, 2017) in various edited volumes and papers over the years.

2 Intellectual and Social Origins

Astronomy got 'physical,' in the modern sense of the word, when Kepler and Newton introduced the idea of central force fields, which Newton then elaborated as a universal theory of gravitation. Newtonian gravitational physics dominated astronomical thought for over two centuries, forming a theoretical framework that yielded models of the formation of the solar system, planets, and stars.

A rough contemporary of Newton, the Danish astronomer Ole Römer, found from timing variations in the eclipses of the Jovian satellites that the speed of light

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S. Gullberg, P. Robertson (eds.), *Essays on Astronomical History and Heritage*, Historical & Cultural Astronomy, https://doi.org/10.1007/978-3-031-29493-8_8

was not infinite, a critical clue to the nature of the physical universe. After him, in the 1720s, James Bradley searched for the annual parallaxes of stars, but found instead the aberration of starlight, a displacement that confirmed that the earth was not stationary and revealed as well that the speed of light is a constant, independent of position or distance traveled. By the end of the eighteenth Century, Newtonian gravitational physics had been extended beyond the solar system after John Michell argued from statistics that physical binary star systems had to exist, and William Herschel, joining the search for annual parallax, found instead evidence of orbital motion among binaries. Herschel was also one of the first to speculate, based upon his observations of the different forms of nebulae, how gravitational attraction could cause the nebulous mists to contract into stars and stellar systems.

There was, therefore, plenty of physics in astronomy well before anyone thought of calling the subject 'astro-physics,' 'cosmical physics,' the 'new astronomy' or even 'sidereal physics,' starting in the 1860s. That physics became explicit in astronomy only at that time is due to several factors, intellectual and social. Most important, the rise of specialties in science in the nineteenth Century contributed to establishing new disciplinary identities based upon topic, mode of investigation, institutional affiliation, and specific research goals. Thus, the introduction of new, distinct modes of physical investigation of the heavens, such as photometry, photography and especially spectroscopy, the rise of the study of how matter and light interact, the entrance of numerous aggressive practitioners like A. Secchi, H. Vogel, W. Huggins, N. Lockyer, J. Scheiner and C. Young, bringing new talents and perspectives, and the appearance of institutions devoted explicitly to the physical study of the heavens, all contributed to the explicit appearance of astrophysics. Though many of the practitioners of astrophysics lacked competitive training in, or a devotion to, physical and mathematical theory, they more than made up for the deficit by mastering a new form of instrumentation and fighting to make it a legitimate new science.

In the 1880s, Allegheny Observatory's Samuel Pierpont Langley, who had developed and refined the bolometer and pyrheliometer for the physical study of the sun, preferred the term 'New Astronomy' for what he was doing. He wanted to create a wider venue, and a new meaning for astronomy itself, one which, to Langley's mind, was distinct from the goals of the priest, who, like the traditional positional astronomer, only wanted to "say *where* any heavenly body is, and not *what* it is." Langley's New Astronomy, mainly the study of the character of the sun and its radiation, bore a new application for humanity, "the conclusion being that, in a physical sense, it made us and re-creates us, as it were, daily, and that the knowledge of the intimate ties which unite man with it brings results of the most practical and important kind, which a generation ago were unguessed at." For Langley, the term 'new astronomy' not only highlighted a new venue, but a new utility, which deserved support from the people and their governments every bit as much as the 'old astronomy' enjoyed (Langley, 1889: 3–4).

Rhetoric like Langley's echoed down through the years, repeated by people like George Ellery Hale and James E. Keeler, who promoted the new astronomy as a mean to understand the behavior of matter at the extreme conditions found in the sun and stars. For those gathered at the dedication of Hale's new Yerkes Observatory, Keeler explicitly repeated Langley's claim that the new astronomy "seeks to ascertain the nature of the heavenly bodies, rather than their position in space—*what* they are, not *where* they are." (Keeler, 1897: 746). Hale went further, likening stars to cosmic 'crucibles' where new knowledge could be forged. Entrepreneurs like Hale created the crucible metaphor not only to engage the interest of physicists in astronomical problems, but also to demonstrate the general applicability of astronomical practice. In 1897 he pointed to how the study of a star's spectrum went beyond astrophysics: "To the physicist, and even to the chemist, this fiery crucible may afford the means of performing experiments far beyond the scope of terrestrial laboratories" (Hale, 1897: 311).

This was the promise of the new astronomy, but before it could become truly physical, a significant change in outlook and methodology had to take place. Here we look at a critical phase in the development of astrophysics as it changed from being a largely descriptive empirical activity to being a generally theory-based problem-oriented pursuit during the first half of the twentieth Century. To appreciate the nature of that change, we start by reviewing what came before. We will then confine our attention to three personalities who played a significant role in articulating the differences between the old and new astrophysics, and who best illustrate the profound changes that were required of astronomers who chose to engage the latter pursuit.

3 The Old Astrophysics

In May 1909, looking out over the assembled students and faculty of the Case School of Applied Science in Cleveland, astronomer Edward Charles Pickering, president of the Astronomical and Astrophysical Society of America, director of the Harvard College Observatory, and the most productive organizer and provider of astrophysical data in the world, held up astronomy as a model to be emulated by the other sciences. Astronomy, Pickering observed, enjoyed enormous appeal; through large endowments, some half a million dollars was spent annually on astronomy in America and some observatory budgets were as high as $50,000 per year. Why the support for astronomy? Pickering wondered, "[I]t was probably because astronomy appealed to the imagination. . . [the] most highly developed of the sciences. Indeed, it should be so, since no other science has ever received such support from royalty, from the state and from the private individual" (Pickering, 1909: 105–116).

Attracting patronage had been Pickering's obsession since 1877, when he was appointed Harvard College Observatory director and was faced with a paucity of reliable physical data about the stars and lacked the means initially to change it. It is in his rhetoric about needs that we can sense his view of what the practice of astrophysics meant to him. Pickering knew that astronomy always needed suitable telescopes, photographic and spectroscopic instrumentation, good climate, constant support, and, above all, manpower. "The case is not unlike that of a battleship" he claimed, using one of the favorite metaphors of his day, "Would [one] a thousand feet long always sink one of five hundred feet?" For Pickering, the answer was simple, the captain had to lead a well-trained crew. Stipends were needed to support new recruits, because only a few could afford to volunteer as apprentices. This form of support was more important than giving large telescopes to universities, he claimed, unless those bequests were "accompanied, first, by a sum much greater than its cost, necessary to keep it employed on useful work..." (Ibid.: 109).

Pickering had long enjoyed an honorable place in astronomy as a broker of funds, including the Rumford Fund, the Elizabeth Thompson Fund, and the Bruce Fund of the National and American academies of science. They had yielded scientific results out of all proportion to their cost, he claimed, because these funds had supported manpower for survey and cartographic work, using tried and true techniques and proven methods of analysis. This model worked best at the larger observatories: "These institutions, if properly managed, have after years of careful study and trial developed elaborate systems of solving the great problems of the celestial universe. They are like great factories, which by taking elaborate precautions to save waste at every point, and by improving in every detail both processes and products, are at length obtaining results on a large scale with a perfection and economy far greater than is possible by individuals, or smaller institutions" (Ibid.: 110).

Pickering viewed the business of astrophysics as a huge, diversified factory, where the observatory director was a captain of industry "The true astronomer of to-day is eminently a practical man. He does not accept plans of a sensational character. The same qualities are needed in directing a great observatory successfully, as in managing a railroad, or factory. Any one can propose a gigantic expenditure, but to prove to a shrewd man of affairs that it is feasible and advisable is a very different matter" (Ibid.: 115). On factory observatories see Smith (1991), Lankford and Slavings (1996), Lankford (1997). A sense of the nature of the community and its sources of support at the end of the nineteenth Century can be gathered from numerous sources, including the above, and Rothenberg (1981), Moyer (1992), Doel (1996), and Plotkin (1978).

Pickering's vision of the nature and needs of his science naturally was a vestige of his generation, when the primary challenge facing astronomers was to collect spectroscopic and photometric data. Since the late 1880s and early 1890s, his mode of patronage was simple. First, he wrote to observatory directors asking what they needed. Then, with their needs in hand, he approached possible benefactors, brokering as he saw fit. His view of what deserved support never wavered; the health of the astronomical profession, he stated in 1895, centered upon "the undertaking of large pieces of routine work, and the employment of numbers of inexpensive assistants whose work is in a great measure mechanical, such as copying and routine computing" (Plotkin, 1990: 47).

By the turn of the Century, even though Pickering's staff had spent the past decade amassing huge amounts of spectroscopic and photometric data in their astrophysical assembly lines, no one system of spectral classification or magnitudes had been accepted by the world's astronomers. There had been some 40 years of effort, in the wake of the teachings of Kirchhoff and Bunsen, resulting in at least 20 distinct classification systems. Partly this was because all systems lacked a physical framework against which they might interpret the data for truly astrophysical purposes, such as measuring the temperatures of the stars, their compositions, ages, and stage of life. Modern physics was then beginning to provide hints about the relationship of color to temperature; but there was no rational link between the physics of matter, radiation, and spectroscopic astronomy. The most powerful tool astrophysicists had at hand was the empirical correlation of astrophysical quantities, such as spectrum, color, brightness, or motions. The most productive application of spectroscopic technique was not really physical in the modern sense; it was the measurement of radial velocities, which gave an instantaneous picture of the kinematics of stars and stellar systems, an extension of traditional astronomical interests, akin to Langley's 'where.'

Even so, a few evangelical astrophysicists, such as Hale, proselytized for the centrality of physical theory and technique at the turn of the century. Chroniclers like Agnes Clerke were sympathetic, arguing that "The pliancy and generality of astrophysics contrasts singularly with the austere exclusiveness of gravitational astronomy. The new mode of celestial inquiry follows every indication, lays hold of every clue…." But when it came to extrapolating between the laboratory and the stars, as in calculating the photospheric temperature of the sun, she observed in 1903, such efforts were "not true enough to bear extension into regions beyond experience. Useful over a moderate compass, they prove treacherous adjuncts to investigation." Clerke knew that in the absence of an acceptable theoretical structure, the clear course of action was as before, the accumulation of observations (Clerke, 1903: 67). Indeed, at the turn of the century astrophysics was almost completely driven by observation, to the point of being a moral imperative. This predilection for data collecting was a necessary first step in the establishment of a new discipline, but there were some who looked at this factory mentality with disdain.

When Ludwig Boltzmann visited San Francisco in the summer of 1905 and taught in the Berkeley summer session, he was wined and dined and taken on tours of local attractions which included the Lick Observatory, sitting atop 4300 foot Mount Hamilton to the east of San Jose. Lick personified the factory system in astronomy, producing great quantities of stellar radial velocities and double star measures. Boltzmann may well have envied what money could do, but outwardly chided how it was being used. He was more impressed by the accomplishments of the Berkeley physiologist Jacques Loeb in his frugal laboratory in Pacific Grove, "How striking is the difference between the great works of industry and the modest workshops of science!" he exclaimed. "How imposing are the colossal ocean steamers! But the frequent traveler will have noticed that the officers and crew are always doing the same routine jobs, over and over again.... Gargantuan masses, but not one new thought!" Lick reminded Boltzmann more of industry than science: "Admittedly in science too some things have been accomplished by large-scale effort (as we saw at the Lick Observatory). But the truly great scientific advance (our Minister of Education mustn't hear this) is always made with the smallest means" (Boltzmann, 1992: 50).

Sarcasm aside, Boltzmann's message was simple. Proper scientists professed to frame hypotheses, and then designed observational or experimental programs to test those hypotheses, or so they said they did. American astronomers tended to be cartographers, not asking questions so much as striving to obtain representative samples of natural data, in the hope that someone, someday, would make sense of it all through deft analysis, typically empirical correlation. Most observatory directors worked that way, such as W.W. Campbell at Lick and Pickering at Harvard. As Otto Struve observed in 1943, at the turn of the century a theorist would have found himself "out of place at an observatory" (Struve, 1943: 477). Hale was the exception, doing all he could to attract both experimental and theoretical physicists who could link his telescopes to laboratory experimentation, and optimizing those telescopes to answer astrophysical questions. And Keeler too had tried to initiate a formal astrophysics program at Allegheny Observatory in the 1890s, but there were no takers. In the late 1890s he was called back to Lick as a reformer, but soon sadly died. These sporadic efforts started to pay off slowly, however, as the new physics emerged. Hale searched for the Zeeman effect in sunspots at his new solar observatory atop Mount Wilson, found it, and verified the existence of magnetic fields in the Sun. That made the news.

4 The New Astrophysics

In the first decade of the twentieth Century, English physicists like Arthur Schuster and Alfred Fowler started encouraging their students to apply theory and experimental analysis to specific problems in astrophysics. George Darwin was doing much the same, more in the realm of geophysics and cosmogony, whereas astrophysicists at Potsdam and a few other European sites were providing advanced formal training in spectroscopic technique. Ejnar Hertzsprung's earliest published application of Planck's radiation law in 1905 led to a prediction for the angular diameter of Antares that was close to the mark. His prediction could not be verified, however, and so was generally ignored, as were the later predictions by Europeans using physically calibrated colorimetry pioneered by Karl Schwarzschild at Potsdam. Henry Norris Russell of Princeton was led to what is today called the 'Hertzsprung–Russell Diagram' by his mission to test an unpopular theory of stellar evolution, and showed that giant stars existed.

There were, to be sure, highly influential and effective practitioners of astronomical mapping and correlating of the various observational quantities acquired. Topmost was J.C. Kapteyn, whose campaigns in the first two decades of the Twentieth Century led to many insights into the dynamics of the stellar system, some of which were interpreted physically. Kapteyn's 1906 'Plan of Selected Areas' envisioned the world-wide cooperation of observatories 'to bring together, as far as possible, all the elements which seem most necessary for a successful attack on the problem of the structure of the sidereal world' (Van der Kruit, 2022: 374, 429). His vision stimulated many American astronomers into action, including Henrietta

Swan Leavitt, Walter S. Adams, and Harlow Shapley, to name a few. Their insights were limited to empirical correlations but stimulated many questions as to why these correlations exist.

A milestone in accepting the power of physical theory came when Niels Bohr explained the co-called second spectrum of hydrogen, observed for over a decade by E.C. Pickering in stars of the Zeta Puppis type, as due to ionized helium. Although Alfred Fowler resisted the new intepretation at first, his continued attention to the detail of the analysis resulted by 1914 in Fowler's modification of Rydberg's theory, the success of which soon converted him to the Bohr model and to a new framework of thinking within which to interpret spectra (Robotti, 1983: 123; McGucken, 1969: Ch 3). There was still an enormous gap between the laboratory and the stars, but it was closing.

Hale did more than anyone alive to establish astrophysics, building a succession of observatories with associated physical laboratories, creating a journal devoted explicitly to astrophysics, with a board of editors composed equally of physicists and astronomers, and finally helping to found a professional society that explicitly recognized astrophysics. But his Mount Wilson Observatory staff still relied on empirical correlation as their chief mode of investigation. More than at most observatories, however, his spectroscopic staff searched out specific problems and planned observations to solve them, but rarely if ever were their problems formed around physical principles or physical issues. Even the physicist A.S. King, whom Hale employed to develop a temperature classification based upon laboratory spectra, produced a detailed, yet empirical classification, based upon correlation. His was only one of many correlations that puzzled astrophysicists. The technique of spectroscopic parallaxes of W.S. Adams and A. Kohlschütter provided access to stellar luminosities far deeper into space than trigonometric parallaxes, but what caused the spectroscopic signature of a star to be sensitive to luminosity? Where was the physics in all of this? In 1924, the English mathematical theorist Edward A. Milne looked back over the previous decade, noting the many correlations that had been found, and the concern of many that they lacked a rational basis:

> It was known that some spectral lines could be produced, in the laboratory, only at high temperatures or under intense discharges, and that such lines were often only to be found in stars with high effective temperatures. But it was not known why this was so, or why the same line tended to disappear at still higher temperatures; and of quantitative explanation there was none. There appeared to be a definite relation between effective temperature and type of spectrum, but the connection was empirical. There was a gap in the logical argument. (Milne, 1924: 95)

In making this observation, Milne highlighted the recent work of the Calcuttan physicist Meghnad Saha, who had shown that there was a definite relationship between temperature and spectrum, based upon his ionization equilibrium theory. Saha's theory of thermal ionization equilibrium rationalized the dominant system of spectral classification from Harvard and drew the attention of observational astrophysicists to the applicability of Bohr theory. Saha, thinking like a physical chemist, combined Bohr theory and the chemical thermodynamics of W. Nernst, letting his reactants and products be light and heat, and the balance the fraction of ionized to neutral atoms in a gas. Saha found that for a given temperature and pressure, a specific degree of ionization will be maintained due to a set rate of absorption and emission of photons. He derived for the first time the interrelationship of the total pressure of a gas, the degree of ionization of a particular element within the gas, the ionization energy (or potential) of the element, and the temperature within the gas (DeVorkin & Kenat, 1983a, b). This application of Bohr theory established an indelible link between astrophysics and physics and changed the professional lives of many spectroscopic astronomers.

Russell was among those most affected by Saha's work. In August 1921, applauding Hale's efforts to build an integrated agenda at Caltech to link the laboratory to the stars by studying all aspects of the structure of matter, Russell cited Saha's work as the master key, unlocking "the immense possibilities of the new field of investigation which opens up before us." There was a great new world to conquer, "and the astronomer, the physicist, and the chemist must combine in the attack, bringing all their resources to bear on this great problem, which is of equal importance to us all" (Russell, 1921: 280). Completing Langley's forty-year-old prophesy, the explanatory power of quantum theory by the 1920s in the hands of those who exploited Saha's work—E.A. Milne, R.H. Fowler, Russell, Cecilia H. Payne, Donald Menzel, Albrecht Unsőld, Antoine Pannekoek—helped to make astrophysics truly the new astronomy, for it is only by that time that astrophysics began to absorb its parent to become the dominant mode of studying the physical universe.

5 Russell's Shift

In the 1920s, Russell became one of the most ardent advocates for a shift to an astrophysics based not only upon the interpretive power of physics, but upon its methods as well. In many ways, his scientific life stands as an example of this shift. Russell came to his famous diagram, for example (now known as the 'Hertzsprung–Russell Diagram' because Ejnar Hertzsprung had found much the same relationship a few years earlier), via a theory of stellar evolution. A theory, in other words, framed his observational agenda, and highlights his then-radical research style. It is not surprising that many popular reviews of his work by astronomers in subsequent years state just the opposite, that he constructed the diagram, showing that giant stars exist, and from it deduced a theory of stellar evolution by the accepted method of empirical correlation. Russell's own papers on the subject imply just that and were carefully crafted to convince skeptical astronomers. Nevertheless, Russell knew how to work from what he called, much later, a 'tissue of approximation' and found indeed that this mode of research was extremely rewarding. Reacting strongly to criticism from a traditional double star observer in Philadelphia in 1914, who had objected to Russell's fast and loose methods of approximation, Russell shot back:

> I have not Professor [S. W.] Burnham's serene and self-denying willingness to let the derivation of results from my work wait until fifty or a hundred years after the probable date of my decease. I am always trying to best Father Time with the aid of what mathematical

> weapons I can bring to bear on things. A hundred years hence all this work of mine will be utterly superseded: but I am getting the fun of it now. The altruistic nature of the work that you double-star people are doing arouses my lively admiration. But I fear that I am too selfish to emulate it. (Russell to Doolittle, 1914)

Russell always strove for computational efficiency, shortcuts, and a problem-oriented approach. In 1912 he devised the first general technique for the solution of the orbits of eclipsing binaries, a feat that contemporary specialists believed was impossible. Constantly striving for methodologies based upon physical insight and intuition, Russell felt that routine work had its place, only if it was driven by specific questions. He thus reacted poorly when his patron, Pickering, continued to insist that the core of activity in astronomy was routine observation, and that the primary needs of astronomy were for more computers and observing assistants, not people who could think for themselves. Russell complained to Pickering, holding up his first graduate student as a shining example of the kind of person astronomy really needed: "[O]ne good man like [Harlow] Shapley, who would have to be paid a good salary to keep him, is worth more than ten ordinary computers" (Russell to Pickering, 27 Feb., 1917). Shapley was one of those rare fellows who thought about what he was doing. Hale was even more critical of Pickering's position. Admitting that purely inductive processes had their place, still one had to be judicious and promote those that stood the best chance to solve "specific sidereal problems." Above all, Hale argued, responsible observatory directors had to find and train staff "capable of thinking for themselves" (Hale to Pickering, 1917).

In later interactions with Pickering, Russell took on Hale's bluntness: "[T]here are two sides of astronomical research, one of which has to do with the collection of facts, and the other with their interpretation." The former was routine; the latter was not. Pickering had always emphasized the routine aspects. Now the other side needed airing: "[I]t is upon studies of this sort that the future advances of any science must very largely depend" (Russell to Pickering, 22 Nov., 1917b).

6 'Some Problems of Sidereal Astronomy'

In 1917, after several frustrating rounds sparring with Pickering about the needs of astronomy, and how astronomers might aid in the war effort, Hale asked Russell to prepare a general essay on the needs of astronomy under the auspices of the National Research Council. This was part of an overall strategy Hale had developed to establish the sciences as a primary resource for ensuring national security, thereby making science the focus of national interest, expressed in continuing and vastly increased levels of corporate philanthropy. Russell was an ardent interventionist, and gladly sought out ways to make science useful in the war effort. But he also was willing to think ahead and drafted an essay that took a fresh look at the needs of astronomy. In his published report after the War in 1919, Russell took aim at what had been, particularly, an American tradition in astronomy:

> The main object of astronomy, as of all science, is not the collection of facts, but the development, on the basis of collected facts, of satisfactory theories regarding the nature, mutual relations, and probable history and evolution of the objects of study. (Russell, 1920: 212)

Observation should guide theory, Russell believed, but the recent mathematical work by Arthur Stanley Eddington on stars as gas spheres in radiative equilibrium and by Jeans on the dynamics of rotating systems, were critical "in the solution of the larger problems of sidereal astronomy" (Ibid.). Jeans and Eddington's style, Russell noted in July 1917, made "theoretical astrophysics . . . a new branch of mathematical astronomy" (Russell to Eddington, 27 July, 1917). Even before Saha's revelations, Russell knew that carefully planned campaigns to collect high-quality solar and stellar spectra were the "master key" to the solution of many of the pending problems he had identified. But he added strongly that observers had to become sensitive to the fact that astronomy was no longer the mere collection of data. Observing programs had to be informed by theoretical questions, such as finding the source of energy which drives the Sun and stars, "at present the greatest of all the unsolved problems of astronomy" (Russell, 1919: 414). And moreover, as Matt Stanley has recounted, Russell appreciated that Eddington and Jeans and their ilk had created:

> a methodological shift towards a phenomenological approach that allowed existing theories and observations to be synthesized in a way that suggested rich new avenues for astrophysical investigation. (Stanley, 2007: 53)

Russell shied away from theoretical modelling, though he often applied their predications. In a way he stood between the observationalists and the pure mathematical theorists, later raising the ire of theorists like E. A. Milne, who complained that Russell lacked 'a theory of knowledge' and his work was deficient in 'grace and elegance.' (Milne to Chandrasekhar, 26 December, 1937: 3–4, a sentiment recently echoed by Cenadelli, 2010: 2154).

Russell's essay certainly attracted attention. In January 1920, Robert Aitken, acting director at Lick Observatory, congratulated Russell: "I read it, as everybody here did, with the greatest interest" (Aitken to Russell, 16 January, 1920). Kind words notwithstanding, it was abundantly clear that Russell was moving against the tide, especially at places like Lick. Eddington, too, sensed this resistance, and fought back in an address on 'The Internal Constitution of the Stars' in August 1920: "If we are not content with the dull accumulation of experimental facts, if we make any deductions or generalizations, if we seek for any theory to guide us, some degree of speculation cannot be avoided" (Eddington, 1920: 356). The watchword of the day remained, however, as even enlightened Mount Wilson astronomers like F.H. Seares articulated in 1922, that: "The most pressing need for further study of the structure of the stellar universe is still the accumulation of observational data" (Seares, 1922: 252). One can appreciate Seares' objection, given his interests in the form and structure of the galactic system.

Indeed, how one reacted to Russell's and Hale's admonitions depended upon one's specialty. Russell was keenly aware of this and often framed his arguments in a manner calculated to convince as broad a constituency as possible. He structured his arguments accordingly. Possibly the best example was his 1929 paper on the composition of the solar atmosphere, wherein he argued for hydrogen's dominance based upon its atomic properties, and only upon this platform did he then present the physical data and Cecilia Payne's critical interpretation of it (DeVorkin & Kenat, 1983b). In this manner, those he knew who were most affected by her findings, like Eddington, were compelled to face her groundbreaking discovery and deal with it in a long process that led to the modern view of stellar structure, evolution, and the nature of the universe.

Establishing physics at the core of astrophysical practice took decades and required powerful adherents. By the end of the 1930s, Struve remained frustrated with the remnants of Pickering's and Campbell's legacy still dominant at most American observatories, observing that "[a] physicist would consider it incomprehensible that anyone should find satisfaction in observing a phenomenon only because it is measurable" (Struve, 1943: 470). But by 1955 he happily recalled that "[m]y own work in astrophysics was stimulated by this article, and even today it forms one of the most inspiring pieces of astronomical literature" (Struve, 1955: 216). Other adherents, many of whom became leaders in American astronomy, included Russell's key students, like Shapley, Donald Menzel, Theodore Dunham, Jr., and Lyman Spitzer, Jr.

The style of research Russell advocated in the second and third decades of the century became mainstream after the Second World War. In the interim many factors brought about the changes Russell sought. First was the influx of well-trained foreign astronomers and physicists in the 1930s, escaping Nazi oppression. Although many American astronomers viewed the migration as a threat to domestic talent, Russell saw it as a thin 'silver lining' where American astronomy would profit from the deepening global tragedy (DeVorkin, 2000a). The second factor was the postwar emergence of large-scale government funding. Again, leading American astronomers, mainly observatory directors, resisted this new source of support far more than physicists, because traditional philanthropic sources had proven adequate for their needs. But faced with the potential of ONR and then NSF support and pressured from below by aggressive younger astronomers who had made many lucrative contacts in wartime, such as Menzel at Harvard and Spitzer at Princeton, the directors soon found themselves caught up in a very new process of proposal writing which more than not favored a problem-oriented approach (DeVorkin, 2000b). Finally, the gradual shift in advanced training from the observatory chamber to the campus continued to weaken the autonomy of the observatory director. These factors, along with the obvious ones of the incredible applicability of first quantum, then nuclear and now particle physics to the solution of astrophysical problems, made the transition complete, and not likely to be reversed.

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