Back to the Future

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What can history say about what works and what does not work when setting priorities for new research programs and methodologies in astronomy? Is there an optimum balance between risk and innovation, such as building upon proven technologies or experimenting with new unproven technologies? Have astronomers been most effective as tool builders or cross-disciplinary entrepreneurs, acquiring expertise in emerging areas of physics, or have they been most effective when they manage to attract this talent and expertise from the new disciplines of physics? Does the degree of correlation between the motives and goals of patrons of astronomy and the goals of astronomers influence the quality and nature of subsequent knowledge production? What produces a better product or more robust discipline: elite-based meritocratic reward structures or egalitarian social, institutional and regional reward structures? While there are no absolute answers to questions like these, there may well be historical indicators that hint at what worked well and what did not prove to be especially effective. This presentation will provide several case studies from 20th century astronomy that offer illustrative guidelines astronomers might find helpful in thinking through these questions.

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1. Introduction

Don Osterbrock, who is well known to all of us, shifted his considerable talents from astronomy to its history many years ago feeling that, as the title to his Doggett Prize lecture at the 199th meeting of the AAS, attests “History is too Important to be left to the Historians.”[1] I agree with that; at the least to the extent that astronomers need to be aware of their disciplinary history for a wide variety of reasons. Given the tenor of this special session at the IAU GA, here I explore only one facet of history’s value: What can history say about what works and what does not work when setting priorities for new research programs and methodologies in astronomy? History can provide clues and possibly templates, but not predictions. The world and the societies on its surface are far too complex. William Shakespeare knew this when he crafted his oft-quoted passage in the *Tempest* "Whereof what’s past is prologue…”[2] Shakespeare is telling us that events of the past make up today's world, but also that it is what we choose to do today that determines our future. The U.S. National Archives has taken Shakespeare's advice, carving “What’s past is prologue” on its main building in Washington, D.C. Indeed, the archives attracts more than historians: policy specialists, congressional staffers, anyone looking for precedent as well as insight. Niels Bohr, however (or was it Yogi Berra?) has cautioned that “Prediction is hard . . . especially about the future.” The Web is full of humorous quotes by famous people predicting the future.

To this preamble, and the cryptic title for my talk meant to evoke a sense of futility, I might only add that knowing your history may well be useful in knowing how and why certain paths were taken in past time that were responsible for producing the world of astronomy we encounter today. The best we can do is examine what worked well in the past, what choices were made, risks taken, and what worked out and what didn’t, and why. Know too that this is far from a prime motivation for doing history, not mine at least. Mindful that there are an infinity of contingencies to consider in any process leading to problem choice, I present here a few ways things seemed to work out in the past. My presentation is anecdotal, not encyclopedic. I'll not be covering all the questions I raised in my abstract, but have chosen two to illustrate what are important aspects of knowledge growth in astronomy. The first might be called “acceptable practice in astronomy.” By that I mean how astronomers as a culture go about agreeing to a particular world view. And the second will deal with taking risks. Both of these are distilled from historical work I’ve conducted in the past, and are therefore derivative works adapted from previously published papers and books. But to avoid any implication that these are the only works worth citing, I’ll begin with a short review of the general historical literature on the history of modern astronomy that I feel would be useful for practitioners seeking enlightenment.

1.1 Brief Introduction to Historical Works of Special Interest to Practicing Astronomers

The history of astronomy is all the richer for the great interest shown by astronomers. Certainly in recent years Don Osterbrock has illuminated many of the major characters of early and mid-20th century astronomy with his biographies and institutional histories, and has lent his own particular views to what works and does not work in astronomy, especially in his study of the rise and fall and rise of the Yerkes Observatory.[3] Allan Sandage as well has traced the scientific contributions and evolution of staff

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[1] Donald E Osterbrock, *The view from the observatory: history is too important to be left to the historians*, In: *Organizations and strategies in astronomy III.* André Heck (ed.). Astrophysics and Space Science Library, Vol. 280. Dordrecht: Kluwer Academic Publishers (2002), 201-215. American Astronomical Society, 199th AAS Meeting, #31.01; Bulletin of the American Astronomical Society, **33,** 1354-1368.

[2] *The Tempest,* actII, scene 1, line 253-54. From *askville.amazon.com/SimilarQuestions.do?...Past+Prologue*

[3] Donald E. Osterbrock, *Yerkes Observatory, 1892-1950, The Birth, Near Death, and Resurrection of a Scientific Research Institution*. Chicago: University of Chicago Press (1997).

and programming at the institution he helped to maintain and prosper.[4] One astronomer who has made a significant departure from this pattern is Martin Harwit, whose *Cosmic Discovery* broke new ground by exploring the relationship of new techniques and new technologies, especially new telescopes, on the rate of discovery of new classes of objects in the universe.[5] Although Harwit at a deeper level was concerned with establishing guidelines and criteria for evaluating the reality or reliability of new discoveries in astronomy, his work has been recognized widely as a landmark statement on the importance of fostering new technologies even in the face of considerable cost and risk. One of the case studies I will review here addresses this question.

If I were asked which works by historians are most illuminating of recent astronomy, but that offer perspectives that are distinctly historical, I would certainly start with works by Owen Gingerich, Karl Hufbauer, Robert Smith, John Lankford and younger scholars like Patrick McCray and Matt Stanley. In his general history of solar research, *Exploring the Sun: Solar Science Since Galileo*, Karl Hufbauer not only explores more thoroughly than most what the discipline “astrophysics” meant to astronomers over time, starting with Germans who saw it as a "coalescence" of "physics and chemistry with astronomy,"[6] but also explores the growth and continual adjustment of this relationship and establishes criteria for what he calls the “robustness” of a discipline, its ability to bring different techniques and methodologies to a particular question and where the answers can be compared in terms of the reliability, power or fidelity of the technique. The nature of the profession that stimulates or restricts this robustness has been explored in Lankford’s *American Astronomy: Community, Careers and Power, 1859-1940*.[7] Although his depiction of the changing nature of the astronomical community during this time is limited to the first half of the 20th Century, it still provides very useful hints for how these changes can influence career paths of individuals and the changing nature of the questions deemed by peers that were appropriate to ask. Hierarchical structure and funding patterns changed drastically in the postwar era, of course, and some of this is captured in recent historical literature, but is best explored generally by reading Robert Smith’s landmark study of the *Space Telescope* and McCray’s recent study of *Giant Telescopes*.[8] In both, one can see how mega-projects that characterize recent astronomy were both influenced by the cultural values of formal and informal social structures within the modern scientific community, but how competition and priority setting can have profound influences on the way the science itself is done.

These are but a very few of the many major works in the history of recent astronomy that I feel could provide useful perspective on what worked, and what did not work, and why. The following case studies, adapted from work I've done over the years, are presented in the hopes that they will offer direct access to instances where behavior as well as new techniques and innovation, were important. The first deals with acceptable practice in the discipline.

2. Acceptable Practice in Astronomy

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4 Allan Sandage, *Centennial History of the Carnegie Institution of Washington. Volume 1 The Mount Wilson Observatory*. Cambridge, UK: Cambridge University Press (February 2004).

5 Martin Harwit (1984).

6 Quoted in Karl Hufbauer (1991), 64. See also Dieter B. Herrmann, *The History of Astronomy from Herschel to Hertzsprung*. Translated and revised by Kevin Krisciunas. Cambridge: CUP (1984), 70.

7 John Lankford; with the assistance of Ricky L. Slavings. *American astronomy: community, careers, and power, 1859-1940*. Chicago: University of Chicago Press (1997).

8 Robert W. Smith with contributions by Paul A. Hanle, Robert H. Kargon, Joseph N. Tatarewicz, *The space telescope : a study of NASA, science, technology, and politi*cs. Cambridge [England] ; New York : Cambridge University Press (1989). W. Patrick McCray, *Giant telescopes: astronomical ambition and the promise of technology*, Cambridge, Mass.: Harvard University Press, 2004. A focused study of AURA and the National Observatories, by Frank Edmondson, and a reconnaissance of funding patterns in postwar American astronomy by the author, would round out this listing: Frank K. Edmondson, *AURA and its US National Observatories*, Cambridge, U.K.; New York: Cambridge University Press (1997). David H. DeVorkin, *Who Speaks for Astronomy? How astronomers responded to government funding after World War II, Historical Studies in the Physical and Biological Sciences* **31**, pt 1, (2000), 55-92.

"Extraordinary claims require extraordinary evidence" Carl Sagan once argued, regarding the results of paranormal research.[9] What might now be taken for granted, like the fact that the dominant element in the sun and stars is hydrogen, was not always so.

A long-held opinion in astronomy, ever since the sun, moon and planets became physical under scrutiny by the telescope, is that they, and all bodies in the universe, are generally composed of the same materials. In 1891 Henry Rowland claimed that "were the whole earth to be heated to the temperature of the Sun, its spectrum would probably resemble that of the Sun very closely." Of course, Rowland was not here suggesting that the proportions were the same, but as each new element was discovered, especially in 1895 when Ramsay identified helium, the conviction that there was chemical unity in the cosmos held sway.[10] Astronomers like Henry Norris Russell shared this view; in a 1916 lecture he pointed out that the concept of unity in Nature meant that "The same chemical elements exist in the stars as in the Earth to the remote limits of the universe."[11] The spectroscopist Alfred Fowler agreed, in 1918 claiming that: "as work proceeds, it becomes less and less probable that the Sun contains any elements which do not also enter into the composition of the Earth. It seems natural to infer that the composition of the Sun may be practically identical with that of the Earth."[12]

But astronomers of that time keenly knew that such pronouncements stood on unsure ground. What do the spectra of the stars reveal about stars themselves? Temperature? Composition? Age? Size? And what about luminosity effects -- what caused the widths of lines to vary? The discovery that there were relations between the spectra of stars and their brightnesses was remarkable, and was interpreted many ways. Much of stellar astronomy in 1920 was based upon empirical correlation; physical theory was only then emerging that promised to reveal the underlying principles. As E. A. Milne observed in 1924:

> .... although most observational astronomers and most astronomers generally, typically Americans and Continental Europeans, were pretty impressed with the empirical correlations they had been finding and building upon for decades (the P/L relation, spectroscopic parallaxes, an observational mass luminosity relation) there were some who worried in the late teens through 1920 that all this lacked physical understanding...[13]

Milne was here speaking of the work of a young Bengalese scholar, Megnad Saha, who was the first to show that there is a relationship between the appearance of a line in a spectrum and the relative number of atoms available to produce that line in terms of the temperature of the gas, the pressure, and the energy needed to ionize the element. Saha's first papers were noticed because people like Alfred Fowler made a lot of noise about them. But people like Milne were skeptical until the equation was based upon solid theory - Nernst's heat theorem, chemical thermodynamics, and a bit of hand waiving, were not acceptable practice. So Milne and R. H. Fowler rederived the relations using the statistical mechanics of gases established by Boltzmann -- hence, in western textbooks, the famous relation - the foundation of quantitative astrophysics, came known as the Saha-Boltzmann equation, or, just the Boltzmann equation, even though Ludwig died in 1906.

But it was through Saha's work that a path was finally hewn to overturn the dominant view of the relative abundances of the elements in the universe. Over the next decade, this view changed radically with the application of Saha's ionization theory to composition studies in stellar atmospheres. In its original form, based upon principles in chemical thermodynamics applied to Bohr theory, Saha's theory was not acceptable to European and American astrophysicists, who quickly set about the task of refining it and elaborating on its use. By the mid-1920s, equipped with those refinements after training under E A

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[9] Often attributed to Carl Sagan, see: http://quotes4all.net/quote_989.html. Variously attributed, see: http://en.wikipedia.org/wiki/Marcello_Truzzi.

[10] Rowland, quoted in David DeVorkin and Ralph Kenat (1983), 186.

[11] H. N. Russell, "The Scientific Approach to Christianity" lecture manuscript, October 1916. Box 72.1(c), Princeton University Library Henry Norris Russell Papers (PUL/HNR).

[12] Alfred Fowler, 1918, 204, quoted in DeVorkin and Kenat (1983), 183.

[13] Edward A. Milne, *Recent Work in Stellar Physics, Proceedings of the Physical Society, London* **36** (1924), 94--113; quote from 95.

Milne and R H Fowler in Cambridge, Cecilia Payne went to Harvard for graduate work and access to the greatest stellar spectroscopic plate vault in existence. Her plan at first was to utilize these data to calibrate the Harvard spectral sequence as a temperature sequence, but soon she added the determination of the relative abundances of the elements in stars. To be sure, there were hints that serious anomalies existed in stellar abundances compared to terrestrial ratios. Another student at Harvard in the spring of 1924, a Russell student from Princeton named Donald Menzel, had already found that hydrogen lines were anomalously strong in the spectra of giant stars. This was indeed a puzzle that needed attention.

By the fall of 1924, Payne was hard at work refining and extending Menzel’s work, to distinguish her thesis from his, and realized that the hydrogen anomaly was even greater than he had expected. Her Cambridge teachers, Milne and Fowler, had already suggested that Saha’s methods could be utilized to determine relative abundances, and though Russell advised her not to take on the additional work for her thesis, she forged ahead. They called their method the determination of the "fractional concentration" of an element. This was the fraction of the total available that contributed to the intensity of a line as a function of temperature. Since this was before the days of transition probabilities, she had to assume that all elements behaved equally. But at the least, using Saha’s marginal appearance technique, Payne could interpret the physical situation where a line became visible--its marginal appearance in the spectral sequence. Assuming that the intensity of any given line at a given temperature was also a function of abundance, she concluded that the relative abundance of an element could be determined by the fractional concentration at marginal appearance. She did have rudimentary ionization and resonance potentials for each element, but had to assume a partial electron pressure, and a temperature provided by her own calibration. With these she used Fowler and Milne's theory to calculate fractional concentrations. Given the crude order-of-magnitude limits she was employing, her assumptions seemed to hold nicely. So she gathered up all her observations of marginal appearances in the Draper sequence, and with her temperatures calculated relative abundances.[14]

Russell was well aware of Payne’s progress since he had been asked to be one of her mentors on her thesis. He was much impressed with the young student, already recommending her for a National Research Council fellowship to support her next year at Harvard. At the end of the year he was delighted that she had received it. Russell was deeply interested in her analysis, but knew that she had made many assumptions, some critical. The real challenge, he lectured her in a letter in January 1925, 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 certain state which will absorb various lines originating in this state." Russell correctly predicted 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." This, of course, is what Russell originally thought Payne wanted to do, and he may have been right. But such a theory was still well in the future, even though there were a number of clues, presented by the physicist William Meggers and also by Russell’s own Princeton colleague John Q. Stewart, that hinted that a full-blown theory was not terribly far off.[15] For the present, however, Payne had come up with a most provocative analysis, and Russell had to deal with it.

We know that her initial findings showed that hydrogen and helium were orders of magnitude more abundant in stellar atmospheres than the rest of the elements she examined.[16] When Russell found this conclusion in her draft, he figured that something was amiss with the theory, just as he did when his own student, Donald Menzel had found discrepancies in his assessment of ionization potentials using much the same data. Russell felt that Payne had uncovered another "very much more serious discrepancy"

[14] Cecilia Payne, 1925. "Astrophysical data . . ." 193-194, in DeVorkin and Kenat (1983), 185-186. See also DeVorkin (2000), 200-205.

[15] Russell to Payne, 14 January 1925. PUL/HNR. Quoted in DeVorkin and Kenat (1983), 187-188, where a full discussion of this letter is found.

[16] Peggy Aldrich Kidwell, *An Historical Introduction to `The dyer's hand'* in Heramundanis (1984), ed., 11--37.; Owen Gingerich, *Henry Draper's Scientific Legacy*, in A. E. Glassgold, P. J. Huggins and E. L. Schucking, eds., *Symposium on the Orion Nebula to Honor Henry Draper, Annals of the New York Academy of Sciences* **395** (1982), 308-320.

> It is clearly impossible that hydrogen should be a million times more abundant than the metals, and I have no doubt that the number of hydrogen atoms in the two quantum state is enormously greater than is indicated by the theory of Fowler and Milne. . . 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.[17]

The hydrogen anomaly had long been an issue with Russell and other astronomers. Russell and A. H. Compton had tried to explain it away with a metastable states argument, and his argument to Payne derives from this. Just how Payne felt about Russell's rejection of her most dramatic conclusion is not known, but evidently from the earliest manuscripts extant, which are close to what was published, she dutifully made the changes he thought were necessary.

The published form of her abundance discussion highlights not the discordances, but the striking similarity between celestial abundances and the relative abundances of the elements in the Earth's crust. Indeed, she seemed to belabor this conclusion, drawing heavily upon the arguments Russell made in his brief study in 1914. This was a brilliant political tactic, given Russell's power.

Cecilia Payne hedged her bets: unable to clearly state her findings as forcibly as she knew how, she decided to give Russell the credit for an old qualitative abundance picture she figured could well be thoroughly altered in time. She knew that Harry Plaskett had found ionized helium to be abnormally strong in the hottest stars, so it was a safe bet that something was wrong with the present scenario. If the problem was due to theory, then she was well advised to acquiesce. But if her abundances were right, then she would be ultimately vindicated. This explains why, in both her National Academy paper on abundances and her eventual thesis, she persisted in displaying her results for hydrogen and helium, both in tabular listings and discussion. In her Academy paper, she concluded, citing Russell's and Compton's metastable state argument as a rational: "The stellar abundance deduced for these elements is improbably high, and is almost certainly not real." She said much the same in her thesis, even quoting from a portion of Russell's letter.[18]

Russell had his reasons for rejecting Payne's hydrogen abundance and they stemmed from his high regard for Eddington's work. Eddington's ability to describe the interiors of the stars using his theory of radiative equilibrium had by the early 1920s strengthened Russell's theory of stellar evolution. Eddington's view of how stars work required, however, that they possess rather little hydrogen. In the early stages of his work, as he pondered how stars powered themselves, he seemed amenable to the possibility that hydrogen was highly abundant, preferring hydrogen fusion to matter annihilation. But he also soon found that the consequences of a significant role for hydrogen created problems for his radiative models.

In Eddington's theory, mean molecular weight was a factor influencing a star's behavior; i.e. how the physical variables of temperature, density and pressure varied with radius in the star. His theoretical interpretation of the mass-luminosity relation, announced in 1924, not only gave theoretical support for a great quantity of empirical evidence suggesting that the luminosities of stars varied regularly with their masses, but also suggested that composition could not be a major factor influencing how a star looked and behaved. At the high temperatures in the stars, the mean molecular weight within stars did not vary much; all elements were highly ionized, and the resulting mix was an arithmetic blend of the weights of the freed electrons and exposed nuclei, with electrons dominating in number. Thus "Stellar atoms," as Eddington quipped in 1927, "are nude savages innocent of the class distinctions of our fully arrayed terrestrial atoms."[19] This colorful scenario worked beautifully in his equations, resulting in a mean molecular weight near 2, which never varied.

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[17] Russell to Payne, 14 January 1925. PUL/HNR. Quoted in DeVorkin and Kenat (1983), 187-188.

[18] Cecilia Payne, 1925, "Astrophysical Data," 197; *Stellar Atmospheres, Harvard Observatory Monograph No. 1*, 188.

[19] A. S. Eddington, *Stars and Atoms.* Oxford: Oxford University Press (1927), 22.

But, as Eddington keenly realized, hydrogen in its ionized state has a mean molecular weight of 0.5, since it possesses an equal number of nucleons and electrons (one proton and one electron). A significant amount of hydrogen would thus lower the mean molecular weight of the gas, which, as Eddington passionately argued in February 1923 before the Royal Institution, played havoc with his physics: "Hydrogen gives results differing widely from all the other ninety-one elements. To assume hydrogen as the material would in most cases destroy the general accordance of theory and observation." Eddington expressed great satisfaction that his models had been "practically independent of the chemical composition of the star" but emphatically added that this condition held "provided it is not made of hydrogen."[20]

Eddington had other arguments for low hydrogen as well, since the mean molecular weight and the coefficient of absorption of the stellar gasses also appeared in his equations describing steps leading to his theoretical mass-luminosity relation. Again everything worked beautifully, according with observation, as long as hydrogen was a minor player. Eddington was so convinced of this requirement that he ignored for the moment the fact that a recently announced physical theory for the coefficient of absorption of gasses by H. A. Kramers, a colleague of Bohr's, differed greatly from one Eddington had created empirically, based upon astrophysical arguments alone. Eddington knew he was on thin ice here, and so did people like Rosseland.

The interlaced arguments Eddington arrayed to make his stars work built up what seemed to be a consistent picture requiring a low hydrogen abundance. It was a basic fact of his theory, expressed time again in his talks, articles and books. And though he certainly had his critics and detractors in the likes of Jeans, Milne and F. A. Lindemann, Eddington's stature in astrophysical theory was largely uncontested in the 1920s. Anyone pressing a high hydrogen abundance in stellar interiors had to face Eddington. Cecilia Payne recalls that when she discussed her original results for hydrogen with Eddington, and admitted "that I believed that there was more hydrogen in the stars than any other atom," he shot back: "`You don't mean in the stars, you mean on the stars.’"[21]

These are but a few of the many reasons why, at the time she published her thesis, Cecilia Payne well knew that extraordinary claims required extraordinary evidence. She accepted Russell’s advice, but made clear in her own writings that she had indeed arrived at these surprising conclusions for hydrogen, but left it for the future to determine acceptance.

Payne’s thesis was very well received by astronomers. Otto Struve reviewed it, fully aware of criticisms that had been laid on Saha’s theories and applications of it by other astronomers, and cognizant of the fact that the entire field was as yet very unsettled. Yet, Struve proclaimed that the published version of her thesis, titled *Stellar Atmospheres*, was a modern landmark study in the new field, “full of useful suggestions for the practical worker…”[22] Struve was then probably more receptive than most to Payne's work among American-based astrophysicists. Yet one can easily see in his criticisms that the field had hardly coalesced to the point where a consensus was possible. Given Struve's criticisms, therefore, Russell’s advice was prudent. There is no evidence that he wished to suppress her findings; rather he realized, as Struve observed in his review, that the field was yet young, largely untested, and was not robust enough to warrant radical conclusions.

In the following four years, as she completed her degree, enjoyed a growing solid reputation within the American astronomical community and was indeed retained by Shapley as a Harvard staff member, Payne watched as Russell and others continually faced and tried to rationalize the hydrogen anomaly. Payne herself moved on to other projects. Her work was certainly not forgotten by Russell, who in 1929 finally accepted her conclusions and published a long paper in the *Astrophysical Journal* that laid out, in ways ApJ readers would accept, all the arguments that led to the still radical conclusion. In those four years, many lines of evidence, including from atomic theory, new and more direct ways to

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20 A. S. Eddington, *The Interior of a Star (1923)*, in B. Lovell, ed., *Roy. Inst. Library of Science: Astronomy* **2** (1970), 256-257.

21 Eddington (1923), 256-257; Heramundanis (1984), 165.

22 Quoted in DeVorkin (2000), 204.

relate the intensities of lines in multiplets to the number of atoms involved in producing each line, and improved techniques for determining what processes were acting to create the most prominent (i.e. strongest, widest, most broadened) features in stellar spectra, aided the effort. Russell was not the only one pursuing these paths. Others, including Albrecht Unsold, Charlotte E. Moore and John Q. Stewart, worked in parallel to gather the evidence. They all were coming to the same conclusion, and finally Russell decided it was time to state the obvious.

Payne remained a very active and highly productive astrophysicist, though as a woman in those days was severely limited in her freedom of problem choice and, working for Shapley, was subject to considerable direction. Even so, some thirty-five years after Struve wrote his initial review of her monograph, he recalled her work as "the most brilliant Ph.D. thesis ever written in astronomy.[23]

Russell was not "wrong," as some have informally suggested in recent years, to advise her in this manner. In his early career he too was rejected for making too grandiose conclusions on weak evidence --- He knew there were powerful reasons not to accept the conclusion at its face value, without an argument that would be iron clad. The abundance created problems for Eddington's standard model , which was built on very little hydrogen, and second, even though Saha's equations were now acceptable in Milne's rederived forms, the assumptions Payne had to use to exploit them for abundances were open to question. She knew this. But she presented her data and conclusions based upon them, cited Russell saying they certainly could not be right, received her PhD (which was the point of it all) and moved on. When Russell finally came to agree with Payne, in 1929, he only used her work as supporting evidence from astrophysical data. By then he was able to tap rudimentary curve of growth techniques, and based his argument squarely upon the physics of the atom. That was fully acceptable. Today, Payne is remembered for this work, and it was a revelation that stimulated a whole rethinking of the nature of stars.

3. Taking Risks

In her thesis, Payne went where her conclusions took her. But she knew when to back off. She was prudent not to risk her yet nascent reputation, yet she was wise to record the fact that she had played by the rules and followed the suggestions of those advising her. Nothing really was at stake other than her job and career. In distinction to the risks she took, which were personal, others took risks that were institutional and expensive. Let's look at the risks George Ellery Hale took to solve a major question: the reality of giant suns.

By the end of the second decade of the 20th century there was a lot riding on the notion that giant stars existed. The Russell theory of stellar evolution and its variants envisioned giant suns contracting to the main sequence. But what proof was there that stars radiated energy according to the laws of Wien, Stephan and Planck that predicted that stars of huge dimension existed? Russell's theory was based upon empirical evidence, augmented by indirect observational studies of eclipsing binaries by Harlow Shapley, his student, as well as the spectroscopic parallaxes of Adams and Kohlschűtter. Much in modern stellar astronomy depended upon stars behaving this way. Eddington keenly knew this, and said as much in his presidential address to the British Association in 1920:

> Probably the greatest need of stellar astronomy at the present day, in order to make sure that our theoretical deductions are starting on the right lines, is some means of measuring the apparent angular diameters of stars. At present we can calculate them approximately from theory, but there is no observational check. We believe we know with fair accuracy the apparent surface brightness corresponding to each spectral type; then all that is necessary is to divide the total apparent brightness by this surface brightness, and the result is the angular area subtended by the star.

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[23] Otto Struve, *Reviews [*C. H. Payne, *Stellar Atmospheres]. ApJ* **64** (1926) 204-208; Otto Struve and Velta Zebergs, *Astronomy of the Twentieth Century.* New York: Macmillan (1962), 220-221.

> The unknown distance is not involved, because surface brightness is independent of distance. Thus the estimation of the angular diameter of any star seems to be a very simple matter. For instance, the star with the greatest apparent diameter is almost certainly Betelgeuse, diameter 0.051".[24]

There was nothing new in Eddington's predictive technique; it was the same in principle as those used by Ejnar Hertzsprung over a decade earlier, and more recently, but quietly, by Russell. But no astronomer had Eddington's clout, nor could Eddington's timing have been better. He had known that George Ellery Hale had been directing his staff to build a huge device for the just completed 100-inch telescope based upon the designs of the physicist Albert Michelson. Eddington had been very excited about some preliminary tests of the interferometric technique on the double star Capella because they gave him all-important data on the physical characteristics of the two stars, data he needed to test his theory of stellar structure. Even more important would be direct measurements of dimensions.

The fact was that Hale had decided to build this expensive device, and occupy much of his best staff with its construction and testing, at a time when neither the 100-inch had been completed, or when acceptable predictions for the sizes of stars were available to them in a manner they would listen to or accept.

Michelson had wanted to make this sort of measurement since the 1890s as yet another application of his interferometric expertise. He approached astronomers knowing that his methods, in theory at least, could be used for measuring extremely small angles, or angular diameters. After a test at Harvard, and another at Yerkes, which were promising, he wrote to E S Holden at Lick proposing an interferometric mask for one of the Lick telescopes. Holden agreed to have a mask built for the 12-inch, and even after it demonstrated that it could match E. E. Barnard's keen eye on the 36-inch discerning the angular diameters of the Jovian satellites, Holden remained reluctant to utilize 36-inch observing time for such risky work. Holden assumed that the atmosphere would not allow such precise measurements, and anyway, his telescopes were not robust enough to hold a large interferometer, nor was this a project directly in line with what Holden considered traditional practice.

Hale, always a risk taker and a builder of mechanically much more robust telescopes, was more than happy to give Michelson's technique a try. Michelson was partly responsible for convincing the University of Chicago to hire Hale as one of those new practitioners of "astro-physics," and when Hale left Chicago for Southern California, remained in close touch with Michelson on many projects and plans. Even before the 100-inch was completed, Hale agreed to try out Michelson's ideas with this still unproven technique. His staff objected, especially W. S. Adams, but Hale was convinced that he had to do something with the 100-inch not possible with any other telescope on earth. So they built the 20-foot beam using the telescope at its broken Cassegrain focus configuration to provide maximum scale. Even as the beam was nearing completion in 1919, no one on Hale's staff had any idea what the angular diameters of stars should be.

Michelson advised Hale in 1919, as he had advised Holden in 1896, that one did not need complete fringe disappearance to be able to extrapolate the angular diameter of an object from the separation of the two interferometric elements. but Hale and his staff, including a young and presumably well trained Harlow Shapley, continued to worry that null readings required separations of over a 100 feet. So did Michelson. Yet Hale continued to urge the building of the beam.

Construction of the beam began in June, 1920 and by the middle of the summer, Michelson and Francis Pease made initial seeing tests and found stable fringe visibility over the entire length of the beam, which negated the fear that seeing would limit its effectiveness. Then, pondering Michelson's résumé on the stellar interferometer and its dour predictions for angular diameters, Hale picked up the September 2 issue of **Nature** to read what Eddington had to say to the British Association about the state

[24] A. S. Eddington, 1920. *The Internal Constitution of the Stars, Nature* **106** (1920), 14-20, on 17.

of astronomy. Halfway into the transcript of his speech, Hale must have let out a howl, and immediately summoned his staff. As he reported to Michelson on 4 October:

> Eddington's estimates of star diameters in his British Association address (Nature, Sept. 2) are so large that we are planning to measure Betelgeuse with the 100-inch as soon as possible. Dr. Anderson will make the trial very soon, and I will report the results to you.[25]

"As soon as possible" meant that the beam was not yet equipped with remote controls. The astronomers therefore devised a means to have a hapless night assistant perched on the beam during the night to manually move the interferometric mirrors, bit by bit. The result made headlines across the globe. Hale's risk had more than paid off. Sometimes risk is a good thing. Hale, of course, had a back-up plan. If the beam could not measure single angular diameters, he and Paul Merrill knew that it would work for close double stars. And in any event, Hale still had his telescope!

But this whole episode demonstrated more to Hale, if he was not aware of it before, and that was that American observatories were severely lacking in theoretical expertise and even the best and most capable observational staff members, like Russell's student Shapley who was then at Mount Wilson, were unable to properly apply basic photometric data to derive critical astrophysical quantities. This episode would help to convince Hale that the presence of people like Russell or Eddington as mentors to his observational staff were critical to the success of his observatory. As a result, Russell's professional position was greatly strengthened by Hale's patronage--he invited him to be in residence every summer for the next 20-odd years--which helped him become the pre-eminent theorist in American astrophysics.

4. The Astrophysicist's Test Bench

Hale grew up in the age of the great refractor but did not share the philosophy of those observatory directors who defined their programming in terms of the traditional and proven capabilities of refracting telescopes. Great refractors defined the capabilities of visual micrometry, spectroscopy and photography. Even in Hale's first observatories, the refractor was king. But as he courted patrons and worked to attract physicists to experiment with his telescopes, he expressed his view of what a modern observatory was all about. In 1905, trying to attract a young and energetic physicist to Mount Wilson, he described the place as follows:

> We have a (laboratory) shop much better than that of the Yerkes observatory already in operation, and when we move into our new building in a few weeks, we shall be still better equipped. If you enjoy work of development, under conditions that will enable us to build special apparatus, and use it under the finest conditions, I am sure the place would appeal to you.[26]

Hale's preoccupation with "special apparatus" required a test bench far more adaptable and stable, both thermally and kinematically, than the moving eye end of a telescope. Thinking more like a physicist, and like more than one Frenchman, Hale sought out means to create fixed focal plane telescopes focus telescopes to link the physics laboratory to the Sun and stars.

In the late 19th Century, Coudé telescopes were well known and very exciting, but beyond certain diameters, they were impracticable. Hale was planning huge mirrors like the 60-inch at the turn of the century, and sought to combine their light gathering power with a stable, controlled fixed focus under laboratory conditions. In the mid-1890s he met Arthur Couper Ranyard at meetings of the RAS; Ranyard had similar dreams of creating a fixed observing chamber fed by a moving equatorial telescope. Ranyard died in the mid 1890s, and his family sent his drawings to Hale. Hale's shop foreman, F. L. O. Wadsworth, suggested a critical modification to Ranyard's design, which envisioned a third mirror akin to a broken Cassegrain, but directing the light to the north tier of an English-style mounting. Wadsworth

[25] Hale to Michelson, 4 October 1920. Hale Papers Microfilm Edn. Caltech.
[26] Hale to Charles E. Mendenhall, 3 March 1905. Hale Papers Microfilm Edn. Caltech.

preferred using a fork form adopted by Common and by Rosse for his 36-inch speculum where the light goes down the polar axis to a lower level that can be fully temperature stabilized. The lower axis would carry all the drive mechanisms.

Hale's 60-inch eventually combined this Coudé focus with Cassegrain and Newtonian configurations, and in essence created 3 telescopes in one. Over time, the versatility of the 60-inch became a hallmark of astrophysical work. As Richard Jarrell has shown, the most common form of large reflector in the first half of the 20th century did not follow the multiple focus 60 inch design - that is because they were built to do normative science - mainly photography and spectroscopic radial velocity work -- competent, needed, but not revolutionary. The galactocentric revolution was started with Hale's 60-inch and culminated with the 100-inch. And designs for great reflectors of the latter half of the century embodied Hale's multiple foci.[27]

5. Fruitful Collaborations

True collaborative work, between astronomers and physicists, where physics becomes a guiding light rather than only a post-hoc explanation of phenomena or an application of new technique, can also aid discovery. Here we present a single example, where true synergistic collaborations finally broke down disciplinary barriers and led to the realization that red giants stars were not young stars but were old evolved stars.

In 1915, after listening to Alfred Fowler review theories of stellar evolution at a meeting of the British Association, Lord Rutherford mused:

> In listening to this discussion it seemed to me that astronomers may be proceeding too much on the assumption that the evolution only proceeds in one direction. That does not seem to be necessary. I see no reason why we should not have some stars condensing and others diverging. " I see no reason why the evolution should always proceed in the direction of condensation.[28]

No one paid any attention to this observation, not even when Eddington's theoretical mass-luminosity relation demolished Russell's giant to dwarf theory in the mid-1920s. Eddington's theoretical mass-luminosity relation, making sense at last of the observed relations noted by Hertzsprung, Hamy and other observers, established two very important things: Eddington's radiative standard model, that all stars behaved as perfect gases, and, as a result, that Russell's giant-to-dwarf theory had to be abandoned, or somehow deeply modified. It also established Eddington's dominance as a theorist, and hence established in turn what historians call the "Eddington era" in theoretical astrophysics.[29]

One of the primary characteristics of this era was disciplinary isolation. And as I argue elsewhere in greater detail, one of the reasons it took astronomy another three decades at least to accept the fact that giants are evolved stars was due to this iconoclasm.[30] For example, unraveling the nature of the stellar energy source was the most significant puzzle to solve for astronomers in the teens and twenties and thirties; very few if any tried their hand, and fewer still tried to collaborate with physicists. Robert Atkinson was the sole exception, with Fritz Houtermanns.[31]

[27] Richard Jarrell, *J. S. Plaskett and the modern large reflecting telescope, Journal for the History of Astronomy* xxx (1999) 359-390 (1999); David DeVorkin, *In the Grip of the Big Telescope Age, Experimental Astronomy* 25 (2009), 63-77.

[28] Rutherford's comments were made during the discussion following Alfred Fowler, *Spectral Classification of Stars and the Order of Stellar Evolution, The Observatory* xxxviii (1915), 379-392; on 390.

[29] There is a solid literature on Eddington, most recently and competently represented in Matthew Stanley, *Practical Mystic: Religion, Science and A. S. Eddington*. Chicago: 2007.

[30] David H. DeVorkin, *The changing place of red giant stars in the evolutionary process. Journal for the History of Astronomy* xxxvii Part 4, No. 129 (2006), 429-469.

[31] Karl Hufbauer, *Stellar structure and evolution, 1924 - 1939, Journal for the History of Astronomy,* xxxvii Part 2, No. 127, (2006), 203-227.

Eddington felt that the most significant thing physicists could do for astronomy would be to improve understanding of the transmutation of the elements and how it liberated energy - new knowledge, he claimed in his Presidential report for Commission 35 report, "for which," Eddington noted prophetically in 1935, "astronomers have long been waiting,"[32] His wish came true in only three years with the contributions of physicists Bethe and von Weizsäcker in 1938. Now, however, with an actual nuclear process at hand that provided energy, there was great stimulus to star builders, and the field also attracted more physicists. One of the first theoretical nuclear physicists to become intrigued with the problem was George Gamow, brilliant and irascible. On the heels of Bethe and Weizsäcker's dramatic work in 1938, Gamow began applying the theory to stellar evolution, and adopted a new concept of shell burning to argue that stars with exhausted hydrogen cores would expand. There had been observational evidence to this effect, starting with B. Strömgren's work earlier in the decade, and continuing with cluster diagrams by G. Kuiper interpreted by Strömgren, but it was Gamow who created the first models. Hoping to compare his theoretical tracks with observations, he pleaded for data on stellar masses, luminosities and radii in 1939, sending lovely hand-sketched evolutionary diagrams. Unfortunately, astronomers like Walter Adams were unimpressed.

A young theorist from India, S. Chandrasekhar, was also at Yerkes and eagerly read Gamow's first papers. He had been brought to Yerkes by Otto Struve, who was convinced that placing good theorists in the observationalist's lair provided the best possible combination for synergistic programming. Chandra had already been keenly interested in hydrogen abundances and stellar evolution as interpreted by cluster diagrams. In 1936, after an early colloquium, Struve reported to Kuiper that:

> Chandra discussed the problem of the hydrogen content of clusters in one of his colloquia at the Yerkes Observatory, and I was very much impressed with the brilliant character of his presentation, with the remarkable results that you [Kuiper] have obtained in explaining Trumpler's diagrams and with the importance of Strömgren's early work on the subject.[33]

In 1939, however, Chandra immediately challenged Gamow, saying his shell burning violated numerous formal conditions derived from stellar structure theory. A strict mathematical theorist, Chandra was annoyed with Gamow's approximations and tomfoolery. Even so, Chandra's closest patron at Yerkes, Gerard Kuiper, was obviously interested in this issue, and so was delighted with the interaction, writing to Gamow:

> As I understand that you have had correspondence with Chandrasekhar about the physical processes of the energy generation, I will not touch upon that part of your letter, only [to] say that it is fortunate that physicists and astronomers have found a new field of common interest.[34]

Gamow did not share Kuiper's hopeful attitude; feeling rather far from a common bond, he replied with a sarcastic cartoon to make his point, that physical theory always trumped astrophysical theory, and both trumped astrophysical data.[35]

True collaboration on the problem, where all sides respected each other, began only in the early 1950s when Martin Schwarzschild and Walter Baade teamed up. Schwarzschild was a bit reluctant at first; in the mid-1940s, fearing to get between Gamow and Chandra, as he advised Geoff Keller that "if giants like Chandrasekhar and Gamow fight, then dwarfs like you and me don't go into their field."[36] Still, with Baade's encouragement, and Baade's boys, including William Baum, Allan Sandage and B.

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[32] A. S. Eddington, *Report of Commission on the Constitution of the Stars, Trans. IAU* **v** 1935, 238.

[33] Struve to Kuiper, 21 March 1936, Kuiper Papers, Box 2. University of Arizona Gerard Kuiper Papers (GPK/UA).

[34] Kuiper to Gamow, 10 October 1938, Kuiper Papers, Box 2. GPK/UA.

[35] Cartoon reproduced in DeVorkin (2006).

[36] Schwarzschild oral history 20 April 1983 (rough draft) 18--19. Space Astronomy Oral History Project, National Air and Space Museum. See also Schwarzschild oral history #III, 16 December 1977, 87-88. Sources for History of Modern Astronomy (SHMA), American Institute of Physics.

Oke starting to produce remarkably detailed cluster diagrams with photoelectrically calibrated photometry reaching below the turnoff points of a range of clusters, the way looked clear. Observed cluster diagrams beyond the turnoff were not reconciled until Schwarzschild teamed up with Fred Hoyle, the already well-known physicist from St. John's College, Cambridge who, invited by Baade and Schwarzschild, was on an extended research and lecturing tour in the United States focused on collaborations at Caltech and Princeton.[37] Further collaborations grew from this exciting atmosphere at Caltech and Princeton and came to mark the center of a whole new specialty, nuclear astrophysics.

6. Turning Points

The last example raises what is probably the most important factor leading to accelerating the rate of discovery in astronomy. Cross-fertilization and collaboration, not only in the migration of, or accretion of, theoretical or methodological disciplinary specialties to address problems in astronomy: atomic structure, quantum theory, nuclear and particle physics, etc., but fields that have created new tools for perceiving the universe. This is possibly the most well-known means of promoting discovery, one that Martin Harwit has effectively presented in his landmark 1981 book "Cosmic Discovery" and so only needs cursory comment here. Harwit identified five turning points in all of the history of astronomy, all based upon the application of new technologies.

1. the telescope and the detection of the physical universe
2. the detection of cosmic rays
3. the detection of the radio universe
4. the detection of the x-ray universe
5. the detection of the gamma ray universe

Historians of course, would want to flesh this out considerably, adding the infrared in 1800 and the ultraviolet in the 1850s, and asking all sorts of questions, like who developed and applied those new techniques, and what were their motivations? And as each new cohort migrated into astronomy, especially people like radio engineers and high-energy physicists, how did the discipline of astronomy, and its institutions change? One only needs to look briefly at how the American Astronomical Society changed in structure over the past 50 years to appreciate why these questions are important.[38]

But Martin's basic point remains, new tools reveal new universes. He also identified trends common to many discoveries in astronomy, only the first being technological. Historians have been concerned that Martin's views oversimplify history, making it seem too linear and causal, and certainly there are issues one still needs to address. Some astronomers narrowly define discovery, and even argue that the expanding universe was not discovered at Mount Wilson. It is true that Hubble's techniques and instruments were not revolutionary. But in fact, because of the efforts Hale took to provide very high light-gathering power and new levels of versatility, Hubble's techniques were afforded new powers of efficiency and discrimination. Thus the hints provided by Slipher and the powerful context created by Lemaitre saw their fullest expression at Mount Wilson, even if Hubble at first was reluctant to interpret his red shifts as velocities.

In sum, reflecting on these case studies, and the larger literature cited here, lets us now revisit the questions posed in the abstract to this paper, making a few general observations and suggestions that hopefully will be worthy of discussion and debate during this special session and in the future, between astronomers, between historians, and, hopefully, across the two disciplinary boundaries:

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[37] Simon Mitton, *Conflict in the cosmos: Fred Hoyle's life in science*. Washington, D.C.: Joseph Henry Press (2005), chapters 6 – 8; see esp. 162-164.

[38] David O. Edge, and Michael J. Mulkay, *Astronomy transformed : the emergence of radio astronomy in Britain*. New York: Wiley, 1976; Richard F. Hirsh, *Glimpsing an invisible universe: the emergence of X-ray astronomy*. Cambridge [Cambridgeshire]; New York: Cambridge University Press, 1983; David DeVorkin, ed. *The American Astronomical Society's first century*. New York, N.Y. : Published for the American Astronomical Society through the American Institute of Physics, [Washington, D.C.], 1999.

First, is there an optimum balance between risk and innovation, such as building upon proven technologies or experimenting with new unproven technologies? One sees this choice continually, most recently in competing efforts in the United States to build the next generation of monster ground-based optical telescopes.[39] As with all questions of this type, what "optimum " really means is elusive. A robust science must employ proven technologies for routine work, or data gathering under unusual conditions where the instrument has to be faithful. But science also needs innovation, and innovation is risky.

Next, once again reflecting on Harwit's *Cosmic Discovery,* we need also consider gaps between first detection of a new phenomena, or a new class of object, and its collective comprehension by astronomers - sometimes those gaps can be considerable. Often they are periods of reorientation. Is there an optimum to strive for? Was Pluto really discovered "before its time" and would its status as progenitor of a major new class of object in the solar system, or zone, have been easier to accept if another body like it had been discovered soon after? If one returns to popular musing about the significance of Pluto in the press at the time of discovery, one sees more than one voice asking if indeed Pluto is the first of a new breed.[40]

Third: Have astronomers been most effective as tool builders or as cross-disciplinary entrepreneurs, acquiring expertise in emerging areas of physics, or have they been most effective when they manage to attract this talent and expertise from the new disciplines of physics?[41] Those who wish to explore this question, however, need to be aware of the personal choices scientists make, and reasons why they do so. In a retrospective essay in 1989, Leo Goldberg reflected on this question, hinting at its complexity:

> The growth of astronomy has always been inseparable from developments in experimental and theoretical physics. In most cases, however, discoveries in physics have been applied to astronomy by physicists, some of whom, like Prof. [V. L.] Ginzburg, Bengt Edlén, Bruno Rossi and Hans Bethe, have retained their identity as physicists, while others, for example S. Chandrasekhar, E. E. Salpeter, M. Ryle and R. Giacconi have chosen to become affiliated with astronomy.[42]

Fourth: Does the degree of correlation between the motives and goals of patrons of astronomy and the goals of astronomers influence the quality and nature of subsequent knowledge production? My historical work has led me to argue that the degree of correlation is highly important in the space sciences, but not the only factor when one considers the scale of operations attainable. The old personal and even the corporate philanthropic modes enjoyed by Pickering and Hale were pretty open-ended and hands-off compared to the mission-oriented style experienced today.

And finally, it is always valuable to revisit the perennial question of what produces a better product or more robust discipline: elite-based meritocratic reward structures or egalitarian social, institutional and national reward structures? Patrick McCray and others continue to explore such questions, looking at succeeding generations of astronomers, and it is hoped that in doing so, we may someday come to a better appreciation of if and how social standards and practices influence the cultural phenomenon we know as astronomy.[43]

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[39] Yudhijit Bhattacharjee. *Race for the Heavens, Science* **326** (23 October 2009), 512-515.

[40] See for instance, F. C. Leonard, *The New Planet Pluto, Astronomical Society of the Pacific Leaflets*, **1** (1930) 121-129.

[41] Leo Goldberg, *Atomic spectroscopy and astrophysics, Physics Today* **41 (**Aug. 1988), 38-45.

[42] Leo Goldberg, *Quantum Mechanics at the Harvard Observatory in the 1930s*, in *Problems in Theoretical Physics and Astrophysics: A Collection of Essays dedicated to V. L. Ginzburg on his 70th Birthday*. Moscow (1989), 21--22. Karl Hufbauer (1991) also makes this point in Part 1 of his *Exploring the Sun: Solar Science Since Galileo.*

[43] See for instance, W. Patrick McCray, *The Contentious Role of a National Observatory, Physics Today* **56** (2003) 55-58; and William E. Howard, Cameron Reed, and Patrick McCray, letters. *National Observatories: Contention Continues, Physics Today* **57** (2004), 13-14.

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[2] Katherine Heramundanis, ed., *Cecilia Payne-Gaposchkin an Autobiography and Other Recollections*, Cambridge: Cambridge U. Pr. (1984).

[3] Martin Harwit, *Cosmic Discovery. The search, scope, and heritage of astronomy. Cambridge: MIT Press, 1984*.

[4] Karl Hufbauer,, *Exploring the Sun: Solar Science Since Galileo. Baltimore: Johns Hopkins, 1991*.

[5] David H. DeVorkin. *Henry Norris Russell: Dean of American Astronomers. Princeton, 2000*.