First Mover: Otto Struve and The Use of Scientific Capital in Astrophysics, 1921–1950

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FIRST MOVER: OTTO STRUVE AND THE USE OF SCIENTIFIC CAPITAL IN ASTROPHYSICS, 1921-1950

A Thesis

by

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Submitted to the Graduate School of the
University of Texas-Pan American
In partial fulfillment of the requirements for the degree of

MASTER OF ARTS

DECEMBER 2011

Major Subject: History

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ASTROPHYSICS, 1921-1950

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December 2011

ABSTRACT

Norquest, Erik P., First Mover: Otto Struve and the Use of Scientific Capital in Astrophysics, 1921-1950. Master of Arts (MA), December, 2011, 89 pp.

Otto Struve (1897-1963) came to the United States in 1921 and became one of the dominant personalities in the field of astrophysics. Struve's career made him, in the words of sociologist Pierre Bourdieu, a "first mover" in a scientific field that was firmly engaged in the process of what Thomas Kuhn called "normal science." Struve pulled astrophysics further away from its empirical roots in categorization and made it more like physics in its unification of theory and observation. The primary way that he accomplished this was through his administration of Yerkes and McDonald observatories, where he brought in theorists to work directly with observers. He also helped to set barriers for entry into the field that emphasized greater knowledge of mathematics, physics, and theory. He also influenced the field through his editorship of *The Astrophysical Journal*, and through his own prolific publication of both scholarly and popular articles.

DEDICATION

I would like to dedicate this thesis to my wife, Joanna Waggoner-Norquest, and my parents, Neil and Virginia Norquest. Their wholehearted support and encouragement throughout my graduate studies has been invaluable to me. Thank you for your support and patience. I love you all.

ACKNOWLEDGEMENTS

I am grateful to everyone who has helped me in the research and writing of this thesis. Particularly Dr. Amy Hay, the chair of my thesis committee, who has mentored me through this process, and who has given me such great advice on the content of my work and encouragement in my development as a historian. Dr. Christopher Miller, also on my thesis committee has also shared many of his valuable insights during this process and helped to make my thesis a work of real scholarship. I would also like to thank Dr. David DeVorkin, who was my mentor at the 2010 History of Science Society Meeting and whose feedback on the history of astrophysics and astronomy has been incredibly valuable in helping me to understand the field as a whole. Dr. DeVorkin also helped me to acquire a set of microfilm of Otto Struve's correspondence without which completing my research would have been near impossible. My deepest thanks goes out to all of them for their feedback and help throughout this process.

I would also like to thank the librarians and archivists at the University of Chicago Special Collections Research Center who helped me to find archival material from the director's office of the Yerkes Observatory, as well as material from the editor's office of *The Astrophysical Journal*. Additionally, I would like to thank everyone who contributed financially to make possible the research and completion of this thesis, including the National Science Foundation, the UTPA College of Art & Humanities, the UTPA Department of History & Philosophy, and the members of the board of the Paul & Florence Crissman Scholarship. Without their support, researching and writing this thesis would have been impossible.

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INTRODUCTION

Otto Struve (1897-1963) came to the United States in 1921 and became one of the dominant personalities in the field of astrophysics. The study that follows is an attempt to trace Struve's career and the peculiar contingencies that made him, in the words of sociologist Pierre Bourdieu, a "first mover" in a scientific field that was firmly engaged in the process of what Thomas Kuhn called "normal science." Struve pulled astrophysics further away from its empirical roots in categorization and made it more like physics in its unification of theory and observation. The primary way that he accomplished this was through his administration of Yerkes and McDonald observatories, where he brought in theorists to work directly with observers. He also helped to set barriers for entry into the field that emphasized greater knowledge of mathematics, physics, and theory. He also influenced the field through his editorship of *The Astrophysical Journal*, and through his own prolific publication of both scholarly and popular articles.

As the use of the term "first mover" implies, the narrative lens through which this study of Otto Struve's career will be viewed is based on the theoretical considerations synthesized by French sociologist Pierre Bourdieu. In analyzing how power and influence unfold and are channeled within a scientific community. Bourdieu uses the analogy of "scientific capital," equating exchanges within science with economic exchanges, suggesting that such scientific capital follows similar rules in terms of accumulation and distribution as does financial capital.[1]

According to this theory, the accumulation of scientific capital allows those who possess it to shape the contours of the field according to their own beliefs. It also gives them power over those who possess less of it, and lets them set the barriers of entry into the field. Those who possess the most capital are dominant agents or "first movers." First movers enjoy the privilege of initiative in that they can set the rules of the game and therefore shift the field so that it is more in line with their own interests. Henry Norris Russell, a Princeton-based astronomer who was seen as the dean of American astrophysics in the years before Struve's ascendency, was one such first mover in astrophysics. He made himself into an obligatory reference point for all others in the field, so that he was not only referenced often in the scientific literature, but was also often asked for his opinion when others were filling staff positions or considering other changes to their own institutions.[2]

Bourdieu's model is particularly useful in examining the career of Otto Struve. Struve's actions during his rise and after he became one of the dominant agents in astrophysics fit into a better narrative using the Bourdieu model than they would in using another, such as Thomas Kuhn's narrative of scientific revolutions. Kuhn's theories are useful in discussing those scientists who radically changed, for lack of a better term, the intellectual content of a particular science. His cycle of normal science, to crisis, to revolution, and back to normal science can help to shape an understanding of how concepts under discussion by scientists change, but it is less useful for a case like the career of Struve. Struve's career took place during a period of what Kuhn would probably call normal science, and yet he was able to change practice in the field of

[1] Pierre Bourdieu, *Science of Science and Reflexivity*, trans. Richard Nice (Cambridge: The University of Chicago Press, 2004), 35–36, 55–57.
[2] Ibid., 35–36.

astrophysics due to his influence as an observatory director and through the nature of some of his own research. Therefore a model like Bourdieu's, which emphasizes the power relationships within the field and how they effect the practice of the field is much more useful in studying Struve. Struve, of course, was not conscious of the fact that he was acquiring and using scientific capital, but it is doubtless that he was ambitious and that he had very definite ideas about the way that observatories and the field of astrophysics in general should be organized.[3]

Bourdieu also points out than in using his theory of scientific capital one needs to examine the values of the field under study in order to understand their impact on the amount of scientific capital that an individual or institution possesses.[4] For this, it is useful to look at John Lankford's study of careers in astronomy in the United States from 1859 to 1940. Lankford created a parameter of analysis called institutional potential that he used to separate American observatories into tiers. He based institutional potential on the quality of an observatory's instruments, the quality of its staff, the ability to provide assistants for research, the presence of a good library, the ability to secure adequate funding, and the ability to grant time to the observatory staff to work on significant research programs. In his top two tiers of institutional potential, Lankford placed the Mount Wilson Observatory, the Dudley Observatory, the Harvard College Observatory, Yerkes Observatory, and the Princeton University Observatory. Struve became director of Yerkes in 1932, as well as its companion observatory, McDonald, which was completed in 1939. In this discussion of Struve's career and his impact on astrophysics, Yerkes, Mount Wilson, Harvard, Princeton, and the Lick Observatory (associated with the University of California and part of Lankford's third tier of institutional potential) will be the institutions most closely examined, as they produced the majority of research in astrophysics during this time

[3] Thomas Kuhn, *The Structure of Scientific Revolutions*, 3rd ed. (Chicago: University of Chicago Press, 1996), 1–9.
[4] Bourdieu, *Science of Science and Reflexivity,* 34.

period. The Dudley Observatory did not do much research in astrophysics, and was not mentioned very often in the primary literature. Following Bourdieu's model and investigating the values of the field therefore leads us to these five primary institutions as holders of scientific capital and shapers of the field.[5]

In his study, Lankford also examined the position of observatory director and found that, during this time period, observatory directors held almost absolute power in shaping the research of the institutions under their direction and, for those at the most successful institutions, in shaping the field as a whole. Before World War II, observatory directors were able to control and guarantee resources, define the research of the observatory staff, and were given a great deal of respect and status within the field. Additionally, these directors often held their positions for long periods of time. This, then, helps to explain why using Bourdieu's idea of scientific capital is particularly useful in the case of observatory directors. Scientific capital was concentrated in the person of the director.[6]

It must be said that while Bourdieu's model is of great use in examining Struve's career, there is not intent to suggest that Struve was in any way conscious of engaging in such "businesslike" behavior or that his decision-making processes were predicated on such accumulative and distributive reasoning. Rather, Bourdieu's structure for analyzing the business of science simply provides a structure and language that can make narrative sense out of Struve's meteoric rise in the astrophysical world and virtually unprecedented influence on the evolution of that world. Of course other models might have been used the same end, but Struve's actions as he accumulated scientific capital and became an observatory director fit well into a narrative framed by Bourdieu's theories much better than they would in using, for example, Thomas

[5] John Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940* (Chicago: University of Chicago Press, 1997), 129–131.
[6] Ibid., 168.

Kuhn's model of scientific paradigms and revolutions. Kuhn's model has much more to do with how the concepts or content of a science change through time, and less to do with how individual actors find their way through the power dynamics of a field over the course of their careers. Kuhn's model is concerned with the way that scientific ideas develop through his cycle of normal science, crisis, revolution, and normal science again. The key to Kuhn's model is the revolution, and given that Struve's career took place during a period of what Kuhn would call "normal science," Kuhn does not have much to say about Struve's influence on the field as a whole.[7] Therefore a model like Bourdieu's, which examines power relationships and how those might influence the practice of a field, can give a better framework for studying a scientific career.

As to the field itself, astrophysics is the study of a star's spectrum to determine its physical characteristics. The field dates back to Gustav Kirchoff's 1859 discovery that the chemical elements that made up a gas could be determined by examining the spectrum of light that passed through it. Upon making this discovery, Kirchoff realized that it could be used to determine what chemical elements make up the sun, and the new branch of astronomy called astrophysics was born. Initially astrophysics developed as a part of classical astronomy, which was concerned with the position and motion of stars. Soon, however, astrophysicists began to separate stars into classes according to their spectra and hypothesize about what those spectra meant in terms of the physical nature and life cycle of stars. Later astrophysics would also contribute greatly to cosmology, the understanding of the structure and nature of the universe,

[7] Kuhn, *The Structure of Scientific Revolutions,* 1–9.

but Struve's work and the work of those under him mostly focused on the physical characteristics, behavior, and evolution of stars.[8]

When Struve entered the field of astrophysics in 1921, it was already entering what Kuhn would have called its normal science phase. The major classification efforts were mostly complete, though they were still being refined, and theorists like Arthur Stanley Eddington at Cambridge University and Henry Norris Russell at Princeton University had worked out fairly firm theories on the physical characteristics and behavior of stars. There was still much work left to do in order to explain the ways that stars behaved and evolved according to their spectra, but there would be no revolutions, in Kuhn's sense, coming during Struve's career.[9]

Struve's early career found him doing observational work based on Eddington's and Russell's theories.[10] Russell in particular believed that astrophysics should become more like physics in its problem-oriented approach to increasing scientific knowledge, and his beliefs fell directly in line with Struve's. During this period he also built up a network of contacts with other dominant agents in astrophysics including Eddington, Russell, and the directors of the major observatories in the United States.[11] The U.S. had come to have the best observatories in the world due to significant private patronage and locations with superior viewing conditions to

[8] Sungook Hong, "Theories and Experiments on Radiation from Thomas Young to X Rays," in *The Cambridge History of Science, Volume 5: The Modern Physical and Mathematical Sciences*, ed. Mary Jo Nye (New York: Cambridge University Press, 2003), 272-288; A.J. Meadows, "The Origins of Astrophysics," in *The General History of Astronomy: Volume 4, Astrophysics and Twentieth-Century Astronomy to 1950: Part A*, ed. Owen Gingerich (Cambridge, U.K.: Cambridge University Press, 1984), 3-15.
[9] Matthew Stanley, "So Simple a Thing as a Star: the Eddington-Jeans Debate Over Astrophysical Phenomenology," *British Journal for the History of Science* 40, no. 1 (March 2007): 53-82; David H. DeVorkin, *Henry Norris Russell: Dean of American Astronomers* (Princeton University Press, 2000), passim.
[10] Examples include Otto Struve, "Interstellar Calcium," *The Astrophysical Journal* 65 (April 1, 1927): 163-199; Otto Struve, "Further Work on Interstellar Calcium," *The Astrophysical Journal* 67 (June 1, 1928): 353-390; B. P. Gerasimovic and O. Struve, "Physical Properties of a Gaseous Substratum in the Galaxy," *The Astrophysical Journal* 69 (January 1, 1929): 7.
[11] Donald E. Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 1999th ed. (Chicago, Ill: University of Chicago Press, 1999), 86–92.

those found in Europe.[12] However, there were few theorists working in America outside of Russell.

Historians have worked hard to uncover and examine the history of astrophysics during the last thirty years. Several volumes have been of particular use in writing this thesis. Donald E. Osterbrock's monograph *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution* has been of particular help. Osterbrock used many of the same primary sources found in this thesis to document the history of Yerkes from its founding by George Ellery Hale until Struve's resignation. Osterbrock trained as an astronomer at Yerkes at the very end of Struve's tenure there, and continued on to become director of the Lick Observatory in California. *Yerkes Observatory* is extensively researched and has excellent end notes, though Osterbrock does not attempt much beyond giving a blow-by-blow account of the happenings at the observatory. He also occasionally allows his own biases to slip into the text in the name of keeping his narrative of the near death of the observatory alive. He is particularly hard on Edwin Frost, who Struve thought had allowed the Yerkes staff to deteriorate, but who was himself a notable and respected scientist. Additionally, Osterbrock seems to sometimes recall events from his own memory or conversations with others without properly citing them as such. This is rare, however. The book therefore makes an excellent and useful chronology, but does not extensively overlap with this thesis due to its lack of analysis.[13]

David S. Evans and J. Derral Mulholland's *Big and Bright: A History of McDonald Observatory* is a brief history of McDonald from the death of W. J. McDonald himself to the end of the agreement with the University of Chicago. The book is useful particularly in documenting McDonald's life and death and how the observatory came to exist. However it is not very well

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[12] Meadows, "The Origins of Astrophysics."
[13] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*.

cited and has no central thesis. It is unclear from the text itself, but the book seems to have been written in preparation for the fiftieth anniversary of the opening of the McDonald Observatory. The authors are two astronomers who have done an able job in briefly documenting the history of the observatory, but who do not provide any sort of critical or historical analysis.[14]

John Lankford's sociological study, *American Astronomy: Community, Careers, and Power, 1859-1940*, previously mentioned, has been of great use in writing this thesis. Lankford researched the career paths of every member of the American astronomical community from 1859 to 1940, and he provides insightful analysis of the power structure of the community and the ways that careers tended to develop over time. Lankford uses methods of quantitative analysis to examine many facets of the education and subsequent employment of astronomers in America, including the women employed as computers. Of particular use to the current thesis have been his examinations of the power of observatory directors and the way that they used that power. Occasionally Lankford draws quantitative conclusions from sample sizes that seem very small, but he does an excellent job at fleshing out his examples of various types of careers and of the power dynamics of the field in general.[15]

The work of David DeVorkin has also been invaluable in helping to situate Struve's career in its proper historical context. His many articles on the history of astrophysics have been uniformly excellent, but of particular use has been his biography of Russell, *Henry Norris Russell: Dean of American Astronomers*. The book is thoroughly researched and cited, and there is some overlap in the sources DeVorkin uses and those used in this thesis. DeVorkin examines the career of Russell and looks at his place within astrophysics and the impact that he had on the field. Since Russell was a predecessor and contemporary of Struve, and since Struve largely

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[14] David S. Evans and J. Derral Mulholland, *Big and Bright : A History of the McDonald Observatory*, 1st ed. (Univ of Texas Pr, 1986).
[15] Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*.

agreed with Russell's views on the correct practice of astrophysics, DeVorkin's book serves as a sort of immediate antecedent to this thesis. In some ways Struve picked up where Russell left off, though the men had very different careers. Russell was primarily a theorist while Struve was primarily an observer with little theoretical training, and Russell accumulated scientific capital primarily through his own innovative research efforts while Struve primarily accumulated his through his administrative responsibilities. DeVorkin recognized this dynamic and specifically comments on it in the Russell biography, noting that Struve was attempting to mix theorists and observers at Yerkes and McDonald.[16]

Matthew Stanley's article "So Simple a Thing as a Star: The Eddigton-Jeans Debate Over Astrophysical Phenomenology," also serves as an antecedent of a sort to this thesis. Historically speaking, Stanley's article concerns the period of astrophysics before Struve's career and addresses a debate between Eddington and mathematician James Jeans over the correct practice of theoretical astrophysics. Although the exact practice of astrophysical theory is not addressed in this thesis, Stanley does an excellent job of providing a historical analysis of two scientists arguing over the correct way to practice their science, and consciously choosing what the nature of their field will be. In that way it serves as a model for an analysis of Struve's career. Even though it is unclear if Struve ever engaged in the type of public argument that Eddington and Jeans did, Struve still clearly and consciously chose what he thought was the right way of practicing astrophysics and applied it to the observatories that he directed.[17]

Finally, here is a brief breakdown of how this thesis is organized. Chapter I is a brief history of astrophysics from 1859 to the 1930s, when Struve became a dominant agent in the field. It includes discussions of how astrophysics broke away from classical astronomy, how

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[16] DeVorkin, *Henry Norris Russell: Dean of American Astronomers.*
[17] Stanley, "So Simple a Thing as a Star: the Eddington-Jeans Debate Over Astrophysical Phenomenology."

classification efforts evolved, how theoretical astrophysics developed, and the development of observatories, particularly in the United States. Chapter II is a biography of Struve and an analysis of his career up until he became director of Yerkes and McDonald. It includes how Struve ended up in the U.S., the research that he focused on in his early career, the influence of Russell's ideas, his ascension to director, and the engineering of the agreement with the University of Texas. Chapter III is a discussion of the ways that Struve accumulated scientific capital including the construction of McDonald Observatory, his hiring of a well-regarded staff, his duties as editor of *The Astrophysical Journal*, and his directorial style. Chapter IV examines how Struve used his scientific capital to move the field by fostering an environment where theorists and observers worked together. It also covers the nature of the work done under Struve's directorship, and Struve's influence on the barriers of entry in astrophysics. Finally, a brief conclusion discusses the changes in astrophysics after Struve's death including the decline of the observatory director as a major center of power and the expansion of astrophysics into areas outside the visible light spectrum, as well as a discussion of areas of further study in the history of astrophysics.

This thesis serves as a continuation of DeVorkin's, and to a lesser extent Stanley's, work by examining the dynamics of the field of astrophysics after the apex of Russell's career and into the middle of the twentieth century. It also gives a fuller account of an individual career in astronomy than Lankford could print when he discussed astronomical careers as a whole, and therefore serves as an individual example of the general patterns that he studied. It therefore expands on previous historical studies while also contributing to a deeper understanding of how the actions of individual scientists shaped the field of astrophysics as a whole. Further research will help to illuminate more details about the relationships between observatory directors and how those relationships helped to shape the field, in addition to providing more information on the individual uses of power by observatory directors during this period.

CHAPTER I

In 1859, Gustav Kirchoff made a discovery that changed the future of astronomy. Scientists had previously been aware that when light passed through a gas and was then split using a prism the resulting spectrum would contain dark lines, but knew neither what caused the lines, nor what the lines signified. Kirchoff determined that each chemical element produced lines that were unique, and that the composition of a gas could therefore be determined by examining the spectral lines that it created. Kirchoff also realized that this could be applied to the spectrum of the sun to determine the composition of the solar atmosphere. This new application signified the beginning of a new branch of astronomy: astrophysics.[18]

Astrophysics was the science of the study of stellar spectra, and it initially developed as a companion to traditional astronomy, which concerned itself with astrometry and celestial mechanics – the study of the positions and motions of the stars, planets, and other celestial objects. Astrophysics was initially used to further these endeavors, as well as to develop catalogues that organized stars into categories according to the similarities of their spectra. As astrophysics developed through the end of the nineteenth and into the twentieth century, its practitioners remained loyal to these empiricist roots, and only in the 1920s and 1930s did astrophysicists begin to move towards a problem-oriented approach that more closely resembled physics in its attempts at explanation, rather than simply observation, of phenomena.[19]

[18] Hong, "Theories and Experiments on Radiation from Thomas Young to X Rays."
[19] Meadows, "The Origins of Astrophysics," 3.

However, at this time celestial mechanics was the most prestigious branch of astronomy. During the eighteenth and nineteenth centuries, celestial mechanics had developed rapidly and been perfected by French mathematical astronomers led by Pierre Simon de Laplace. Celestial mechanicians created more and more complex theoretical models of the orbits of solar system objects and then compared them to telescopic observations. Thomas Kuhn would have said the field was firmly embedded in the "normal science" phase of its life cycle.[20] The field had a well-established theoretical grounding, and the intense mathematics of the process lent celestial mechanics much of its prestige. As Langley wrote, classical astronomy had as its object "to say *where* any heavenly body is, and not *what* it is."[21]

Because classical astronomy was so well developed, with little in the way of major discoveries that could revolutionize the field, classical astronomers took a long-term view of their science and did not expect any new or foundation-shaking discoveries to arise from it. J. S. Plaskett, director of Dominion Observatory in Canada described astronomy as "a science which requires long continued and systematic investigations to be carried through with faithfulness, unselfishness, and untiring perseverance before any definite results can be attained." He went on to illustrate the moral view of positional astronomy, which was held by many of its practitioners, and gave "honor" to "astronomers of the past, who spent their lives making observations of which they themselves could not hope to reap any fruit," and further praising current astronomers for "unselfishly collecting data which only a future generation [could] use."[22] In a science in which precision was given the highest value, only better instruments and more data

[20] Kuhn, *The Structure of Scientific Revolutions*, 23–34.
[21] Samuel Pierpont Langley, *The New Astronomy* (Boston: Ticknor and Company, 1888), 3; Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*, 26–27.
[22] J.S. Plaskett, "Some Recent Interesting Developments in Astronomy," as found in Smithsonian Institution. Board of Regents, *Annual report of the Board of Regents of the Smithsonian Institution* (The Smithsonian Institution, 1912), 256.

could advance the complicated mathematical models that had become the driving force behind astronomers' actions.

In contrast, spectral analysis suddenly opened up whole new vistas of information that promised new discoveries at every turn. As William Wallace Campbell, director of Lick Observatory from 1900 to 1930 said, "Kirchoff's immortal discovery of the fundamental principles of spectrum analysis opened a gateway which many were eager to enter."[23] Kirchoff started off studying the sun, and soon others also began to apply the new techniques of spectral analysis to solar observation. Astronomers were surprised to discover that many of the elements that made up the sun and other stars were the same as those found here on Earth. Samuel P. Langley, a solar physicist at the Allegheny Observatory in Pennsylvania and later secretary of the Smithsonian Institution, expressed some of the excitement surrounding the new branch of astronomy when he wrote his book *The New Astronomy* in 1888, which was aimed at a popular audience.

> You and I are not only like each other, and brothers in humanity, but children of the sun and stars in a more literal sense, having bodies actually made in large part of the same things that make Sirius and Aldebaran. They and we are near relatives.[24]

Still, there was little theory on which to build a physical understanding of the new data, but that meant that astrophysicists were given some leeway to speculate about their discoveries in the new science. Kirchoff, for his part, used his study of the sun to advance a theory that the sun was a liquid core surrounded by a gaseous envelope. Though other astronomers later disproved this theory, the fact that Kirchoff had proposed such an ambitious theory based on little evidence went against everything that astronomy had become during the nineteenth

[23] W.W. Campbell, "An Address on Astrophysics," *The Popular Science Monthly*, February 1905, 298.
[24] Langley, *The New Astronomy*, 222.

century.[25] Practitioners of the old astronomy frowned upon this however, as exactly what caused the differences in spectra between stars was not understood. In their minds, astrophysics suffered in comparison to the older science because classical astronomy was already so grounded in theory.[26] As a result, many leading astronomers were unhappy with astrophysics. Otto W. Struve, director of the Pulkovo Observatory in Russia, warned of straying too far from the traditions of astronomy:

> As yet, astrophysical investigations are far from the standard of scientific accuracy possessed by classical astronomy, which, with its solid mathematical base and constant progress in both observation and theory, rightfully occupies the premier place among the experimental sciences. God forbid that astronomy should be carried away by a fascination with novelty and diverge from this essential basis, which has been sanctified for centuries, and even millennia.[27]

This difference between the approaches to the two kinds of astronomy was also illustrated by the discovery of Pluto in 1930 by astronomers at the Lowell Observatory in Arizona. The staff of the observatory were used to doing astrophysical work that allowed some leeway in the precision of observations in the name of quickly publicizing new discoveries so that others in the field could take immediate advantage of them. However, the celestial mechanicians of the old astronomy scolded them for the lack of precision in their models of the orbit of the new planet.[28]

Many young astronomers, however, were drawn to astrophysics precisely because it was new and interesting, and did not promise to use an entire career for only incremental advances. For early practitioners, astrophysics promised a way to advance careers quickly and without having to move through the well-established hierarchy of classical astronomy. As Mary Agnes

[25] Meadows, "The Origins of Astrophysics," 6.
[26] Ibid., 13.
[27] Otto W. Struve to the Academy of Sciences in St. Petersburg, as quoted in A.J. Meadows, "The New Astronomy," in *The General History of Astronomy: Volume 4, Astrophysics and Twentieth-Century Astronomy to 1950: Part A* (Cambridge, U.K.: Cambridge University Press, 1984), 61.
[28] DeVorkin, *Henry Norris Russell: Dean of American Astronomers*, 297–300.

Clerke, an early historian and advocate of astrophysics wrote, classical astronomy was not of interest to young astronomers because it was a "tale of discoveries [already] told, and whose farther advance must be in the line of minute technical improvements, not of novel and stirring disclosures."[29] She went on to describe astrophysics in terms that probably made classical astronomers shudder. "It is full of the audacities, the inconsistencies, the imperfections, the possibilities of youth," Clerke wrote, and her positive attitude towards these characteristics was reflected by many astrophysicists.[30]

That astrophysics was initially a young man's game was born out by John Lankford's recent sociological study of astrophysicists in America. Lankford noted that until the late 1880s, astrophysicists were elected to the National Academy of Sciences after fewer years of work in their field than were astronomers who focused on astrometry or celestial mechanics. Those elected during this early period included Henry Draper, a medical doctor who built his own observatory in New York and pioneered photographic methods in astrophysics and before leaving an endowment to Harvard College Observatory for the creation of a massive spectrographic catalogue. Also included among the nominees was Edward C. Pickering, who developed photographic techniques of measuring the amount of light that stars produced and, as director of the Harvard College Observatory, went on to oversee the creation of the catalogue that Draper endowed. It is also worth noting that this group of early astrophysicists came from diverse educational backgrounds that included chemistry, physics, engineering, and mathematics. This highlights the fact that the new astronomy required practitioners to bring together knowledge from scientific fields that had not traditionally been considered part of astronomy.[31]

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[29] Agnes Mary Clerke, *A Popular History of Astronomy During the Nineteenth Century* (A. and C. Black, 1902), 142.

[30] Ibid.

[31] Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*, 49–51.

The debate over the legitimacy of astrophysics continued until after World War I, when new developments in physics and chemistry began to put it on a firmer theoretical foundation. During the nineteenth century, astrophysics continued along two paths that contributed to its acceptance. The first was as a companion and helper to celestial mechanics. Astrophysicists on the second path used their catalogues of stellar spectra to begin to classify stars of like type in a way that reflected early models of stellar evolution. When faced with the variety of colors of stars revealed by their spectra, astrophysicists often fell back on the idea that stars began hot and blue and cooled to red over the course of their lives. This idea was fed by the growth of the field of thermodynamics in the nineteenth century, and allowed astrophysicists to give some sort of physical interpretation to their classification efforts, though there were still many things that they did not understand.[32]

W. W. Campbell pointed out the ways that astrophysicists could help classical astronomers in the latter's pursuit of greater accuracy in stellar positions. Astrophysicists could determine the radial velocities of stars by studying their spectra, and this information would primarily be of interest to "the student of astrometry." Analysis of spectra also allowed astrophysicists to determine the motion of stars towards or away from the earth, a dimension that had long been closed to celestial mechanics, but was of great interest to classical astronomers. Using the Doppler effect, in which the spectral lines from a star approaching the earth would "shift" towards the blue end of the spectrum, while the lines of a star retreating would shift

[32] Meadows, "The New Astronomy," 61; Otto Struve and Velta Zebergs, *Astronomy of the 20th Century* (New York: The Macmillan company, 1962), 26–27; Joann Eisberg, "Solar Science and Astrophysics," in *The Cambridge History of Science, Volume 5: The Modern Physical and Mathematical Sciences*, ed. Mary Jo Nye (New York: Cambridge University Press, 2003), 505-521.

towards the red, astrophysicists could finally show astronomers how stars moved in this crucial third dimension.[33]

Classification, however, was the primary concern of astrophysicists for the rest of the nineteenth and into the twentieth century. As previously stated, the physics behind various characteristics in spectra was not yet properly understood, and devising a system of classification proved complex because of it. The physical factors behind differences in spectra could be attributed to temperature, pressure, chemical composition, or some combination of all three.[34] Julius Scheiner, a German astrophysicist, explained that the problem with astrophysics was that it had gathered "a large mass of observational data," that could not be resolved due to the "indefinite number of hypotheses and solar theories, which are with few exceptions radically wrong at the start and often contradictory to the most simple physical view of today."[35]

As new classification schemes and physical interpretations of spectral data were being developed, the observatory itself was undergoing a fundamental shift in both the nature of its instrumentation and in its organizational structure. Observatories increasingly came to resemble factories in the way that they operated. Labor was increasingly divided and specialized, in much the same way that it was in American factories during the same period. Changes in photographic technology were one of the key causes of this transition. Photography had become a useful tool in making astronomical observations in the mid-nineteenth century, but it did not become indispensible until around 1880, with the invention of dry plate photography, which allowed much longer exposure times than had previously been available, thus increasing the sensitivity of telescopes. It also simplified the photography process such that the photographer no longer

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[33] Campbell, "An Address on Astrophysics," 298; Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*, 60–62.
[34] Meadows, "The Origins of Astrophysics," 12–13.
[35] Julius Scheiner in *Der Spectralanalyse der Gesterne* as quoted in Meadows, "The New Astronomy," 69.

needed to worry about carrying and using dangerous chemicals. Further improvements in the sensitivity of the photographic plates also gave a boost to utility of astronomical photography. The plates kept well, which meant that suddenly observatories began to build up their stores of data faster than it could be evaluated. This in turn fueled the process of the division of labor, as semi-skilled workers, many of them women, were now required to reduce the photographic plates to usable data. Photography, however was only part of the reason for the rise of the factory-style observatory.[36]

The late nineteenth century also saw improvements to reflecting, as opposed to refracting, telescopes. Refracting telescopes magnified light by passing it through a large lens, called a refractor. Refractors always had a certain degree of chromatic aberration built in and were expensive to produce. The expense grew exponentially with the size of the refractor, as did the chances of flaws in the lens. Reflectors, which were essentially large, curved mirrors, had no chromatic aberration, but tarnished easily and thus had not been used much in the construction of large telescopes. However, the invention of silvered-glass reflectors that did not tarnish, or only tarnished very slowly, allowed reflectors to surpass refractors in size and light sensitivity. By the twentieth century all large telescopes were built with reflectors. The investment of financial capital necessary to build the large new reflectors meant the decline in significance research of smaller observatories, which could no longer keep up with the advances in instrumentation that were suddenly necessary to produce research at the highest levels. By the 1890s a few large, wealthy institutions came to control astronomical research in the United States.[37]

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[36] Owen Gingerich, *The General History of Astronomy: Volume 4, Astrophysics and Twentieth-Century Astronomy to 1950: Part A* (Cambridge, U.K.: Cambridge University Press, 1984), ix; John Lankford, "The Impact of Photography on Astronomy," in *The General History of Astronomy: Volume 4, Astrophysics and Twentieth-Century Astronomy to 1950: Part A*, ed. Owen Gingerich (Cambridge, U.K.: Cambridge University Press, 1984), 23, 34–36.

[37] Albert Van Helden, "Telescope Building, 1850-1900," in *The General History of Astronomy: Volume 4, Astrophysics and Twentieth-Century Astronomy to 1950: Part A*, ed. Owen Gingerich (Cambridge, U.K.:

The rise of the factory observatory with its need for unskilled and semi-skilled labor also led to the increased employment of women in observatories, beginning in the late nineteenth century. A modern observatory with a large telescope had to employ teams of computers—individuals trained in mathematics who could do all of the basic but labor-intensive calculations necessary to make observations useable. These positions came to be filled generally by women who were paid about half of what men were paid for equivalent labor. Their cost-effectiveness allowed more money to be dedicated to other areas of the observatory. Boss, director of the Dudley Observatory, noted that the women employed as computers were "as rapid and accurate as men and much more patient." In Boss's estimation women's patience and tendency to produce fewer errors meant that they were "superior" to men in the role of computer.[38] It no doubt helped that they allowed more money to be diverted to instruments, which were always the first priority of an observatory director.

The majority of women employed as computers had only a high school education, though there was a steady flow of recent college graduates from Vassar and other women's colleges as well. The calculations performed by computers in an attempt to create useable data from the photographic plates were often broken down into small, repeated steps that could be mastered by unskilled workers. The computers were given standardized forms and procedures that guided them through the calculations and helped to check for errors. There was little chance for advancement, or for doing any of the actual observing or interpretation. That being said, there were notable exceptions to this rule. Pickering at the Harvard College Observatory gave Williamina P. Fleming a great deal of responsibility in the creation of the Henry Draper

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Cambridge University Press, 1984), 40; Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*, 184–185.

[38] Lewis Boss, "History of the Dudley Observatory," (Unpublished, 1903) as quoted in George Wise, *Civic Astronomy: Albany's Dudley Observatory, 1852-2002* (Dordrecht: Kluwer Academic, 2004), 101.

Catalogue, a large-scale classification project being undertaken there. Fleming oversaw the work of eleven other computers and edited manuscripts by male astronomers. However, she was paid only a little more than half of what male observatory assistants made and her salary was fixed. Annie J. Cannon, who will be discussed later, also worked on the catalogue and had a major hand in creating the Harvard classification scheme that would be used for decades to come.[39]

As telescopes and observatories grew larger in the late nineteenth century, more emphasis was put on where they were to be placed geographically. Particularly in the United States, the spots for new observatories were chosen for their distance from cities, in order to cut down on light pollution, and for their good atmospheric conditions, which allowed more light to pass through the air and into the telescopes. After visiting the Lick Observatory, opened in 1887, Clerke remarked that "nowhere in the world is there to be found such a happy combination of instrumental power with atmospheric translucency as at Lick."[40] Lick was the first observatory to be sited deliberately on a mountaintop that had excellent atmospheric conditions. James Lick, who endowed the observatory, chose a site that was close to San Francisco, but still took the advice of astronomers in placing it at a site that ensured minimal atmospheric interference.[41]

As the nineteenth century gave way to the twentieth, it became increasingly clear that the United States had surpassed Europe to become the center of observational astrophysics. Part of the reason was the numerous favorable locations in the U.S. for building large telescopes. European observatories were dogged by heavy cloud cover or close proximity to population centers, but in the U.S. three major new observatories were built in optimal locations near the turn of the century: Lick, Yerkes Observatory, built in 1897, and Mount Wilson Observatory,

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[39] Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*, 287–309.
[40] A. M. Clerke, "Stellar Spectroscopy at the Lick Observatory," *The Observatory* 13 (January 1, 1890): 46.
[41] Trudy E. Bell, "Lick Observatory," in *The General History of Astronomy: Volume 4, Astrophysics and Twentieth-Century Astronomy to 1950: Part A*, ed. Owen Gingerich (Cambridge, U.K.: Cambridge University Press, 1984), 127-130.

built in 1904. Yerkes was a compromise in that it was built in Williams Bay, Wisconsin, far enough from its parent institution, the University of Chicago, to ensure dark skies, but close enough that the weather was often an impediment to viewing conditions. The Mount Wilson observatory in California had the best geographical placement of the three.[42]

Observatory placement was not the only reason that astrophysics in the U.S. began to outpace its European counterpart, however. Additionally, observatories in the U.S. were increasingly funded by large, private donations, while observatories in Europe were funded largely by governmental bodies. Lick, Yerkes, and Mt. Wilson were all built with money from private donors or foundations. Although these large observatories sometimes had trouble finding money after the initial instrument had been built, they were still able to operate with better primary instruments than other observatories.[43]

The best example of the ability of American astronomers to secure funds from private donors is undoubtedly George Ellery Hale. Born in 1868, Hale came from a wealthy family in Chicago and aggressively pursued his interest in science. While at the Massachusetts Institute of Technology he built the first successful spectroheliograph, an instrument that photographed the sun at one particular wavelength of light, which he used to observe solar prominences. Hale became an Associate Professor of Astral Physics at the University of Chicago in 1892, and soon convinced streetcar magnate C.T. Yerkes to donate money to the university for the construction of an observatory. When Yerkes Observatory opened in 1897, it had a 40-inch refracting telescope – the largest in the world. Hale was director of the observatory until 1905, and in that

[42] D. H. P. Jones, "Was the Carte Du Ciel an Obstruction to the Development of Astrophysics in Europe?," in *Information Handling in Astronomy: Historical Vistas* (Dordrecht: Kluwer Academic Publishers, 2003), 271; Meadows, "The Origins of Astrophysics," 62; "The Development of Astronomy in America," *Publications of the Astronomical Society of the Pacific* 12, no. 74 (June 1, 1900): 113.
[43] Jones, "Was the Carte Du Ciel an Obstruction to the Development of Astrophysics in Europe?," 271; Struve and Zebergs, *Astronomy of the 20th Century*, 17–26.

time he made sure that Yerkes stayed focused on astrophysics. Hale left when he convinced the Carnegie Institution to fund the construction of the Mount Wilson Observatory, which opened with a 60-inch reflecting telescope in 1907, and added a 100-inch reflecting telescope, the largest in the world, in 1917, thanks to both the Carnegie foundation and J.D. Hooker, a wealthy Los Angeles man that Hale convinced to help fund the project. Hale's career, which has only partially been illuminated here, was a demonstration of American astronomers putting to work resources for the advancement of astrophysics that were not available elsewhere.[44]

Some historians have also advanced the argument that astrophysics was more acceptable and less controversial in the U.S. than it was in Europe because the U.S. did not have as strong a tradition in classical astronomy. Also, American astronomers may have been more willing to adopt the new approach because it gave them a chance to help establish a brand new science. However, this would seem to be undermined by the fact that Germany, France, and England all established observatories in the late nineteenth century dedicated to astrophysics.[45]

Yet another theory on how the U.S. came to dominate astrophysical observation is that the *Carte du Ciel*, a major project undertaken at the end of the nineteenth century, consumed too much time and too many resources from European observatories and therefore allowed the Americans to get a head start on astrophysics. First coordinated at an international conference held in Paris in 1887, the *Carte du Ciel* was supposed to first create paper charts of all stars in the sky down to a magnitude of 14, and secondly produce a high-precision catalogue of all stars brighter than a magnitude of 11 (stellar magnitudes are measured such that higher numbers mean

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[44] Brück, M. T., ed., *Agnes Mary Clerke and the Rise of Astrophysics* (Cambridge, U.K.: Cambridge University Press, 2002), 102; Albert Van Helden, "Building Large Telescopes, 1900-1950," in *The General History of Astronomy: Volume 4, Astrophysics and Twentieth-Century Astronomy to 1950: Part A*, ed. Owen Gingerich (Cambridge, U.K.: Cambridge University Press, 1984), 139–142.

[45] Meadows, "The New Astronomy," 72; Robert W. Smith, "Remaking Astronomy: Instruments and Practice in the Nineteenth and Twentieth Centuries," in *The Cambridge History of Science, Volume 5: The Modern Physical and Mathematical Sciences* (New York: Cambridge University Press, 2003), 162–163.

dimmer stars). It was therefore primarily an exercise in precision astrometry, and not concerned with astrophysics. Three Americans were among the fifty-six astronomers present at the conference, but no American observatories took part in the project. Opinions on the impact of the project on European astrophysics varies, though Otto Struve, director of Yerkes and McDonald Observatories from 1932 to 1950 (and grandson of previously mentioned Otto W. Struve), noted in 1943 that it was a common sentiment that projects such as the *Carte du Ciel* had "done much harm by virtually killing the ambitions and scientific aspirations of hundreds of the younger astronomers in Europe."[46] No matter what the reasons were, astrophysical observation and the classification of stellar spectra proceeded primarily in the United States at the end of the nineteenth and into the twentieth century.

While European scientists continued to focus on mechanical systems like the *Carte du Ciel*, investigators in the U.S. focused increasingly on astrophysical analysis. One area of spectral analysis that claimed attention from the emerging astrophysical community related to the apparent color qualities associated with different stars. Astrophysicists had long noticed that individual stars were often brightest in a particular part of the spectrum, and could be categorized according to that color. The sun was brightest in the yellow portion of the spectrum and was therefore considered a yellow star, other stars ranged from red to blue. Even though they were not sure exactly what that meant physically for each star, astrophysicists generally agreed that the fact that stars could be grouped into a few major categories was somehow significant. In Clerke's words, it was "scarcely doubtful that these spectral distinctions correspond to differences in physical condition of a marked kind."[47] Many color-based classification systems were proposed; twenty-three were being used by 1900. Additionally, there was no consensus on

[46] Otto Struve, "Fifty Years of Progress in Astronomy," *Popular Astronomy* 51 (1943): 474–475; Jones, "Was the Carte Du Ciel an Obstruction to the Development of Astrophysics in Europe?," 267–272.
[47] Clerke, *A Popular History of Astronomy During the Nineteenth Century*, 373.

what differences in the characteristics of spectral lines meant. Lines could be different widths with sharper or more diffuse edges, and astronomers were not sure what this meant in terms of temperature, pressure, or density.[48]

The most popular of the early classification systems was that of Father Secchi, a Jesuit astronomer at the Collegio Romano. Secchi looked at the spectra of 4,000 stars between 1863 and 1867, and divided them into four types. Type I were bluish or white stars whose strongest spectral lines were those caused by hydrogen. Type II were yellow and included the sun as a member. Type III were red and had as their strongest lines those attributed to metals. Type IV were also red, but with different characteristics than Type III. At the end of the century, however, Pickering at the Harvard College Observatory would lead an effort to create a more thorough classification scheme.[49]

Pickering used the "objective prism" method, which allowed the Harvard College Observatory to simultaneously photograph the spectra of hundreds of stars. The results formed the basis of the Henry Draper Catalogue, a comprehensive catalogue of stellar spectra funded by Henry Draper's widow, Mary Anne Palmer Draper. Draper, as previously mentioned, had contributed greatly to the use of photography in astrophysics, even though he was an amateur in the sense that he was trained as a medical doctor and was unaffiliated with any institution. In its early years the project, which would span decades, was headed by Fleming, who released the first part of the catalogue in the 1890 volume of the Harvard *Annals*. This first part contained spectra for over ten thousand stars. Pickering for his part created an alphabetical classification system using the letters A through Q. Pickering's main criterion was the strength of hydrogen

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[48] David H. DeVorkin, "Stellar Evolution and the Origin of the Hertzsprung-Russell Diagram," in *The General History of Astronomy: Volume 4, Astrophysics and Twentieth-Century Astronomy to 1950: Part A*, ed. Owen Gingerich (Cambridge, U.K.: Cambridge University Press, 1984), 92–95.
[49] Clerke, *A Popular History of Astronomy During the Nineteenth Century*, 373–374.

lines, so that they were strongest in A stars, and became weaker as the alphabet progressed. The system roughly corresponded with Secchi's. Classes A to D were Secchi's Type I, E to L corresponded to his Type II, M to Type III, and N to Type IV. O through Q were stars that contained unusual bright lines and did not fall into any of the other categories. When the first edition of the full catalogue was published in 1918, it doubled the number of stars classified by spectra and allowed astrophysicists to begin statistical studies of stars.[50]

Annie J. Cannon, working under Pickering, later rearranged the sequence of the Harvard classification system. Pickering had put women in charge of this project, and Cannon was the most qualified of them, with a science degree from Wellesley. She dropped some letters from the scheme and re-ordered it as O, B, A, F, G, K, M, P, Q. The new scheme was based on the strength of the spectral lines associated with hydrogen, the most fundamental element that constituted stars. This order and system are still used by astronomers. Cannon further divided the system using numbers, so that a star that was B2, for example, was one-fifth of the way between B and A in its spectral characteristics. She did this by paying attention to the lines that were designated as helium, and created a more continuous scheme than Pickering's original.[51]

By the twentieth century, an increasing number of astrophysicists came from physics backgrounds, and they began to apply some of the methods of physics to astrophysics. While the initial cohort of astrophysicists had included men from different fields, astrophysics in the twentieth century became increasingly aligned with physics, a field with its own barriers for entry than included formal academic training. Combined with the fact that it was now necessary

[50] DeVorkin, "Stellar Evolution and the Origin of the Hertzsprung-Russell Diagram," 95–96.
[51] Ibid., 98.

to have large telescopes and good funding to perform astrophysics, amateur astronomers had little room to operate in astrophysics at the beginning of the twentieth century.[52]

Ever since it had become apparent that spectral analysis would allow astronomers to probe the physical characteristics of stars, the big question in astrophysics was: how did stars work? What were their interiors like and how did they create the stupendous amounts of energy that they seemed to? What were their life cycles like? Beginning in the second decade of the twentieth century, two British theoretical astrophysicists (still a relatively new thing) debated the proper way to perform theoretical astrophysics and find answers to these questions. The United States had the best telescopes, but still lagged behind Europe in producing first-rate theoretical physicists and mathematicians. James Jeans emphasized completeness in his theorizing, refusing to believe that a theory was sound or useable before it had a thoroughly solid grounding in already-understood physics principles. Arthur Stanley Eddington, on the other hand, believed that the value of a theory lay in its utility, and that any theory that allowed for a merger with observation and that suggested further investigation was worth pursuing, even if it did not have a firm footing. In his analysis of the Eddington-Jeans debate, Matthew Stanley named Eddington the winner of this particular struggle.[53]

As an example of their differences, Eddington came up with equations to describe the apparent correlation between mass and luminosity that was observed by astronomers. His equations accurately described this relationship and allowed observers to make predictions that might help determine if this was indeed a relationship that had some physical significance. Jeans was aghast, however, that Eddington had not deduced these equations from already-established physical principles, and that they were not part of a complete description of how stars

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[52] Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*, 58–60.
[53] Stanley, "So Simple a Thing as a Star: the Eddington-Jeans Debate Over Astrophysical Phenomenology," 53–54.

functioned. But Eddington maintained that a theory was valuable in "its ability to provide understanding and the means to investigate further, not in its deductive relation to established facts."[54] Observational astrophysicists agreed, as they now had something to work with and investigate as they observed.[55]

In the United States, Eddington had an admirer and ally in Henry Norris Russell, a Princeton astrophysicist who, like Eddington, was ready to begin creating some meaningful physical interpretations out of the mountains of data that had been collected, even if these were not on the firmest footing, theoretically. In his review of Eddington's culminating work *The Internal Constitution of the Stars*, published in 1926, Russell approvingly quoted Eddington on the difference between mathematics and physics: "Cases could be cited where physicists have been led astray through inattention to mathematical rigour; but these are rare compared with the mathematicians' misadventures through lack of physical insight."[56] Russell and Eddington clearly both approved of physicists' method of charging ahead wherever possible over mathematicians' slower deduction. In America, Russell would lead a similar charge to pull astrophysics towards physics and away from its empirical roots.[57]

Russell obtained his doctorate in astronomy from Princeton in 1899, and spent his entire career there after being hired as an instructor in 1905. He was trained in mechanics, gravitation, and the theory of fluid bodies, and believed that astronomy had to become more like physics, where research was dictated by specific problems in the derivation of theory, and less of an empirical investigation with no specific endpoint. During his early career, astrophysicists were

[54] Arthur S. Eddington as quoted in Ibid., 57.
[55] Ibid., 68–69.
[56] A. S. Eddington, *The Internal Constitution of the Stars* (Cambridge University Press, 1926), 103, as quoted in Henry Norris Russell, "REVIEW: The Internal Constitution of the Stars by A. S. Eddington," *The Astrophysical Journal* 67 (January 1, 1928): 84.
[57] Ibid., 83–84.

still largely avoiding attaching much physical meaning to their work and some wanted to avoid adopting any sort of theoretical framework at all. A survey of astronomical publications by historian H. A. Abt bears this out. Papers published in the early twentieth century had no need to explain the importance of the measurements presented, nor did they draw conclusions.[58]

Russell's attempts at theorizing the field really took off when he discovered the work of Meghnad Saha in 1920. Saha was an Indian scientist who had visited European centers of learning, but was forced to remain in India due to his politics and low social standing. He developed a theory of thermal ionization that greatly advanced understanding of the O-B-A-F-G-K-M sequence in the Harvard classification scheme by looking at spectral line characteristics. His ionization theory "demonstrated that the ordinary laws of physics could be safely and successfully applied to the study of stellar spectra."[59] Saha at first had trouble getting his theories noticed, but Russell was one of those who latched on fairly quickly, as they made specific predictions that could be checked and allowed the advancement of physical theory.[60]

By this time Russell had worked out an agreement with Walter S. Adams, the director of the Mount Wilson observatory, that allowed him to visit the observatory and gain access to the massive number of photographic plates that the observatory and its large telescopes were producing. As there were few other theoretically-minded astrophysicists in the United States, Russell was the only one of this bent with access to such a large number of high-quality observations. Russell was also able to direct observers at Mount Wilson towards problems that were relevant to him.[61]

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[58] DeVorkin, *Henry Norris Russell: Dean of American Astronomers*, xi–xii; H. A. Abt, "Changes in Astronomical Publications During the 20th Century," in *Information Handling in Astronomy: Historical Vistas* (Dordrecht: Kluwer Academic Publishers, 2003), 127–128.
[59] Otto Struve, "Some New Trends in Stellar Spectroscopy," *Popular Astronomy* 43 (1935): 485.
[60] David H. DeVorkin, "Quantum Physics and the Stars (IV): Meghnad Saha's Fate," *Journal for the History of Astronomy* 25 (August 1, 1994): 158.
[61] DeVorkin, *Henry Norris Russell: Dean of American Astronomers*, 180.

While Russell's work began to move the field of astrophysics in the United States away from its overwhelming empiricism and more towards something resembling physics, Russell was not interested in building up America's institutional base of theoretical astrophysicists. Unlike most observatory directors, who built up their staffs and their own particular institutions, Russell stayed focused on his research and only created networks and relationships that could directly contribute to it. In addition, Russell corresponded primarily with European theoreticians, lessening the chances that theory would get a foothold in America beyond his own work. Russell viewed himself as a "headquarters scientist," to use historian David DeVorkin's term. He shifted the field by making himself into a touchstone that all other American astronomers had to acknowledge and consult in their work.[62]

Thus, in 1932, astrophysics was beginning to change. It was moving away from its empirical roots, though they still dominated the field. Thanks to new advances in theory, *The Astrophysical Journal*, the primary scholarly publication in the field, saw an abrupt increase in the number of pages published in each issue.[63] However, according to Horace Babcock at Mount Wilson, the observatory with the best research facilities in the world, "it was difficult for active research astronomers to keep up with the current advances in physics and spectroscopy, quantum theory and the like."[64] Astronomers there had to rely on visiting lectures from theorists, many of them European. That would begin to change when Otto Struve was appointed director of Yerkes Observatory and put into action a plan to bring theory and observation together under one roof in

[62] Ibid., 218, 310, xiv.
[63] Abt, "Changes in Astronomical Publications During the 20th Century," 129.
[64] Horace W. Babcock, "Interview with Horace W. Babcock," interview by Spencer Weart, July 25, 1977, Niels Bohr Library & Archives, American Institute of Physics, http://www.aip.org/history/ohilist/1038.html.

order to fully synthesize the field. Like Russell, who desired to make astrophysics more like physics, Struve wanted "to make astronomy the tool of modern physics."[65]

[65] Otto Struve, "Memorandum", February 4, 1941, Otto Struve Papers, Reel 2, Special Collections Research Center, University of Chicago Library.

CHAPTER II

Struve was born on August 12, 1897 into a family whose relationship with astronomy went back generations. His great-grandfather, grandfather, uncle, and father were all observatory directors, which may have given Struve an advanced understanding of the workings of observatories and scientific capital. When Struve won the Gold Medal from the Royal Astronomical Society in 1944, he was the fourth in his family to do so.[66] The Struve family was German, but moved to Russia when Otto's great-grandfather Wilhelm Struve founded the Pulkovo Observatory near St. Petersburg in 1839. Wilhelm was the first of seven Struve's in five generations to obtain a PhD or its equivalent in astronomy. His son Otto Wilhelm, the younger Otto's grandfather, was director of Pulkovo after Wilhelm, and spoke out against the rising tide of astrophysics for its tendency to stray from the "solid mathematical base and constant progress" of the old positional astronomy.[67] The younger Otto's father Ludwig was then director of the observatory at the University of Kharkov, in the Ukraine, which was where Otto completed his undergraduate education.[68]

In 1916, during World War I, Struve left his undergraduate studies at Kharkov to enlist in the Imperial Russian Army, where he served in the artillery on the Turkish front until 1918. Struve then rushed to complete his degree at Kharkov. Shortly after receiving his degree in 1919, Struve became an army officer in the White Army, who fought against the communist Red Army

[66] Leo Goldberg, "Otto Struve (obituary)," *Quarterly Journal of the Royal Astronomical Society* 5 (September 1, 1964): 284.
[67] Otto W. Struve to the Academy of Sciences in St. Petersburg, as quoted in Meadows, "The New Astronomy," 61.
[68] Kevin Krisciunas, "Otto Struve, 1897-1963," *Biographical Memoirs of the National Academy of Sciences* 61 (1992): 352; Otto Struve, "Footnote to History," *Science* 129, no. 3341, New Series (January 9, 1959): 60.

but were defeated in 1920. After the defeat, Struve was amongst those who were evacuated from Sevastopol across the Black Sea into Turkey. Struve made camp with the other refugees near Gallipoli until he was given permission to travel to Constantinople. There he worked as a laborer while living in a cramped loft. During this time Struve tried desperately to find out what had become of his family. When he finally made contact with a half-aunt in Germany, he found out that his father, brother, and sister had all died during the civil war. Struve later described his memories of the Russian Revolution as "those of a family disrupted; of the cruel, bloody, and hopeless struggle of Russian against Russian during the Civil War of 1918-1920."[69] Despite his loss and exile, Struve remained proud of his homeland and regarded his decision to enlist in the White Army as "the most self-sacrificing act of my life," an act that he hoped would some day be recognized "when the Russian people will recognize that patriotism was not the exclusive privilege of those who fought on the winning side."[70]

Struve's half-aunt probably informed the director of the Berlin-Babelsberg Observatory, Paul Guthnick, of his situation in Constantinople. Struve's uncle Hermann Struve had founded the Berlin-Babelsberg in 1913, and so Guthnick undoubtedly knew the family well. Guthnick wrote to Edwin Frost at the University of Chicago, Director of the Yerkes Observatory, to ask for a position for Struve, as Frost had previously expressed an interest in helping any Russian astronomers who had been displaced by the war, including Struve's father Ludwig. Frost found a spot for Struve shortly thereafter at Yerkes, and sent for him. Frost then worked with the American Central Committee for Russian Relief and the State Department to ensure that Struve would have no trouble being allowed into the United States. Struve thus arrived at Yerkes Observatory in November 1921, where he would spend the next twenty-nine years of his career.

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[69] Struve, "Footnote to History," 60.
[70] Ibid.; Goldberg, "Otto Struve (obituary)," 285.

Struve worked with Frost when the latter began a program in 1922 to ship food to Russian observatories in order to help astronomers during the famine that gripped that country after the civil war. Scientifically speaking, Struve set to work almost immediately upon his arrival and had earned his Ph.D. in astronomy by December 1923. He was then made an instructor, and became an assistant professor in 1927 (the same year that he was granted United States citizenship) and an associate professor in 1930, before eventually succeeding Frost as Director of Yerkes in 1932.[71]

During this period, in addition to familiarizing himself with astrophysics and beginning his own research career, Struve took the time to begin networking with the major figures in American astronomy. After meeting Walter S. Adams, the director of Mount Wilson, he travelled to the observatory in 1926 to comb through the photographic plates archived there. While in California he also took the time to visit the Lick Observatory, the Dominion Observatory in British Columbia, and the Lowell Observatory outside of Flagstaff, Arizona. The Lick director, Robert G. Aitken, had tried to poach Struve from Frost and Yerkes during the previous spring. In 1927 he visited Harvard College Observatory for two months after writing to Harlow Shapley, the director of the observatory and a former student of Russell's. Thus, only a few years into his career Struve was already known to most of the major observatory directors.[72]

During this early career period, Struve poured energy into his astrophysical research. After first hearing from Frost but before coming to America, Struve admitted to Frost that he was "only marginally familiar with the area of astronomical spectral analysis."[73] However, he must

[71] Goldberg, "Otto Struve (obituary)," 284; Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 78–79; Struve and Zebergs, *Astronomy of the 20th Century*, 29; Krisciunas, "Otto Struve, 1897-1963," 354–357.

[72] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 86–92.

[73] From a letter from Otto Struve to Edwin Frost, dated March 11, 1921 as quoted in Krisciunas, "Otto Struve, 1897-1963," 356.

have taken immediately to the field as he spent the rest of his career churning out publication after publication using spectroscopy. Apparently at some point Struve also read Russell's 1919 article "Some Problems in Sidereal Astronomy," which laid out Russell's beliefs on the what the future program of astrophysics should be, because he later wrote that the article "stimulated and directed" his own research. This may also explain how Struve began to adopt Russell's problem-oriented approach to astrophysics.[74]

In the article, Russell first defined the objective of astronomy (and all science) as "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."[75] He then asserted that this objective might be approached in one of two possible ways. The first was for astronomers to gather large amounts of reliable data and "leave it to the insight of some fortunate and future investigator to derive from the accumulated facts the information which they contain regarding the general problems of science."[76] The second possible way to go about doing things was for astronomers to keep the greater problems in mind while planning their researches so that they could generate "practicable data" with which to go about solving specifically defined problems. Russell, with his preference for problem-orientation over empiricism naturally advocated the second method as superior.[77]

In the article, Russell then went on to lay out exactly what problems he saw as most pressing in astrophysics so that others would have a guide to use in their attempts to generate "practicable data." Struve, with his limited, but rapidly growing, knowledge of astrophysics and

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[74] Ibid.; Otto Struve, "The General Needs of Astronomy," *Publications of the Astronomical Society of the Pacific* 67 (August 1, 1955): 216.
[75] Henry Norris Russell, "Some Problems of Sidereal Astronomy," *Proceedings of the National Academy of Science* 5 (October 1, 1919): 391.
[76] Ibid.
[77] Ibid.

spectroscopy seems to have latched onto Russell's outline of problems as a sort of program for his early career. Viewed through the lens of Bourdieu's scientific capital, Russell clearly had placed himself in a position to influence his younger peers such as Struve. Russell had attained this position due to the scientific capital that he had acquired during his career leading up to 1919. His position in the field and connections had placed him on the radar of the newly-formed National Research Council, who commissioned leading experts in various sciences to produce articles outlining current problems in the field. Russell was tapped as astronomy's representative. A young Struve, looking for a program of research to guide his entrance into astrophysics, would naturally have been drawn to a prestigious document such as this. And thus Russell's accumulation of scientific capital helped him to shape the path of this new entrant into the field.[78]

Russell took what he considered the biggest problems in astronomy and subdivided them into smaller areas for researchers to attack. The first was the need for a clear theory of stellar evolution. Various theories had been proposed, including by Russell himself, but none were rigorous enough to attain general consensus. Russell subdivided this problem into three smaller problems. First, how did binary star systems form? Second, what is the mechanism that allows some stars to vary their brightness? And finally, what is the tremendous source of energy that powers the stars? Russell next explored the problem of creating a model for the galaxy based on the motions of stars. He subdivided this problem into the two sub-problems of determining the "character, distribution and gravitational influence" of matter between the stars, and the relationship between the age of a star and both its position and motion within the galactic structure. He went on to state his belief that the "master-key" to these problems lay in further

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[78] Ibid., 391–392; Bourdieu, *Science of Science and Reflexivity*, 62–63; Struve, "The General Needs of Astronomy," 216.

investigation of spectra and the correlation of characteristics of spectra with the physical properties of stars and gas.[79]

Struve, showing his willingness to embrace theoretical investigations as jumping-off points for observation, tackled the binary star problem in 1924, working with Frost, still director of Yerkes, and Storrs B. Barrett, another astronomer at Yerkes. He studied hundreds of stars, often using photographic plates that had already been accumulated by the observatory, and worked to determine the radial velocities of hundreds of B-type stars. The study had been started by Frost and Barrett before his arrival, but Struve threw himself into his work in order to gain more practice at spectroscopy and get his career started.[80]

During this early part of his career, Struve also had the first stroke of luck with his family since the beginning of World War I. He was able to get his mother, the last surviving member of his immediate family, a visa to the US, where she arrived in January, 1925. During her first several years she worked part time at Yerkes as a computer. Then in May, 1925, Struve married Mary Martha Lanning, who had been a part-time secretary at Yerkes. The three of them lived together for thirty-eight years—the rest of Struve's life. The ordering of his domestic life allowed Struve to focus on his astronomical pursuits.[81]

In his research, Struve next studied clouds of interstellar matter, continuing to address the questions raised by Russell. He worked with Soviet astronomer B. P. Gerasimovic to determine the disposition of clouds of calcium that appeared to be distributed throughout the galaxy, thus partially addressing Russell's question regarding the distribution of matter between the stars. In his attempts to explain his work, Struve looked closely at the theories of J. S. Plaskett, director of

[79] Russell, "Some Problems of Sidereal Astronomy," 392–394.
[80] E. B. Frost, S. B. Barrett, and O. Struve, "Radial Velocities of 368 Helium Stars," *The Astrophysical Journal* 64 (July 1, 1926): 1-77; Goldberg, "Otto Struve (obituary)," 286.
[81] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 82; Krisciunas, "Otto Struve, 1897-1963," 357–358.

the Dominion Observatory, and the theories of Eddington, still considered one of the best theorists in the world. Struve came down on the side of Eddington's theories.[82]

Struve continued to work on problems that were linked to current physical theory. Next he worked on studying the Stark effect as a cause of line broadening in stellar spectra, which had also been noted by Russell in his 1919 paper.[83] The effect, which was evidenced by broad spectral lines in the presence of intense electric fields, had been demonstrated in the laboratory, and Struve wanted to examine whether it was a cause of broad spectral lines in stellar spectra.[84] But in order to successfully explore the effect, Struve first had to learn more physics, an area in which he had always been deficient. Struve thus applied for a Guggenheim fellowship to go and study with Eddington at Cambridge. During his application process he consulted with Adams at the Mount Wilson Observatory and Shapley at the Harvard College Observatory. In his application to the Guggenheim trustees, Struve expressed his belief that there were almost no theorists in the younger generation of American astronomers, though there were plenty of good observers. (The lone exception, as always, was Russell with his student Donald Menzel, and his supervision of Shapley's student Cecilia Payne.) Struve thought that by studying under Eddington he might become more literate in theory and fill the gap at Yerkes.[85] With his modest training in physics, however, Struve was not able to learn much from Eddington on either interstellar matter or, more broadly, quantum mechanics.[86] Nonetheless, Struve was still able to do enough work to publish several papers on the Stark effect, the final one with Christian Elvey, who was one of the first students to train under Struve. So while Struve may not have been able

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[82] Struve, "Interstellar Calcium"; Struve, "Further Work on Interstellar Calcium"; Gerasimovic and Struve, "Physical Properties of a Gaseous Substratum in the Galaxy."
[83] Russell, "Some Problems of Sidereal Astronomy," 394.
[84] Goldberg, "Otto Struve (obituary)," 287.
[85] Otto Struve, "Plan for Study," November 11, 1927, as cited in Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 95.
[86] Ibid., 95–99.

to deepen his theoretical understanding of astrophysics as much as he would have liked, he was still able to do things that built up his own scientific capital: strengthen connections with high-ranking members of the field such as Adams, Shapley, and Eddington, and turn a project into multiple published items, each of which built his reputation in the field of astrophysics. He was also undeterred in his belief that more theorists were needed in the United States, even if he himself would never be one of them.[87]

Struve agreed with Russell that it was time for astrophysics to become more like physics, with theorists driving research problems and interpreting results. Subrahmanyan Chandrasekhar, an astrophysical theorist who would join the Yerkes staff in 1936, described Struve as "farsighted" in his desire to have theorists become active in observatory research. Chandrasekhar also said that Struve greatly admired the English theoreticians such as Eddington, E.A. Milne and James Jeans.[88] In 1941, Struve wrote that, with the exception of Russell at Princeton, "the educational institutions in the United States [had] depended upon European countries to provide nearly all theoretical work in astrophysics." The American observatories had a "personal inclination toward experimental work" and so they worked with "purely observational matters" and made no attempt to "evaluate the results in terms of theory." According to Struve, it would be best for there to be an organization in the United States that would "digest the results of observation and would furnish the final fruits of scientific investigations."[89] Though these sentiments were written after he had become director of Yerkes Observatory, it seems clear from his work and his Guggenheim application that these views

[87] Otto Struve, "The Stark Effect in Stellar Spectra," *The Astrophysical Journal* 69 (April 1, 1929): 173; C. T. Elvey and O. Struve, "A Study of Stellar Hydrogen Lines and Their Relation to the Stark Effect.," *The Astrophysical Journal* 72 (December 1, 1930): 277-300.
[88] Subrahmanyan Chandrasekhar, "Interview with S. Chandrasekhar," interview by Spencer Weart, October 17, 1977, Niels Bohr Library & Archives, American Institute of Physics, http://www.aip.org/history/ohilist/4551_1.html.
[89] Struve, "Memorandum."

originated before his directorship. Struve's attitude may have been influence by his European origins, though his sentiments are backed by American, and future Yerkes astronomer, Jesse Greenstein's testimony on the nature of the work conducted at Mount Wilson Observatory.[90]

In 1932, Struve had come to a crossroads in his career. He was now the most productive researcher at Yerkes, he created and analyzed more spectrograms than anyone else at the observatory, and he published more papers each year than anyone else. Yet he had already been forced to use plates gathered at Mount Wilson and Lick in order to do his research. The old forty-inch refractor at Yerkes was no longer a first-rate instrument in the field. Not only were large reflectors becoming dominant, but the Yerkes instrument operated at a "reduced efficiency" due to the often-cloudy weather, poor atmospheric conditions, and extreme cold during the winter.[91] Additionally, Struve would later describe the astronomy department during the 1920s as "stagnant," and filled with "incompetent or unproductive persons."[92] According to Lankford's institutional potential metric, Yerkes, at least in Struve's view, was beginning to fade due to its outdated instrumentation and low quality of staff. It was under these circumstances that Struve got an offer from Shapley for the position of assistant director at the Harvard College Observatory, which promised access to the observatory's two sixty-inch reflectors. The University of Chicago and its new president, Robert Hutchins, however, were not going to let go of Struve easily.[93]

Struve had learned of Hutchins' appointment to the presidency while finishing his fellowship with Eddington in 1929. Hutchins, who was only thirty years old when he received

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[90] Jesse Greenstein, "Interview with Dr. Jesse Greenstein," interview by Paul Wright, July 31, 1974, Niels Bohr Library & Archives, American Institute of Physics, http://www.aip.org/history/ohilist/4642.html.
[91] Otto Struve, "Memorandum", November 13, 1939, Otto Struve Papers, Reel 4, Special Collections Research Center, University of Chicago Library.
[92] Otto Struve to K. K. Link, June 25, 1938, Otto Struve Papers, Reel 4, Special Collections Research Center, University of Chicago Library.
[93] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 119–121.

the appointment, was dubbed the “boy president” in the Chicago newspapers, and he preferred to pick young and energetic deans and department chairs when he could. Edwin Frost was set to retire in 1932, and would need to be replaced. Struve was not Frost’s first choice, but he was the first choice of Henry Gale, the Dean of Physical Sciences at the university. Gale had noted Struve’s productivity and his popularity with the directors of other observatories and made it a priority to make him Director of Yerkes. Since scientific capital naturally flows to other scientific capital, the scientific capital that Struve had already built up through his research and his connections put him in a position to gain even more as an observatory director. Luckily Struve was not completely enamored with Shapley’s offer. For one thing he would have to work under Shapley, who was not going anywhere anytime soon, which would limit his ability to set his own research agenda. For another, Struve knew that the offer of the two sixty-inch telescopes was not all that it seemed. The telescopes had been cheaply built and never achieved their promised efficiency. Additionally, one was located on the east coast of the U.S., not ideal for viewing conditions, and the other was in South America. Struve wanted something like Mount Wilson, a large reflector at a site in the American southwest where conditions were favorable year-round. In an astounding bit of serendipity, Frost had ensured that the University of Chicago would have access to just such an instrument – the newly conceived McDonald Observatory in Texas.[94]

William Johnson McDonald was a Texas banker who died in 1926, and left his million-dollar fortune to the University of Texas “for the purpose of aiding in erecting and equipping an Astronomical Observatory to be kept and used in connection with and as a part of the University

[94] Ibid., 105–121.

for the study and promotion of the study of Astronomical Science."[95] McDonald was a life-long bachelor who had always had an interest in science, and had even taken courses on botany during the summers of 1895 and 1896 at Harvard, where he also visited the observatory. In his final decades he apparently lived in an apartment over a cabinetmaker's shop. McDonald owned a small telescope that he and his landlord, S. W. Wilson, would use to stargaze in the backyard. Because he had enjoyed the sciences so much during his life, McDonald evidently wanted to leave something behind that would contribute to them. Much to the consternation of his family, he left almost his entire fortune to the University of Texas in the hopes that Texas would have its own large observatory to rival those on the east and west coasts. The university was caught completely off guard.[96]

At the time, the University of Texas had no astronomy department and thus no one to guide them in how to proceed, so the president of the university, Harry Y. Benedict, immediately dispatched letters to the large observatories asking for advice. One of the letters went to Edwin Frost at Yerkes Observatory, who seems to have had a talent for conscientious letter writing, even if, in Struve's opinion, he had let the quality of the Yerkes staff degrade late in his tenure. He wrote back to Benedict with advice on how to use the money effectively. Then, when McDonald's family challenged the bequest on the grounds that McDonald had not been of sound mind when he wrote it, Frost wrote a detailed defense of the idea, citing examples of other wealthy men who had given large donations for the building of observatories. This help probably kept Yerkes and the University of Chicago in Benedict's mind when President Hutchins wrote to him in 1932 with an offer of cooperation. So Frost's social awareness ended

[95] William Johnson McDonald, "The Last Will and Testament of William Johnson McDonald," in *Big and Bright: A History of the McDonald Observatory*, by David S. Evans and J. Derral Mulholland (Austin, TX: University of Texas Press, 1986).
[96] Evans and Mulholland, *Big and Bright*, 7–11.

up saving Yerkes. He brought in Struve when Struve had nowhere else to go, and made it possible for Yerkes to supplement its aging facilities with a new telescope in Texas.[97]

Struve, meanwhile, was aware of the possibility of collaborating with Texas even as he was pondering Shapley's offer of the assistant directorship at Harvard. When Hutchins and Dean Gale met with Struve in March, 1932, he already had a plan worked out to cooperate with the University of Texas. After meeting, Hutchins phoned Benedict in Texas and proposed that the universities work together on building and staffing the McDonald Observatory while Struve went to visit Harvard to learn more about his possible appointment there.[98] Shapley and Harvard ranked high on Lankford's institutional potential scale because they were good at securing funding and had a steady stream of first-class astronomers and physicists, as well as easy access to Russell.[99] However, it is clear that that quality of instrumentation, which includes good location, was the most important part of an observatory's institutional potential, and Struve had a chance at getting what might become the second-best telescope in the world, after the one-hundred inch reflector at Mount Wilson. After leaving Harvard, Struve travelled to Austin in April with a formal proposal prepared, which Hutchins signed and presented to the Board of Regents, who approved it. Even if they built the observatory on their own, the University of Texas never would have been able to staff it with the caliber of astronomers that Chicago would be able to provide. Under the agreement, the director of Yerkes would also be the director of McDonald and would report directly to both university presidents, ensuring a high degree of autonomy.   The agreement also dictated that the new observatory would be built according to the plans of the director and under his direct supervision. The agreement was set to last for thirty

[97] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 122–124.
[98] Ibid., 124–127.
[99] Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*, 188–189.

years, easily a career for the thirty-four-year-old Struve. Struve had thus leveraged his scientific capital into the opportunity to gain more. He used his desirability and the offer from Harvard to push Hutchins to act quickly and secure a new observatory with a large reflector in a good climate, just like he wanted. With an agreement in place with the University of Texas, Struve declined the offer from Harvard and took over as director of Yerkes in the summer of 1932.[100]

[100] McDonald Observatory, *Addresses Made at the Dedication Exercises of the W.J. McDonald Observatory on Mount Locke, Jeff Davis County, Texas, May 5, 1939* (Austin?, 1939), 28–29; Evans and Mulholland, *Big and Bright*, 26–29.

CHAPTER III

Once he was appointed director of Yerkes Observatory, Otto Struve quickly became a first mover in astrophysics. He continued to gather scientific capital through his prolific research, but most of his influence came from his administrative responsibilities. Struve gained this power by becoming director of two observatories with large telescopes, Yerkes and McDonald, and hiring a well-regarded staff to work at them. As previously noted, in his discussion of institutional potential John Lankford listed quality of instrumentation as one key factor in the ranking of astronomical observatories, along with the availability of assistants, adequate funding, research time and quality of colleagues. With his takeover of Yerkes and the founding of McDonald, Struve had ensured all of these except the quality of colleagues, which he felt Frost had allowed to decay. Thus Struve quickly set about improving the quality of the Yerkes staff. In addition, he became managing editor of *The Astrophysical Journal*, the primary peer-reviewed publication in the field, a role that had traditionally gone to the director of Yerkes because the journal was a University of Chicago publication.[101]

Struve had already increased his scientific capital before his directorship by publishing research that aligned him with Russell, Eddington, and other major names in the field who had already banked large amounts of scientific capital, and by publishing journal articles prolifically. The Struve name had gotten him a job at Yerkes, and had given him easy access to the other major holders of scientific capital and guaranteed that they would at least listen to what he had to

[101] Bourdieu, *Science of Science and Reflexivity*, 35; Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*, 129–131.

say.[102] By 1932 that combination of factors had made him the desired candidate for the position of Director of Yerkes Observatory, which follows Bourdieu's idea that, much like monetary capital, scientific capital flows to those who already have it.[103]

The McDonald Observatory would not open until 1939, but in 1932 Struve had a vision for the direction that he wanted to take his two observatories. He wanted to hire a staff that would mix theorists, often foreign, with observers from the U.S. in order to study individual stars in depth and thus answer some of the questions that Russell had raised in 1919. When the new 82-inch reflecting telescope at McDonald was finally dedicated in 1939, Struve asked some questions remarkably similar to Russell's:

> Why are there no stars which exceed in mass a few hundred times the mass of the Sun? Why is it that nearly all stars and nebulae consist of the same chemical elements in roughly the same relative proportions as we find them in the Sun? Where and how do the stars generate their stupendous energies of light and hear, and what is the ultimate fate of their radiation?[104]

In order to answer these and similar "why" questions, Struve decided to hire a staff that had theoretical depth to go with its observational capabilities.[105]

In order to fulfill his vision for Yerkes and McDonald, Struve needed to first be sure that he had complete control over his observatories. He had already written into the agreement with Texas that he would be accountable directly to the university presidents. He next needed to clean out the "incompetent or unproductive persons" who made up "a large fraction of the staff" in 1932. In 1938 he argued strongly against switching to a voting-based hiring system, which

[102] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 77–105.
[103] Bourdieu, *Science of Science and Reflexivity*, 56.
[104] McDonald Observatory, *Addresses Made at the Dedication Exercises of the W.J. McDonald Observatory on Mount Locke, Jeff Davis County, Texas, May 5, 1939*, 16–17.
[105] Struve, "Memorandum."

was being considered by the faculty senate at the University of Chicago. Struve asserted that "a powerful administration is much to be preferred in spite of the possibility of errors in some individual cases." He went on to say that "the results would have been disastrous" had a voting-based system been in place when he took over, for he never would have been able to get rid of the old guard in favor of the young astronomers he had brought in, astronomers who were rapidly making Yerkes one of the best observatories in the world.[106]

Struve's attempt to clean out those staff members and research programs that he found unproductive was aided by the fact that he took over during the worst years of The Great Depression. The University of Chicago's endowment had taken a major hit, and all programs were being asked to cut back on expenses. Struve fired any staff members that did not contribute publishable research, but kept on those that did. Though Struve did not think of it in this way, only employees that contributed directly to the buildup of scientific capital were considered valuable. He terminated the contract of Arthur S. Fairly, an instructor who produced very little research, and stopped the daily solar observation program that Frost had continued despite the fact that it was not part of any larger research effort. He also allowed Clifford C. Crump's appointment to expire in 1933. Crump was much more of a teacher than a research scientist. He had only ever published two pieces of research, one of which was his Ph.D. thesis.[107]

Struve was later described by Subrahmanyan Chandrasekhar as "autocratic" in the way he ran the observatories. But Chandrasekhar described this style as necessary in order for Struve to achieve his goals. He wanted to push and develop the field of astronomy "to the maximum that was possible." He kept a close eye on the work being done by all of those under him because

[106] Struve to Link, June 25, 1938.
[107] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 133–143.

the study of astronomy "was the only thing that mattered to him."[108] Struve's desire to control all aspects of the observatories is evident in a letter to Hutchins wherein Struve asked him to intercede with the department of Buildings & Grounds at the University of Chicago so that Struve could decide who exactly would be hired to do work at Yerkes. He had already worked it out so that all work orders crossed his desk, but there were still things slipping through the cracks. One of the heaters had been painted without Struve's knowledge, and he wanted to make sure that anything having to do with equipment at the observatory would be under his full control.[109] Struve's control over the observatory also provides a useful demonstration of Lankford's hypothesis that observatory directors before World War II were like chief executive officers of their institutions. They were responsible for everything from raising capital, to setting research agendas, to the daily operations and upkeep of the observatory equipment. They also held a corresponding level of power, which is why it is useful to look first to observatory directors when looking for Bourdieu's first movers.[110]

As he hired his new staff, Struve followed through on his idea of mixing theorists and observers. Of the older staff member, Struve kept on George Van Biesbroeck and Frank E. Ross. Van Biesbroeck was a productive observer at Yerkes who had helped to train Struve and also ran the instrument shop, building auxiliary pieces of equipment as they were necessary. Ross productively studied stars with high proper motions (meaning that they moved through the sky relatively quickly), but spent half of each year in Pasadena, California, working on the two hundred inch telescope project being put together for the Palomar Observatory, which was funded by the Rockefeller Foundation. Struve also fought to find the money to keep on three

[108] Chandrasekhar, "Interview with S. Chandrasekhar."
[109] Otto Struve to Robert Hutchins, November 23, 1936, Otto Struve Papers, Reel 4, Special Collections Research Center, University of Chicago Library.
[110] Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*, 188–192.

young researchers whom he had helped to train himself. These were Christian T. Elvey, William Wilson Morgan, and Philip C. Keenan. All three men were experts with observatory equipment who knew how to capture spectra, but none were particularly strong when it came to theory. Elvey spent his career studying the nebulae and the effects of the atmosphere on observation, while Morgan was mostly concerned with updating and improving the old Harvard classification system – he believed that there was still much to be done at the "pre-interpretive" level. Struve had put together a team of observers, including himself, whom he knew to be productive researchers that would publish and bring more scientific capital to the institution.[111]

For theorists, Struve looked to those who had European training, and in 1936 he hired three young men around whom he intended to build the mixed observational/theoretical group that he hoped "might lead to many new types of investigations of a non-routine character that would be especially valuable in the present state of astrophysics."[112] Here Struve began to show the innovative approach to how he believed astrophysics should be practiced if it was going to move towards a greater understanding of the many "why" questions that still needed to be addressed. Gerard Kuiper was a Dutch astronomer who was also a capable observer, and often mixed the two in his work. Bengt Strömgren was a Danish theorist who had studied theoretical physics at Neils Bohr's famous Institute of Theoretical Physics. The third was Chandrasekhar, an Indian physicist who had studied with Eddington at Cambridge. With these three, Struve hoped to open a "new chapter in the history of Yerkes Observatory." He believed that having

[111] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 138–141; The conclusion that Elvey's work was on nebulae and the atmosphere was drawn based on a survey of his published work using the SAO/NASA Astrophysics Data System, http://adsabs.harvard.edu; William Wilson (W.W.) Morgan, "Interview with Dr. William Wilson Morgan," interview by David H. DeVorkin, August 8, 1978, Niels Bohr Library & Archives, American Institute of Physics, http://www.aip.org/history/ohilist/4786_1.html.

[112] Otto Struve to Subrahmanyan Chandrasekhar, February 25, 1936, Otto Struve Papers, Reel 2, Special Collections Research Center, University of Chicago Library.

three theorists of their caliber at one institution would mean having "the best group of astronomers in the world."[113]

Kuiper had studied under Enjar Hertzsprung and received his Ph.D. in 1933. He first came to the United States on a two-year postdoctoral fellowship at Lick Observatory. When the fellowship was completed, Kuiper wanted to stay on in order to maintain his access to a large telescope, but there were no positions available for him, at least partly because of a strong vein of nativism that ran through some observatories during this period. Those leaders immune to this were Russell, his former student Harlow Shapley, director of Harvard College Observatory, and Struve, who was of course foreign himself.[114] After leaving Lick, Kuiper obtained a one-year position at Harvard and stopped by Yerkes while on his way across the country. There he met with Struve, who was impressed by his energy and devotion to research, and offered him a position after he had gotten to Harvard. Happy to be able to remain in America near the big telescopes, Kuiper accepted.[115]

Strömgren agreed to come to Yerkes on an eighteen-month contract. Struve wanted him to come permanently, but Strömgren was not ready to leave his family for that long, though he did return to Yerkes and McDonald on a more permanent basis in 1947. Strömgren was the son of Elis Strömgren, director of the Copenhagen University Observatory, and showed an extraordinary aptitude for mathematics from a young age. He completed his doctoral thesis in 1929 at the age of twenty-one. He published his first major finding in astrophysics in 1932 when

[113] Otto Struve to Subrahmanyan Chandrasekhar, March 23, 1936, Otto Struve Papers, Reel 2, Special Collections Research Center, University of Chicago Library.
[114] DeVorkin, *Henry Norris Russell: Dean of American Astronomers*, 327–338.
[115] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 170–172.

he determined that hydrogen was the main component of stellar interiors. Given his famous name and obvious potential, Struve could see great value in adding him to the staff at Yerkes.[116]

For his part, Strömgren was very interested in the possibilities that Yerkes offered. There were no large telescopes in Europe doing the type of spectroscopic work that Strömgren needed to advance his research, so he knew that he wanted to spend some time in the United States. Since he was interested in working closely with observers, Struve's plan was a natural fit for him. Said Strömgren, "I wanted to have close contacts with the efforts in observational astronomy, which were ever so much more important in the thirties in the States than in Europe."[117] Sure enough, Strömgren was able to work with Struve and Morgan, during which time he learned much more about observational astrophysics than he had been able to in Europe. He was also interested in teaching classes in theory, and this ruled out visiting the large telescopes on the west coast, who "weren't the least interested in that," because it would "disturb their activities of observing." Even though he was only at Yerkes for eighteen months, Strömgren added to the growing reputation of Struve's staff and set up his return in 1947, when he would be able to work at the McDonald Observatory, which by then had become a major hub for combined theoretical/observational research.[118]

Chandrasekhar had studied with the famous Eddington and was beginning to garner a reputation as a first-rate theorist. He received his Ph.D. from Cambridge in 1933 at the age of 22, and had already been publishing productively for several years. He went to visit Harvard in 1935 and met Struve, who was also visiting. Struve, like many others, was impressed with

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[116] Otto Struve to Robert Hutchins, February 18, 1936, Otto Struve Papers, Reel 4, Special Collections Research Center, University of Chicago Library; M. Rudkjobing, "Obituary - Stromgren, Bengt - 1908-1987," *Quarterly Journal of the Royal Astronomical Society* 29 (June 1, 1988): 282.
[117] Bengt Strömgren, "Interview with Dr. Bengt Stromgren," interview by Lilllian Hoddeson and Gordon Baym, May 6, 1976, Niels Bohr Library & Archives, American Institute of Physics, http://www.aip.org/history/ohilist/5070_1.html.
[118] Ibid.

Chandrasekhar's brilliance and ability, and invited him to come visit Yerkes. Before Chandrasekhar had even arrived, Struve discussed the matter with Hutchins and decided to go ahead and offer him a position on the Yerkes staff. Chandrasekhar later recalled being "astonished" that he was being asked to join the observatory staff, and not just the faculty of the university.[119] But Struve made it clear that he wanted Chandrasekhar to work with the observers: "It seems to me that your brilliant theoretical work could be made even more valuable to astronomers if it could be combined with practical investigations by the observers." Struve then went on to state his belief that having a theoretician work directly with the observers "might lead to many new types of investigations of a non-routine character that would be especially valuable in the present state of astrophysics."[120]

Chandrasekhar was also being wooed by Shapley at Harvard. Shapley had Russell, his old mentor, attempt to convince Chandrasekhar to accept his offer. Shapley tried to get Struve to reconsider his offer to Chandrasekhar by telling him that Chandrasekhar was a communist because of his sympathies with the Indian nationalist movement, but Hutchins was disbelieving and was able to convince Struve that Chandrasekhar's political views would not be a concern. In the end Chandrasekhar chose Yerkes, where he began in January, 1937, just a few months after Kuiper and Strömgren. Coming from Eddington at Cambridge and drawing the personal attention of Russell showed just how much scientific capital Chandrasekhar had already acquired through his work, and as he began at Yerkes he rapidly began to accumulate more.[121]

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[119] Chandrasekhar, "Interview with S. Chandrasekhar."
[120] Struve to Chandrasekhar, February 25, 1936.
[121] Chandrasekhar, "Interview with S. Chandrasekhar"; Otto Struve to Robert Hutchins, January 13, 1936, Otto Struve Papers, Reel 4, Special Collections Research Center, University of Chicago Library; Robert Hutchins to Otto Struve, January 15, 1936, Otto Struve Papers, Reel 4, Special Collections Research Center, University of Chicago Library.

Since scientific capital flows to scientific capital, and since Struve's prestigious staff and the promise of the new large telescope at McDonald increased the institutional potential of Yerkes in instrumentation and quality of colleagues, the observatory began to draw more astrophysicists interested in pushing the boundaries of theoretical/observational collaboration. One such scientist was Jesse Greenstein, who finished his Ph.D. at Harvard in 1938. While Greenstein did not characterize himself as a theorist, he studied under Donald H. Menzel, who had studied under Russell and who was characterized by Greenstein as the only real theorist in the Harvard program. When Greenstein won a postdoctoral fellowship to study at Yerkes, he jumped at the chance:

> Struve impressed me; he was an observer who thought also about theory… he impressed me very very much, and he was, to my eyes, much closer – since they were generating new observations – to the interaction of theory and fact than Menzel's group was.[122]

Greenstein was at first intimidated by the intellectual abilities of the Yerkes group, but was drawn to Yerkes and McDonald as the leading center in the world for astrophysics. The presence of Struve, Chandrasekhar, Kuiper, and Morgan, in conjunction with the new telescope at McDonald, made it so. Just as Harvard was, at least according to Greenstein, flawed by their inability to work effectively at the interface of theory and observation, Mount Wilson was hampered by their lack of theorists on staff. They had the largest telescope in the world, but they did not "innovate" in the field, according to Greenstein. Struve's buildup of scientific capital, just as in Bourdieu's model, and working through the mechanism of Lankford's institutional potential, had made it easier to build more.[123]

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[122] Jesse Greenstein, "Interview with Dr. Jesse Greenstein," interview by Spencer Weart, May 7, 1977, Niels Bohr Library & Archives, American Institute of Physics, http://www.aip.org/history/ohilist/4643_1.html.

[123] Greenstein, "Interview with Dr. Jesse Greenstein"; Greenstein, "Interview with Dr. Jesse Greenstein."

Struve's own research benefited from the presence of visiting professor Pol Swings, a Belgian astrophysicist who became Struve's closest collaborator on his individual astrophysical research. Swings was not a permanent staff member, but when World War II broke out he was stranded in the United States and decided to make the best of it by staying on and working with Struve. Swings was more of a theorist than an observer, and he and Struve worked very well together, with Swings providing the theoretical insight and Struve bringing his considerable observational ability to bear on the problems. Swings was strongest in theory, just where Struve was weakest, and their friendly collaboration spanned years and many publications.[124]

Most of Struve's publications were in *The Astrophysical Journal*, which was the major peer-reviewed journal for publishing spectroscopic studies of stars. It had been founded by George Ellery Hale, founder of Yerkes Observatory, and later Mount Wilson Observatory, in 1895. It was published by the University of Chicago press and had therefore traditionally been organized and edited by the staff at Yerkes. Astronomers from other major observatories were also editors, but most of the work fell to the director of Yerkes and whatever members of his staff he appointed to editing positions. When Struve took over as director of Yerkes, he therefore inherited the journal from Frost. Under Struve, the number of pages published increased abruptly, though this may have been due to the changes in astrophysics during the 1920s that allowed for more physically meaningful explanations of observations. The style of writing in astronomical publications in general had changed during the course of the twentieth century. In the beginning, papers did not need to explain why measurements or observations were important in order to be published, nor did they need to draw conclusions based on the

[124] Strömgren, "Interview with Dr. Bengt Stromgren"; Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution,* 104–105.

measurements. As a result, the average length of articles in *The Astrophysical Journal* increased from two or three pages to five pages by mid-century.[125]

Struve was determined to keep the journal's place as the dominant publishing organ in the field. He worked with the other major observatory directors to ensure that all concerns were addressed. In 1941, he worked with the American Astronomical Society to reorganize the board of collaborating editors so that all American astronomers, or at least those at the major observatories, would feel responsible for the journal, and would feel that their work was adequately represented. Struve had to work extra hard during the war years to keep the journal, never a moneymaking enterprise in the best of times, afloat when all of its foreign subscriptions dried up. After the war, the combined output of Yerkes and McDonald in the journal outpaced that of the other major observatories. During the 1946-1947 academic year, when Yerkes and McDonald were in their prime, they spent $2,200 on publishing in the journal, which was as much as the next three largest contributors, Harvard, Mount Wilson, and Lick, spent combined. Considering that Yerkes and McDonald were operating with the same staff, the difference seems particularly large. The editorship was another of the administrative ways that Struve accumulated scientific capital.[126]

While managing his various administrative responsibilities, all of which increased his scientific capital, Struve also found time to publish a tremendous amount of his own research, accumulating scientific capital of "strictly scientific authority," as Bourdieu puts it. This is capital of the type that is accumulated purely through scientific work. Struve published an

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[125] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 18–19, 236; Abt, "Changes in Astronomical Publications During the 20th Century," 127–134.

[126] Otto Struve to W.S. Adams, December 19, 1941, Otto Struve Papers, Reel 1, Special Collections Research Center, University of Chicago Library; Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 256; William Wilson (W.W.) Morgan, "Memorandum", 1948, Astrophysical Journal Records, Box 15, Folder 6, Special Collections Research Center, University of Chicago Library.

average of twenty-two items per year over the course of his career, including both peer-reviewed and popular articles. During the period from 1932 to 1947, he published 125 papers in *The Astrophysical Journal* alone. At the same time, his popular articles, which were aimed at astronomers and scientists outside the field, as well as the lay public, were important for keeping those outside of the field abreast of the latest developments. It is unclear if these popular articles helped to keep private money flowing into astrophysics, but it is a possibility.[127]

The staff at Yerkes later remembered Struve as remarkably, almost manically, industrious. Strömgren recalled that Struve's administrative duties never cut into his research, so far as he could tell. "He managed to produce more than most people. And I don't see that he could have done much more, had he concentrated on that work alone."[128] "He was just a great strong person with a moral sense about science," recalled Greenstein. "It was our duty to work harder, and if we'd done that, to work still harder. And if we didn't work sixteen hours a day, it was our fault."[129] Struve's work was not defined just by its volume, but by its quality as well. He won the Gold Medal of the Royal Astronomical Society in 1944. He also seemed to have an eye for finding stars that would turn out to be of significant research interest. Greenstein later analogized it to being able to "smell a Rembrandt behind the dirty wall." Struve would notice something odd about a particular star, and that something odd would turn out to be physically significant in a way that could generate years of work for an individual astronomer.[130]

Struve's drive to spend as many waking hours as possible conducting research reflected sociologist Max Weber's ideas on science as a vocation. Weber believed that scientists had to possess an intrinsic motivation for pursuing their work because it was almost guaranteed that

[127] Bourdieu, *Science of Science and Reflexivity*, 57; Krisciunas, "Otto Struve, 1897-1963," 358–359.
[128] Strömgren, "Interview with Dr. Bengt Stromgren."
[129] Greenstein, "Interview with Dr. Jesse Greenstein."
[130] Greenstein, "Interview with Dr. Jesse Greenstein"; Greenstein, "Interview with Dr. Jesse Greenstein."

most of their research and ideas would be overturned by future generations of scientists. They therefore could not think of themselves as building some edifice that would last indefinitely into the future. Instead, they had to possess an internal drive that was tied to the work itself. This drive may have been especially pronounced in a European like Struve, whose family would have reflected the exact traditions that informed Weber, who was German himself.[131] Lankford also noted this internal drive in the most eminent astronomers in his study. He cited a 1951 study by psychologist Anne Roe, who interviewed many of the most elite physical scientists, including astrophysicists, and concluded that one of their common characteristics was a relentless and continuous work that was driven by internal psychological pressures.[132]

Beginning when he assumed the directorship of Yerkes in 1932, Struve thus accumulated an immense amount of scientific capital in a short period of time – more than he could have with his research alone. He proved adept at accumulating both the "capital of strictly scientific authority" as well as the "capital of power over the scientific world."[133] It was particularly this second type, generated by Struve's administrative positions – director of his highly regarded staff of Yerkes and McDonald, controller of the new reflector at McDonald and the still-operating refractor at Yerkes, and editor of *The Astrophysical Journal* – that Struve excelled at accumulating. Becoming observatory director had placed him among the power brokers in the field, W. S. Adams at Mount Wilson, Harlow Shapley at Harvard, and Henry Norris Russell at Princeton, all of whom were observatory directors. He did this through luck, good social skills, and the fact that, once accumulated, scientific capital will flow to scientific capital. Struve thus attained the position of a first mover within the field, and was free to

[131] Max Weber, "Science as a Vocation," in *From Max Weber: Essays in Sociology*, trans. H. H. Gerth and C. Wright Mills (New York: Oxford University Press, 1946), 129-156.
[132] Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*, 186.
[133] Bourdieu, *Science of Science and Reflexivity*, 57, 63.

redefine the methods and borders of astrophysics so that it became the science that he envisioned.

CHAPTER IV

The question remains as to how Struve, as a first mover in astrophysics, would choose to move the field. As Bourdieu states, dominant agents in a field impose on the field "the principles that they themselves consciously or unconsciously engage in their practices."[134] Following Bourdieu's model, Struve then would move astrophysics towards what he already saw as the right way to perform astrophysics, and how he was already in the process of performing astrophysics: by bringing theory and observation together so that they could inform one another more easily. Struve worked hard to ensure that Yerkes and McDonald would have a strong theoretical presence on staff to work with the observations coming out of the two large telescopes. He wanted astrophysics to begin taking a more problem-oriented, physics-like approach to its research, much like the methods that Henry Norris Russell had used to build his own scientific work. By virtue of having accumulated a large amount of capital, primarily through his administrative position as an observatory director, Struve was able to reshape the field so that the appropriate or proper way of doing things was the way that he had ensured they were done at Yerkes and McDonald.[135]

Struve, despite having little training in theory himself, had come to see theoretical discussion as a necessary part of an observatory's work. For this reason he brought in Bengt Strömgren, Gerard Kuiper, and Subrahmanyan Chandrasekhar, and was very disappointed when Strömgren did not accept a full-time position. Struve specifically asked Chandrasekhar to

[134] Ibid., 62–63.
[135] Ibid.

cooperate closely with the observers both to increase the value of Chandrasekhar's work, and to help guide the observers to "new types of investigations" that would contribute to the progress of astrophysics.[136] Struve wanted to develop a first-class theoretical department at the University of Chicago of the kind that had never existed in America. This theoretical department would be "invaluable" to the observational astronomers as "it would direct the observational work along those lines which are particularly valuable from the theoretical point of view." It would also "digest the observational results which would come from the observatory in Texas."[137]

By 1941 Struve had noticed a marked decline in the productivity of British theoretical astrophysicists, a decline that had only been hastened by the outbreak of World War II. Without the ability to rely on theoretical direction to come from Europe, and with Russell, long "the sole exception" to this distinction between astrophysical practice in America and Europe, rapidly moving towards retirement, Struve became even more convinced America needed a strong theoretical presence at its observatories. Struve had long since noted the tendency of the large American observatories—Mount Wilson, Harvard, Lick, and Yerkes (before Struve)—to deal in "purely observational matters" with "no attempt… to evaluate the results in terms of theory." He chalked this up to the "personal inclination" of the astronomers at these observatories, but he had determined that it was "now emphatically necessary to provide in this country for an organization which would digest the results of observations and would furnish the final fruits of scientific investigations." Struve wanted, in his words, "to make astronomy the tool of modern physics."[138]

Strömgren, when he came to Yerkes for his eighteen-month stay in 1936, was delighted to be able to work closely with observers. Strömgren had been working on his theory of stellar

[136] Struve to Chandrasekhar, February 25, 1936.
[137] Struve, "Memorandum."
[138] Struve, "Memorandum."

interiors and needed observational results in order to continue his work. To obtain those results he needed to come to America, the location of all of the large observatories, and he found in Struve a director who was as interested as he was in combining theoretical and observational research. It turned out that Strömgren was unable to use the observational work at Yerkes to advance his research on stellar interiors, possibly because he was there too briefly, though this is unclear, but his proximity to Struve, Morgan, and the other observers gave him new ideas about possible avenues of study, just as Struve had hoped that they would. Strömgren later said that he worked primarily with Chandrasekhar and Struve during his visit to Yerkes. With Chandrasekhar he discussed his theories on stellar interiors, and he in turn helped Chandrasekhar as he was developing his monograph *An Introduction to the Study of Stellar Structure,* which was published in 1939. Strömgren and Chandrasekhar had almost daily contact as Chandrasekhar wrote the book, and Strömgren gave him comments on the chapters as Chandrasekhar wrote them. He later reviewed the book for *The Astrophysical Journal*.[139]

Strömgren's work with Struve during this period must have gone a long way towards convincing Struve that he had made the right decision in bringing theoreticians to his observatory since it showed observers and theoreticians working together on a problem that could not be solved be either alone. Struve had worked on the spectroscopic binary star Epsilon Aurigae with his student Christian Elvey since 1930, but had not been able to puzzle out the unusual changes in its spectrum. He had presented two possible physical theories about how the behavior of the two stars making up the binary might account for the spectral peculiarities, but had not been able to do more than hypothesize. With the arrival of Kuiper and Strömgren, however, Struve was able to use their expertise—Kuiper's in mathematically modeling stellar motions and

[139] Strömgren, "Interview with Dr. Bengt Stromgren"; Bengt Strömgren, "Book Review: An Introduction to the Study of Stellar Structure, by S. Chandrasekhar," *Popular Astronomy* 47 (1939): 287-289.

Strömgren's in modeling the behavior of light shining through gases—to create a complete and theoretically-backed account of Epsilon Aurigae's spectrum. The three men worked out a physical model where the smaller of the two stars that composed the binary shone through the atmosphere of the larger star and thus accounted for the unusual variations in its spectrum. The presence of the theoreticians had thus led to the publishing of a joint paper in *The Astrophysical Journal* of the exact kind that Struve had been hoping for.[140]

Another example of the observers and theoreticians working together in novel ways came from the creation of the nebular spectrograph at Yerkes in 1938. This was a device specially designed to photograph the spectra of nebulae—diffuse clouds of interstellar matter that emitted radiation when they were struck by radiation from nearby stars. Photographing the spectrum of a nebula accurately was difficult because most observatory equipment was geared towards photographing stars, which are not as large, nor as irregular in shape. The nebular spectrograph at Yerkes attached to the end of the large telescope and was conceived and built by Struve, Jesse Greenstein, Louis Henyey, and George Van Biesbroeck. Henyey had just finished his Ph.D. in Struve's revamped department at Yerkes and was more theoretically and mathematically-minded. According to Greenstein, he, Struve, and Henyey were at Yerkes one night and were unable to observe due to the cloudy weather conditions. Struve turned to the two younger astronomers and asked them what the "ideal, most efficient" spectrograph was. This quickly led to a discussion where the three men conceived of the design for the new spectrograph, which Van Biesbroeck then built in the workshop during the next week. The new spectrograph worked

[140] Strömgren, "Interview with Dr. Bengt Stromgren"; O. Struve and C. T. Elvey, "Preliminary Results of Spectrographic Observations of 7 Epsilon Aurigae.," *The Astrophysical Journal* 71 (March 1, 1930): 136-149; Edwin B. Frost, Otto Struve, and C. T. Elvey, "A study of the spectrum of 7 [epsilon] Aurigae.," *Publications of the Yerkes Observatory* 7 (1932): 81-132; G. P. Kuiper, O. Struve, and B. Strömgren, "The Interpretation of ε Aurigae," *The Astrophysical Journal* 86 (December 1, 1937): 570.

as predicted and Struve and the others immediately set about putting it to good use.[141] A larger, better version of the nebular spectrograph was then built at the McDonald Observatory site, which had been chosen for its clear sky and could therefore be put to this use even before the large, new telescope was completed.[142] Walter S. Adams, director of the Mount Wilson Observatory and William H. Wright, director of the Lick Observatory both wrote to Struve to congratulate him on the innovative new instrument.[143]

Strömgren's interest in interstellar matter was piqued by the observations being made with the new spectrograph. He and Struve sometimes drove together between Yerkes and the campus in Chicago, and their discussions naturally turned towards Struve's new work on nebulae. Strömgren created a theoretical model of the kinds of radiation produced from a star embedded in a nebula that explained particular lines that Struve was observing in the nebulae that he studied. He then worked out his theory more completely after he left Yerkes to return to Copenhagen in 1938, which allowed him to calculate an approximation of the amount of hydrogen that existed between the stars, a number that was important for its potential to inform theories of star formation including how often new stars were formed. Even though by this point Strömgren had left the observatory, Struve's work with him demonstrated the unique opportunities for research that could arise when theorists and observers were brought into close proximity.[144]

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[141] Greenstein, "Interview with Dr. Jesse Greenstein"; Otto Struve, "A New Slit Spectrograph for Diffuse Galactic Nebulae," *The Astrophysical Journal* 86 (December 1, 1937): 613.
[142] For a complete description of the workings of the new spectrograph, see O. Struve, G. van Biesbroeck, and C. T. Elvey, "The 150-FOOT Nebular Spectrograph of the McDONALD Observatory," *The Astrophysical Journal* 87 (June 1, 1938): 559-571.
[143] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 199–200.
[144] Strömgren, "Interview with Dr. Bengt Stromgren"; Bengt Strömgren, "The Physical State of Interstellar Hydrogen.," *The Astrophysical Journal* 89 (May 1, 1939): 526.

Greenstein and Henyey worked productively on various nebulae and on the general interstellar medium for the next several years. The two men also developed a camera that could cover 140 degrees of the sky and so was optimal for photographing the Milky Way. They developed it while working for the National Research Council during World War II to help train gunners on large bomber aircraft, and were able to keep two of the five cameras that they had built with them when the war ended. The two men worked at the Yerkes Optical Bureau that Struve had set up during the war specifically to keep some of his staff present at Yerkes. Throughout their working relationship, Greenstein supplied the observational expertise and Henyey worked through the mathematical theory to give physical explanations for the observations.[145]

Struve, for his part, worked most extensively with Pol Swings. The two men jointly published thirty-eight papers between 1936 and 1948, twenty-one in *The Astrophysical Journal*. Swings was a spectroscopist who specialized in understanding the conditions that formed certain spectral lines for various elements. He supplied much of the theory that backed up Struve's observations and allowed the two men to make stabs at physical interpretation of their observations. Together they worked on the lines formed by molecules—the joining of more than one atom together—and on "peculiar" stars, meaning those that had spectra that were unexpected when compared to most other stars. Their work on the molecular lines helped to determine the relative abundance of oxygen in stellar atmospheres, when compared to hydrogen. Struve needed Swings' ability to understand the latest research in physics, since he had never been

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[145] Greenstein, "Interview with Dr. Jesse Greenstein"; L. G. Henyey and Jesse L. Greenstein, "The Theory of the Colors of Reflection Nebulae.," *The Astrophysical Journal* 88 (December 1, 1938): 580; J. L. Greenstein and L. G. Henyey, "Studies of Diffuse Nebulae.," *The Astrophysical Journal* 89 (June 1, 1939): 653; J. L. Greenstein and L. G. Henyey, "The Ratio of Interstellar Absorption to Reddening.," *The Astrophysical Journal* 93 (March 1, 1941): 327; For a full account of the formation of the Yerkes Optical Bureau during World War II, see David H. DeVorkin, "The Maintenance of a Scientific Institution: Otto Struve, the Yerkes Observatory, and Its Optical Bureau During the Second World War," *Minerva* 18, no. 4 (December 1, 1980): 595-623.

strong in that area relative to the theorists, and through Struve Swings had access to someone who could generate huge amounts of high-quality data.[146]

Chandrasekhar, a future Nobel laureate, became the foundation of the theoretical astrophysics work done during Struve's tenure at Yerkes and McDonald, but his ability to work together with observers did not always produce results in the way that Struve had probably envisioned. He was more often inspired to explore new research areas not by the results of the observational program at Yerkes and McDonald, but rather by the latest work by other theorists throughout the world. Similarly, the observers that he worked with often struggled to find useful ways to integrate Chandrasekhar's latest theories into their own work. *An Introduction to the Study of Stellar Structure* became a foundational text in astrophysics programs for decades after it was published, but it took Strömgren to make deductions from the text that could be of use to observers.[147]

The most impactful work that Chandrasekhar did during Struve's tenure, as far as the observational astronomers were concerned, was on the negative hydrogen ion. Rupert Wildt, a Princeton theorist working under Russell, had published a paper in 1939 on the negative ion of hydrogen (a hydrogen atom that has more than the usual one electron) that showed that when this particular ion was taken into account the relative abundance of hydrogen to the other elements in stellar atmospheres was greatly increased, and this explained certain observations much better.[148] It was a breakthrough for astrophysics in determining what stars were made of, and

[146] The number of articles by Struve and Swings was determined by searching the SAO/NASA Astrophysics Data System, http://adsabs.harvard.edu/; Strömgren, "Interview with Dr. Bengt Stromgren"; Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 104–105; Otto Struve to W.S. Adams, June 8, 1942, Otto Struve Papers, Reel 1, Special Collections Research Center, University of Chicago Library.
[147] Chandrasekhar, "Interview with S. Chandrasekhar"; Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 218–221.
[148] Chandrasekhar, "Interview with S. Chandrasekhar"; Rupert Wildt, "Negative Ions of Hydrogen and the Opacity of Stellar Atmospheres.," *The Astrophysical Journal* 90 (November 1, 1939): 611.

Chandrasekhar set about improving on Wildt's work. This culminated in papers that determined exactly how much light was being absorbed by this hydrogen ion. The observational astronomers "immediately recognized" the value of Chandrasekhar's work, published in two 1945 papers, and incorporated it into their own.[149]

During the late 1940s, Chandrasekhar worked primarily on his theory of radiative transfer, which showed how the light produced by stars moved outward through their high-temperature atmospheres. He later said that the five-year period between 1944 and 1949, which he spent working on this problem and hammering out his theory was "the happiest period of my scientific life."[150] For Chandrasekhar, the subject "moved with its own momentum" from paper to paper until he had completed twenty-four papers on the subject.[151] It was all highly mathematical in nature, and of a type of mathematics that he found particularly pleasing to work on. His inspiration during this period was the work of other theorists and physicists, and not the work of his colleagues at Yerkes, who often did not understand his work fully. Greenstein later recalled that he could only understand about half of Chandrasekhar's papers, and Chandrasekhar was never sure that Struve really understood much of his work, as evidenced by the fact that in Struve's book *Astronomy of the 20th Century*, published in 1962, he got some of Chandrasekhar's work wrong.[152]

Despite his colleagues' occasional lack of understanding, Chandrasekhar must have seen some value in being so closely connected with large observatories like Yerkes and McDonald,

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[149] Chandrasekhar, "Interview with S. Chandrasekhar"; Wildt, "Negative Ions of Hydrogen and the Opacity of Stellar Atmospheres."; Subrahmanyan Chandrasekhar, "On the Continuous Absorption Coefficient of the Negative Hydrogen Ion," *The Astrophysical Journal* 102 (September 1, 1945): 223.
[150] Chandrasekhar, "Interview with S. Chandrasekhar."
[151] Ibid.
[152] Greenstein, "Interview with Dr. Jesse Greenstein"; Subrahmanyan Chandrasekhar, "Interview with Dr. S. Chandrasekhar," interview by Kevin Krisciunas, Transcript, October 6, 1987, Niels Bohr Library & Archives, American Institute of Physics, http://www.aip.org/history/ohilist/4552.html; Struve and Zebergs, *Astronomy of the 20th Century*.

because when he turned down a chance to replace Russell at Princeton in 1946, he specifically cited the fact that, at Princeton, "the close contacts I have here with observational work [would] be lost," as one of the deciding factors.[153] It is also possible, however, that this was partly a rationalization by Chandrasekhar, as Russell was beginning to doubt his offer to Chandrasekhar at the same time as Chandrasekhar was deciding to stay at Yerkes.[154] Regardless of how the Princeton offer played out, the mere fact that he had been offered the chance to replace the universally respected Russell also shows exactly how highly Chandrasekhar had come to be regarded in the astronomical community. The quality of his scientific work had gained him a large amount of scientific capital of the type conferred by research, which only contributed to the capital of Struve as an administrator. Chandrasekhar's attitude regarding the importance of observers also shows how Struve's perception of the proper way to conduct astrophysics had penetrated down into his staff.[155]

One program that continued at Yerkes that had nothing to do with the mixture of theory and observation was W. W. Morgan's work on spectral classification. Morgan had received his Ph.D. at Yerkes in 1931 under the supervision of Struve but, unlike Struve, he saw a definite need for more, in his own words, "pre-interpretive" classification work to be done in order to give astrophysics a more solid foundation. Whether the classification work could possibly have been pre-interpretive given the point to which astrophysics had progressed during the previous half-century is debatable. Nonetheless, Morgan viewed the work as both pre-interpretive and necessary. Morgan thus worked with his assistants Philip C. Keenan and Edith Kellman to produce the monograph *An Atlas of Stellar Spectra, with an Outline of Spectral Classification*,

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153 Subrahmanyan Chandrasekhar to Otto Struve, October 17, 1946, Otto Struve Papers, Reel 2, Special Collections Research Center, University of Chicago Library.
154 DeVorkin, *Henry Norris Russell: Dean of American Astronomers*, 354–355.
155 Chandrasekhar to Struve, October 17, 1946; Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 276–279.

which was published in 1943. In the monograph, Morgan and his collaborators altered the Harvard classification system, which was still very much in use, to add another dimension—that of luminosity classes. Morgan had been frustrated by the Harvard system in that he thought it was based on a system of floating values that were continually being redefined as equipment improved. *An Atlas of Stellar Spectra* used the ratio of the intensities of various spectral lines within the same star, rather than comparisons to certain standard stars, to define its luminosity classes and therefore set the classification scheme on firmer footing. He respected Struve's attempts to get directly to physical interpretation in his work, but believed that too much still needed to be done at this pre-interpretive level to justify working on anything else. He also later said that he had "no point of contact" with Chandrasekhar's work, and that the two of them lived in "different worlds."[156]

Morgan, Keenan, and Kellman's work paid off as the new "MKK" system was widely adopted. Greenstein later said that the three of them had "invented spectral classification in the modern sense."[157] Thus, even though Morgan's work did not generally fit in with the other work being done under Struve, he and his team still managed to add to the scientific capital of Yerkes. One possible reason that Struve allowed Morgan's work to continue under him was that Morgan never asked for any telescope time on the new 82-inch at McDonald. He only used the old 40-inch refractor at Yerkes, which at this point was nowhere near as good of an instrument and therefore not good for much of the other work being done at Yerkes and McDonald. He did sometimes complain, however, that the Yerkes instrument was suffering from neglect due to the

[156] Morgan, "Interview with Dr. William Wilson Morgan"; Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 255–256; David Leverington, *A History of Astronomy from 1890 to the Present* (London: Springer-Verlag, 1995), 146.
[157] Greenstein, "Interview with Dr. Jesse Greenstein."

money and attention pouring into McDonald. Nonetheless, Morgan seemed satisfied to use the old telescope on his big project.[158]

So, with a few exceptions, it is clear that Struve wanted his observatory to produce work at the interface of theory and observation, and he encouraged his staff to pursue projects along those lines. Since Struve had obtained the use of a first-rate instrument at McDonald and had built up a staff that grew in scientific capital and produced many publications, he was already shifting the field in the direction that he thought it should go. To use Bourdieu's terminology, Struve was a "dominant player" who imposed "as the universal standard of the scientific value of scientific productions the principles that [he himself] consciously or unconsciously [engaged] in [his] practices."[159]

It is unclear from the sources examined so far, but it is possible that Struve's ideas on how to pair theorists and observers came from his own limitations as a scientist, and the ways that he worked around them by working with Swings and others. It should also be noted that these pairings did not produce revolutionary results, which is the primary reason why Thomas Kuhn's ideas regarding the development of a scientific field are not as useful in examining Struve's effect on astrophysics as Bourdieu's scientific capital. Following Kuhn, the field of astrophysics could more accurately be described as having been in its "normal science" phase during Struve's tenure as director of Yerkes and McDonald. For Kuhn, this meant that most of the work consisted of "puzzle solving," where scientists in a given field had fairly complete conceptual models to work with and were therefore not looking for any revolutionary ideas, but were rather trying to develop those models to fit all available data. Struve, however, did succeed in altering the practice of astrophysics by bringing theorists into a major observatory to work

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[158] William Wilson (W.W.) Morgan to Otto Struve, May 23, 1939, Yerkes Observatory, Office of the Director, Records, Box 182, Folder 2, Special Collections Research Center, University of Chicago Library.
[159] Bourdieu, *Science of Science and Reflexivity*, 62–63.

with observers. So changes were made in the field, but not the kind of changes that Kuhn would have been interested in.[160]

Struve's role as a dominant agent or first mover also allowed him to help set the barriers to entry into the field of astrophysics. As one of the major holders of scientific capital, he was able to define the education necessary for someone to become an astrophysicist. It is telling, then, that Struve almost always put the theorists working at Yerkes in charge of teaching graduate programs at the University of Chicago. Struve believed that America had traditionally lagged behind Europe in its theoretical training. In an article published in *Popular Astronomy* in 1949, Struve called for American students to receive more training in "formal knowledge," by which he meant, "advanced mathematics and advanced physics."[161] While he praised American universities for their ability to produce students who were both driven and excellent observers and instrumentalists, he argued that there was a great need for training in the latest, most advanced physics for astronomy students. He pointed out that, at Yerkes, he had been forced to institute courses in modern atomic theory, which was more physics than astrophysics, and yet was necessary in order for the astronomers there to learn fundamental principles that were necessary to their work. Presumably he still wanted their graduate studies to include work at observatories that would gain them practical experience, but he wanted to supplement that experience with a greater understanding of theory. He also proposed for adoption by American astrophysics programs a set of minimum requirements for undergraduates who wanted to pursue graduate study in astrophysics. In mathematics, these included "a very thorough knowledge of ordinary differential equations," as well as familiarity with "elementary aspects of analysis." In

[160] Kuhn, *The Structure of Scientific Revolutions*, 35–42.
[161] Struve, "Memorandum."

physics, they included “a very good general knowledge of atomic theory, including the more recent advances in quantum mechanics, optics, thermodynamics, and electromagnetic theory.”[162]

Just as with the work being done at the theoretical/observational interface, Struve was setting boundaries on the field of astrophysics that reflected what was already in place at Yerkes, and had been for over a decade by the time the *Popular Astronomy* article was published. He had put Chandrasekhar in charge of graduate studies at the University of Chicago, and Chandrasekhar naturally taught those things that he was working on at the time, which included stellar dynamics and stellar atmospheres, both highly theoretical subjects. Chandrasekhar, in fact, during the late 1930s and early 1940s, was teaching somewhere between one-third and one-half of all graduate astronomy courses, ensuring that graduate students at the University of Chicago were getting a very thorough grounding in current astrophysical theory. Similarly, when Strömgren came to Yerkes in 1936 and again in 1947, he also taught graduate courses on theory. Strömgren also recalled that during this period neither of the large observatories in California, Mount Wilson and Lick, were teaching theory. Since these were the only two observatories with telescopes and observing programs that rivaled those at Yerkes and McDonald, this made Yerkes unique in the world in the way that it was able to teach theory and observation at the highest levels. The other institutions that rivaled Yerkes, according to Lankford’s institutional potential metric, were Harvard and Princeton, neither of which had access to the quality of instruments available at McDonald or the California observatories. At Mount Wilson, which was associated with the California Institute of Technology, Strömgren recalled that the astronomers did not want to sacrifice the time and effort that they could spend observing, and at Lick, which was associated with the University of California at Berkeley, the

[162] Otto Struve, “The requirements for a graduate student at an observatory,” *Popular Astronomy* 57 (1949): 382-386.

faculty did not really have any theorists on staff. Tellingly, neither of these things would change until the two schools hired Greenstein and Henyey, respectively, both fresh off of their time at Yerkes under Struve. As a final illustration of the way that the program at Yerkes emphasized theory, out of the eight Ph.D. graduates produced between 1940 and 1944, five had completed their degrees under Chandrasekhar.[163]

By the late 1940s, Struve was beginning to look for a way out of his position at Yerkes. Even though he was influencing the field with the very large amount of scientific capital that he had gathered at Yerkes and McDonald, it is very doubtful that Struve thought of it this way. The accumulation had been largely unconscious, and Struve had benefitted from the power that observatory directors held during this period.[164] Struve had naturally used this power and his scientific capital to help shape the field, but from his perspective his administrative duties had left him unable to focus on his own research to the degree that he wanted. It had been very difficult for him to keep Yerkes and McDonald running during the war, when many of his young staff had gone off to do war work, and when everyone came back and McDonald was in its heyday, there was suddenly much more work for an administrator to do.[165]

Struve initially tried to just reorganize the astronomy department so that most of the day-to-day work would be handed off to others. In 1947 he made Kuiper the director of Yerkes and McDonald, and put Chandrasekhar in charge of teaching and of theoretical astrophysics at Yerkes. He also made Morgan the managing editor of *The Astrophysical Journal*; Chandrasekhar had been an associate managing editor since 1945. Struve would remain in

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[163] Chandrasekhar, "Interview with S. Chandrasekhar"; Strömgren, "Interview with Dr. Bengt Stromgren"; Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 261.
[164] Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940*, 188–199.
[165] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 286–296.

charge of all of them, though, and soon tension arose between Struve and his three senior staff members.[166] Kuiper and Chandrasekhar both found Struve to be a micro-manager, and the two men were by this point prestigious enough in their own right to want to dictate the terms of their careers. During this period, Kuiper recalled that Struve "was often overworked, suffered from insomnia which caused him sometimes to be in somewhat of a daze following two or three hours sleep."[167] Thus, after several uncomfortable years that were nonetheless very productive in terms of astrophysical research, Struve left Yerkes altogether to become head of the astronomy department at the University of California at Berkeley in 1950.[168]

Before discussing the final phase of Struve's career before his death in 1963, it is important to look at the way that, when he gave up much of the scientific capital that he had accumulated at Yerkes, he left it in the hands of astronomers who would continue to practice astrophysics as he had set it up at Yerkes and McDonald—uniting theory and observation. Chandrasekhar could have replaced Russell at Princeton and continued to integrate observation with the latest physics, just as Russell had always done, but that likely would have been true of anyone qualified enough to replace Russell, and by staying at Yerkes, Chandrasekhar ensured that there would always be a particularly strong theoretical presence at the University of Chicago. By staying at Chicago, he was also able to take over as managing editor of *The Astrophysical Journal* when Morgan resigned in 1952, a position that Chandrasekhar held for the next nineteen years, at which point the journal was turned over to the American Astronomical Society. During his tenure the journal flourished, as Chandrasekhar demanded much of authors

[166] Otto Struve to Subrahmanyan Chandrasekhar, January 27, 1947, Otto Struve Papers, Reel 2, Special Collections Research Center, University of Chicago Library; Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 286–296.
[167] Kuiper as quoted in Krisciunas, "Otto Struve, 1897-1963," 367.
[168] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 295–296.

and editors, and expanded the journal to keep at the forefront of each new subfield as it developed, including the new post-*Sputnik* space science. Strömgren, who had always been one of the best at uniting theory and observation, took over the directorship of Yerkes and McDonald, ensuring that Struve's style continued to be the norm.[169]

Struve's method of mixing theory and observation spread to Mount Wilson and Lick observatories through both himself and the astronomers that had trained under him. In 1948 Jesse Greenstein was selected to head the department of astrophysics at the California Institute of Technology, which was associated with the new two-hundred-inch reflector at the Palomar Observatory. Before his arrival, the annual reports from the observatory had no heading specifically for theoretical studies, the way that the Yerkes reports did. In 1949, however this changed so that theoretical studies became a part of each annual report on the work being done both at Mount Wilson and Palomar. At the university Greenstein immediately began teaching theoretical astrophysics courses, which had previously not been offered. Greenstein was at first thrown by the way that theory seemed to have no place at the observatory. One of the astronomers there had even suggested that all theoretical papers should be taken out of *The Astrophysical Journal* and put into a supplement that would not be part of the journal itself. In the coming years, however, Greenstein was able to hire more theoretically-minded faculty like himself. Even though Greenstein had always considered himself more of an observational astronomer than a theorist, he had clearly been influenced by his time at Yerkes, where he described the work as a "blend" of theory and observation.[170]

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[169] Chandrasekhar, "Interview with S. Chandrasekhar"; Strömgren, "Interview with Dr. Bengt Stromgren"; Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 310–312.
[170] Ira S. Bowen, "Theoretical Studies," *Mount Wilson and Palomar Observatory Annual Report* 2 (December 1, 1949): 20-21; Greenstein, "Interview with Dr. Jesse Greenstein"; Greenstein, "Interview with Dr. Jesse Greenstein."

Lick Observatory was associated with Berkeley, and had hired Greenstein's frequent collaborator Louis G. Henyey in 1947 as the only theoretical astrophysicist in the department. This changed when Struve accepted his position there. Although Struve had left Yerkes specifically to make more time for his own research, he still could not help but build up the theoretical presence at Berkeley. He hired Su-shu Huang, who had completed his Ph.D. at Yerkes under Chandrasekhar, and who worked with Struve in much the way Swings once had, providing theoretical input to complement Struve's observations. He also hired Jorge Sahade, an Argentine astronomer who had worked with Struve at McDonald. In 1953 Struve considered an offer to take over the Harvard College Observatory from Harlow Shapley, with whom Struve had often competed for staff members. However, Struve was not interested in leaving the wonderful large telescopes in California to which he now had regular access. When he did leave in 1959, he made sure to leave Henyey in charge of the department at Berkeley. Struve had thus seeded the American observatories with the best telescopes with members of the Yerkes staff who would ensure that his model of astrophysics stayed alive at those institutions.[171]

Struve's final job before his death was as director of the National Radio Astronomy Observatory in Green Bank, West Virginia. He wanted to build up this newest method of observation, but soon resigned as he felt that he did not have the necessary scientific acumen to be a leader on the project. He travelled and did some more observing in California before being hospitalized and dying in 1963 of complications from hepatitis, which he had most likely contracted during the Russian Civil War.[172]

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[171] Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 284, 306–309.
[172] Leo Goldberg, "Interview with Leo Goldberg," interview by Spencer Weart, May 16, 1978, Niels Bohr Library & Archives, American Institute of Physics, http://www.aip.org/history/ohilist/4629_1.html; Osterbrock, *Yerkes Observatory, 1892-1950: The Birth, Near Death, and Resurrection of a Scientific Research Institution*, 307–308.

Struve, over the course of his career, had changed the way that astrophysics was practiced. Russell wanted to make astrophysics more physical, and Struve similarly wanted to "make astronomy the tool of modern physics."[173] The difference between the two men was in the type of scientific capital that they built up. In addition to the form of capital gained through well-regarded research, which Struve had and Russell had in abundance, Struve built up the scientific capital granted by administrative positions. From there he was able to influence the field more widely than Russell ever had. In this Struve was aided by the historical circumstances that made observatory directors the most powerful men in the field. Struve put one of the two best telescopes in the world, McDonald, at the service of teams of observers and theorists working together. He edited *The Astrophysical Journal* and made Chandrasekhar associate managing editor so that the danger that the journal might stop printing theoretical articles, however small it may have actually been, would never come to pass. He put in place a graduate teaching program that emphasized physics and theory, so that American astronomers would benefit from what he saw as the advantages enjoyed in Europe, and they would do it in an environment where they could work with one of the best telescopes in the world. Some of those students spread to the Mount Wilson and Lick observatories in California, where they began to change the cultures of those observatories so that they were more like Yerkes. Finally, when Struve left Yerkes, and then later when he left Berkeley, he made sure to leave those who had similar ideas to his in a position to inherit the scientific capital that he had accumulated. The power of observatory directors went into steep decline with the advent of large-scale government funding brought on by the Cold War, so some of the capital that Struve had left to others

[173] Struve, "Memorandum."

dissipated. He nonetheless had succeeded in bringing theorists firmly into the fold in America, where the best instruments were found.[174]

[174] For more on the influence of government spending on the field of astronomy, see 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, no. 1 (January 1, 2000): 55-92.

CONCLUSION

By the time of Otto Struve's death in 1963, the era of supremely powerful observatory directors was coming to a close. The government funding and multi-institution projects that had come to dominate American physics since World War II slowly made their way into astronomy as well.[175] Just as the government as a major new source of funding decreased the power and administrative scientific capital of observatory directors, new technologies expanded astrophysics and devalued the scientific capital held by large-reflector observatories. It is not that the major American observatories such as Mount Wilson, Palomar, Lick, and McDonald were suddenly able to contribute less to the science, but rather that the science expanded around them with the arrival of radio and upper-atmosphere astronomy. New technology allowed astronomers to build large radio telescopes that observed spectra in the previously invisible radio band of the electromagnetic spectrum. Similarly, the development of rocket technology allowed them to begin observing x-rays and other high-frequency electromagnetic radiation up above the earth's atmosphere, which absorbed it before it could reach the surface. Both of these areas required great sums of money to be built up and were largely funded by the government, which put them out of the hands of the observatory directors.[176]

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[175] Ibid.

[176] Woodruff T. Sullivan, "Early Radio Astronomy," in *The General History of Astronomy: Volume 4, Astrophysics and Twentieth-Century Astronomy to 1950: Part A*, ed. Owen Gingerich (New York: Cambridge University Press, 1984), 190-198; Smith, "Remaking Astronomy: Instruments and Practice in the Nineteenth and Twentieth Centuries."

Struve regretted the intrusion of government funding in astronomy, calling it the end of the "Golden Age" of astronomy, which Struve believed had stretched back into the seventeenth century, a time during which astronomy was pursued without any need for practical applications and "only sought truth for its own sake."[177] Nevertheless, Struve had influenced the practice of astrophysics, particularly in America, at least through the 1950s, when the prominence of large observatories began to fade. Whether the teaming of theorists and observers continued, or whether those categories even continued to hold any meaning is an area that requires further study. Struve had also played a large role in getting astrophysical theory into American universities, particularly at the University of Chicago and the University of California at Berkeley.

The shape of Struve's career and how he was able to influence the field of astrophysics come particularly into focus when viewed through the lens of Pierre Bourdieu's theory of scientific capital. Bourdieu's idea that scientific capital is not just wielded by those whose scientific research is considered extraordinary, but also by those who hold the best administrative positions in the field helps to reveal how Struve both gathered and used scientific capital despite his relative lack of training in advanced mathematics and physics. Additionally, by looking at what was most valued in the field—superior instrumentation and high-status colleagues—we are able to determine just what sort of a power base Struve built at Yerkes and McDonald during his tenure as director. Bourdieu's ideas make visible the influence of a scientist who would have been largely invisible to Thomas Kuhn's model of paradigms and revolution.[178]

There are several areas that merit further research in order to fully understand the role of Struve in astrophysics, as well as the distribution and influence of scientific capital in the field

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[177] Struve and Zebergs, *Astronomy of the 20th Century*, 5.
[178] Bourdieu, *Science of Science and Reflexivity*, 34–36, 57; Kuhn, *The Structure of Scientific Revolutions,* passim.

during the first half of the twentieth century. There are a great many archival sources still left to examine at the University of Chicago, which houses the archives from Yerkes Observatory. These include thousands of pages of correspondence as well as old laboratory notebooks and other miscellanea that were moved there from the observatory. Some of this material may reveal more about how Struve went about pairing together his observers and theorists. How active a role did he take in getting them together? Did he assign projects to groups of staff members or did he let them form organically? There are also questions regarding Struve's relationships with the other observatory directors. The correspondence between Struve and other directors that was examined in writing this thesis is all very cordial, but that may be professional courtesy hiding the true nature of these relationships. How much notice did the other major directors take of the particular way that Struve was changing practice at Yerkes and McDonald, and did any of them object to it? These questions can only be answered by delving further into Struve's letters as well as the archives of the other major observatories. Additionally, applying Kuhn's model to astrophysics in general during this period and the period immediately after may reveal more about the state of the field itself. It seems that there were no major upheavals in scientific understanding during Struve's career, but a more careful examination may reveal otherwise. These and many other questions need to be answered in order to develop a full understanding of the dynamics of the field of astrophysics during the twentieth century.

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BIOGRAPHICAL SKETCH

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