1993

The birth of American astrophysics: the development of a science in its cultural context

Orville Roderick Butler
Iowa State University

Order Number 9321124

The birth of American astrophysics: The development of a science in its cultural context

Butler, Orville Roderick, Ph.D.

Iowa State University, 1993

U·M·I
300 N. Zeeb Rd.
Ann Arbor, MI 48106

The birth of American astrophysics:

The development of a science in its cultural context

by

Orville Roderick Butler

A Dissertation Submitted to the

Graduate Faculty in Partial Fulfillment of the

Requirements for the Degree of

DOCTOR OF PHILOSOPHY

Department: History
Major: History of Technology & Science

Approved:

Signature was redacted for privacy.

In Charge of Major Work

Signature was redacted for privacy.

For the Major Department

Signature was redacted for privacy.

For the Graduate College

Members of the Committee:

Signature was redacted for privacy.

Iowa State University
Ames, Iowa

1993

DEDICATION

To Hsieyun Serena Butler-Yang, who has encouraged her father in ways that she will never know, and to Xiaohua Yang, whose support and organizational skills have made finishing this dissertation so much easier.

TABLE OF CONTENTS

INTRODUCTION

Most historians have found the birth of astrophysics in the application of the spectroscope to the study of the sun and stars. They have generally traced its roots to Bunsen and Kirchhoff's 1859 paper which suggested the usefulness of the spectroscope for astronomical research and have found the development of astrophysics as a profession in the employment of the spectroscope, and other appliances in the laboratories of nineteenth century physicists, for research into the chemical and physical makeup of heavenly bodies.

A history which concentrates on the role of instrumentation in the development of astrophysics is, at best, incomplete. As important as instrumentation was, and is, in astrophysical research, separating astrophysics from the intellectual questions and the cultural issues which gave rise to those questions removes from the historical record answers to some questions about the nature of the science and why it came to be a specialized professional field.

Contrary to the views of the positivists, and others who see science as providing objective truth, scientific ideas do not develop in a cultural vacuum any more than political, religious or social ideas. To the extent that the motivation and even ideas for research in a field result from the impact of broader social and cultural issues, science takes place in a cultural context. The questions raised by scientists, the answers they give and the way they answer those questions are often part of a response to issues raised by the broader culture.

The role of these questions and answers in the birth of American astrophysics helps to illuminate one aspect of the role of a culture in the development and eventual professionalization of a science. Research into the application of the laws of mechanics, and particularly the application of Newton's theory of gravitation, to the development of an understanding of the makeup and evolution of heavenly bodies, as well as the study of solar heat and its influence on the earth, took place within the context of broader cultural interest, input and demands upon the criteria which the answers would meet.[1]

The nebular hypothesis was widely discussed in American literature as well as scientific papers beginning in the early 1840s. In the nineteenth century the nebular hypothesis included components which "explained" the collapse from nebulous material into stars, the nature of the sun and possible explanations of the sun's heat source. It

---

[1]From the perspective of many modern astronomers and astrophysicists, the field of astrophysics, as it was defined at the end of the nineteenth century, was closer to the "new astronomy" then the specialized research of today. The modern self-described "astronomer" is as likely to use the astrophysical principles and study the problems reserved to the astrophysicist in the 1890s. The astrophysicist of today is more likely to specialize in the study of specialized fields of physics--high energy physics or plasma physics for example--in which stars play as much the role of a laboratory as they do the object of modern astrophysical study itself.

influenced American thought, providing a challenge to traditional theological explanations of creation and ultimately of the origin of humankind. It provided an issue around which scrutiny into the nature, role and realm of valid scientific inquiry, as well as into the interrelationship between scientific and theological truths, took place.

Utilized by Herbert Spencer and his American disciples the nebular hypothesis became a crucial starting point for their development hypothesis and notions of progress which enamored the American public in the second half of the nineteenth century. The application of the nebular hypothesis, by Lord Kelvin and others, to calculations of the age of the solar system and the earth, created turmoil within the geological community when the results provided an inadequate time frame for then current theories of geological and biological evolution.

The extra import given the nebular hypothesis by those seeking to reconcile science and religion or by those seeking to provide their philosophy with scientific credibility leavened the debate over its validity and encouraged American astronomers, along with their European counterparts, to search for a mechanism by which it might be validated or disconfirmed for all time. The attempts by American astronomers to affirm or falsify the nebular hypothesis can be seen as one cultural component of the development of astrophysics in America.

In many respects the study of the causes and nature of the sun's heat provided a more intimate connection between American culture and the rise of astrophysics in the United States. With the westward movement of the American population onto the plains, land speculators, farmers and advocates of westward expansion eagerly sought after scientific theories which would allow farmers to plan their production or potentially control their environment. The connection between sunspot activity and terrestrial magnetism had already been established.[2] Speculation, and the resulting scientific studies, regarding the connection between solar heat and terrestrial meteorology was encouraged by those seeking to settle the West.

Some astronomers and early astrophysicists, most notably Samuel Pierpont Langley, were equally influenced by the rise of pragmatic philosophy. Pragmatic philosophy gave meaning to terms and validity to beliefs and theories based upon the sensible measurable consequences of those assumptions and propositions. In America pragmatism began as a

---

[2]In 1722 James Graham noticed a daily cycle in the declination of a magnetic needle. In 1851, John Lamont found that the range of this variation was periodic and the period was approximately ten years.

In 1828 Alexander von Humboldt called for a study of terrestrial magnetism. A wide ranging network of stations were set up in the hopes of finding some law explaining occasional spasmodic vibrations of magnetic needles, which Humboldt called storms. In 1852 Edward Sabine noted that the occurrence of these vibrations was cyclic and the period was roughly equal to that established by Heinrich Schwabe for sunspots. Rudolf Wolf and Alfred Gautier independently came to similar conclusions later the same year.

philosophy of science. As such it influenced astronomers and physicists who validated their theories by the consequences they would have in everyday experience.

The nebular hypothesis provided the dominant source for accepted theories of solar heat, again enhancing its import. But the connection or potential connection between solar spots and earthly patterns of rainfall led some astronomers and physicists to attempt to develop theories of the sun which would provide testable measurements of their accuracy in long-range weather prediction.

This dissertation is a study of the development of these two components of astrophysical research in America prior to the explicit composition of astrophysics as a separate field of study. It will trace the reasons scientists attempted to answer questions regarding the nebular hypothesis and the relations between solar heat and terrestrial meteorology in an attempt to show the evolution of these subjects in America and how they were eventually assimilated into a body of knowledge called astrophysics compiled by a community of researchers that became known as astrophysicists. Finally, this dissertation provides some insights into how the development of a scientific community, working in a defined field, affects relationships with and attitudes towards those in the broader culture who addressed corresponding questions.

CHAPTER 1

THE NEBULAR HYPOTHESIS IN AMERICAN THOUGHT: CONFLICT AND UNITY, 1830-1860

The nebular hypothesis--the term given to theories regarding the evolutionary condensation of the solar system from a nebula--played a major role in mid-nineteenth-century thought. Immanuel Kant had proposed a nebular hypothesis in 1755, while similar theories found independent expression in the writings of the French mathematician-astronomer Pierre Simon Marquis de Laplace in 1797.[1] In Europe Hermann von Helmholtz would derive his 1853 theory of solar heat, drawing support from Kant's nebular hypothesis.[2] Sir William Huggins's spectroscopic research verifying the gaseous nature

---

[1]A.J. von Oettingen, foreword to Allgemeine naturgeschichte und theorie des himmels, by Immanuel Kant, ed. A.J. von Oettingen (Leipzsig: Wilhelm Engelmann, 1898); Immanuel Kant, Universal natural history and theorie of the heavens, ed. Stanley Jaki (Edinburgh: Scottish Academic Press, 1981), 27; Ludwig Ernst Borowski, Immanuel Kant: Sein leben in darstellungen von zeitgenossen/Die Biographien von L.E. Borowski, R.B. Jachmann und A. Ch. Wasianski (Berlin: Felix Gross, 1912), 89; Ernst Cassirer, Kant's life and thought (New Haven and London: Yale University Press, 1981), 40. According to F. Krafft, most copies of Kant's Allgemeine naturgeschichte were destroyed in a fire. Immanuel Kant, Allgemeine naturgeschichte, ed. F. Krafft (Munich: Kindler, 1971), 193. However, others have called this assertion into question. Cf. Stanley Jaki in Kant, Universal natural history and theorie of the heavens, p. 222, n. 8. According to Jaki, there are six extant copies of Kant's first edition in European and seven in North American Libraries. p. 222, n. 7.

[2]Stanley L. Jaki, "Introduction," in Immanuel Kant, Universal natural history and theory of the heavens (Edinburgh: Scottish Academic Press, 1981), 51-54.

of the Orion nebula in the 1860s was at least partially motivated by concerns about the nebular hypothesis.

Kant's nebular hypothesis had relatively little influence in America until about the middle of the nineteenth century, although his philosophy would influence American transcendentalists who were drawn to speculation about the evolution of the heavens as well. Historians have found Laplace's influence in America to be even greater than in Europe.[3] Speculations derived from the nebular hypothesis motivated research by Stephen Alexander, Daniel Kirkwood, Pliny Earl Chase, Benjamin Gould, Simon Newcomb, and Gustavus Hinrichs. Joseph Henry and Benjamin Peirce provided strong support for research on the nebular hypothesis, while Ormsby Macknight Mitchel's lectures on astronomy provided one of many popular expressions of Laplace's theory.

The interest of these mid-nineteenth-century astronomers in the nebular hypothesis went well beyond simple observation. Several, though not all, lacked the instrumentation to adequately survey the details of many known nebulae. Their interest included theoretical speculation, models and analogies which helped to illustrate the operation of the laws of physics in the construction of heavenly bodies from nebulae to the solar system and its components. Some went even further to speculate on a

---

[3]Ronald L. Numbers, Creation by natural law: Laplace's nebular hypothesis in American thought (Seattle: University of Washington Press, 1977), 65-66.

connection between the operation of these laws and more general development hypotheses sometimes, though not necessarily, tied to a theory of evolution. The astronomers and physicists who initiated astrophysical research programs in the United States at the end of the century were profoundly influenced by their mid-nineteenth-century predecessors.

In philosophical circles critics of common sense philosophy, while condemning the development of a nebular hypothesis based on empirical science, followed Kant in deriving a nebular hypothesis from a priori ideas. The popularity of German idealism and transcendentalism in mid nineteenth century American thought encouraged the acceptance of theories similar to the nebular hypothesis even though they remained critical of scientific methodologies. American astronomers in the second half of the nineteenth century could hardly have avoided the discussion of the nebular hypothesis of either Kant or Laplace. They would find in the spectroscope and the new astronomy mechanisms to test the speculative theories of their milieu.

The essential features of Kant's cosmology were spread in America in articles which focused on the theological issues surrounding the discussion of the nebular hypothesis in America. As a result Kant's nebular hypothesis remained closely tied to his philosophical and religious polemics. The nebular hypothesis, as it was discussed in scientific circles would generally be explicitly associated with Pierre Simon Marquis de Laplace, one of the leading late eighteenth-century French Newtonians, who set forth his evolutionary cosmological views in his 1796 publication Le Système du Monde.[4] Unlike Kant's Allgemeine naturgeschichte, Laplace's work was readily available to English as well as French readers with five French editions by 1825 and English translations of the first edition in 1809 and the fifth edition in 1830.[5] His cosmology was further summarized in a chapter of his famed Mécanique Célèste which became a standard reference work for nineteenth century astronomers. The Mécanique Célèste's influence on American astronomy was abetted by Nathaniel Bowditch's four-volume American translation published in the 1830s.[6]

Strictly speaking, Kant's nebular hypothesis and that of Laplace cannot be equated. Kant's primary concern dealt with the formation of the universe as a whole according to necessary laws, while Laplace sought to show the evolutionary development of the solar system. The indirect dissemination of Kant's cosmology, however, aided by the numerous

---

[4]P.S. Laplace, Exposition du Système du Monde (Paris: Impr. du Cercle-Social, [1796]).

[5]P. S. Laplace, The system of the world, trans. J. Pond. (London, R. Phillips, 1809) and P.S. Laplace, The system of the world, 5th edition, trans. Henry H. Harte (Dublin: University Press, 1830).

[6][Benjamin Peirce], "Bowditch's translation of the Mécanique Célèste," North American Review 48 (January 1839): 173-177; Laplace, Mécanique Célèste, trans. and ed. Nathaniel Bowditch. (Boston: Hillard, Grey, Little & Wilkins, 1829-1839). A second edition was published in 1840.

similarities in both theories, led to an assertion, at least occasionally, of a Kant-Laplace nebular theory.[7]

In America, supporters of the two cosmologies divided not so much on the nebular hypothesis itself as on the means for supporting the nebular hypothesis. Followers of Laplace's cosmology tended to emphasize the empirical and observational evidence in support of the nebular hypothesis. Kantian idealists, on the other hand, criticized the efficacy of sense data and preferred to develop _a priori_ support from which they derived the necessary laws of the universe. Some scientists, such as Benjamin Peirce at Harvard, would attempt to reconcile the two and at the same time reconcile tensions between evolution and theology by finding in the nebular hypothesis the working out of the mind of God as manifested in the heavens.[8]

A version of the nebular hypothesis was independently developed in America by Isaac Orr and Alexander Metcalf Fisher. Orr, a minister and teacher at Gallaudet's Asylum for the Deaf and Dumb before serving as secretary of the African Education Society, was assisted in his cosmological

---

[7]By the 1870s the nebular hypothesis occasionally was associated with both Kant and Laplace in the _Journal of the Franklin Institute_ and in the lectures of Charles Augustus Young. No study has yet been made of the use of the Kant-Laplace terminology. Cf. "Constitution of nebulae," _Journal of the Franklin Institute_, 107 (March 1879): 202 and "The planetary system. Professor Young's fourth lecture." New York _Daily Tribune_, 24 January 1883, 3.

[8]Benjamin Peirce, _Ideality in the physical sciences_, ed. James Mills Peirce (Boston: Little, Brown & Co., 1881).

developments by Fisher, professor of mathematics and natural philosophy at Yale. Fisher was to have presented their theory to European scientists but drowned when his ship sank in route to Europe in 1822. Orr turned to Nathaniel Bowditch for intellectual support and legitimacy only to learn that he had been anticipated by Laplace.[9] Nevertheless his theory was published in the American Journal of Science in 1823, eliciting but little attention.[10]

Laplace's theory received occasional mention in American journals in the late 1820s and early 1830s,[11] but widespread discussion awaited the theological debates surrounding the publications of William Whewell's Astronomy and general physics considered with reference to natural theology, Thomas Chalmers' On the power, wisdom, and goodness of God as manifested in the adaption of external nature to the moral and intellectual constitution of man, and John Pringle Nichol's Views of the architecture of the heavens.[12]

---

[9]Isaac Orr, "An essay on the formation of the universe," American Journal of Science 6 (1823): 148.

[10]Ibid.

[11][Nathaniel Bowditch], "Modern astronomy," North American Review 20 (April 1825): 354, 364-366 and "Astronomy of Laplace," American Quarterly Review 7 (June 1830): 279.

[12]William Whewell, Astronomy and general physics considered with reference to natural theology, 3d ed. (London: William Pickering, 1834); Thomas Chalmers, On the power, wisdom, and goodness of God as manifested in the adaption of external nature to the moral and intellectual constitution of man (London: William Pickering, 1835); and John Pringle Nichol, Views of the architecture of the heavens (Edinburgh: William Tait, 1837).

Both Whewell's and Chalmers' comments on the nebular hypothesis appeared in their contributions to the Bridgewater Treatises. Funded by a provision in the will of Reverend Francis Henry Egerton, the eighth Earl of Bridgewater, the treatises were supposed to be written "On the power, wisdom, and goodness of God, as manifested in the Creation."

The youngest of the eight authors, William Whewell had been elected a fellow of Trinity College, Cambridge in 1817. In the first half of the nineteenth century, he became one of England's leading historians and philosophers of science. A disseminator, rather than a practitioner of science, Whewell saw his object in writing a Bridgewater treatise "to lead the friends of religion to look with confidence and pleasure on the progress of the physical sciences, by showing how admirably every advance in our knowledge of the universe harmonizes with the belief of a most wise and good God."[13]

Whewell devoted a chapter to a discussion of Laplace's nebular hypothesis. According to Whewell, Laplace believed that the stability of the solar system was not the result of chance but that a "primitive cause" had guided planetary motions. However, unlike Laplace, Whewell went on to conclude that the arrangement of the solar system must be "the work of an intelligent and most powerful being."[14]

---

[13]Whewell, Astronomy and general physics, iv.

[14]Ibid., 181.

Whewell began Laplace's conjectures with a nebulous sun surrounded by an atmosphere extending beyond the orbits of the present day planets. As the nebulosity cooled and the solar atmosphere contracted, the rotation of the sun about its axis increased and the exterior regions of the atmosphere would spin off into rings, such as Saturn's rings, or coalesce into a planetary mass orbiting the sun. While clearly sympathetic to the nebular hypothesis, he refused to call it any more than "a conjecture only."[15] As such the nebular hypothesis presented no threat to Christianity if its doctrines could not be reconciled with Laplace's "arbitrary guesses and half-formed theories."[16]

Thomas Chalmers published the only Bridgewater Treatise with comments hostile to Laplace's nebular hypothesis. While Chalmers' primary concerns were with theology, he was already well known and widely read for his publications reconciling astronomy with evangelical theology. In 1817 he published <u>A series of discourses on the Christian revelation, viewed in connection with the modern theology</u>. The work went through nine editions, including three American printings by 1818, becoming perhaps the most read series of sermons of the early nineteenth century. Eventually twelve editions were published with regular printings and inclusion in collections published in both Britain and America through

---

[15]Ibid., 183.

[16]Ibid.

the 1860s. The Discourses did not discuss the nebular hypothesis, but Laplace's theory was soundly criticized in Chalmers' On the power, wisdom, and goodness of God, as manifested in the adaptation of external nature to the moral and intellectual constitution of man which went through six American printings in two editions by 1849.[17] Chalmers accused Laplace of weakening the argument for the existence of God from design by reducing the formation of the solar system to the results of the law of gravitation. He referred to the nebular hypothesis as merely an "attempted demonstration." Its implications for the argument from design could be countermanded, he suggested, by arguments, such as those drawn from the construction of an eye, which emphasized "the dispositions of matter more than in the laws of nature."[18]

Whewell and Chalmers provided the framework for the religious debates over Laplace's nebular hypothesis during the religious revivals in the first half of the nineteenth century. But John Pringle Nichol did far more to garner popular support in America for Laplace's views. Nichol has been credited with familiarizing tens of thousands in the

---

[17]Thomas Chalmers, Discourses on the Christian revelation, viewed in connexion [sic] with the modern astronomy, (Edinburgh: Thomas Constable & Co., 1854) and Thomas Chalmers, On the power, wisdom, and goodness of God, as manifested in the adaptation of external nature to the moral and intellectual constitution of man (London: William Pickering, 1835), 1:132-35.

[18]Ibid., 33-34.

English-speaking world with the nebular hypothesis.[19] George Gilfillan in 1855 described Pringle as the "prose laureate of the stars," claiming that he had "done more than any man living to uncase science from its mummy confinements, and to make it walk abroad as a free and living thing."[20]

Nichol had studied for the ministry but had turned to a career in science. He became professor of astronomy at the University of Glasgow in 1836, remaining there until his death in 1859.[21] His Views of the architecture of the heavens went through eleven editions, including two American editions, between 1836 and 1855. Written as a series of letters to a lady, the seventh letter dealt with what Nichol called "Laplace's bold and brilliant induction," which he declared "includes and resolves all."[22] The zodiacal light and the retardation of Encke's comet in an apparently resist-

---

[19]Stanley L. Jaki, Planets and planetarians: A history of theories of the origin of planetary systems, (New York: John Wiley & Sons, 1978), 140.

[20]George Gilfillan, "John Pringle Nichol" in A gallery of literary portraits, (Edinburgh, 1855), 2:254-5.

[21]Stanley L. Jaki, Planets and planetarians, 140.

[22]Nichol, Views of the architecture of the heavens, 179. Nichol's American audience certainly was not limited to scientists and academics. Ronald Numbers, in Creation by natural law, asserted that Nichol's Views of the architecture of the heavens was the "single book most responsible for bringing the nebular hypothesis to the attention of the American reading public." While Pringle's readership included literary figures such as Ralph Waldo Emerson and Edgar Allen Poe, it also apparently included many among the general reading public who also made his speaking tour in 1848 a public success.

ing medium provided sure evidence, in Nichol's opinion, that the sun was once a nebular body.[23]

Additional support for Laplace's theory was drawn from Herschel's research on the rotation of nebulae and globular clusters. Herschel's observations of ring nebulae were cited as a particular stage which a nebula would pass through in condensing into a solar system.[24] Readers were provided with diagrams illustrating the detachment of rings from a rotating solar nebula. These rings, Nichol asserted, would condense into a single planet. The nebular hypothesis even answered Newton's concerns about planetary stability. Laplace's theory inferred that all planets would have basically a circular motion in the same direction. Their

---

[23]Some astronomers argued that the sun's reflection of nebulous particles was the cause of the zodiacal light. Similarly, the retardation of Encke's comet was explained by resistance caused by remnants of the nebular material out of which the sun was made.

[24]While Herschel initially viewed nebulae and star clusters as providing evidence of "attractive powers" collecting stars into regions where they had initially been slightly more dense, he also believed that all nebulae were resolvable into stars. In 1790 he observed the ring nebula NGC1514 which convinced him that some nebulae were condensing into stars. By 1811 he argued that nebulosity could gradually condense into stars and from stars into dense star clusters. William Herschel, "On nebulous stars properly so called," Philosophical Transactions 81 (1791):71-88 and Herschel, "Astronomical observations relating to the construction of the heavens, arranged for the purpose of a critical examination, the result of which appears to throw some new light upon the organization of celestial bodies," Philosophical Transactions 101 (1811): 269-336. In the mid-nineteenth century, Lord Rosse reduced Herschel's classification of nebulae to approximately that of today.

perturbations, Nichol maintained, would, therefore, most likely be periodical.[25]

In the winter of 1847-1848 Nichol toured the United States lecturing on astronomical topics[26]. His seven lectures before the Mercantile Library Association of New York in January and February, 1848, were reported in the New York Tribune. He claimed they were designed to bring before his listeners the "contemplations which are pressed upon us by a consideration of the laws which govern the great Universe. . . ." Those laws had been discovered in the "grand task of exploring through the mazes of creation the mighty ordinances of God."[27] The recent declarations by Lord Rosse in England and by William Cranch Bond at Harvard that they had resolved the Orion nebula into stars apparently led Nichol to modify his assertions supporting the nebular hypo-

---

[25]Ibid., 180-182.

[26]Nichol's itinerary included at least Harvard and Cincinnati in addition to New York. Cf. Frederick W. Conner, "Poe & John Nichol. Notes on a source of Eureka" in All these to teach: Essays in honor of C.A. Robertson, ed. Robert A. Bryan et. al. (Gainesville: University of Florida Press, 1965), 199.

[27]John Pringle Nichol, Views of astronomy. Seven lectures delivered before the Mercantile Library Association of New York in the months of January and February, 1848. (New York: Greeley & McElrath, 1848), 5.

thesis.[28] In the fifth lecture, which was on the nebular hypothesis, Nichol would avow that Laplace's theory was no more than a "hypothetical cosmogony."[29] However, he left little doubt that he was convinced of its verity.

Nichol began expressing caution about the validity of the nebular hypothesis in his Thoughts on some important points relating to the system of the world published in 1846. The resolution of the Orion nebula "into a superb cluster of Stars" necessitated "important changes in previous speculations in Cosmogony."[30] He contended that it was not likely "that anyone who now well considers its logical position, will endeavor to erect it [the nebular hypothesis] into a truth."[31] While Laplace's nebular hypothesis could no longer

---

[28]William Parsons, third earl of Ross, was best known in the 19th century for his astronomical research on nebulae utilizing giant reflecting telescopes. His "Leviathan of Parsonstown," completed in 1845, utilized a seventy-two inch mirror and had a focal length of fifty-four feet. It had the greatest light gathering power of any telescope in its day. Parsons served as President of the Royal Society from 1848 to 1854.

William Cranch Bond, originally a Boston clock maker, gained a reputation as a chronometer rater for several early American survey expeditions. In 1839 he was invited to set up his equipment at Harvard and serve as an astronomer there. In 1843 popular interest in a comet, utilized by some supporters of William Miller as evidence in the heavens of Christ's imminent return, resulted in funding or a 15" refracting, equal to the then largest ad most powerful refractor in the world.

[29]Ibid., 29.

[30]John Pringle Nichol, Thoughts on some important points relating to the system of the world, 1st American ed. (Boston and Cambridge: James Munroe & Co., 1848), x.

[31]Ibid., 120, 125.

be endowed with a "demonstrative or strictly deductive character," it could still be supported on the grounds of its inherent beauty.[32] According to Edgar Allen Poe, a letter purporting to be from Nichol to an American friend was printed in some papers in 1846 in which Nichol admitted the necessity of abandoning the nebular hypothesis.[33] By the third edition of Contemplations on the solar system, Lord Rosse's claim of resolving the Orion Nebula into stars led Nichol to all but reject the nebular hypothesis.

> The magnificent telescope of Lord Rosse--an instrument which has probably carried us to the farthest verge of space that man's vision will ever reach--has resolved the great nebula of Orion into stars. . . . On the occasion of this discovery, I performed what I considered a duty especially owing by me to the public, by avowing my opinion that the hypothesis in question is no longer tenable. . . .
> No such fact as the condensing of nebulous matter into organized stars can now be seen in the heavens; so that LAPLACE'S fundamental tenet--that the sun originated in the gradual condensing of a gaseous or vaporous mass, must henceforth be regarded as a pure hypothesis; and, as a matter of course, the whole of his ingenious deductions can be received only as a plausible cosmogony.[34]

Even with this caveat, he went on to

> confess a disposition still to receive the agreement of its results with so many of the vital arrangements of our system, as recommendations of the general truth of the theory, or at least of its neighbourhood to the truth--recommendations of as strong a kind as we are possibly entitled to expect in reference to processes of nature so

---

[32]Ibid., 106-107.

[33]Roland W. Nelson, "The definitive edition of Edgar Allan Poe's Eureka: A prose poem" (Ph.D. diss., Bowling Green State University, 1974), 80.

[34]John Pringle Nichol, Contemplations on the solar system, 3d ed. (Edinburgh: John Johnstone, 1847), viii-ix.

far removed from any epoch of observation, and of magnitude so overwhelming.[35]

In spite of this and other caveats, the book focused on countering objections to the nebular hypothesis, going so far as to explain the retrograde motion of Uranus' satellites by some unknown external disturbance. These same issues would become the focus of debate surrounding the nebular hypothesis for the next few years.

Nichol's 1850 publication of The planetary system: Its origin and physical structure included little new information. However, a footnote added in press became the first published discussion in a work on the nebular hypothesis of Daniel Kirkwood's Law of the Spheres of Attraction. Kirkwood had taken a job as a teacher while a teenager in Maryland. After one year of teaching, he entered York County Academy in Pennsylvania to further his education. Four years later he was appointed instructor in mathematics. By 1849 he was teaching in the local school in Spottiswood, Pennsylvania.[36] In 1839 while still at York County Academy, Kirkwood had read a passage in Thomas Young's Mechanics which maintained that "both the progressive and rotary motions of

---

[35]Ibid., x.

[36]Kirkwood to Benjamin Apthorp Gould, 23 January 1850, American Journal of Science, 2d ser., 9 (May 1850): 398-399. In my discussion of Kirkwood I rely heavily on Ronald L. Numbers, "The American Kepler: Daniel Kirkwood and his analogy," Journal for the History of Astronomy 4 (1973): 13-21 and Numbers, "Daniel Kirkwood's analogy" in Creation by natural law: Laplace's nebular hypothesis in American thought (Seattle and London: University of Washington Press, 1977), 41-54.

the heavenly bodies were originally communicated by the same impulse."

For the next several years, Kirkwood sought to find a mathematical law relating the orbit and rotation of planets. Finally, after studying Laplace's nebular hypothesis in 1846 he derived his law based on the diameter 'D' of a planet's sphere of action and the number 'n' of rotations in each orbit about the sun.[37] The diameter of a planet's sphere of action was defined as the width of the ring or rings which had condensed into that planet and was generally calculated to be the distance between the midpoints between the neighboring planetary orbits. Kirkwood found that the ratio of the squares of the rotations of two planets was equal to the ratio of the cubes of their diameters of sphere of action. Kirkwood's law received support at the American Association for the Advancement of Science meeting held at Cambridge, Massachusetts, in 1849, where Kirkwood was

---

[37]In mathematical formula, Kirkwood's law was

$$n^2/n'^2 = D^3/D'^3$$

where n and n' were the number of rotations about the respective planet's axis during one orbit of the sun. D and D', the spheres of attraction for the respective planets, were found by first obtaining the distance to the point of equal attraction between the planet and its next interior planet when the two planets were in conjunction. Then one repeated the process for the point of equal attraction for the next exterior planet. D and D' was sum of these two distances for the respective planets. Letter, Daniel Kirkwood to Sears C. Walker. Quoted in <u>Proceedings of the American association for the advancement of science. Second meeting, held at Cambridge, Mass. August, 1849</u> (Boston: Henry Flanders & Co., 1850), 208-210.

acclaimed a second Kepler. In The planetary system Nichol endorsed Kirkwood's law, feeling confident that with this evidence the essential truth of Laplace's hypothesis could no longer be doubted.[38]

Nichol's changing views regarding the nebular hypothesis probably reflected the changing public debate in both Europe and America. American scientists in the 1840s, like the populace at large, had serious doubts about Laplace's nebular hypothesis, in large part because of the claimed resolution of the Orion Nebula by Lord Rosse, in England and Professor George Bond, in America.[39] However, Kirkwood, as an American astronomer, provided American scientists with the possibility that the nebular hypothesis could be confirmed. The widespread debate over the nebular hypothesis combined with the belief that astronomy could find ways of testing Laplace's theory would be a powerful inducement for subsequent American astronomers to enter the debate. Kirkwood's law revived their faith and interest in the nebular hypothesis while Nichol's support enabled it to capture the imagination of America's reading public. Kirkwood's analogy gave new life to debates over the nebular hypothesis. Certainly Nichol's subsequent publications again extolled the veracity of Laplace's nebular hypothesis.

---

[38]John Pringle Nichol, The planetary system: Its order, and physical structure (London: H. Bailliere, 1850), 245-249.

[39]Numbers, Creation by natural law, 28, 35-36.

Nichol's final word on the nebular hypothesis came in his Cyclopaedia of the physical sciences published in 1857.[40] Here Nichol admitted that the nebular hypothesis could not be empirically verified. As a result, "the highest evidence that can be attained is a degree, greater or less, of probability."[41] The foundation of Laplace's speculations was "hypothetical," but that only meant that the assumptions of the nebular hypothesis could not be directly tested. Instead they could be indirectly tested by their ability to explain or solve numerous problems that "seemed otherwise inexplicable."[42] In fact, according to Nichol, a hypothesis was a heuristic device shaping research and predicting the results. Whether a hypothesis was valid was less important than whether it "stimulated men to think, and so did good service."[43] Laplace's nebular hypothesis provided a "causal solution" for the formation of Herschel's double stars, as well as deducing the general laws "characteristic of the

---

[40]John Pringle Nichol, "Nebular hypothesis," in Cyclopaedia of the physical sciences, comprising acoustics, astronomy, dynamics, electricity, heat, hydrodynamics, magnetism, philosophy of mathematics, meteorology, optics, pneumatics, statics, &c. &c. (London: Charles Griffin & Co., 1857). Citations are taken from the third edition (London: Charles Griffin & Co., 1868), 605-613.

[41]Ibid., 605.

[42]Ibid., 606.

[43]Nichol, "Hypothesis," in Cyclopaedia of the physical sciences, 457.

planetary system" and explaining the apparent retardation of the orbit of Encke's comet.[44]

If after the development of Kirkwood's analogy, Nichol was again willing to propound the veracity of the nebular hypothesis, his explication became increasingly mystical and pantheistic. The nebular hypothesis presented, what philosophy had long taught, "the universe united, compact, tending to one end--a type of its august CREATOR."[45] While such pantheistic views would be often attacked, they seemed to infiltrate the discussions of even those who objected to pantheism.

Most reviewers of The architecture of the heavens did not see that work leading towards pantheism. The New York Observer declared that the volume was "remarkably calculated to show the religious and even devotional tendencies of true science,"[46] while its more conservative competitor, the New York Evangelist only cautioned its readers:

> There is one reflection, which continually crowded upon the mind while on the wing, that the greatest themes of astronomic science could awaken no emotions of reverence towards the Eternal Creator, which the language of revelation is not adequate to express.[47]

---

[44]Nichol, "Nebular hypothesis," 610-612.

[45]Ibid., 608.

[46]Review of The architecture of the heavens, by John Pringle Nichol, In New York Observer 18 (1840): 158.

[47]Review of Views of the architecture of the heavens, by John Pringle Nichol, In New York Evangelist 11 (1840): 158.

In spite of this caution, Nichol's works remained a favorite source for conservative theologians seeking to glorify and confirm the existence of God through God's nature. As late as 1869, the Southern Review published an article drawn from three of Nichol's works. The anonymous author announced the object of the paper to be "to consider the solar system, not merely as a machine for turning out the greatest happiness of the greatest number, but as the manifold and sublime manifestations of an infinite and eternal Mind."[48]

Nichol was only one of many popularizers of science and astronomy in the 1840s, giving lectures on the nebular hypothesis in America. Indeed reaction to Nichol's 1848 lectures in New York suggests the extent to which the nebular hypothesis was being discussed. A letter in the New York Herald complained:

> "The general details and explanations were certainly appropriate but they were not new. In numerous publications and at repeated lectures we have listened to the same interesting theories. . . ."[49]

Some were not as popularly received as Nichol, while others had perhaps even broader hearings throughout America. All increased the popular interest in the nebular hypothesis and presumably the interest of both the untrained and the professional scientists and astronomers.

48"The solar system," Southern Review n.s., 6 (July 1869): 204.

49"Prof. Nichol's lecture of Friday last," New York Herald, 1 February 1948, 4.

Between 1840 and 1845, Dionysius Lardner, former professor of natural philosophy and astronomy at the University of London, toured the "chief cities and towns" of the United States, giving a series of sixty lectures on science and the arts. Lecture fifty-five presented William Herschel's theory of the structure of the universe and Laplace's nebular hypothesis. While Lardner emphasized the speculative nature of the nebular hypothesis, he nevertheless defended it against those who decried the atheistic moral tendency of the hypothesis.[50]

After 1845 Ormsby Macknight Mitchel presented a series of popular lectures on astronomy in Cincinnati and other cities, which included sections reconciling the nebular hypothesis with a belief in God. The popularity of Mitchel's lectures led to the subscription of funds to build the Cincinnati Observatory, at that time one of the largest in the world.[51]

During the 1850s and 1860s, Daniel Vaughan, a self-taught professor of chemistry in Cincinnati, presented a series of lectures and published several tracts presenting alternative solutions to theoretical problems raised by the nebular hypothesis. His interests suggested both the

---

[50]Dionysius Lardner, "The stellar universe," in Popular lectures on science and art; delivered in the chief cities and towns in the United States (New York: Greeley & McElrath, 1846), 395-396.

[51]Stephen Goldfarb, "Science and democracy: A history of the Cincinnati Observatory, 1842-1872," Ohio History 78 (1969): 172-178.

influence of the nebular hypothesis and an attempt to retain the fluid interpretation of light and heat.[52]

While lecturers on science and astronomy inspired popular acceptance of the nebular hypothesis in the 1840s and 1850s, both the acceptance of and the debate over the grounds for verification of the hypothesis were reinforced by the popular literature of Ralph Waldo Emerson and Edgar Allen Poe. Emerson was clearly aware of the nebular hypothesis by 1832. His journals included John Herschel's Discourse on the Heavens and Mary Somerville's Mechanism of the heavens among

---

[52]Otto Juettner, "Daniel Vaughan," in Daniel Drake and his followers: Historical and biographical sketches (Cincinnati: Harvey Publishing Co., 1909), 300-304; "The late Daniel Vaughan," Popular Science Monthly 15 (1879): 127-129; and "Professor Vaughan on the origin of asteroids," Popular Science Monthly 15 (1879): 570-571. Daniel Vaughan was generally critical of the nebular hypothesis as espoused by Laplace. While he wrote his papers within the context of the debate over the nebular hypothesis, his theories appear to be largely a return to the older view of heat and light as fluids. Vaughan's articles, peripherally related to the nebular hypothesis, included: Daniel Vaughan, "Researches in meteoric astronomy," in Report of the twenty-fourth meeting of the British association for the advancement of science; held at Liverpool in September, 1854 (London: John Murray, 1855), 26-27; Daniel Vaughan, Phenomena of the material world (Cincinnati: Longley Bros., 1856); D. Vaughan, "Secular variations in lunar and terrestrial motion from the influence of tidal action," in Report of the twenty-seventh meeting of the British association for the advancement of science; held at Dublin in August and September, 1857 (London: John Murray, 1858), 40-42; D. Vaughan, "On the light of suns, meteors, and temporary stars," in Report of the twenty-seventh meeting of the British association for the advancement of science; held at Dublin in August and September, 1857 (London: John Murray, 1858), 42-44; Daniel Vaughan, "On the solar spots and the variable stars," Philosophical Magazine, 15 (1858): 359-362; Daniel Vaughan, "On the phenomena which may be traced to the presence of a medium pervading all space," Philosophical Magazine, 21 (1861): 507-515; and Daniel Vaughan, "The origin of worlds," Popular Science Monthly, 15 (1879): 1-10.

his reading lists for that year. The latter work has been described as a digest of Laplace's Mécanique céleste.[53] However, the young Emerson did not yet accept evolutionary world views. While he proclaimed that the study of astronomy resulted in a corrected and exalted view of God in his sermon on astronomy, delivered four times between 1832 and 1836, he ignored the evolutionary consequences of the most recent advances of the science.[54] As late as 1836, he wrote in a discussion of idealism, that appeared in his Nature, that it necessarily

> beholds the whole circle of persons and things, of actions and events, of country and religion, not as painfully accumulated, atom after atom, act after act, in an aged creeping past, but as one vast picture which God

---

[53]Frederick William Conner, Cosmic optimism: A study of the interpretation of evolution by American poets from Emerson to Robinson (Gainesville: University of Florida Press, 1949), 381. The role of cosmology in the development of transcendentalism appears to be an important but inadequately studied topic. Transcendentalism was not a well defined philosophy and delineating the various components of influential transcendentalists is beyond the scope of this dissertation. Certainly science played an important role in Emerson's thought as he developed transcendentalism in the 1830s. Harry A. Pochman points out that Emerson before 1830 had only a secondary interest in science. After 1830 his interest in science became a "passionate pursuit for facts to reinforce his religion and his philosophy." German culture in America. Philosophical and literary influences 1600-1900 (Madison: University of Wisconsin Press, 1957), 167. Since American awareness of Laplacian cosmology began to develop at about the same time, it no doubt played an important role along with other sciences Emerson read.

[54]Ralph Waldo Emerson, "Astronomy," in Young Emerson speaks: Unpublished discourses on many subjects, ed. Arthur Cushman McGiffert, Jr. (Boston: Houghton Mifflin Co., 1938), 170-179.

> paints on the instant eternity for the contemplation of the soul.[55]

However his private writings and correspondence in the late 1830s and early 1840s increasingly approved of an evolutionary world view. Harry Hayden Clark has suggested that those writing on astronomy, including Mary Somerville and John Herschel, who discussed Laplace's nebular hypothesis, greatly influenced Emerson's decision to resign as the Unitarian minister at Boston's Old North Church in 1832.[56] Certainly Emerson was to attest late in his life that

> the paramount source of the religious revolution was Modern Science. . . . Astronomy . . . showed that our sacred history as our profane history had been written in gross ignorance of the laws which were far grander than we know; and compelled a certain extension and uplifting of our views of the Deity and his Providence.[57]

As early as 1826 Emerson had named Laplace as one of four "true _de facto_ sovereigns."[58] In his journal he regularly listed Laplace among the writers he read in the 1830s. By 1841 he had read Nichol's _Views of the architecture of the heavens_ and had spent a night at Nichol's

---

[55]Ralph Waldo Emerson, _Nature_, in _The complete works of Ralph Waldo Emerson_, Centenary ed., ed. with biographical introduction E.W. Emerson (Boston: Houghton Mifflin Co., 1903-4), 1:60.

[56]Harry Hayden Clark, "Emerson and science," _Philological Quarterly_ 10 (July 1931): 234.

[57]Emerson, _Works_, 10:335-336.

[58]Ralph Waldo Emerson, _Journals of Ralph Waldo Emerson_, ed. Edward Waldo Emerson and Waldo Emerson Forbes (Boston and New York: Houghton Mifflin Co., 1909-1914), 2:82.

Edinburgh Observatory during his 1842 visit to Europe.[59] He again visited Nichol in 1847 just before Nichol left on his American tour. Emerson provided him with letters of introduction to Henry Wordsworth Longfellow and Theodore Parker.[60] His relationship with Nichol was sufficiently close for him to recommend Nichol as a Lowell Institute lecturer and to entrust Nichol with correspondence to Mrs. Emerson.[61]

Emerson's cosmological speculations found public expression in an 1841 address at Waterville College titled "The method of Nature." As in Nature, Emerson contended that the world originated in the mind. However, his expression of the evolutionary nature of the universe was now clear and the cosmology on which it was based was evidently Laplacian:

> We can point nowhere to anything final; but tendency appears on all hands: planet, system, constellation, total nature is growing like a field of maize in July; is becoming somewhat else; is in rapid metamorphosis. The embryo does not more strive to be a man, than yonder burr of light we call a nebula tends to be a ring, a comet, a globe, and parent of new stars.[62]

In his "Woodnotes II," being revised during this period Emerson wrote

---

[59]R.L. Rusk, The life of Ralph Waldo Emerson (New York: C. Scribner's Sons, 1949), 339.

[60]R.W. Emerson to Henry Wordsworth Longfellow, 4 November 1847; and R.W. Emerson to Theodore Parker, 4 November 1847 in The letters of Ralph Waldo Emerson, ed. Ralph L. Rusk (New York: Columbia University Press, 1939), 3:433-434.

[61]Ibid, 434fn.

[62]Emerson, Works, 1:202-203. See also, Frederick William Conner, Cosmic optimism, 59.

Sweet the genesis of things,
Of tendency through endless ages,
Of star-dust, and star-pilgrimages,
Of rounded worlds, of space and time,
Of the old flood's subsiding slime,
Of Chemic matter, force and form,
Of Poles and Powers, cold, wet, and warm.[63]

Again in "The Song of Nature" written in the 1850s, Emerson's universal dame's assertions again suggest the influence of the nebular hypothesis.

I wrote the past in characters
Of rock and fire the scroll,
The building of the coral sea,
The planting of the coal.
And thefts from satellites and rings
And broken stars I drew,
And out of spent and aged things
I formed the world anew;
What time the gods kept carnival,
Tricked out in star and flower,
And in cramp elf and saurian forms
They swathed their too much power.
Time and Thought were my surveyors,
They laid their courses well,
They boiled the sea, and piled the layers
Of granite, marl and shell.[64]

Emerson's cosmology, while drawing from the nebular hypothesis, also reflected the idealistic concepts of German romanticism. In this respect his cosmology reflected the influence of Immanuel Kant's philosophy with which he was familiar.[65] Creation began and was traced out in a Primal

---

[63]Emerson, Works, 9:52.

[64]Emerson, Works, 6:15.

[65]According to Harry Hayden Clark, Emerson's idealist view of natural history was encouraged by Kantian transcendentalism. In his essay "The Transcendentalist" Emerson wrote: "The Idealism of the present day acquired the name Transcendental from the use of that term by Immanuel Kant, of Königsberg, who replied to the skeptical philosophy of Locke, which insisted that there was nothing in the intellect which was not previously in the experience of the senses, by showing that there was a very important class of

Mind of which all human minds were but a part. In its essence Emerson's cosmology was idealistic and therefore opposed to the mechanistic explanations of scientists. In Cosmic optimism, Frederick William Conner alleged that Emerson felt the sciences dealt only with surfaces while he saw the parts of nature to be only a reflection of the "Over-Mind" which was the ultimate reality. Plato's prime mover was at least one source for Emerson's Over-Mind and Emerson clearly connected Plato's cosmology with that of Laplace, claiming that he had been able "to anticipate the astronomy of Laplace."[66] But for Emerson each individual's mind was a part of this "Over-Mind." Thus Emerson could argue that

> Man carries the world in his head, the whole of astronomy and chemistry suspended in a thought. . . . Every known fact in natural science was divined by somebody, before it was actually verified.[67]

As a result, where empirical scientists might relegate the nebular hypothesis to laws of matter and motion, Emerson found only a "manifestation of a Creative Mind."[68] Some American astronomers, wrestling with the apparent dichotomy

---

ideas or imperative forms, which did not come by experience, but through which experience was acquired; that these were intuitions of the mind itself; and that he denominated them Transcendental forms." Cited in "Emerson and science," 229. Clark believes that many of the ideas of Transcendentalism derived from Kant came to Emerson via the works of Coleridge and Carlyle.

[66]Emerson, Works, 4:39.

[67]Emerson, Works, 3:183.

[68]Ibid., 184.

between religion and the nebular hypothesis, resolved it in a manner which sounded hauntingly like Emerson.

While Emerson's acceptance of Laplace's cosmology clearly influenced his view of nature, it resulted in no clear cosmological essay. His readers would no doubt acknowledge his support for Laplace's nebular hypothesis, but they would not obtain a detailed explication of it from his writings. The same can not be said of Edgar Allen Poe, who saw his cosmological work Eureka, published in 1848, as the culmination of his life work.[69]

Several reviewers have emphasized Eureka's assimilation of current scientific views. Certainly Poe drew from a large number of scientific and popular writers. Eureka was dedicated to Alexander von Humboldt whose Cosmos Poe briefly summarized in the book.[70] Poe had also read John Herschel's Preliminary discourse on the study of natural philosophy and his A treatise on astronomy, Laplace's The system of the world, which had been translated into English in 1830, and

---

[69]Roland W. Nelson, "Eureka: An introduction," in "The definitive edition of Edgar Allen Poe's Eureka: A prose poem (Ph.D. dissertation, Bowling Green State University, 1974), xxxvii. I have ignored some twentieth century writings comparing Poe's cosmology with the big bang theory. Such comparisons do not illuminate Poe's cosmology. See, for instance, Clayton Hoagland, "The universe of Eureka: A comparison of the theories of Eddington and Poe, Southern Literary Messenger 1 (May 1939): 307-313; George Nordstedt, "Poe and Einstein," Open Court 44 (March 1930): 173-180.

[70]Nelson, "Definitive edition of Eureka," 186-187.

both Whewell's and Chalmers' Bridgewater Treatises.[71] Perhaps it is not surprising that John Pringle Nichol provided the most significant influence, since he had been lecturing in New York in February 1848 at the same time that Poe had given his lecture on Cosmology which he turned into Eureka by the following July.[72] In fact, as Frederick W. Conner has pointed out, several of the quotes which Poe attributed to Nichol's The architecture of the heavens were drawn from the published accounts of Nichol's New York lectures.[73] That Poe was quite concerned that his acquaintances not confuse his theories with those of Laplace, suggests that he saw important similarities.[74]

In spite of the fact that Poe drew heavily from the popular science of his day, he was critical of the methods which scientists used. Early in Eureka, he announced, "I

---

[71]Poe notes the Bridgewater Treatises in his 1844 "Marginalia" published in the Democratic Review. Cf. James A. Harrison, ed., The complete works of Edgar Allan Poe, 2d ed. (New York: AMS Press Inc., 1979), 9-10. Poe cites Humboldt's, and Nichol's works by title in Eureka while Herschel's and Laplace's works cited only by author. Edward H. Davidson, Poe: A critical study (Cambridge: Harvard University Press, 1957) also lists Thomas Dick's, Celestial scenery: or the wonders of the planetary system displayed (New York, 1838) and The sidereal heavens and other subjects connected with astronomy (New York, 1840) and Nichol's The phenomena and order of the solar system (New York, 1842) as books which Poe most likely consulted.

[72]Frederick W. Conner, "Poe & John Nichol notes on a source of Eureka," 190-208.

[73]Ibid., 200.

[74]E.A. Poe to George W. Eveleth, February 29, 1848, in John Ward Ostrum, ed. Letters of Edgar Allan Poe (New York: Gordian Press, 1966), 360-362.

shall be so rash . . . as to challenge the conclusions, and thus, in effect to question the sagacity, of many of the greatest and most justly reverenced of men."[75]

Poe attacked Baconian empiricism, satirically calling it Hogianism, which he alleged "proceeded by observing, analyzing, and classifying facts . . . and arranging them into general laws."[76] In a letter to George Washington Eveleth, Poe identified "the chief of the . . . Hogites" as John William Draper.[77] Eveleth had apparently sent Draper a copy of Eureka for comment and had received less than a positive reply. But Poe was just as critical of empirical scientists generally in a letter to George E. Isbell:

> One thing is certain; that the objections of merely scientific men--men, I mean, who cultivate the physical sciences to the exclusion, in a greater or less degree, of the mathematics, of metaphysics and of logic--are generally invalid except in respect to scientific details. Of all persons in the world, they are at the same time the most bigoted and the least capable of using, generalizing or deciding upon the facts which they bring to light in the course of their experiments. And these are the men who chiefly write the criticisms against all the efforts at generalization--denouncing these efforts as "speculative" and "theoretical."[78]

---

[75]Edgar A. Poe, "Eureka: A prose poem" in The complete works of Edgar Allan Poe, 2nd edition, ed. James A. Harrison (New York: AMS Press, 1979), 16:185.

[76]Nelson, "Definitive edition of Eureka," 189.

[77]Edgar A. Poe to George W. Eveleth, June 26, 1849, in John Ostrum, ed., The letters of Edgar Allan Poe, 2:449.

[78]E.A. Poe to George E. Isbell, 29 February 1848 quoted in Arthur Hobson Quinn, Edgar Allan Poe: A critical biography (New York and London: D. Appleton-Century Co., 1941), 560.

In Eureka Poe was equally critical of those such as Aristotle and his disciples--Poe specifies Euclid and Kant--who began with "self-evident truths" and proceeded to results. Using such a system, Poe satirically proclaims that Aristotle obtained his place in history by demonstrating that sneezing was a "natural provision, by means of which over-profound thinkers are enabled to expel superfluous ideas through the nose."[79]

Having dispelled of empiricism and á priori deduction as sources of scientific truth Poe asserts that science "makes its most important advances . . . by intuitive leaps."[80] While for Emerson, consistency was "the hobgoblin of little minds,"[81] for Poe a "perfect consistency" was "nothing but an absolute truth."[82] The great scientists--he cites Kepler and Laplace--would speculate and theorize or guess. Their theories would be "merely corrected . . . ; cleared, little by little, of their chaff of inconsistency" until there remained "an unencumbered Consistency." Poe's Eureka was then, by his own account, speculation, but it was also, by his own account, truth. In his preface, Poe declared:

---

[79]Poe, Eureka, 188.

[80]Ibid., 189.

[81]Ralph Waldo Emerson, "Self reliance," in The collected works of Ralph Waldo Emerson (Cambridge, Mass.: Harvard University Press, Belknap Press, 1979), 2:33.

[82]Poe, Eureka, 196.

> to those who feel rather than to those who think--to the dreamers and those who put faith in dreams as in the only realities--I offer this Book of Truths. . . . What I here propound is true:--therefore it cannot die:--or if by any means it be now trodden down so that it die, it will "rise again to the Life Everlasting."[83]

Poe began his cosmology with God. From God, existing as Spirit,[84] matter was created. But this matter was initially a unitary particle "without form and void," which was "Irradiated spherically--in all directions--to immeasurable but still to definite distances in the previously vacant space. . . ."[85] The now innumerable "atoms" would tend to return to unity--a principle generally recognized as Newtonian gravity. This attractive principle drew matter together until eventually the entire universe would again be an undifferentiated unitary particle at that same point in the universe where God created the original unity. But this attractive principle would be countered by another principle Poe called repulsion, encompassing the terms heat, magnetism and electricity.[86] This principle of repulsion, while weaker than the principle of attraction, would periodically gain sufficient strength to reject the crust of the condensing body, allowing that condensing body to incandesce.[87] Having

---

[83]Ibid., 184.

[84]Poe distinguished Spirit as that which was "not matter." Ibid., 205.

[85]Ibid., 208.

[86]Ibid., 212.

[87]Ibid., 256.

intuited matter into all of space and "established" a connection between the tendency to return to unity and Newton's law of gravitation, Poe turned to the empirical science that he criticized to support his theory. "So far," he states,

> we have gone on _à priori_, from an abstract consideration of _Simplicity_, as that quality most likely to have characterized the original action of God. Let us now see whether the established facts of the Newtonian Gravitation may not afford us, _à posteriori_, some legitimate inductions.[88]

Drawing heavily from Nichol, he went on to suggest that his theory led directly to Laplace's nebular hypothesis. In fact, Poe suggested that his cosmology is to Laplace's nebular cosmology what Newton's law of gravitation was to Kepler's laws of planetary motion.[89] In a letter to his editor he made the stronger claim that Laplace's nebular hypothesis was to his cosmology as a bubble was to the ocean.[90]

In spite of his disparaging of empirical science, Poe's approach provided an explanation of the nebular hypothesis which incorporated the idea of God as the creator and prime mover of the universe. Poe's God was similar to Emerson's:

> Each soul is, in part, its own God--its own Creator:--in a word, that God--the material and spiritual God of the Universe--now exists solely in the diffused Matter and Spirit of the Universe; and that the regathering of this diffused Matter and Spirit will be but the re-

---

[88]Ibid., 215.

[89]Ibid., 313.

[90]Ibid.

> constitution of the purely Spiritual and Individual God.[91]

But Poe was careful to explain that by Spiritual he meant all that was not matter, including, for instance, the heavenly ether. Matter itself was but a combination of spiritual and material principles, which Poe defined as the principles of repulsion and attraction.[92]

Poe's cosmology can be seen as an attempt to resolve the problem of God in a universe consisting of only matter in motion. That he did not resolve it in a fashion acceptable to many Americans is not as important as the fact that he saw the importance of the problem. Others provided different answers. Many of the mid-nineteenth-century scientists argued that from nature and nature's sense experience, they could rise to a knowledge of nature's God. Theologians and philosophers critical of empirical science would counter that such a knowledge of God would be limited to properties available to sense experience. They in turn argued that beginning with a notion of God, either revealed by the Bible or by God himself in the creation of the rational mind, one could derive à priori the necessary laws of the universe. Many scientists, seeking to reconcile their religion with their science, confirmed nature's God from nature, while their critics derived nature from nature's God. In America one side began with the nebular hypothesis; the other ended with

---

[91]Ibid., 205.

[92]Ibid., 214.

it. Both ultimately kept the nebular hypothesis before the public and elevated its importance among those scientists seeking to confirm or deny a particular world view based on the nebular hypothesis.

CHAPTER 2

FROM COSMIC METEOROLOGY TO SOLAR PHYSICS: POPULAR THEORIES OF METEOROLOGY AND THE SETTLING OF THE WESTERN PLAINS

The 1877 edition of the Encyclopaedia Britannica made no mention of American meteorological work prior to the 1850s; about the time the settlement of the western plains began.[1] Nor does it appear to be the only nineteenth century work ignoring earlier commentary on the weather and climate of the United States. Americans studying the weather and potential changes in the climate were active much earlier. These early studies often encouraged westward expansion and agricultural development. Thomas Jefferson, Constantin François Chasseboeff Volney, Edward Holyoke and Samuel Williams all asserted that the coastal climate had become more moderate due to the clearing of forests and cultivation of the land. They implied that the harsher inland climates would also improve through increased cultivation.[2]

---

[1]Encyclopaedia Britannica, 9th ed., s.v. "Meteorology."

[2]Thomas Jefferson, "Query #7," in Notes on the state of Virginia (Richmond, Virginia: J.W. Randolph, 1853), 89-97; C.F. Volney, View of the climate and soil of the United States of America (London: J. Johnson, 1804); Edward Augustus Holyoke, "An estimate of the heat and cold of the American atmosphere beyond the European, in the same parallel of latitude; with some thoughts on the causes of the excess," Memoirs of the American Academy of Science 2 (1793): 65-88; and Samuel Williams, The natural and civil history of Vermont (Walpole, N.H.: Isaiah Thomas & David Carlisle, 1794), 58-63.

These pronouncements expressed the hope of new settlers, faced with harsh conditions, for easier climates and served as propaganda to draw additional inhabitants further west. In spite of refutations by Daniel Webster, Thomas Stewart Traill and Samuel Forry in the 1840s[3], the connection between cultivation and improved climate remained a popular view throughout much of the rest of the century, taking the form of "rain follows the plow" during the settlement of the dry western plains in the 1860s and 1870s.

Charles Lyell's endorsement of the easing of America's climate by cultivation in his Principles of geology did much to promote this view.[4] Even the German meteorologist, Conrad Malte-Brun, commenting on America's climate, asserted that "vanquished nature" yielded "its

---

[3]Noah Webster, "On the supposed change in the temperature of winter," Memoirs of the Connecticut Academy of Arts and Sciences 1 (1810): 1-67, reprinted in Webster, A collection of papers on political, literary, and moral subjects (New York: Webster & Clark, 1843), 1-40; Encyclopaedia Britannica, 5th ed. s.v. "Physical geography" by Thomas Stewart Traill; Samuel Forry, Meteorology: Comprising a description of the atmosphere and its phenomena, the laws of climate in general, and especially the climatic features peculiar to the region of the United States; with some remarks on the climates of the ancient world, as based on fossil geology (New York: H.G. Langley, 1842); Forry, "Researches in elucidation of the distribution of heat over the globe, and especially of the climatic features peculiar to the region of the United States," American Journal of Science 47 (1844): 226-241.

[4]Charles Lyell, Principles of geology: Or the modern changes of the earth and its inhabitants considered as illustrations of geology (New York: D. Appleton & Co., 1857), 113.

empire to man" by ameliorating its climate as it became "thickly peopled and generally cultivated."[5] While climatologists such as Dr. Forry challenged the cultivation hypothesis, they emphasized the role of geological formations--mountain ranges, large land masses, the relative location of the ocean, and the influence of the trade winds as factors in determining the climate of a particular locality.

The details which justified these claims, by the 1870s, increasingly emphasized the role of the sun as the source of heat for the earth. At the same time, meteorologists, concerned with what they believed to be the false hopes raised by many farmers and others on the western plains, would try to explicate scientific theories explaining why such climatological predictions were inadequate. Some on both sides of the debate would be influenced by the development of thermodynamics and the new theories of heat and radiation. Both scientists and weather prophets ultimately found the root of meteorological patterns in the heat which the earth received from the sun. While the source of the heat influencing weather patterns on the earth could be found in the sun, the nature of that heat, how it was transferred to the earth and the details of its influence on terrestrial

---

[5]Quoted in Forry, "Researches in elucidation of the distribution of heat over the globe, and especially of the climatic features peculiar to the region of the United States," American Journal of Science 47 (1844): 240.

meteorology was much debated throughout the nineteenth century.

Meanwhile propagandists and land speculators successfully used the new theories to justify their predictions of better climates. When climates failed to improve, or even got worse, the inhabitants increasingly called for further research from which long-range weather predictions that would assist them in planning their crops might be derived. The solar heat/terrestrial meteorology connection and the hope that a better understanding of the sun might lead to long-range weather predictions helped to motivate research, eventually defined by the term astrophysics. Some American astronomers and physicists drew on this popular support to gain funding for their research. Others would find the connection between solar physics and the weather to be more than a matter of research economics.

The clear connection between the sun and weather patterns, although often cited by both meteorologists and astronomers, was only slowly explicated. To a large extent, acceptance of the new meteorology and the new solar physics had to await widespread acceptance of the principles of thermodynamics and the belief that heat was but one manifestation of energy.

Connections between terrestrial meteorology and the sun had been suggested in American scientific journals as early as 1830 when the Journal of American Science reported European scientists' claims regarding the electric and magnetic capacity of solar light.[6] While these theories apparently did not motivate American meteorologists, they did reflect the growing scientific discussion regarding potential connections between the sun and terrestrial phenomena.

A direct influence on subsequent research on solar heat can be found, however, in the research of James Pollard Espy, probably the most influential American meteorologist in the first half of the nineteenth century.[7] Espy's theory of storms presumed that the winds in storms flowed into a central point where there was an uplift related to warm air rising relative to cooler surrounding air. While his theories were later substantially modified by those who supported William C. Redfield's whirlwind theory of storms and

---

[6]In 1812 a Professor Morichini had claimed that the violet portion of the solar spectrum was able to magnetize a steel needle. Mrs. Somerville had allegedly duplicated the experiment in 1826. Subsequent experiments in 1829 by a Professor Zantedeschi of Pavia confirming Morichini and by P. Riess and L. Moser disconfirming the magnetic influence of solar light was covered by the American Journal of Science. "Magnetic influence of the violet ray," American Journal of Science 18 (1830): 171-172 and "Magnetic influence of the solar beam," American Journal of Science 18 (1830): 181. Even more directly connected to meteorology was Carlo Matrucci's claim to the discovery of electricity in solar rays. In calling for further research, Matrucci pointed out the potential connection between solar electric rays and meteorology which, at that time, included a wide spread belief that electricity was the cause, or at least a cause, of storms. "Electricity in solar rays," American Journal of Science 17 (1830): 389-390.

[7]The French astronomer Arago is reported to have asserted: "England had its Newton, France its Cuvier, and America its Espy." Quoted in National cyclopaedia of American biography (New York: James T. White & Co., 1929), s.v. "James Pollard Espy."

by the rise of thermodynamics in the 1850s, they held a dominant position among the leading American scientists at the middle of the nineteenth century. Many, such as the first director of the Smithsonian Institution, Joseph Henry, came to Espy's support in the latter's longstanding controversy with Redfield over the nature of storms.[8] Espy did not emphasize the role of the sun in his theory of storms, but Henry, in supporting Espy, pointed to the sun as the primary source of heat affecting the earth's environment.

Americans had been treated to a translation of Joseph Fourier's remarks on the sources of terrestrial temperature as early as 1836.[9] Fourier argued that the earth's temperature derived from the sun, the internal heat of the earth and the temperature of space being the collective heat from the stars. Since the heat of space and terrestrial heat remained constant, the diurnal and annual diversity of the earth's climate resulted from the unequal distribution of solar heat.

---

[8]Joseph Henry, "On the application of the telegraph to the premonition of weather changes," Proceedings of the American Academy of Arts & Sciences 4 (1857-60): 274.

[9]Baron [Jean Babtiste Joseph] Fourier, "General remarks on the temperature of the terrestrial globe and the planetary spaces," American Journal of Science 32 (1837): 1-20. Fourier's early career was interrupted by the French Revolution in which he took a role on the side of moderates. In 1798 he joined Napoleon's Egyptian campaign, becoming Secretary of the Institut d'Egypte until 1801 when he returned to France. Napoleon conferred the title of Baron on Fourier in 1808. In 1822 he became Secrètaire Perpétual of the Académie des Sciences and in 1827 he was elected to the Académie Française. Throughout his career he undertook research on the diffusion of heat on which he published several papers.

Fourier assumed that the temperature at the poles would approach that of interplanetary space, mitigated slightly by the circulation of the atmosphere heated at the equator. Fourier further tied the study of the three sources of terrestrial temperature with cosmogony, asserting that such concerns motivated his attempts to establish a mathematical theory of heat. But such concerns, he pointed out, necessitated a study of the modifying influences of the atmosphere and of the oceans on the distribution of heat over the globe.

Shortly after the publication of Fourier's article, R.W. Haskins, submitted to the American Journal of Science a translation of Simon Poisson's more recent work on the same topic. Poisson claimed to presume Laplace's nebular hypothesis in his theory of terrestrial temperature, but he argued, contrary to Fourier, that the earth had completely cooled and there remained no central terrestrial heat derived from contraction of the earth. Rather the hot central portions of the earth were the result of periodic motion of the solar system through hotter regions of space. The surface of the earth would, of course, cool much more rapidly than its center.[10]

In America, early interest in the nature of solar heat appears to have been less directly tied to cosmological

---

[10][Simon Denis] Poisson, "Memoir upon the temperature of the solid parts of the globe, of the atmosphere, and of those regions of space traversed by the Earth," American Journal of Science 34 (1838): 57-69.

concerns, such as those raised by Poisson. During the total solar eclipse of 1831, Joseph Henry's brother-in-law, Stephen Alexander, had noticed a significant reduction in temperature.[11] He assumed that this cooling had resulted from the blockage of the solar rays by the moon. Nevertheless, he asked several fellow scientists for information on the accepted theoretical basis for this temperature loss.[12] In preparation for the 1834 solar eclipse, Alexander sent circulars asking for meteorological information during the eclipse.[13]

Regardless of the beginnings, Henry and Alexander

---

[11]Alexander was not the first to speculate on the differences of temperature during a total eclipse of the sun. In fact some twelve years earlier Honoré Flaugergues' paper, "Observations sur la chaleur que produisaient les rayons du soleil pendant l'éclipse de cet astre, le 7 septembre 1820, faites à l'observatoire de Viviers," Journal de Physique 92 (1821): 435-444, detailed that very phenomenon. "Observations" was reprinted in the Quarterly Journal of Science 12 (1822): 313-314.

[12]Nathan Reingold, ed., The papers of Joseph Henry (Washington, DC: Smithsonian Institution Press, 1972-), 1:359. See also Stephen Alexander "Astronomical observations made at Berlin, Worcester County, Md (February, 1831), with some of their results," Transactions of the Albany Institute 2 (1833-52): 84-96.

[13]Joseph Henry to John Torrey, 20 December 1834, quoted in The papers of Joseph Henry, 2:305-306 and Joseph Henry to James Henry, December 22, 1834, quoted in Ibid., 2:308-309.

cooperated on thermometric solar research in the 1840s.[14] In 1842 Henry admitted to James Henry Coffin that he had once been extremely interested in astronomy but had for the previous seven or eight years limited his research in that field to assisting Alexander.[15] In the 1840s Alexander had returned to researching the relationship between terrestrial phenomena and solar eclipses and presented a paper titled "On the physical phenomena which accompany solar eclipses" at the American Philosophical Society's 1843 centennial celebration.[16] Henry's own intention to study solar heat is sug-

---

[14]Stephen Alexander, "On the physical phenomena which accompany solar eclipses," American Philosophical Society Proceedings (1843): 183-211; Joseph Henry, "On the heat of the solar spots," in Report of the fifteenth meeting of the British association for the advancement of science; held at Cambridge in June, 1845 (London: John Murray, 1845), pt. 2:215-217; Joseph Henry, "Experiments relative to spots on the sun," Walker's Electrical Magazine 2 (1846): 321-324; Stephen Alexander, "Miscellaneous contributions to astronomical science," American Philosophical Society Proceedings 4 (1847):219-229; Stephen Alexander and Joseph Henry, "Experiments relative to the spots on the sun," American Philosophical Society Proceedings 4 (1847): 173-176.

[15]Henry to James Henry Coffin, 9 September 1842 quoted in The papers of Joseph Henry, 5:266.

[16]Stephen Alexander, "On the physical phenomena which accompany solar eclipses," American Philosophical Society, Proceedings 3 (1843): 183-211. Cf. The papers of Joseph Henry, 5:354. Alexander's paper provides a descriptive enumeration of the physical phenomena observed during solar eclipses and published in various accounts of eclipses. After comparing these with some of the phenomena observed during the transits of various planets, Alexander conditionally suggests possible causes asserting that "in the present state of our knowledge we are not prepared to assign the physical cause or causes of the phenomena in question. He speculates about the influence of a lunar atmosphere, but concludes "that an atmosphere properly so called is probably peculiar to the earth.[emphasis his] While Alexander notes observations on various effects of the

gested by the acquisition of a thermopile of Macedonio Melloni's design so that he could determine "the heat of the different parts of the solar spectrum."[17]

During the 1840s Alexander and Henry began work on the problem of solar heat, studying the temperature difference between sunspots and other portions of the sun. Henry appears to have placed a strong connection between this work and his meteorological interests. His research on the heat of sun spots had been influenced by Alfred Gautier's speculations about the relationship between sun spots and terrestrial temperature. William Herschel had suggested that sun spots seemed more prevalent during periods of greater vegetable production. Speculating that sun spot activity resulted in greater radiation of heat, he had attempted to confirm his thesis by developing a connection between sun spot activity and the price of corn. Since corn prices would be influenced by a number of other factors, Gautier had attempted to measure the average temperature of the earth directly, comparing the changes with sun spot activity. While his data suggested that the earth's mean temperature

---

eclipse on the temperature and pressure of the atmosphere during near total eclipses, he does not discuss them in detail. His primary concern appears to be determining the role of the moons atmosphere, if it has one, in the phenomena observed during an eclipse.

[17]"Record of experiments," April 27, 1843, in The papers of Joseph Henry, 5:327.

was slightly less during years of high sun spot activity, the data were generally viewed as statistically insignificant.

Henry sought to avoid complications, which both Herschel and Gautier faced, by adapting Melloni's thermopile to directly measuring the temperature of sun spots relative to the rest of the sun. He had been influenced during his visit to Europe by Claude Pouillet who in 1839 sent him a copy of his memoir on solar Heat and the temperature of space.[18] Henry's and Stephen Alexander's initial research confirmed that sun spots radiated less heat than other portions of the sun. Henry's and Alexander's paper promised further research and publication although no further reports on their research were forthcoming due, at least in part, to Henry's acceptance of the directorship of the Smithsonian in 1846. However, Henry's and Alexander's sun spot research influenced a young Italian Jesuit astronomer, then a professor of mathematics and natural philosophy at Georgetown College, Angelo Secchi, who might have assisted Henry in similar experiments during 1848 and 1849.[19] There is no question

---

[18]Claude M. Pouillet, "Memoire sur la chaleur solaire, sur les pouvoirs rayonnants et absorbants de l'air atomospherique, et sur la temperature de l'espace," Comptes Rendus 7 (1838): 24-65 and The papers of Joseph Henry, 4:234-35.

[19]According to Joseph Lovering, Secchi assisted Henry in his 1845 experiments. However, Secchi spent only two years--1848 and 1849--in the United States as professor of mathematics at Georgetown College. During this time Secchi was researching electrical rheometry and sought and received Henry's advice. Henry may have redone his 1845 experiment during this time, he certainly informed Secchi of those experiments, for Secchi, using Henry's methodology, performed similar experiments after he returned to Italy as

that Henry's and Alexander's research on the heat of sun spots played an important role in determining the direction the research of the future astrophysicist would take when Secchi returned to Italy.[20]

Although he did not implement solar physics research as a part of his Smithsonian meteorological program, Henry

---

head of the Rome Observatory in 1850. The director of the Georgetown Observatory, Benedict Sestini, began his study of sun spots during Secchi's stay in the United States. In the publication outlining his research, Sestini credited Henry for pushing sun spot research at the Georgetown Observatory beginning in 1849. Benedict Sestini, "Sun-spot drawings made at Georgetown College Observatory in 1850, September 20 to November 6," U.S. Naval Observatory Astronomical Observations (Washington, DC: n.p., 1853), 3: Appendix A. To my knowledge only Lovering claimed Secchi assisted Henry in these experiments. However, most of Henry's fellow American scientists pointed out Henry's influence on the early development of solar physics and particularly on Secchi's early research. Joseph Lovering, "Obituary memoir of Joseph Henry," Smithsonian Miscellaneous Collections 21 (1881): 431-432. See also William B. Taylor, "The scientific work of Joseph Henry," Smithsonian Miscellaneous Collections 21 (1881): 270; Simon Newcomb, "Biographical memoir of Joseph Henry," in Memorial of Joseph Henry, Smithsonian Miscellaneous Collections, vol. 21, no. 2 (Washington, DC: Smithsonian Institution, 1881), 448; and Alfred M. Mayer, "Henry as a Discoverer," in Memorial of Joseph Henry, 503. Mayer goes even further to suggest a direct lineage from Henry to Secchi to the work of Samuel Pierpont Langley.

[20]Secchi returned to Rome in 1850 after a two year ban against Jesuits was lifted. There he took over the directorship of the observatory of the Collegio Romano. By 1851 he began a study of the sun, measuring the intensity of radiation of various portions of the suns disk using the techniques developed by Henry in 1845. Charles Coulston Gillispie, ed., Dictionary of scientific biography (New York: Charles Scribner's Sons, 1973), s.v. "(Pietro) Angelo Secchi," by Giorgio Abetti. Between 1851 and 1855 he published over twenty papers on the topic of solar heat including several on the connections between solar heat and terrestrial meteorology.

did acknowledge a close connection between the two fields, particularly in their connection with the development of agriculture. In an article titled "On the conservation of Energy" published in 1860, Henry traced, in broad strokes, the transformation of solar energy into the various forms of energy available for man's use. In order to understand the role of the sun's heat in the energy of the earth, Henry asserted: "It is essential that we know something of its nature; and a lifetime of labor of many individuals, supported at public expense, would be well applied in exclusive devotion to this one subject."[21]

Henry's commitment to the careful expenditure of Smithsonian funds did not permit him to develop such a program of solar research. Indeed, throughout his career as director of the Smithsonian Institution, Henry repeatedly dropped research programs as soon as he saw adequate funding from other sources available for that research. However, as director of the Smithsonian Institution, then America's premier institution for the advancement of knowledge, Henry became the acknowledged facilitator of original research during the mid-nineteenth century. Several scientists that he encouraged turned, at least briefly, to the study of solar heat.

---

[21]Henry, "Meteorology in its connection with agriculture. Part I.--General considerations," in Scientific writings of Joseph Henry (Washington, DC: Smithsonian Institution, 1886), 2:9.

Among the first of these was L.W. Meech. He published a mathematical paper in 1850 that provided a formula for calculating the sun's daily intensity at any particular place. His calculations explained, on astronomical and mathematical grounds, why winters were colder and summers were hotter in the southern hemisphere than in the northern hemisphere.[22] Henry encouraged Meech's work, which resulted in the publication of his paper "On the relative intensity of the heat and light of the sun at different latitudes of the

---

[22] L.W. Meech, "On the computation of the sun's daily intensity at the exterior surface of the Earth, and secular changes of heat," American Journal of Science 2nd ser., 10 (1850): 49-55. Meech sought to answer two popular meteorological questions with his calculations--Is the temperature of summer and of winter the same at equal latitudes in both hemispheres? and are winters in the Northern hemisphere as cold as at the first settlement of New England? Both of these questions had been popularly answered in the affirmative. Meech derived a formula for the sun's daily intensity of

$$\text{Daily intensity} = \delta^2 * \sin L * \sin D * (H - \tan H)$$

where δ = the sun's angular radius.
D = the sun's meridian declination.
L = the 'apparent' or astronomical latitude.
and H = the hour angle from noon.

Meech's formula may have had significance for later controversies between American astronomers and the Scottish geologist James Croll who suggested that the ice ages were the result of changes in the eccentricity of the earth's orbit. Meech showed, however, that a shift in the orbit from its present eccentricity to that of a circle would result in less than 0.025° F reduction in the average temperature of the equator. Similarly, his formula confirmed the belief that the southern hemisphere had hotter summers--by 4-5° F.--and cooler winters--1 to 2° F.--than the Northern hemisphere. However, the claimed moderation of weather in New England could not be credited to astronomic causes and, according to Meech, had to be left to "the effect of cultivation or other terrestrial causes."

earth" in the Smithsonian Annual Report for 1857.[23]

Henry also helped to facilitate Jonathan Homer Lane's research on the nature of solar heat, which played a significant role in the development of solar physics as late as the turn of the nineteenth century. Lane must be seen as one of the brilliant little known figures of American science. Upon receiving his A.B. from Yale College in 1846, he spent a brief period teaching in Vermont before working with the U.S. Coast Survey. Between 1848 and 1857 he worked as an examiner in the U.S. Patent Office, a position which Henry might have helped him get. A careful researcher, he was overly hesitant to publish his work. Consequently many of his research projects were never published.[24] Henry became interested in his research apparently after Lane presented a paper on the nature of solar heat, sometime in 1848.[25] Lane continued his research on solar heat through

---

[23]L.W. Meech, "On the relative intensity of the heat and light of the sun at different latitudes of the Earth," in Smithsonian Annual Report for 1870, (Washington DC: Smithsonian Institution, 1858), 141-142.

[24]For instance between 1840 and 1870 he is said to have spent a significant portion of his time working on cryogenics attempting to develop a low temperature device. Yet none of his research in that field resulted in publication. Indeed, for all the acclaim given him by other leading American scientists, he published only six papers.

[25]Charles Coulston Gillispie, ed., Dictionary of scientific biography (New York: Charles Scribner's Sons, 1973), sv "Jonathan Homer Lane," by Nathan Reingold. Apparently Lane's early work on solar heat either was unpublished or published in a local journal. I have not uncovered any publications thus far and none are listed in Reingold's article.

the 1850s but did not publish it until 1870 and then only after encouragement from the Washington Scientific Club. This club was an informal group of Washington scientists led by Henry, which met every Saturday evening, generally at the home of one of its members, to discuss scientific issues.[26] According to Lane, he had been attracted to the study of the constitution of the sun by discussions in the scientific community, after Helmholtz had published his theory of the sun.

Based largely on Kant's and Laplace's nebular hypotheses, Helmoltz's theory asserted that the sun maintained its heat primarily, if not exclusively, by contraction. Lane thought that if Helmholtz's theory was true, sunspots could be explained by assuming the sun to be a mixture of transparent gases, of which some, for instance

---

[26]Hugh McCulloch, Secretary of the Treasury during Lincoln's second term and Johnson's administration described the scientific club as having "neither a constitution nor by-laws, and no officer but a secretary." Formed sometime in the 1850s, it appears to have been disbanded--probably by the death of most of its active members--by the turn of the century. McCulloch lists its membership in addition to himself as including: Joseph Henry, Alexander Dallas Bache, Peter Parker, Simon Newcomb, J.E. Hilgard, George C. Schaeffer, A.A. Humphreys, Jonathan H. Lane, William B. Taylor, Titian H. Peale, Benjamin N. Craig, J.M. Gilliss, J.N. McComb, O.M. Poe, M.C. Meigs, and F.A.P. Barnard. Hugh McCulloch, Men and measures of half a century (New York: Charles Scribner's Sons, 1889), 262. Simon Newcomb, however, adds General William Tecumseh Sherman and Chief Justice Salmon P. Chase to the list. Simon Newcomb, The reminiscences of an astronomer (Boston and New York: Houghton Mifflin & Co., 1903), 243. While much has been written about the alleged role of the Scientific Lazzaroni in the development of American science, I know of no study of the role suggested by the apparent interrelationship of scientific and political figures in the membership of the Washington Scientific Club.

carbon, would precipitate in the cooler upper atmosphere and revaporize after falling into hotter layers of atmosphere beneath. Lane pointed to Espy's theory of storms, then in vogue in the American scientific community, as the source for the basic components of his idea. He had become familiar with Espy's theory after attending one of the Espy's lectures around 1850. After some initial calculations based upon the current accepted values of the highest temperature and least density possible at the sun's photosphere, Lane concluded that the theory could be made to fit known laws of gases only with extreme difficulty. Nevertheless, sometime around 1860 Lane discussed his theory of solar heat with Henry, who informed him that they followed, in general outline, ideas which had been presented by Hervé Faye in Comptes Rendus.[27] After reading Faye's article, Lane still thought that the theory could not be made to fit the known laws of gases and set aside his research in that field.

Around 1868, Dr. Benjamin N. Craig, unaware of Lane's previous work, proposed a similar theory based again upon

---

[27]Lane does not specify in detail when he discussed his theory with Henry. However, Faye first published his analysis of the solar atmosphere in 1859. Cf. Hervé Faye, "Sur l'atmosphere du Soleil," Comptes Rendus 49 (1859): 564-571, 594-600. Simon Newcomb's account of Lane's work, published in 1903 in his Reminiscences. . ., 246-248, indicates that he was not aware of Lane's solar theory until 1867. It appears likely that Lane was reticent in presenting his theory, which conflicted with the then accepted measurements of the density and temperature of the sun.

Espy's theory of storms to the Scientific Club.[28] About the same time Simon Newcomb addressed a similar problem in his general survey of the nebular hypothesis. At this point Lane decided to invert his problem and, instead of assuming the accepted values of the temperature and density of the solar atmosphere, he assumed that the sun consisted of a gas which obeyed the gas laws as expressed by Poisson and Edme Mariotte.[29] Implicit in Lane's paper and, according to Newcomb, explicit in Lane's presentation was a thermodynamic exposition explaining the relationship between the mass, density, and temperature of a perfect gaseous body.[30] This

---

[28]J.H. Lane, "On the theoretical temperature of the sun; under the hypothesis of a gaseous mass maintaining its volume by its internal heat, and depending on the laws of gases as known to terrestrial experiment," American Journal of Science 100 (1870): 57-74. Lane read this paper to the National Academy of Sciences in April of 1869. Lane implies in his paper that Dr. Craig's theories were either published or formally presented in a paper the previous spring. However, I have found no such publication.

[29]Lane's citations of Poisson and Mariotte suggest a strong French influence on his research. Mariotte's law, as it is known in France, is better known as Robert Boyle's law.

[30]Newcomb appears to present Lane's 1867 argument in a footnote to his Popular astronomy: "If a globular gaseous mass is condensed to one-half its primitive diameter, the central attraction upon any part of its mass will be increased fourfold, while the surface upon which this attraction is exercised will be reduced to one-fourth. Hence, the pressure per unit of surface will be increased sixteen times, while the density will be increased only eight times. Hence, if the elastic and gravitating forces were in equilibrium in the primitive condition of the gaseous mass, its temperature must be doubled in order that they may still be in equilibrium when the diameter is reduced one-half." Simon Newcomb, Popular astronomy, 5th ed. (New York: Harper & Bros., 1884), 520. According to David Devorkin, Lane's unpublished notes, held in the Smithsonian Archives, confirm that he had analyzed the behavior

exposition would be used by subsequent astrophysicists in the development of Stellar evolution theories associated with the Hertzsprung-Russell diagram in the twentieth century.

Recognition of the implications of Lane's research appears to have been almost immediate--at least by Simon Newcomb. Newcomb arranged for Lane to observe the August, 1869, solar eclipse with him in Des Moines, Iowa, using a 5.9 inch aperture Naval Observatory telescope, rather than the 3 inch aperture Coast Survey telescope to which he had originally been assigned.[31] Observation of the beginning and end of totality was left to Lane's discretion so that he could attend to observing the solar atmosphere during totality.[32] Lane was directed to report to the director of the Naval Observatory.

Local newspaper accounts emphasized that the most important observations being undertaken for the first time

---

of a star maintaining a perfect gas condition. As the star contracted its density and heat would increase. When the density reached a point that the star could no longer be described by perfect gas laws, any further contraction would be accompanied by cooling. This came to be known as Lane's law, although the German Scientist Auguste Ritter independently developed a similar analysis published extensively in the late 1870s.

[31]J. Homer Lane, "Report of J. H. Lane, Esq." in Report of the superintendent of the United States Coast Survey showing the progress of the survey during the year 1869 (Washington, DC: Government Printing Office, 1872), 167-169. Lane also presented his work to the National Academy of Sciences in April, 1869.

[32]Ibid., 167.

during this eclipse, were spectroscopic studies to determine the chemical constituents of the solar atmosphere. This, the Iowa State Register assured its readers, would allow astronomers to determine if the sun and the earth had a common origin as proposed by the nebular hypothesis.[33]

The following year, Benjamin Peirce, then director of the Coast Survey, asked Lane to accompany the Survey's eclipse expedition to Europe. Peirce made a point to introduce Lane to British spectroscopist J. Norman Lockyer, who assisted in outfitting Lane with a special spectroscope for visual observations of the sun.[34] No doubt Lane discussed his theory with Lockyer at this time. Certainly Lockyer was subsequently influenced by it, for in his meteoric hypothesis of stellar evolution he assumed periods of ascending and descending temperature which fit Lane's theory of solar heat.[35]

Lane's meteorologically based theory of solar heat also influenced Kelvin's theory of the age of the earth. Kelvin visited America in 1876 during the centennial

---

[33] Iowa State Register, 6 August 1869, 1.

[34] Report of the superintendent of the United States Coast Survey, showing the progress of the survey during the year 1870 (Washington, DC: Government Printing Office, 1873), 120-121.

[35] A. Pannekoek, A history of astronomy (London: George Allen & Unwin, Ltd., 1961), 456. J. Norman Lockyer, The chemistry of the sun (London and New York: Macmillan & Co., 1887) and Inorganic evolution as studied by spectrum analysis (London: Macmillan & Co., Ltd., 1900).

celebrations of the American Revolution. According to Simon Newcomb, they spent an evening at the Smithsonian Institution discussing various matters, including Lane's theory. Kelvin had doubts following Newcomb's exposition and Newcomb, being unable to reproduce Lane's thought experiment, promised to write Kelvin after he had again discussed it with Lane. By 1887, Kelvin had added a footnote to his paper on the heat of the sun, pointing out Lane's law and its implications for his calculations on the age of the earth.[36]

Lane's law, developed by applying theories of terrestrial meteorology to solar physics, would influence the development of theories of stellar evolution--including playing a role in the formation of the Hertzsprung-Russell diagram--until well into the twentieth century. Lane had been one of the earliest physicists to apply theories of terrestrial meteorology to an analysis of solar heat.

With the arrival of the Civil War, Henry's meteorological program declined.[37] He never succeeded in garnering support for solar research, although he advocated

---

[36]Newcomb, Reminiscences, 248. Kelvin also includes among the points discussed, Newcomb's doubts about the reliability of the earth orbit or rotation as a standard measure of time. In fact, Kelvin makes no mention of Lane's law when he referred to his conversation with Newcomb. However, Kelvin does credit Lane, citing Newcomb in a footnote to his 1887 lecture on solar heat. Sir William Thompson, "The sun's heat," Proceedings of the Royal Institute 12 (1888): 16.

[37]Joseph Henry, "Systematic meteorology in the United States," in Smithsonian Annual Report for 1865 (Washington, DC: Smithsonian Institution, 1866), 52.

the development of "physical" observatories as distinguished from "ordinary astronomical" observatories, which most countries needed "but one or two." Physical observatories equipped with "the spectroscope, different modifications of the telescope and other lately invented appliances" later developed programs "to study the nature and changes of the constitution of the heavenly bodies; to study the various emanations from these in comparison with the results of experiments, and to record and investigate the different phenomena which are included under the general term of terrestrial physics."[38] Henry's physical observatory combined meteorological, magnetic and celestial physical observations. Henry found such observations covering common ground required training in physics. Whether one studied celestial or terrestrial phenomena made little difference. They were inexorably intertwined.

While Lane and others were applying terrestrial meteorological analogies to their studies of solar heat and the constitution of the sun, other Americans were attempting to uncover causal connections between solar phenomena and terrestrial magnetism and meteorology. Elias Loomis and Cleveland Abbe both carefully followed European research by Heinrich Schwabe, Rudolf Wolf, and others on the periodicity

---

[38]Joseph Henry, "Suggestions as to the establishment of a physical observatory," in Smithsonian Annual Report for 1870 (Washington DC: Smithsonian Institution, 1871), 141-142.

of sun spots and the possible connection between sun spots and terrestrial phenomena. While Loomis sought a correlation between sun spots and magnetic declination and auroras, Abbe attempted to deduce relationships between sun spots and terrestrial temperature.

By the mid-nineteenth century, Elias Loomis was considered one of the leading meteorological scientists in the United States. He was among those whom Henry consulted when he set up the meteorological research program at the Smithsonian Institution. However, unlike Henry, who asserted the dangers of meteorological observation unguided by abstract science[39], Loomis preferred a more Baconian method of collecting a mass of facts or observations, determining the average condition and then deducing laws which created variations from the norm.[40]

Loomis first attempted to connect auroras with thunderstorms in 1860, following the view that thunderstorms were caused by electrical currents near the surface of the

---

[39]Joseph Henry, "Meteorology in its connection with agriculture. Part I ," 2:10-11.

[40]On Loomis's scientific methodology see: Charles Coulston Gillispie, ed., Dictionary of scientific biography (New York: Charles Scribner's Sons, 1973), s.v. "Elias Loomis," by Gisela Kutzbach; F. Waldo, "Some remarks on theoretical meteorology in the United States, 1855 to 1890," Bulletin. Weather Bureau, United States Department of Agriculture 2 (1895): pt. 2:323-324; and H.A. Newton, "Memoir of Elias Loomis, 1811-1889," National Academy of Sciences, Biographical Memoirs 3 (1895): 230-231.

earth.[41] For the next ten years, the relationship between the sun, auroras and terrestrial magnetism would be the subject of most of Loomis' publications.[42] Loomis' work followed closely that of Schwabe, Wolf, and Karl Fritsch in Europe. Much of his research consisted of attempts at reducing the data collected by others so that comparisons could be made and conclusions drawn. Nevertheless his research in terrestrial magnetism and his conviction that there was some connection between fluctuations in terrestrial magnetism and

---

[41]Elias Loomis, "The great auroral exhibition of 28th Aug. to 4th Sept. 1859; and the geographical distribution of auroras and thunder-storms," American Journal of Science 30 (1860): 79-100, 339-361; 32 (1861): 71-84, 318-335. See also Loomis' 1862 paper "On electrical currents circulating near the earth's surface, and their connection with the phenomena of the Aurora Polaris," American Journal of Science 34 (1862): 34-46, for his explicit connection between Auroras and the electrical currents which "caused" thunderstorms.

[42]Of the thirteen papers Loomis published between 1860 and 1870, nine were on various aspects of this connection. In addition to the papers mentioned in the previous footnote, see: Elias Loomis, "The Auroral-Borealis, or polar light; its phenomena and laws," in Smithsonian Report for 1865 (Washington, DC: Smithsonian Institution, 1866), 208-248; Elias Loomis, "On the physical condition of the sun's surface, and on the motion of the solar spots," Proceedings of the American association for the advancement of science. Fifteenth meeting, held at Buffalo, N.Y. August, 1866, ed. Joseph Lovering (Cambridge, Mass.: Cambridge Press, 1867), 1-5; Elias Loomis, "Notices of auroras extracted from the meteorological journal of Rev. Ezra Stiles, S.T.D., formerly president of Yale College; to which are added, notices of a few other auroras recorded by other observers, at New Haven, Conn.," Transactions of the Connecticut Academy of Arts and Sciences 1 (1866-71): 164-172; Elias Loomis, "Comparison of the mean daily range of the magnetic declination, with the number of Auroras observed each year, and the extent of the black spots on the surface of the sun," American Journal of Science 50 (1870): 153-172; 5 (1873): 245-260.

solar activity indicate the extent to which views about the relationship between sun spots and terrestrial meteorology had been accepted by American scientists.[43]

Cleveland Abbe, who subsequently played an important role in the development of the Army Signal Service Weather Bureau, researched the relationship between sun spots and terrestrial temperature during his two-year directorship of the Cincinnati Observatory.[44] Abbe had spent two years

---

[43]Loomis was careful to point out that not all the fluctuations in terrestrial magnetism could be connected to changes in sun spot activity. In a letter to the British physicist/meteorologist Sir Edward Sabine he challenged the asserted relationship between auroral displays, terrestrial magnetic and sun spot periods: "If then these magnetic disturbances have a period which coincides with movements taking place upon the sun's surface, it seems by no means absurd to suppose that Auroral displays may exhibit a periodicity and this period may be connected with some movement upon the sun. The period of ten years, however, does not appear to be the principal period of the Auroral inequality." Elias Loomis to Sir Edward Sabine, 14 October 1862, quoted in A.J. Meadows and J.E. Kennedy, "The origin of solar-terrestrial studies," Vistas in Astronomy 25 (1982): 423. See also Elias Loomis, "Comparison of the mean daily range of the magnetic declination, with the number of Auroras observed each year, and the extent of the black spots on the surface of the sun," American Journal of Science 50 (1870): 160. It should be noted that by 1870 this connection was being severely criticized by European scientists, including some such as Hervé Faye whose views on the nature of the sun were generally accepted in subsequent years by American scientists.

[44]Abbe graduated from the College of the City of New York in 1857, obtained a M.A. from the same institution in 1860. He taught engineering at Michigan Agricultural College and at the University of Michigan from 1859 to 1860. After being rejected by the army for excessive myopia in 1860 he assisted Benjamin Gould in longitude determinations for the Coast Survey until 1864, when he became a "guest" astronomer at Russia's Pulkowa Observatory for two years. In 1867 he returned to the United States serving briefly at the Naval Observatory before going to Cincinnati in February of 1868 to become director of the Observatory there. Among the research he proposed for the Cincinnati Observatory was the

visiting Russia's Pulkowa Observatory, then directed by Otto Struve, in 1865 and 1867. After spending a year at the Naval Observatory, on his return to the United States, he accepted the directorship of the Cincinnati Observatory. In his inaugural report to the board of directors he set forth a research program which embraced "on the one hand, scientific astronomy, meteorology and magnetism, and, on the other, the application of these sciences to geography and geodesy, to storm prediction, and to the wants of the citizen and the land surveyor."[45]

Research on the sun and its connection with meteorology became increasingly intertwined after the Civil War. The program of research encouraged by Henry prior to the Civil War would be taken over by the Army Signal Service at the end of the war. There, research on the connection between solar physics and terrestrial meteorology would be further encouraged by Noyes' and others' attempts to counter the popularity of weather prophets and the influence of those claiming that rain follows the plow.[46] The Signal Service

---

"application" of the sciences of "astronomy, meteorology and magnetism" to storm prediction. Quoted in W.J. Humphreys, "Biographical memoir of Cleveland Abbe," Biographical Memoirs of the National Academy of Sciences 8 (1919): 473.

[45]Ibid.

[46]Martin Bowden, John Brinkerhoff Jackson and others have defined the discussion of "rain follows the plow" in terms of the extravagant propositions of western land boomers and the "myth of the desert" propagated by eastern intellectuals and scientists. They assert that Western farmers, while concerned about rainfall, adapted their crops to their environment. Older histories generally identify the plains as a desert which the settlers fought to over-

would also provide encouragement and financial support for the research of Samuel Pierpont Langley who eventually developed the Smithsonian Astrophysical Observatory to culminate the research which he thought Henry had envisioned.[47]

---

come. Walter Prescott Webb, The great plains (Boston: Ginn & Co, 1931); Martin Bowden, "The great American desert and the American frontier, 1800-1882: Popular images of the plains," in Anonymous Americans: Explorations in nineteenth century social history, ed. Tamera K. Hareven (Englewood Cliffs, NJ: Prentice Hall, 1971), 48-79; Bowden, "Desert wheat belt, plains corn belt. Environmental cognition and behavior of settlers in the plains margin, 1850-99," in Images of the plains, ed. Brian W. Blouet & Merlin P. Lawson (Lincoln: University of Nebraska press, 1975), 189-201; John Brinkerhoff Jackson, "The plains," in American space. The centennial years, 1865-1876 (New York: W.W. Norton & Co., 1972), 166-180; and John Opie, "The environment and the frontier," in American frontier and western issues, ed. Roger L. Nichols (New York: Greenwood Press, 1986), 7-26.

[47]The Army Signal Service, formed under the direction of General Albert J. Myer in 1870, grew out of attempts to coordinate army signals during the Civil War. Among the duties during the war was the signalling of weather reports. Under the influence of Cleveland Abbe, these reports became the primary focus of the newly organized Signal Service. Cleveland Abbe, "The meteorological work of the U.S. Signal Service, 1870-1891," U.S. Department of Agriculture, Weather Bureau, Bulletin 11 (1894): 282-283.

CHAPTER 3

UP TO GOD AND DOWN TO NATURE: CONFLICTING COSMOLOGICAL METHODOLOGIES

American theologians, philosophers and scientists all addressed cosmological issues in the mid-nineteenth century. Each brought their own set of concerns and their own criteria for establishing a valid cosmology. Many theologians, concerned about the materialism of positivistic science, sought to reconcile religion with the facts and theories of scientific cosmologies by deriving the same from the necessary consequences of the Creator. They claimed to find, in the explication of the concept of God, often as presented in their interpretation of Genesis, facts and theories similar to those espoused by scientists.[1] Their theories

---

[1]Scientist here refers to active members of regional as well as national scientific communities and those who sought some form of empirical/rational based knowledge about the world. While most of the scientist practitioners immediately involved in the development of the Smithsonian Astrophysical Observatory and Harvard College Observatory, the ideas by which they justified their research were far more widely held than just by members of a Washington or Boston based scientific community which some studies suggest made up the essence of the American scientific community. Indeed the notion of a American scientific community may itself be suspect. The exclusionary policies implicit in the professionalization of the various scientific fields precluded acceptance in professional societies by many, both reputable and eccentric, members of smaller local and regional societies. Contrary to the predominant historiographic view, national scientific communities did not, by their exclusionary nature, include all practitioners of science. Yet many standard works imply that American science was limited to an East Coast or Washington based clique. Both George H. Daniels and Sally Gregory Kohlstedt, for example, follow Alexander Dallas Bache in defining the American scientific community by way of the American Associ-

provided security and defense against the onslaughts of a materialistic science.

On the other hand, many scientific practitioners were equally concerned about reconciling the results of their research with generally accepted divine revelation. Beginning with the theories of science, they sought to show the inevitable conclusion of the existence of God derived from a proper understanding of His nature. The two groups would often clash in their battle to provide a safe haven and proper hierarchy for the study of both revelation and nature.

From Nature to Nature's God: The Empirical Cosmology of American Scientists

By the time Laplace included his cosmological views in the fifth edition of Mécanique célèste, Nathaniel Bowditch's American translation and commentary had been completed. While Bowditch's own publications indicate no great interest in Laplace's cosmology, that was not the case

---

ation for the Advancement of Science. Both seem to ignore the fact that Bache was one of a number of scientists actively seeking to limit control of the AAAS to a select group of Washington based scientists, of which they presumed themselves to be the leaders. More recently Robert V. Bruce has even more explicitly accepted Bache's position. George H. Daniels, Science in American society (New York: Alfred A. Knopf, 1971), 169; Sally Gregory Kohlstedt, The formation of the American scientific community: The American Association for the Advancement of Science, 1848-1860 (Urbana: University of Illinois Press, 1976); Robert V. Bruce, "Bache and company, architects of American science," in The launching of modern American science, 1846-1876 (New York: Alfred A. Knopf, 1987), 217-225.

for many of the next generation of American scientists whose careers spanned the 1840s through the 1870s. Although none of this generation of scientists may be considered astrophysicists in the current sense of the term, many became the mentors and senior scientists to the next generation, which spawned the astrophysical community.

Bowditch's protégé Benjamin Peirce and his Harvard astronomer colleague Joseph Lovering openly advocated the nebular hypothesis, as did Princeton's Joseph Henry. After Henry became the director of the Smithsonian Institution in 1847, he encouraged the research of his astronomer brother-in-law, Stephen Alexander, on the nebular hypothesis.

The acceptance of Daniel Kirkwood's analogy by the American scientific community, and its usefulness for the nebular hypothesis, vaulted him into a leadership role in scientific research related to the nebular hypothesis; a role he carried on as professor of mathematics and astronomy, first at Delaware College and then at Indiana University. Other leading American astronomers, including Benjamin Apthorp Gould of the Dudley Observatory, Ormsby Macknight Mitchel of the Cincinnati Observatory and Sears Cook Walker of the Philadelphia High School Observatory and subsequently of the Coast Survey, supported the nebular hypothesis as well.

Unlike their European counterparts, many American scientists saw the nebular hypothesis as a guiding theory for subsequent research. The fact that the nebular hypothesis was caught in the middle ground between evolutionists and creationists only increased the importance for finding a mechanism to verify or disconfirm the theory. On the one hand, the evolutionary nature of the nebular hypothesis created concern on the part of creationists, who feared that it would provide indirect, if not direct, support for Darwin's theory of evolution. On the other hand, after Kelvin used the nebular hypothesis to calculate the approximate age of the sun, many anti-evolutionists found in it a means to challenge both biological and geological evolution--sciences which some creationists eventually charged as being of satanic origin. Astronomy, for many of these people, still declared the glory of God.

The role of the nebular hypothesis in Spencer's "developmental hypothesis" was not lost on Americans either. In the second half of the nineteenth century, it flourished under the influence of William Graham Sumner and John Fiske. The fact that the nebular hypothesis could play a crucial role on both sides of the evolution debate only enhanced its importance in the minds of American scientists. Some seeking to find a confirmation of their theology would eventually perceive the mechanisms of the nebular hypothesis as equivalent to ideas in the mind of God. Others would find, as it has been claimed that Laplace did, that the nebular hypothesis would necessitate no need for hypotheses about a Supreme Being.

That aspects of the nebular hypothesis could be used by both sides of the evolution debate highlighted the importance of the theory. It also ensnared advocates of the nebular hypothesis in the cross fire resulting from the broader debate over evolution.

In mid-century America many advocates of the nebular hypothesis suggested that it led directly to evidence of God and his handiwork in the universe. They advocated this view in spite of the European opinion that belief in the nebular hypothesis tainted its followers with atheistic influences. While some American scientists, no doubt, had their faith strained by advances in modern astronomy and a few probably rejected the nebular hypothesis on theological grounds, most of the advocates of Laplace's nebular hypothesis declared its value in either improving or reinforcing their belief and understanding of nature's God. For them the nebular hypothesis led from nature to nature's God.

Benjamin Peirce feared that the nebular hypothesis might be rejected by ill informed Christians because of Laplace's presumed atheism. In 1839 he would plead that "the character of the votary must not condemn the science."[2] Indeed Peirce suggested that the nebular hypothesis revealed to man the process God adopted in creating the universe. Peirce had some doubts about the nebular hypothesis after his

---

[2] [Benjamin Peirce], "Bowditch's translation of the Mécanique Céleste," North American Review 48 (January 1839): 177.

fellow Harvard astronomer, William Cranch Bond, claimed to resolve the Orion nebula into stars. But those doubts did not prevent him from discussing his research on comets in terms of the nebular hypothesis at the 1849 meeting of the American Association for the Advancement of Science.[3] His doubts were further ameliorated by Kirkwood's analogy, which was first presented at the same meeting.[4] Two years later Peirce provided additional evidence in favor of the nebular hypothesis in a paper on the structure of Saturn's ring. He concluded his paper by proclaiming that

> "the farther I extend my researches into the physical universe, the stronger appears to me the evidence that the process of creation was conducted by the divine geometer in a modified form of that very hypothesis."[5]

In his Ideality in the physical sciences, Peirce further developed the nebular hypothesis as the idea in the mind of God.[6] His role as a leader in astronomical and physical science at Harvard College and his efforts to influence the direction and leadership of the Harvard College Observatory

---

[3]Benjamin Peirce, "On the connection of comets with the solar system," in Proceedings American association for the advancement of science. Second meeting, held at Cambridge, August, 1849 (Boston: Henry Flanders & Co., 1850), 121.

[4]Sears C. Walker, "Examination of Kirkwood's analogy," in Proceedings American association for the advancement of science. Second meeting, held at Cambridge, August, 1849 (Boston: Henry Flanders & Co., 1850), 217.

[5]Benjamin Peirce, "On the constitution of Saturn's ring," American Journal of Science 2d ser., 12 (1851): 108.

[6]Benjamin Peirce, Ideality in the physical sciences (Boston: Little, Brown & Co., 1881), 18, 194.

suggest potential connections between his views and subsequent astronomical research there.[7]

Peirce's colleague at Harvard, Joseph Lovering, explicitly avowed the usefulness of the nebular hypothesis in guiding subsequent research. Astronomers, he was convinced, would deduce from the nebular hypothesis many important discoveries and mathematicians might in the future "be able to settle with exactness the genealogy of our system according to Laplace's theory."[8]

Peirce's and Lovering's heuristic interest in the nebular hypothesis possibly influenced the research of Pliny Earle Chase, one of the most prolific writers on the nebular hypothesis between 1860 and 1890. Chase had graduated in 1839 from Harvard College where he had excelled in mathematics. For several years he had taught secondary school until illness in 1848 led to a thirteen-year hiatus from academic life. During the interim he became the partner in a stove and foundry firm. In 1871 Chase was appointed professor of natural sciences at Haverford College, where he

---

[7]On Some aspects of Peirce's influence on the direction of the Harvard College Observatory see Bessie Zaban Jones and Lyle Gifford Boyd, The Harvard College Observatory: The first four directorships, 1839-1919 (Cambridge, Mass.: Harvard University Press, Belknap Press, 1971), 46, 50, 90-91, 99-100, & 139.

[8]Joseph Lovering, "On the application of mathematical analysis to researches in the physical science," Cambridge Miscellany of Mathematics, Physics, and Astronomy, no. 3 (October 1842): 128-129.

remained until his death in 1886.[9] In 1864 Chase won the American Philosophical Society's Magellanic Medal for his paper on the numerical relations between gravity and magnetism. By the 1870s, however, his interest in gravity and magnetism had expanded to the correlation of all forces in a cosmological world view. Between 1876 and 1878 he wrote a series of ten papers in the Philosophical Magazine, suggesting that the nebular hypothesis provided an explanation for all the forces of nature.[10] His research and publication on the nebular hypothesis increasingly focused on an attempt to establish a law that "All physical phenomena are due to an Omnipotent Power, acting in ways which may be represented by harmonic or cyclical undulations in an elastic medium."[11]

---

[9]Dumas Malone, ed., Dictionary of American biography (New York: Charles Scribner's Sons, 1943), s.v. "Pliny Earl Chase," by Marjory Hendricks Davis, and Clark A. Elliott, ed., Biographical dictionary of American science (Westport, CN: Greenwood Press, 1979), s.v. "Pliny Earl Chase." During his life Chase would publish some one hundred fifty articles of which nearly half would deal with various aspects of the nebular hypothesis.

[10]Pliny Earle Chase, "On the nebular hypothesis. 'In the beginning.'--1. Mass and position," Philosophical Magazine 1 (1876): 315-319; "2. Interaction," Philosophical Magazine 1 (1876): 507-10; "3. Our binary star and its attendants," Philosophical Magazine 2 (1876): 29-36; "4. Correlations of central force," Philosophical Magazine 2 (1876): 198-202; "5. Aetherial nodes," Philosophical Magazine 3 (1877): 203-211; "6. Momentum and vis-viva," Philosophical Magazine 4 (1877): 291-298; "7. Undulation," Philosophical Magazine 5 (1878): 292-297; "8. Criteria," Philosophical Magazine 5 (1878): 362-367: "9. Radiation and rotation," Philosophical Magazine 6 (1878): 128-132; "10. Predictions," Philosophical Magazine 6 (1878): 448-454.

[11]Malone, ed., Dictionary of American biography s.v. "Pliny Earl Chase."

In the 1850s, however, Daniel Kirkwood was the leading American figure in research surrounding Laplace's nebular hypothesis. The presentation of his analogy to the 1849 meeting of the American Association for the Advancement of Science had vaulted him from an unknown amateur astronomer into a prominent position in the American astronomical community. In 1851 he was appointed professor of mathematics at Delaware College advancing to president of the college in 1854. In 1856 he accepted a professorship of mathematics and astronomy at Indiana University, where he remained until 1886. He then moved to California where, in 1891, he was appointed a nonresident lecturer in astronomy at Stanford University.[12] For Kirkwood, the validity of the nebular hypothesis was found in its usefulness in explaining and directing research on a variety of astronomical problems. The nebular hypothesis would be useful in developing theories explaining the asteroid belt. Further research should be done on Saturn's rings since they appeared to have been "left by the Architect of Nature as an index to the creative process."[13] The nebular hypothesis could be applied to geological theory and the theory of double stars to direct research in these fields.[14]

---

[12]Elliott, ed., Biographical dictionary of American science, s.v. "Daniel Kirkwood."

[13]Daniel Kirkwood, "On the nebular hypothesis," American Journal of Science 2d ser., 30 (1860): 165.

[14]Ibid.

If the nebular hypothesis provided insights into and a guide for astronomical research it also, for many astronomers, led to an acknowledgement of the Deity. The nebular hypothesis required the existence of a universal law, which in turn implied the existence of a law giver.[15]

Like Kirkwood, Princeton's professor of astronomy, Stephen Alexander, treated the nebular hypothesis as a heuristic device.[16] His successor, Charles Augustus Young, one of the earlier American astrophysicists, maintained that Laplace's nebular hypothesis had always a powerful fascination for Alexander. "He made it the basis of endless speculations as to the origin and genesis of the present state of things, and though he sometimes reached conclusions difficult to reconcile with it, as commonly understood, he was always persuaded of its essential verity."[17]

In fact the nebular hypothesis guided Alexander's research on star clusters and on various planetary problems. He, for instance, used it to direct his research in support of Bode's law. Bode's law asserted that the distances of the planets from the sun could be described as a mathematical

---

[15]C.A. Young, "Memoir of Stephen Alexander, 1806-1883," Biographical Memoirs of the National Academy of Sciences 2 (1886): 256-257.

[16]Stephen Alexander, "On the origin of the forms and the present condition of some of the clusters of stars and several of the nebulae," Astronomical Journal 2 (1852): 97, 109.

[17]C.A. Young. "Memoir of Stephen Alexander, 1806 - 1883," 256.

progression. However, the planet Neptune, discovered at the turn of the century, provided an exception to the law. Alexander suggested that Bode's law could be saved if it was presumed that the sun was surrounded by two systems of rings, similar to Saturn's. Alexander went on to show that the satellites of Saturn and Jupiter were arranged in a similar pattern as that of the planets. However, Jupiter was missing an interior moon, which Alexander explained by the fact that a ring had failed to coalesce into a satellite. Since Benjamin Peirce had "proven" that the sustenance of a ring required several satellites to counter the attraction of the planet, the ring was, no doubt, the remains of a moon which had collapsed into Jupiter.[18]

If the nebular hypothesis guided Alexander's astronomical research, he taught his students that it also led to a glorification of God. A former student wrote about the close of his lecture on the nebular hypothesis:

> I vividly recall . . . the magnificent sweep of his ideas concerning the formation of the material universe with its countless suns and systems; his happy application of Scripture phrase when, pointing to the drawings of certain nebulae of remarkable form, he would quote: "They all shall wax old as doth a garment, and as a vesture

---

[18]Stephen Alexander, "On the origin of the forms and present state of some of the clusters of stars," in Proceedings of the American association for the advancement of science. Sixth meeting held at Albany (N.Y.), August 1851 (Washington, DC: S.F. Baird, 1852), 128, 129 and "Statement and exposition of certain harmonies of the solar system," Smithsonian Contributions to Knowledge 21 (March 1875): 42-43, 77-80. Peirce's "proof" included the incorrect assertion that Saturn's rings had to be fluid in nature. Benjamin Peirce, "On the constitution of Saturn's ring," American Journal of Science 2d ser., 12 (1851): 106-108.

> shalt thou change them, and they shall be changed;" the outburst of eloquence, seeming to our young minds akin to inspiration itself, with which he ascribed all the beauty and glory of creation to Him who is enthroned in majesty above all spheres, evermore controlling and guiding all, the Personal God, glorious in holiness, fearful in praises, doing wonders.[19]

Alexander received much encouragement from his brother-in-law Joseph Henry, director of the Smithsonian Institution, who promised Alexander that research guided by the nebular hypothesis was a fertile field from which "a valuable harvest may be reaped."[20] Henry was by this time one of the leading facilitators of scientific research in America. His encouragement of astronomical research guided by the nebular hypothesis carried no little weight.

One of the strongest advocates of the spiritual compatibility of the nebular hypothesis, Ormsby Macknight Mitchel, founder and director of the Cincinnati Observatory, regularly incorporated the topic into his popular lectures as well as his professional research. Mitchel had been trained at West Point and had come to Cincinnati in 1832 to practice law. However, his interest turned to astronomy in 1834 when he was elected professor of mathematics, philosophy and astronomy at the newly opened Cincinnati College. The college was dissolved after the buildings were destroyed by

---

[19]Rev. Horace G. Hinsdale quoted in C.A. Young, "Memoir of Stephen Alexander, 1806-1883," 258.

[20]Joseph Henry to Stephen Alexander, 4 July 1857, quoted in Ronald Numbers, Creation by natural law (Seattle: University of Washington Press, 1977), 63.

fire. In 1842, through a series of popular lectures, he garnered public subscriptions to build and equip the Cincinnati Observatory. Until 1860, when he took over the directorship of the Albany, New York, Dudley Observatory, he continued to fund research at the observatory through his popular lectures.[21]

Mitchel's lectures invariably included a discussion of the nebular hypothesis and its compatibility with the Biblical account of creation. Mitchel's interpretation of Mosaic creation did not allow for the six literal days of modern fundamentalists, but neither did the interpretation of most religious devotees who discussed the topic in the mid-nineteenth century. Mitchel instead emphasized God's role in the creation of the matter of the universe and the laws of nature by which creation unfolded. He was also careful to separate the nebular hypothesis from the alleged atheistic opinions of Laplace by finding the fundamentals of the nebular hypothesis in the works of William Herschel. Laplace, Mitchel would claim, had only put the final touches on Herschel's theory.[22] Having redeemed the nebular hypothesis from the hands of atheists, Mitchel goes on to develop

---

[21]The details of Mitchel's life are drawn from "Biographical Notice" in O. M. Mitchel, The astronomy of the Bible (New York: Blakeman & Mason, 1863), 13-76 and Dumas Malone, ed., Dictionary of American biography (New York: Charles Scribner's Sons, 1943), s.v. "Ormsby MacKnight Mitchel" by Jermain G. Porter.

[22]Mitchel, The astronomy of the Bible, 136-137.

a teleological view of the nebular hypothesis in which God actively chose which portions of nebulosity are to condense into solar systems and worlds.

> Here was truly a grand and magnificent work. The selection of the great centers of aggregation was the work of Omniscience. These must be so located that the forming worlds shall in no degree interfere with each other. They must be so selected that in the development of the mighty systems to be brought into being, there should be space commensurate with the grand movements which were to be evolved. Matter was not left to itself. God was in all and over all; His wisdom sketched the mighty plan of creation on a scale commensurate with the glory and majesty and grandeur of His divine perfections.[23]

Mitchel was not above allowing that the nebular hypothesis might be "defective" or even "radically wrong." However, it could be reconciled with a consistent interpretation of the Mosaic account of creation and, if it were incorrect, then God would in His own time

> permit the human mind to rise higher and still higher in its researches in the universe, until, God aiding, it shall reach, by its own struggles, to the knowledge of the plan by which this world we inhabit, these planets that roll and shine, and yonder sun, luminiferous and resplendent with all the host of Heaven, were brought to people the unlimited regions of vacuity.[24]

While Peirce, Mitchel, Alexander, Kirkwood, and Henry found in the nebular hypothesis a mechanism for interpreting the Mosaic account of creation and of reaching nature's God through nature, other, no less devout, Christians would attack the view that one could interpret the Bible using

---

[23]Ibid., 191-192.

[24]Ibid., 211.

science or even arriving at an understanding of God, primarily relying on God's creation. They did not thereby necessarily reject the nebular hypothesis. Instead some followed Immanuel Kant in deriving the nebular hypothesis from à priori principles. For these American Kantians the nebular hypothesis could be deduced from the revealed nature of God.

From God to God's Nature: The Nebular Hypothesis in Rational Cosmology

Among those who criticized empirical science, one of the most acerbic was Tayler Lewis, Union College professor of Greek and Oriental languages. While some of his writings have been interpreted as being hostile to the nebular hypothesis[25], he was first and foremost opposed to the view that one could draw knowledge of nature's God from sinful imperfect sense data of nature. Empiric science led to atheism or pantheism since it could at best speak only of the attributes of a God in creation. But the Creator was not a part of creation, therefore, while one could deduce the character of creation from the revealed attributes of God,

---

[25]See for instance Numbers, Creation by natural law, 32. Numbers finds Lewis opposing the nebular hypothesis in his review of Robert Chambers Vestiges of the natural history of creation, while accepting similar ideas to the nebular hypothesis even though attacking scientists' views in his The six days of creation; or the scriptural cosmology, with the ancient idea of time-worlds, in distinction from worlds in space (Schenectady, N.Y.: G.Y. Van Debogert, 1855) and The Bible and science; or, the world-problem (Schenectady, N.Y.: G.Y. Van Debogert, 1856).

one could not accurately derive the character of God from a knowledge of His creation. Lewis, convinced of Platonic ideas and to a lesser extent influenced by Coleridge,[26] argued that a true knowledge of creation would be found not in science, which was valid only to the extent that it reflected and began with revelation, but in a philological

---

[26]In Natural religion the remains of primitive revelation. A discourse, pronounced at Burlington, before the literary societies of the University of Vermont, August 6th, 1839 (New York: University of New York Press, 1839), 4, Lewis suggests that the term natural religion might be made theologically safe by redefining it to emphasize the study of ancient philosophers as opposed to the development of theological conclusions induced from contemplating the natural world. For examples of Lewis's use of Platonism in defense of Christianity see his Plato contra atheos. Plato against the atheists; or, the tenth book of the dialogue on laws, accompanied with critical notes, and followed by extended dissertations on some of the main points of the Platonic philosophy and theology, especially as compared with the holy scriptures (New York: Harper & Bros., 1845). Lewis would not replace the study of the Bible and revelation with ancient philosophy, however. "The writings of Plato may show us afar off the land of Beulah and the gates of the heavenly city, but the gospel alone points out the narrow road which leads to them. From the Grecian sage may we derive many a sublime precept for drawing away the mind from earthly and sensual objects, for mortifying the carnal affections, and directing the thoughts to heavenly and spiritual contemplations, but the gospel alone removes the heavy chains which bind us down to earth, and gives us the wings of redeeming love to soar to regions where philosophy can only follow with its gaze." Lewis, Natural religion the remains of primitive revelation, 11. In referring to Coleridge, Lewis asserted "notwithstanding certain errors which we believe exist in his statement of the plan of redemption, we can never think but with respect almost amounting to veneration--There have been, however, gross perversions of some of his views, (connected with the extravagances of certain writers,) which, under the appearance of high spirituality, are at bottom, the very dregs of materialism." Ibid., 50.

exegesis of the Bible.[27]

Lewis was not the first to derive a cosmology from Biblical exegesis. The American natural historian, Constantine Samuel Rafinesque, had argued that one could derive from the Hebrew Bible, interpreted according to the principles of OBRI Hebrew, the principles of modern cosmology. According to this interpretation, Rafinesque avowed, the Hebrew term generally translated star, should be literally translated "highest father or producer of intense centralization," while the word translated sun, meant "celestial condensation."[28] He had endorsed Laplace's nebular hypothesis and its implications for geology in his short lived Atlantic Journal and Friend of Knowledge in the fall of 1833[29] However, his cosmological views became increasingly mystical in his 1836 publication of The world, or instability and 1838 Celestial wonders and philosophy, or

---

[27]"The moral poison is too virulent to be cured by the aid of the physical sciences, and it may be doubted whether they tend much to ameliorate even the temporal condition of mankind. Ibid., 47.

[28]C.[onstantine] S.[amuel] Rafinesque, Genius and spirit of the Hebrew Bible. Including the Biblic philosophy of celestial wisdom, religion and theology, astronomy and realization, ontology and mythology, chronometry and mathematics (Philadelphia: Printed for the Eleutheriun of Knowledge, 1838), 115 and 132. Just what Rafinesque meant by OBRI Hebrew remains unclear. Apparently it is a variant on the Hebrew word for creation, "briah," upon which he based his translation of other Hebrew words.

[29]C.S. Rafinesque, "Some essential views of geology, by Dr. Hibbert and Rafinesque," Atlantic Journal and Friend of Knowledge 1 (Autumn 1833): 191-195.

the structure of the visible heavens with hints on their celestial religion and theory of futurity. The latter work claimed to derive its science from the discoveries of "the 3 Hershels and the explanations of Nichol, Mrs. Somerville and others."[30] However widely Rafinesque's cosmological works may have been read, they do not appear to have had much influence on popular thought.[31] Tayler Lewis, on the other hand, appears to have profoundly influenced Laurens P. Hickok, perhaps America's most influential rationalist philosopher in the mid-nineteenth century.

In The six days of creation Lewis attacked two prevailing views of creation and science. The first view attempted to reinterpret the Bible in some sense reconcilable with established science.

> Their argument runs thus: The bible may have this sense; it must have this sense to be consistent with acknowledged science; and, therefore, on the principle that all truth must be consistent with other truth, it actually has this sense.[32]

Such a position was self deceiving since it suggested a belief based on scripture when in fact it rested mainly on

---

[30]C.S. Rafinesque, The world, or instability: A poem in twenty parts with notes and illustrations (Philadelphia: J. Dobson, 1836) and Celestial wonders and philosophy, or the structure of the visible heavens with hints on their celestial religion and theory of futurity (Philadelphia: Printed for the Central University of Illinois, 1838).

[31]Charles Coulston Gillispie, ed., Dictionary of scientific biography (New York: Charles Scribner's Sons, 1973), sv "Samuel Constantine Rafinesque."

[32]Tayler Lewis, The six days of creation, i.

scientific writings.

The second perspective, Lewis asserted, was merely an inversion of the first.

> They take as indisputable a certain interpretation which they choose to call the literal. Modern science does not agree with this; therefore, science, they say, is false in its deductions, and infidel in its spirit.[33]

Lewis was not concerned with defending modern science. If it disagreed with his interpretation of revelation, science rather than revelation would have to give way. Even where science was valid, it was limited in what it could say. "Science may boast as she pleases," Lewis would chide,

> but according to her own most vaunted law, she can only trace the footsteps of a present or once passing causation. When those footsteps cease--as from the very nature, not only of things, but ideas, they must cease, when we come to the question of origin--she can teach us nothing. This <u>seems</u> to have been before <u>that</u>, she may say; or between this and that there <u>seems</u> to have been many mediate stages of transition or development. Such is the <u>apparent</u> lesson she reads in the rocks, the mines, the lava, the beds of coal. . . . The untaught Esquimaux [sic] stand on an equal footing here with La Marck [sic], or La Place [sic], or Auguste Comte. Without light coming from above the plane of physical causation, one is just as ignorant as the other.[34]

Lewis may have been conservative in his demand for a literal interpretation of the Bible, but he insisted that a philologically accurate reading of Genesis would not allow for a literal six-day creation.[35] In fact, his Biblical cosmology was remarkably like the nebular hypothesis, even

---

[33]Ibid., ii.

[34]Ibid., 220-221.

[35]Ibid., 4.

though he attacked the "confident views on this subject, presented in such books as Nichol's Architecture of the heavens, or by the great mass of our popular scientific lecturers" as "alike baseless in their premises and conclusions."[36] Science was in no position to make the assumptions necessary for the nebular hypothesis. But the Bible, philologically interpreted, could present as fact that of which telescopic observations might give evidence.[37]

In spite of his attack on those who condemned science as impious, Lewis did little to counter that position and much to encourage it. In response to Lewis' diatribe, James Dwight Dana entered the foray in the January, 1856 issue of Bibliotheca Sacra. While The six days of creation included "much truth, well expressed and argued," Dana implied that Lewis, rather than scientists, might be guilty of "a decidedly infidel tendency" by degrading science. Indeed, Dana suggested, that the study of nature leading to a knowledge of God was just as spiritual as the study of the scriptures.[38]

Nature, Dana insisted, had been specially adapted to finite minds. God had appointed it to direct human minds to

---

[36]Ibid., 140.

[37]Ibid., 145-147.

[38]James D. Dana, "Science and the Bible. A Review of 'The six days of creation' of Prof. Tayler Lewis," Bibliotheca Sacra 13 (January, 1856): 91 and "Science and the Bible, II," Bibliotheca Sacra 13 (July, 1856): 632-633.

Him and to be a revelation of one aspect of His attributes.[39] Just as the mind rose, in the process of induction, from specific to general truths, so it continued on to a veiled conception of God revealed through nature.[40] Lewis's Scriptural cosmology in denying that the attributes of God could be found in nature left the defenders of the Bible existing only among the superstitious.[41]

Dana argued that, on the one hand, Lewis failed to distinguish between atheists who used science to falsely attack Christianity and scientists seeking truth. On the other, Lewis misrepresented what science, in fact, said. Lewis, he pointed out, described the formless void of precreation as a "huge nebulosity" out of which the world developed largely through "natural causes." At the same time he attacked the notion of "nebular condensation." How, Dana asked, could Lewis's nebulosity become the world without a process of condensation?[42] In fact, Dana marveled, Lewis accorded his cosmogony with the views of science in all the essential points, yet in every case found some opportunity to denounce science.[43]

---

[39]Ibid., 635.

[40]Ibid., 637.

[41]Ibid., 649.

[42]Dana, "Science and the Bible," 103-104.

[43]Ibid., 105.

The real point, Dana concluded, was that Lewis wanted, not a mosaic cosmology, but a Platonic cosmology. Lewis's attacks on science were not based upon a defense of Christian revelation. Lewis's cosmology, while perhaps good Platonism, was "neither scriptural theism, nor true naturalism." Most of Dana's arguments, as well as Lewis's, dealt with the fine points of the creation story rather than those of nebular hypothesis.[44] Yet the mechanisms for defining and determining the validity of the nebular hypothesis lay behind their central arguments.

Lewis responded to Dana's defense of scientists, as well as David Nevin Lord's attack on his lack of twenty-four hour literal days of creation, with his 1856 publication of The Bible and science, or the world problem. Both Lord and Dana, Lewis alleged, were concerned with reconciling revelation with their notion of the world than with interpreting the Bible according to its own light. Lord insisted on literal twenty-four hour days of creation, in spite of the fact that the first four "days" of creation did not include a sun by which days could be measured. Dana, likewise, insisted on interpreting the Bible by the light of current scientific theories. But Dana's view, Lewis claimed:

---

[44]Dana admits in his conclusion that he purposely avoided discussing the nebular hypothesis in his early articles criticizing Lewis. James D. Dana, "Science and the Bible III," Bibliotheca Sacra 14 (July 1857): 522.

> either makes Moses a mere mechanical revealer of facts of which he had no conceptions at all, or gives him a science which we are certain he could not have possessed.

However, Moses

> no more thought of nebulae, and nebular rings, than he did of sun-risings and twenty-four hours for the first great day, or for any of the great days that followed. He thought of no other world than this, and the sky around it which he called the heavens, and in which the heavenly bodies appeared.[45]

Dana relied on science to interpret scripture, but by its own pronouncements science could only make generalizations from facts or appearances. It could not go from nature to the supernatural.[46] Lewis did not deny the possible validity of the nebular hypothesis.

> The nebular view of the universe may be physically right in itself. It strongly challenges our admiration. But, after all, it has mainly guesses for our science, whilst it presents but a cold waste for the imagination.[47]

Rather, he objected to the philosophical underpinnings of the nebular hypothesis and, for that matter, all modern science.

Modern science was based on Aristotelian or Baconian philosophy which presumed the eternity of matter and a world of eternal causation. Modern science, based on these presumptions, was "fundamentally and irreconcilably at war" with Christianity and Platonism.

> The very essence of the Platonic system is involved in its great gradations. Matter is younger than life, life

---

[45]Lewis, The Bible and Science, 147.

[46]Ibid., 170-175.

[47]Ibid., 267.

> is junior to law, law is junior to ideas, [and] ideas belong to mind, . . .[48]

Dana had attacked the Platonic element in Lewis's The six days of creation, to which Lewis responded "This may be all nonsense to the Editor of the Theological Review and the Silliman Professor of Mineralogy in Yale College; but, without having the fear of either before our eyes, we must still talk Platonism."[49] Platonism applied to the principles of creation would, like God's revelation, provide at least a safe spiritual guide:

> We may, indeed, see something when we cast among its shadows the reflection of certain a priori ideas, or carry with us amid its caverns the torch of revelation, if not as a scientific guide, at least as an assurer of the divine wisdom.[50]

Only in this way could one avoid the theistic "God in Nature" which Lewis found in Dana's critique. Instead, Lewis argued one must begin with God and deduce God's nature. Two years later, Lewis's call for an à priori account of creation was answered by his colleague at Union College, Laurens P. Hickok.

Hickok had trained for the Presbyterian ministry after graduating from Union College in 1820. In 1836 he became professor of theology at Western Reserve College, accepting a similar position at Auburn Theological Seminary

---

[48]Ibid., 141.

[49]Ibid., 94.

[50]Ibid., 259.

in 1844. In 1852 Eliphalet Nott called him back to Union College to serve as Professor of Moral and Mental Philosophy and Vice-President of the College, a position he held until Nott's death in 1866, when he assumed the presidency. During the mid-nineteenth century, he became the first major American expositor of German idealism. Relegated by modern historians to a minor position in the history of American philosophy, Hickok was nevertheless viewed by his disciples as the hope for an "American philosophy" in the mid nineteenth century.[51]

In 1848 he had published his Rational psychology in response to Lewis's call, in Plato against the atheists, for arguments against skepticism based on the primal truth that God exists.[52] While Rational psychology developed the necessary laws of the mind based on an à priori knowledge of God, Hickok's Rational cosmology, published in 1858, detailed the necessary laws of the universe in a manner Hickok thought

---

[51]Dumas Malone, ed., Dictionary of American biography (Charles Scribner's Sons, 1946), s.v. "Laurens Perseus Hickok," by Ernest Sutherland Bates; National Cyclopaedia of American biography (James T. White & Co., 1929), s.v. "Laurens Perseus Hickok;" Herbert W. Schneider, A history of American philosophy, 2d ed. (New York and London: Columbia University Press, 1963), 379 and Joseph Blau, "Laurens P. Hickok: The orthodoxy of reason," in Men and movements in American philosophy (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1952), 93.

[52]Laurens P. Hickok, Rational psychology (Schenectady, N.Y.: G.Y. Van Debogert, 1854) and Lewis Tayler, Plato against the atheists, iv.

would answer the skepticism of modern positivism.[53] The necessary laws, which Hickok derived from the idea of God, were remarkably similar to the Newtonian laws and principles which made up the nebular hypothesis. Highly critical of Baconian science, Rational cosmology, with its focus on *à priori* reasoning, appears to have had little influence on actual scientific thought of the mid-nineteenth century. However, it provided some theologians and laymen an apparently satisfying framework for reconciling scientific explanations with conservative religious beliefs.

In the 1870s James Dana would write Henry Guyot that he had come to accept Lewis' interpretation of the conflict between science and theology which he had so criticized in the 1850s.[54] That view included his acceptance of the nebular hypothesis. After the publication of Hickok's Rational cosmology many American scientists and theologians adopted the view that the nebular hypothesis was a physical exposition of ideas in the mind of God. One might not be able to begin with God and derive the nebular hypothesis in total or begin with the nebular hypothesis and prove God's existence, but the two, held together, led to a harmonious confluence which gave both a greater understanding of God and

---

[53] Laurens P. Hickok, Rational cosmology; or The eternal principles and the necessary laws of the universe, 2d ed. (New York: D. Appleton & Co., 1859), 53.

[54] Dana to A. Guyot, January 30, 1875, quoted in Daniel C. Gilman, The life of James Dwight Dana (New York & London: Harper & Bros., 1899), 330-331.

provided a safe explanation of some of the troubling developments in evolution theory. This position, at least as espoused by members of the American scientific community, would find its culmination in Harvard physicist-mathematician Benjamin Peirce's Ideality in the physical sciences.[55]

James Mills Peirce, who edited his father's last book, asserted that it was written to offer Benjamin Peirce's personal contribution to evolution theory from the perspective of his deep rooted belief that every development of scientific thought necessarily contributed positively to religion.[56] In fact, the work appeared more to be an attempt to give secular or even atheistic interpretations of science a theological slant.

Benjamin Peirce began by introducing the concept of Ideality into the scientific realm, but did this within the context of reinterpreting August Comte's three stages in the development of science. Comte's stages begin with the theological and its vague notions about the role of God in nature, move to the metaphysical and finally to the positive. Peirce interpreted the latter stage in a way that limited it to observational statements which would ultimately reduce all

---

[55]Ideality in the physical sciences (Boston: Little, Brown & Co., 1881), published after Peirce's death, was originally presented as a series of six lectures delivered in February and March, 1879, to the Lowell Institute and subsequently to the Peabody Institute of Boston.

[56]James Mills Peirce, "Introduction," in Benjamin Peirce, Ideality in the physical sciences, iv.

causes to one cause. But, Peirce points out, facts of observation are not merely a function of the material world. While one component of most facts is what Peirce calls "earthy" another component was mental and dependent upon the constitution of the mind. Referring to his own field of astronomy he pointed out that there existed physical facts "of which the knowledge is wholly mental, and of which there is no direct evidence to the senses."[57]

Beyond this "ideal" component of facts, science also dealt with the classification and arrangement of facts. These, Peirce insisted, were mental processes which were not automatically performed in nature.[58] The physical sciences were permeated with an ideal element which could not be eliminated.[59]

Having established that ideality existed in the physical sciences, Peirce attempted to show how that ideality was a manifestation of the mind of God. He began with the assertion that there was no physical manifestation which did not have a counterpart ideal representation in the mind of man. The determination of the reality of a manifestation lay

---

[57]Ibid., 12. Peirce's "earthy" appears to refer to that which is the source of sensation. The "mental" component referred to the organization of sense data and the derivative conclusions from that organization and the structure imposed on sense data.

[58]Ibid., 14.

[59]Ibid., 16-17.

in man's ability to represent it ideally.[60] But, Peirce argued, identity of "law, structure or material" was evidence of commonality of origin. Therefore, the identity of the ideal laws of mind and the physical laws of the universe gave support to a belief in their common origin. Peirce implied that there were but three possible explanations for the origin of the universe; that "the mind of man first constructed the world with its great harmonies, and then shrank to its present stature," that mind had grown out of "brute matter, which is now utterly unconscious and incapable of intellectual argument or spiritual emotion," or that both originated in the decree of a creator.[61] Because positive science was devoted to rigid observation, it could not see the divine, which was neither in nature nor in the mind but was the first and final cause of both.[62] This, Peirce asserted, was the necessary deduction of the common laws found in both mind and nature.

Having established nature as a manifestation of the mind of God, Peirce went on to discuss, among other things, cosmological evolution. Beginning with a discussion of the battle between Vulcanists and Neptunists over the origin of the earth, he contended that scientific communities were often limited by the realm of their observations, as well as

---

[60]Ibid., 27.

[61]Ibid., 30-31.

[62]Ibid., 35.

by their desire for their theory to win over competing theories. Implicit in this discussion was the argument that creationists and evolutionists set aside their desire for victory and look for truth.

That truth, however, was found by limiting the Mosaic cosmogony to a simple declaration that God created the universe and that the process of creation was to be found in the created product.[63] The nebular theory--Peirce refused to call it a hypothesis--provided the "trunk" of the latest form into which cosmogony had grown under the guidance of modern science.[64] According to the theory, as Peirce espoused it, the universe began with an all pervading homogeneous substance in which there was no apparent structure or division into parts. After going through innumerable transformations, it eventually terminated in a system in which disorganization was totally eliminated and where each individual component would be "perfect in itself" and "combined in indestructible harmony." As such it would display the final plan "of an unbounded imagination."[65]

Peirce's nebular hypothesis, ultimately was neither that of Laplace nor that of Sir William Thompson, whose utilization of Laplacian theories at least partially introduced thermodynamic principles into cosmological discussions.

---

[63]Ibid., 50.

[64]Ibid., 51.

[65]Ibid., 51-52.

Peirce, in fact, criticized Thompson for introducing the concept of entropy into the nebular hypothesis and for his analysis of solar heat.[66] Peirce subsequently became quite concerned with components of the nebular hypothesis which suggested the eventual cooling and death of the sun, going so far as to develop the ad hoc hypothesis that invisible meteors, which provided as much as five-sixths of the sun's heat, existed. Their fall into the sun, he believed, was the source of solar flares. Their fall into the earth provided a significant source of the earth's heat as well as endowing the earth with the substance carbon, necessary for the beginning of life.[67] Developing this hypothesis became the focus of his final years. His presentations, both formal in seminars he directed at Harvard and informal in discussions with his colleagues, played an important role in heightening the importance of research related to the nebular hypothesis at the Harvard College Observatory.[68]

---

[66]Ibid., 156-160.

[67]Presumably the frictional heat derived from the meteorite burning up in the atmosphere provided a significant component of the temperature of the atmosphere in Peirce's cosmology at this point. No detailed study has been completed on Peirce's cosmological developments after his writing of Ideality in the physical sciences.

[68]Ibid., 110, 198-211. James Mills Peirce asserted that he presented his ideas in a series of informal scientific meetings at Harvard during 1879 and 1880. Cf. 198.

The Physicists versus Darwin and the Geologists: The Nebular Hypothesis and the Age of the Earth and solar system

Philosophers and theologians might begin with God and develop a priori forms of the nebular hypothesis. Some scientists and advocates of natural theology might begin with an acceptance of the nebular hypothesis and find in its principles the hand of God. Others focused on the development of a consistency among the sciences along the lines of what the British philosopher William Whewell had called the "concilliance of inductions." Many had commented on the similarities between the evolutionary process in the nebular hypothesis, the evolutionary process in geology and Darwin's biological theory of evolution. Some conservative theologians, such as David Nevin Lord, had rejected the nebular hypothesis on the grounds that stellar evolution, geological evolution, and biological evolution were substantially the same and equally irreconcilable with the Biblical record.

Within the American scientific and philosophical community, the tone for the correlation of diverse physical phenomena was set by the 1864 publication of The correlation and conservation of forces, edited by Edward L. Youmans. The work, dedicated to John William Draper, was a compilation of lectures and papers showing the conservation of "force" and its transformation from one form of energy to another. It introduced to American readers essays by W.R. Grove, Herman Helmholtz, J.R. Mayer, Michael Faraday, Justus Liebig and

William Benjamin Carpenter on the conservation of "force" in their respective fields. Both Helmholtz's[69] and Mayer's[70] papers included a discussion of the nature and origin of the sun's heat from the perspective of Newton's theory of gravitation and the nebular hypothesis.

Even more significant was the expanded view of the conservation of force, which Youmans put forth in his introduction. The new doctrine of forces, Youmans announced, opened such an extended range of research fields, in comparison to older doctrines, as to preclude any resistance towards its acceptance.[71] The book, he argued, would clearly present to American readers the importance of the new doctrines in the realm of the physical sciences. However, he went on to suggest their equal importance for the fields of psychology and sociology. He suggested that any disturbance of the physical forces, such as would result in floods or droughts would be "felt throughout the entire social organism." Where the local effect was not relieved by aid from other regions there would be economic decline, fewer marriages and a general "depression of the social

---

[69]Herman Helmholtz, "Interaction of natural forces," in The correlation and conservation of forces: A series of expositions, by Prof. Grove, Prof. Helmholtz, Dr. Mayer, Dr. Faraday, Prof. Liebig and Dr. Carpenter, ed. Edward L. Youmans (New York: D. Appleton & Co., 1865), 211-249.

[70]J.R. Mayer, "On celestial dynamics," in The correlation and conservation of forces, 259-315.

[71]Edward L. Youmans, "Introduction," in The correlation and conservation of forces, iv-xv.

energies."[72] He went on to tie the notion of the conservation of force with the philosophy of Herbert Spencer, suggesting that Spencer had shown the "Persistence of Force" to be "the underlying principle of all being," as well as "the fundamental truth of all philosophy."[73] Spencer, even more explicitly than Youmans, would draw on the nebular hypothesis and the concilliance of scientific theories to support the validity of his philosophical speculations.

Spencer's Illustrations of universal progress, first published in American in 1864,[74] went through two American editions and seventeen printings by 1889. His "new and enlarged edition of Recent discussions in science, philosophy and morals, printed in America in 1873,[75] was equally successful, with eleven American impressions made by 1890. Each was profoundly influenced by its author's adoption of the nebular hypothesis.

Youmans maintained that Spencer had developed his general principle of progressive evolution while writing a paper on the nebular hypothesis in 1858.[76] Spencer's

---

[72]Ibid., xxxvii.

[73]Ibid., xxxix.

[74]Herbert Spencer, Illustrations of universal progress (New York: D. Appleton & Co., 1864).

[75]Herbert Spencer, Recent discussions in science, philosophy and morals, new and enlarged ed. (New York: D. Appleton & Co., 1873).

[76]E.L. Youmans, "Spencer's evolution philosophy," North American Review 129 (1879): 396 and [Herbert Spencer], "Recent astronomy and the nebular hypothesis," Westminster Review 70 (1858): 185-225.

influence on late nineteenth-century American thought was significant, providing one of the most widely read popular expositions of evolution to the American public.[77] The nebular hypothesis played an important role in much of his writings[78] and few American writers, whether critics or allies, failed to make a connection between Spencer and the nebular hypothesis when discussing his philosophical writings.[79]

---

[77]Richard Hofstadter, "The vogue of Spencer," in Social Darwinism in American thought, rev. ed. (Boston: Beacon Press, 1955), 31-50. and Elizabeth Flower and Murray G. Murphey, A history of philosophy in America, Vol. II, (New York: G.P. Putnam's Sons, 1977), 529. More recent scholars have disputed the role of Darwinism in the evolutionary thought of Spencer and his American disciples. See for example: Edward S. Corwin, "The impact of the idea of evolution on the American political and constitutional tradition," in Evolutionary thought in America, ed. Stow Persons (New Haven: Yale University Press, 1950) and Robert C. Bannister, Social Darwinism: Science and myth in Anglo-American social thought (Philadelphia: Temple University Press, 1979)

[78]Herbert Spencer, Essays: Scientific, political and speculative (London: Longman, Brown, Green, Longman and Roberts, 1858-1874); Herbert Spencer, First principles of a new system of philosophy (New York: D. Appleton & Co., 1864); Herbert Spencer, Illustrations of universal progress (New York, Appleton & Co., 1864); Herbert Spencer, Recent discussions in science, philosophy and morals (New York: D. Appleton & Co., 1873). D. Appleton & Co. first published part one of First principles separately in 1860. The complete work was not printed in America until 1864. It went through 6 editions and numerous printings by 1900.

[79]See for example: "The philosophy of Herbert Spencer," North American Review 100 (1865): 423-476; Francis Ellingwood Abbot, review of The principles of biology by Herbert Spencer, In North American Review 107 (1868): 377-422; F.H. Johnson, "Mechanical evolution," Andover Review 1 (June 1884): 631-649; and W.R. Benedict, "Theism and evolution," Andover Review 6 (1886): 336-350, 601-622.

Spencer's influence was especially strong at Harvard, where one of his most influential American followers, Jonathan Fiske, gave a series of lectures in 1869 which formed the basis for his Outlines of cosmic philosophy.[80] Fiske's Outlines of cosmic philosophy, published in 1875, went through eight printings that first year and went through another twelve impressions by 1900. Fiske's writings focused less explicitly on the role of the nebular hypothesis than did Spencer's, yet they remained an important element in his philosophy. Evolution was exemplified by, among other things, the "development of our planetary system from a relatively homogeneous ball of vapor."[81] Even his definition of evolution--the process of differentiating from homogeneity to heterogeneity--suggests its influence.[82] However, many supporters of Fiske went beyond the issue of the nebular hypothesis to advocate that it and other aspects of evolution were manifestations of God's creative process. George Harris, in his review of Fiske's The destiny of man viewed in the light of his origin condemned those who viewed humans as

---

[80]Barbara MacKinnon, American philosophy. A historical anthology (Albany, N.Y.: State University of New York Press, 1985), 112.

[81]Quoted in review of Outlines of cosmic philosophy, based on the doctrine of evolution, with criticisms on the positive philosophy, by John Fiske, In North American Review 120 (January 1875): 202.

[82]John Fiske, John Fiske's miscellaneous writings, Vol. 2, Outlines of cosmic philosophy (Boston and New York: Houghton Mifflin & Co., 1902), 218-223.

"a local incident in an endless and aimless series of cosmical changes." Rather, he asserted, they needed to be viewed, as Fiske viewed them, "as the consummate fruition of creative energy."[83] The nebular hypothesis was not to be rejected, but it was necessary to transcend its scientific framework. Fiske, Harris would later allege, advocated a "tellurian teleology," finding no evidence of purpose "in the vast astronomic story of the universe." Although it was impossible to derive a teleological law from the nebular hypothesis, Harris claimed that Fiske, realizing the unity of the laws of nature, found it necessary to recognize that one thought or plan formed both the telluric and heavenly universe. The notion of progress could only be derived from experience and cosmology lay outside of experience. In brief, Harris concluded, Fiske's philosophy was based on the theory "that the idea of God is a deduction from the universe." In order to get a full idea of the scope of God's plan, one had to use the imagination "to push the extent of the universe out beyond the horizon of sight."[84] Beginning with the universe one could arrive at a concept of God.

If Benjamin Pierce, Jonathan Fiske, and many philosophers who lived on the periphery of Harvard in the

---

[83]George Harris, Review of The destiny of man viewed in the light of his origin, by John Fiske, In Andover Review, 3 (January 1885): 83.

[84]George Harris, Review of The idea of God as affected by modern knowledge, by John Fiske, In Andover Review 5 (1886): 100.

1860s and 1870s could either begin with the nebular hypothesis and find the mind of God or begin with the mind of God and work out evolutionary cosmologies, opponents of the connection between science and theology explicitly attacked the nebular hypothesis and the crux of the affiliation. In spite of Spencer's developmental hypothesis, the "concilliance" between the nebular hypothesis, geology and evolutionary biology had become problematic during the 1860s when William Thomson, subsequently Lord Kelvin, calculated the age of the solar system based on the amount of contraction required, on the gravitational collapse hypothesis, to maintain the sun's heat. His estimation of between twenty and four hundred million years,[85] while far short of the time presumed by both geological and biological evolutionists, was also far longer than the approximately 6,000 years held by many Biblical literalists. Since most traditional literalists had already rejected the nebular hypothesis they did not involve themselves in the debate over the age of the earth based on the nebular hypothesis. Geologists and Darwinians, on the other hand, attacked Kelvin's estimate, in particular, and the nebular hypothesis, in general.

The focal point of the debate over the age of the solar system remained in Europe where William Thomson and

---

[85]W[illiam] Thomson, "Physical considerations regarding the possible age of the sun's heat," in Report of the thirty-first meeting of the British association for the advancement of science; held at Manchester in September, 1861 (London: John Murray, 1862), 28.

Huxley and their respective supporters periodically locked horns from 1862 until shortly before Kelvin's death in 1907. However, it did influence views in America. Some American physicists and astronomers, most notably Simon Newcomb,[86] sided with Kelvin; and several American geologists sought ways of reconciling Kelvin's drastically reduced age of the earth with geological chronology.[87]

---

[86]Simon Newcomb, Review of Climate and time, by James Croll, In American Journal of Science 3d ser., 11 (1876): 263-273; Simon Newcomb, "On some points in climatology, a rejoinder to Mr. Croll," American Journal of Science 3d ser., 27 (1884): 21-26; "The sun. Professor Young's second lecture. A mass of information crowded into a talk of an hour and a half," New York Daily Tribune, 10 January, 1883, 2; James Campbell Irons, Autobiographical sketch of James Croll LL.D., F.R.S., etc. with memoir of his life and work (London: Edward Stanford, 1896), 314.

[87]See Joe D. Burchfield, Lord Kelvin and the age of the Earth (New York: Science History Publications, 1975), 106-107. While the age of the earth controversy received less attention in America than in Europe, it may have had a major influence on speculative works on the nature of solar heat in both America and England. Many such writers, apparently coming out of an engineering tradition, appear to have adopted some aspects of W.J.M. Rankine's 1852 paper suggesting that at some point in time or space the tendency of other forms of energy to convert to heat might be reversed. Rankine, responding to Kelvin's assertion of the tendency of mechanical energy to be diffused into heat, suggested that the interstellar medium might be perfectly transparent and diathermos. Supposing there to be a limit to the interstellar medium, just as there is for an atmosphere, there would be eventually be a boundary where all unused radiant heat would be reflected back to its foci. If the star at the foci had become "extinct" this reflected heat would vaporize it resolving it into a store of chemical and potential energy. British engineers such as William Mattieu Williams and Charles William Siemens and numerous American writers, for example John Hume Kedzie, William McKendree Bryant and I.W. Heysinger, attempted to develop similar ideas. W[illiam] J[ohn] M[acquorn] Rankine, "On the dynamical theory of heat," Philosophical Magazine 27 (1864): 194-196; Rankine, "On the reconcentration of the mechanical energy of the universe," Philosophical Magazine 4th ser., 4 (1852): 358-360.; William Mattieu Williams, The fuel of the

One early and perhaps the leading American attack on the teleological aspect of the nebular hypothesis also expressed concerns about the limitations it placed on geological time.[88] Chauncey Wright would influence both philosophers and scientists coming out of Harvard.[89] In the 1860s he published a series of essays, mainly in the North American Review, defending Darwinism from what he felt were unscientific critiques. The first of these essays, "A physi-

---

sun (London: Simpkin Marshall & Co., 1878); C. William Siemens, "On the conservation of solar energy," Nature 25 (9 March 1882): 440-444; J[ohn] H[ume] Kedzie, Speculations. Solar heat, gravitation and sunspots (Chicago, S.C. Griggs & Co., 1886); William M[cKendree] Bryant, The world-energy and its self-conservation (Chicago: S.C. Griggs & Co., 1890) and I.W. Heysinger, The source and mode of solar energy (Philadelphia: J.B. Lippincott Co., 1895). Thomas Sterry Hunt, a Canadian chemist, tried to claim priority over some aspects of Siemens' theory. T. Sterry Hunt, "On the conservation of solar energy," Nature 25 (27 April 1882): 602-603.

[88] [Chauncey Wright], "A physical theory of the universe," North American Review 99 (1864): 1-33. Wright subsequently republished his article in his 1877 Philosophical discussions. Cf. Chauncey Wright, Philosophical discussions (New York: Burt Franklin, 1971 reprint of 1877), 1-33.

[89] Chauncey Wright, Secretary of the American Academy of Arts and Sciences, graduated from Harvard University in 1852 where he had excelled only in mathematics and philosophy. Soon after commencement he received appointment as one of the computers for the American Ephemeris and Nautical Almanac. His occasional papers, primarily in the Mathematical Monthly, had gained him a modest reputation as a mathematician and physicist, but he is probably best known for a series of philosophical essays defending Darwinian evolution. With the exception of brief intervals he spent his entire life after graduating from Harvard in the Cambridge community. Cf. Charles Eliot Norton, "Biographical sketch of Chauncey Wright," in Chauncey Wright, Philosophical discussions, ix-x.

cal theory of the universe," published in 1864, was purportedly a review of Spencer's Essays: Scientific, political and speculative.[90] However, Spencer's work was not even mentioned for the first third of the essay and in discussing Spencer's book Wright took notice only of his essays on the nebular hypothesis and on geology.[91] Wright was apparently concerned about the time limits which the nebular hypothesis placed on Darwin's theory of evolution, but his attack on it focused on its teleological susceptibilities. The nebular hypothesis had been popularly accepted because it could be adapted to the principles of creation and because of its simplicity.[92] It had initially passed scientific acceptance because it explained a "large number of facts and relations" as yet unaccounted for. However, Wright argued, it had fallen from the esteem of astronomers generally, because it had not developed past its character of a happy guess and had been unable to be confirmed in "precise and definite," i.e. empirical, ways demanded by the physical sciences.[93] Wright further appeared to ignore the evidence supporting Helmholtz's nebular condensation theory of solar heat which had led most astronomers to abandon Mayer's meteoric theory.

---

[90][Chauncey Wright], "A physical theory of the universe," North American Review 99 (1864): 1-33.

[91]Ibid., 11-12.

[92]Ibid., 4.

[93]Ibid., 4-5.

In fact, Wright sought to revive the meteoric theory in what he called the dynamical theory, of solar heat and to account for the evolution of the earth through a process of meteoric aggregation.[94]

In spite of his attack on the nebular hypothesis, which he viewed as a threat to Darwinism, Wright conceded that if the "mechanical conditions" assumed by the nebular hypothesis were

> the only ones by which similar effects could be produced, the hypothesis would, without doubt, acquire a degree of probability amounting almost to certainty, even in spite of the absence of independent proof that matter has ever existed in the nebulous form.

Still, he was so convinced that he had discredited the nebular hypothesis that he subsequently dismissed astronomical books which included a pronouncement of the nebular hypothesis for virtually that reason alone.[95]

Wright would continue to challenge the nebular hypothesis, in the Harvard community in which he lived, if not in his writings, until his death in 1875. That he was a mentor of some of the subsequent pragmatists trained at Harvard and that he influenced Simon Newcomb and others at the Nautical Almanac and that he was considered a senior member of the "Metaphysical Club" suggests the potential

---

[94]Ibid., 22-23.

[95][Chauncey Wright], Review of <u>The origin of the stars, and the causes of their motions and their light</u>, by Jacob Ennis, In <u>North American Review</u> 104 (April 1867): 618.

importance of his ideas.[96] While he was less than successful in convincing those around him of the dangers of the nebular hypothesis, his argument that it needed further empirical support to be convincing undoubtedly struck a responsive chord with any astronomer on the receiving end of his arguments.

If Wright was concerned with the encroachment of theism into science, others sought to condemn the nebular hypothesis on the grounds that it diminished the differences between theism and pantheism. Less influential at Harvard than the unorthodox followers of Wright, the more orthodox found little difference between arguments suggesting the nebular hypothesis as the working out of the mind of God and the equation of the laws of the universe with the mind of God. They were less concerned about the validity of the nebular hypothesis than they were about attempts to utilize scientific knowledge to encroach on theological domain.

One such critic of the nebular hypothesis was W.R. Benedict, who in 1886 wrote in the Andover Review that, while Spencer's "'cosmic theism' may represent truth, it can never represent theism."[97] Benedict's primary concern was with the claims which Fiske and others were making about the nature of

---

[96]Edward H. Madden, Chauncey Wright and the foundations of pragmatism (Seattle: University of Washington Press, 1963), 14-29.

[97]W.R. Benedict, "Theism and evolution," Andover Review 6 (1886): 359.

God based upon their evolutionary cosmology. Physical science had shown much that was true, so much that its materialism was a disturbing influence when science addressed issues beyond its domain.[98] The physical sciences were limited to the material realm and, as Benedict's fellow Cincinnatian, J.B. Stallo, had pointed out, support for the essential features of the nebular hypothesis was based on a medieval notion of realism which objectified such notions as force and power.[99] Others would make similar arguments.[100]

Throughout the 1860s and 1870s, and, for that matter, at least until the end of the nineteenth century, the nebular hypothesis played a foundational role among advocates of a compromise theology giving grounds to evolution, as well as among those who sought to limit the effects of evolution theory. The fact that it could be incorporated into arguments on both sides did not seem to mitigate its importance in those arguments. Rather, both sides appear to have become increasingly aware of the limitations on their respective arguments and of the need for further empirical research either providing additional empirical support or at least addressing some of the apparent anomalies arising from

---

[98]Ibid., 609.

[99]Ibid., 616-617. Cf. J.B. Stallo, The concepts and theories of modern physics (New York: D. Appleton & Co., 1882).

[100]Physicus, A candid examination of theism (Boston: Houghton, Osgood & Co., 1878).

the nebular hypothesis. Its role in providing a foundation for Spencerian evolution, as well as an empirical temporal revelation of the mind of God made its veracity extremely important to anyone trying to reconcile science and religion or trying to provide scientific support for the developmental hypothesis. Few trained in astronomy at Harvard could have escaped the broader implications of the nebular hypothesis as advocated by Fiske or as challenged by Wright. Whether or not they addressed those broader implications in their research, the nebular hypothesis was in the air about Harvard.

Astronomers, however, could become the focal point for those seeking to use, or misuse, the nebular hypothesis for their own ends. Eventually they had to address the issue of their responsibility for how their theories and research would be used. In fact, within ten years of Benjamin Pierce's publication of Ideality in the physical sciences, the notion of science as the working out of the mind of God would be openly rejected by Simon Newcomb. In a discussion seeking to raise thoughtful discourse between advocates of "scientific philosophy" and "religious philosophy," Newcomb, representing the former, and Noah Porter, Joseph Cook, James Freeman Clarke and James McCosh, representing the latter, tried to establish the grounds for the debate in essays in the North American Review.[101] What little mention that was

---

[101]Simon Newcomb, Noah Porter, Joseph Cook, James Freeman Clarke and James McCosh, "Law and design in nature," North American Review 128 (1879): 537-562 and Simon Newcomb,

made of the nebular hypothesis suggested the extent to which it had become an argument used by all sides in the past.

Among the advocates for religious philosophy, only Joseph Cook sought to form an argument for the necessity of a teleological theory over a mechanical theory of force based on the "chasm between the primordial star-dust and the solar system."[102] In both his introduction and his response, Newcomb clearly sought to limit the role of science to enunciating the laws of nature based solely on antecedent causes and without regard to teleological consequences.[103] The nebular hypothesis, Newcomb asserted, had only partially moved thought away from a theological notion of special creation. Beyond making the creation of the world a natural process, it still allowed the possibility for a teleological interpretation and it had not been until after Darwin's publication of Origin of Species that it had been able to even partially escape from the clutches of religious, i.e. teleological, philosophy.[104]

By the end of the nineteenth century, most American astrophysicists would accept Newcomb's position separating

---

"Evolution and theology. A rejoinder," North American Review 128 (1879): 647-663.

[102]Newcomb, Porter, Cook, Clarke and McCosh, "Law and design in nature," 552.

[103]Ibid., 540-541.

[104]Newcomb, "Evolution and theology. A rejoinder," 659.

theological implications from the nebular hypothesis. Even so, for those trained at Harvard just after the Civil War the interrelationship between the theory of evolution, the nebular hypothesis and the mind of God must have been an important influence. Whatever position they took on the matter, it could not have but helped enhance the import of resolving anomalies and uncovering additional support for or evidence to reject the theory. Philosophical debates founded on interpretations of the nebular hypothesis were not the only influence motivating research in the fields that would become astrophysics. Pragmatic concerns about the relationship between solar heat and terrestrial meteorology would provide an equal, if not greater, influence on the genesis of astrophysics.

CHAPTER 4

POPULAR METEOROLOGY AND THE RISE OF ASTROPHYSICS: WEATHER PROGNOSTICATORS, THE U.S. ARMY SIGNAL SERVICE AND THE DEMAND FOR LONG-RANGE WEATHER PREDICTION.

The development of a coordinated program of meteorological research in 1870, under the auspices of the Army Signal Service, provided the system of meteorological observations which Henry had been pushing for since the decline of the Smithsonian meteorological network during the Civil War. It also provided increased support for research in fields subsequently defined by astrophysical research.

The government distribution of Western lands under the Federal Homestead Act of 1862, the Union Pacific Act of the same year, and the Timber Culture Act of 1873 all encouraged increased Western settlement. Railroads encouraged settlement as a means of financing their roads, soldiers sought western lands as part of their payment for war services. Increased rainfall in the early 1870s encouraged the belief that adequate rainfall existed or that increasing rainfall would result from the process of settling the land.[1] In his 1878 report on western lands Grove Karl Gilbert reported interpretations of the cause for increase in rainfall varying from the laying of railroad tracks and/or

---

[1]John Brinkerhoff Jackson, "The plains," in American space. The centennial years, 1865-1876 (New York, W.W. Norton & Co., 1972), 173.

telegraph lines to cultivation of the soil and even Divine providence.[2] That he also pointed out the unacceptable presumptions which had allowed the advocates of these theories to make their conclusions did not lessen their impact.

Increasing rainfall on the plains right after the civil war gave implicit support to just about any theory which connected increased rainfall with increased western settlement. But the subsequent decline in rainfall after the mid 1870s led to increasing uncertainty regarding their viability for agricultural pursuits. Some land developers, advocates of western expansion and farmers who had settled in the region sought explanations which would justify maintaining their positions. Historians have thus far focused either on the search for water or limited the debate to land boomers advocating a garden of Eden opposed by east coast scientists and intellectuals who declared the plains to be a great American desert. Whether the demand for research into long range weather prediction, to allow farmers to adjust their crops to wet and dry periods, resulted from a debate between the promoters of western settlements and scientists asserting the existence of a desert, or grew out of legitimate farmer concerns about the extent of rainfall in the Western plains cannot be concluded here. That such a demand

[2]Quoted in Walter Prescott Webb, "The search for water in the great plains," in The great plains (Boston: Ginn & Co., 1931), 377-378.

was created as part of the aftermath of western settlement after the Civil War is beyond question.[3]

During the 1870s and 1880s the connection between terrestrial meteorology and solar physics increasingly attracted the attention of popular weather prognosticators. These "weather prophets" competed with the Signal Service to provide weather "information" to the public. The alleged ability to make long-range weather predictions made the "weather prophets" particularly popular among the farmers and land merchants of the western plains who hoped to be able to plan agricultural practices based upon their predictions.

General Myer, who was placed in charge of the new Signal Service in 1870, appears to have been interested in the application of physics to the study of terrestrial atmospheres and possibly to the study of a connection between solar phenomena and terrestrial meteorology. In fact, Cleveland Abbe, one of the scientists hired to do meteorological research in the Signal Service, claimed in an 1891 lecture that he had felt constrained to "dissuade" Myer

---

[3]For instance Walter Prescott Webb, "The search for water in the great plains," in The great plains (Boston: Ginn & Co., 1931), 318-384 and Henry Nash Smith, Virgin land; the American West as symbol and myth (Cambridge, Harvard University, 1970) both find the great plains to have been a desert where settlers struggled to find an adequate water supply. On the other hand John Brinkerhoff Jackson, "The plains," in American space. The centennial years, 1865-1876 (New York: W.W. Norton & Co, 1972), 166-180, argues that settlers adapted their crops to the plains and that the literature on the plains never or rarely described them as a desert.

from too strong an "invasion" of astrophysical research.[4]

Myer's proposed projects included spectroscopic observation of the sun on the horizon. From the data thus collected he hoped to determine atmospheric conditions facilitating weather predictions. A spectroscope made by Lockyer was obtained for the purpose of these studies, but according to Abbe the program was never implemented.[5] Myer also proposed a study of the atmospheres of other planets. Apparently this was, at least in part, a motivation behind Myer's 1873 agreement with the Dudley Observatory for the use of its equipment. In spite of Abbe's dissuasion, Myer arranged to obtain the sun spot observations of the British Astronomer

---

[4]Cleveland Abbe, "The meteorological work of the U.S. Signal Service, 1870 to 1891," U.S. Department of Agriculture, Weather Bureau, Bulletin 11 (1894): 283. Abbe may have been confusing General Myer with General Hazen in his 1891 lecture. Certainly his testimony before the Allison Commission in 1884 portrayed General Myer's interests in weather prediction as limited to the establishment of probabilities while General Hazen encouraged the development of the principles by which those probabilities could be improved.

[5]Ibid., 283. The connection with Lockyer is significant since Lockyer's own research at this time included studies of the relationship between sun spots and terrestrial meteorology. See J. Norman Lockyer and W.W. Hunter, "Sun-spots and famines," 19th Century 2 (1877): 583-604. "Sun-spots and famines" was reprinted in Popular Science Monthly--Supplement 2 (1877): 128-134; and provided an excellent summary of research on the relationship between the sun and terrestrial meteorology until 1877.

David P. Todd. Again the project seems not to have developed further.[6] Many of Myer's contemporaries noted his political savvy and considering the subsequent problems in the Signal Service over theoretical research under General Hazen, Myer may have abandoned his solar physics projects when important politicians raised discouraging questions.[7]

If Abbe's 1893 recollections about his concerns in invading a cognate field are accurate, Abbe may have played a significant role in limiting the early Signal Service Weather Bureau's astrophysical activity. By the 1870s science had begun to specialize, but Abbe was not so much concerned with invading astrophysicists turf as he was of supporting astronomical research. When he first began meteorological research, he viewed it as a means of drawing support for astronomical observatories. His 1893 pronouncements suggest he had transferred his professional interest from astronomy to meteorology. Even Abbe, however, found the meteorological implications of solar eclipse observations important enough for Signal Service work and for the support of Samuel Pierpont Langley, one of America's early astrophysicists.

---

[6]Todd's sun spot observations, however, were regularly published in the Signal Service's Monthly Weather Review.

[7]T.B. Maury, "The telegraph and the storm: The United States Signal Service," Harper's New Monthly Magazine 42 (July 1871): 417; "Sketch of General Albert J. Myer," Popular Science Monthly 18 (1880): 408-411; New York Times, 26 August 1880, 5.

When Langley took over the directorship of the Allegheny Observatory, he immediately focused his research on the sun.[8] Langley's first publication was of his observations during the 1869 solar eclipse expedition, where Lane's theory of solar heat played an important role in the observations.[9] Langley's first connection with the Signal Service came in 1878 when he was invited to join the Signal Service's eclipse expedition to Pikes Peak. According to Abbe, General Myer had turned to the Pikes Peak expedition in 1878 in part because of the information it would provide for atmospheric physics. The meteorological problems formulated as a result of that expedition, Abbe suggests, were central to meteorological research for at least the next fifteen years. Langley's inclusion in the Signal Service's Pike's Peak expedition emphasizes Myer's and Abbe's awareness of the intimate connection between the developing field of "astronomical physics" and meteorology--an awareness which Langley also acknowledged in his subsequent publications.

---

[8]Langley claimed that the atmospheric conditions at Allegheny Observatory were so bad as to make stellar astronomy virtually impossible. He turned to the study of the sun and particularly to the use of the spectroscope for solar analysis in order to avoid these otherwise serious atmospheric impediments. Charles D. Walcott, "Biographical memoir of Samuel Pierpont Langley, 1834-1906," National Academy of Sciences, Biographical Memoirs 7 (1912): 248.

[9]"Report by Professor S.P. Langley of observations at Oakland, Kentucky," United States Coast Survey observations of total eclipse of Aug. 7, 1869 (Washington, DC: Coast Survey, 1870), 21-22.

Myer's encouragement of research on the connections between solar physics and meteorology foreshadowed the role of astrophysical research sponsored by the Signal Service in the 1880s. Whether he intended to actively push research on solar physics, as Abbe claimed in 1891, or sought merely to provide accurate weather predictions based on existing knowledge, as Abbe testified before the Allison Commission in 1884, Myer's death in 1880 left those developments to his successor General William Babcock Hazen. By 1880 a number of factors, many associated with Hazen's recent military career on the western plains, had influenced his decisions to encourage further research on the physics of the sun and its relation to terrestrial meteorology.

While he continued Myer's encouragement of solar physics research, Hazen's motivation clearly drew from his objections to land speculator's claims encouraging the settlement of dry western plains. Many early expansionists insisted that the desert did not exist. By 1872 agents for Jay Cooke's Northern Pacific Railroad were extolling the agrarian virtues of the territories through which their track ran. These claims had offended the sensibility of Colonel William B. Hazen. At the time Hazen was stationed at Fort Buford in the Dakota Territory and he had spent most of his time since the Civil War in the West. With the publication of a fanciful article in Harper's Monthly, titled "Poetry and philosophy of Indian summer," Hazen's "indignation" at these "shameless falsehoods" reached its limit. The article repeated Northern Pacific's claims that the section west of the 100th meridian would be agriculturally valuable.[10]

Hazen challenged these claims in a letter to the New York Daily Tribune warning those holding bonds in the Northern Pacific Railroad to exchange them for good land in the Red River valley and not to exchange them for land west of the 100th meridian where the humidity was "insufficient for any general agriculture."[11] Hazen's letter was reprinted in newspapers nationwide and elicited strong responses from both detractors and supporters.[12] Hazen's most vocal detractor,

---

[10]T.B. Maury, "Poetry and philosophy of Indian summer," Harper's Monthly Magazine 48 (December 1875): 89-98. For further discussion of the debate over the viability of Western lands, which continues to take place in the historical literature, see page 113. Both Hazen and Custer began their debate while in the West. That Hazen eventually returned to the East and continued the debate while a part of the Washington based political and scientific communities does not diminish his roots as a soldier based in the West. For a more detailed, but thoroughly sympathetic, discussion of Hazen's role in the conflict over the agricultural viability of the high Western plains, see Marvin E. Kroeker "The arid lands controversy," in Kroeker, Great plains command: William B. Hazen in the frontier west (Norman, Oklahoma: University of Oklahoma Press, 1976), 120-142 and Marvin E. Kroeker, "Deceit about the garden: Hazen, Custer and the arid lands controversy" North Dakota Quarterly 38 (Summer, 1970): 5-21.

[11]W.B. Hazen, "Worthless railroad land. The Northern Pacific Railroad country--Views of Maj.-Gen. Hazen," New-York Daily Tribune, 7 February 1874, 2.

[12]Over the next ten days the New York Daily Tribune reprinted comments supporting Hazen from the Cleveland Herald, the Providence Press, the Cincinnati Commercial, the Toronto Leader, and the Omaha Herald as well as critical letters to the Minneapolis Tribune by a General Rosser and letters objecting to Hazen's assertion from Representative Martin Maginnis of Montana. "Worthless Northern Pacific railroad lands," New York Daily Tribune, 14 February 1874, 2. A strong supporter of Hazen's position in its editorial pages, the Daily Tribune informed its readers that Maginnis'

then Colonel George Custer, had been involved in regular conflicts with Hazen ever since Hazen had arrested Custer after the latter had violated some West Point regulations while a student there. Custer's attacks on Hazen in the press[13] and his claims about the agricultural viability of Western lands, published in "Life on the Plains," would embroil the two in additional controversy.

In 1875 Hazen responded to Custer in a pamphlet, Some Corrections to 'Life on the Plains', and presented a more general attack on the climatological theories of western expansionists in Our barren lands: The interior of the United States west of the 100th meridian and east of the Sierra Nevadas.[14] His subsequent report to Congress on the plains between Fort Kearney and the Rocky Mountains--the region

---

letter had been forwarded to them through the Northern Pacific Railroad offices. The Tribune conveniently ignored the more influential criticisms published in the Minneapolis Tribune and elsewhere by General George Custer.

[13]"The Northwest. General G.A. Custer in reply to General Hazen," Minneapolis Tribune, 17 April 1874, 4.

[14]W.B. Hazen, Some corrections of "Life on the plains" (Saint Paul, MN: Ramaley & Cunningham, 1875) and William Babcock Hazen, Our barren lands: The interior of the United States west of the 100th meridian and east of the Sierra Nevadas (Cincinnati: R. Clarke, printers, 1875). Both works were attacks on claims by George Armstrong Custer that the high western plains were suitable for intensive settlement. Hazen was at the time commander of Fort Buford, Dakota territory. The Hazen-Custer conflict over the quality of western plains for settlement, particularly Hazen's claim that Custer was not a good observer, might provide insights into Custer's subsequent judgment leading to the Custer's "last stand."

neighboring that on which Ferdinand Hayden and his followers based their theories--not only reported overt corruption in the War Department's post-trader system, but probably contributed to his appointment by President Hayes to head the Signal Service after General Myer's death in 1880.

By the 1880s, however, the belief that "rain follows civilization" had taken on even more legitimate support, much of it being derived from Hayden's Geological Survey reports. As head of the Geological and Geographical Survey, Ferdinand V. Hayden had encouraged the settlement of western plains suggesting

> that the planting of ten or fifteen acres of forest-trees on each quarter-section will have a most important effect on the climate, equalizing and increasing the moisture and adding greatly to the fertility of the soil.[15]

The change in climate resulting from appropriate agricultural activities, Hayden claimed, had already increased the rainfall of eastern Nebraska.[16] Hayden's annual reports during

---

[15]General Land Office, Report of the commissioner of the general land office for the Year 1867 (Washington: Government Printing Office, 1867), 135-136.

[16]Regardless of the discussion of the scientific validity of Hayden's claims, they should be viewed in light of their inseparable union with his promotion of western settlements. In Hayden's 1871 Preliminary report of the United States geological survey of Wyoming and portions of contiguous territories, he declared: "Never has my faith in the grand future that awaits the entire West been so strong as it is at the present time, and it is my earnest desire to devote the remainder of the working days of my life to the development of its scientific and material interests, until I shall see every Territory, which is now organized, a State in the Union." Ferdinand V. Hayden, Preliminary report of the United States geological survey of Wyoming and portions of contiguous territories, 42d Cong., 2d Sess., House Executive Document, vol. 15, No. 325, (1871), 6-8.

the 1870s became the focus for land speculators seeking justification for hopes of greater rainfall.

One scientist employed by Hayden, Samuel Aughey, subsequently taught at the newly opened University of Nebraska. There he met Charles Dana Wilber, a land speculator and amateur scientist, who coined the phrase "rain follows the plow."[17] Aughey's and Wilber's publications were widely circulated in the 1880s, providing additional "scientific" support for views popularized by Hayden and others advocating western expansion in the 1870s.

Professor F.H. Snow at the University of Kansas also developed a thesis that rain followed civilization on the western plains. Snow admitted that the nebular hypothesis required a reduction of rainfall over thousands of years. The cooling of the sun and the concomitant reduction in solar

---

[17]Aughey after working for Hayden became Professor of Natural Sciences at the University of Nebraska when it opened. Wilber, who coined the phrase "rain follows the plow" was a land speculator and amateur scientist. Along with Hayden, they became the leading scientific authorities encouraging settlement on the dry western plains. For a general discussion of their work see Henry Nash Smith, "Rain follows the plow: The notion of increased rainfall for the Great Plains, 1844-1880," Huntington Library Quarterly 10 (February, 1847): 169-193 and Smith, "The garden and the desert," in Virgin land: The American West as symbol and myth (Cambridge, MA: Harvard University Press, 1970), 174-183. See also, Samuel G. Aughey and Charles Dana Wilber, Agriculture beyond the 100th meridian or a review of the U.S. Public Land Commission (Lincoln, NE: n.p., 1880); Samuel G. Aughey, Sketches of the physical geography and geology of Nebraska (Omaha, NE: n.p., 1880) and Charles Dana Wilber, The great valleys and prairies of Nebraska and the Northwest (Omaha, NE: n.p., 1881).

heat received by the earth would necessarily lessen rain cloud formation. This reduction, over recorded meteorological history would be virtually unmeasurable. On the other hand, local factors would often result in increased rainfall.[18]

The Indians, claimed H.R. Hilton, another member of the Kansas Academy of Science, had created the great American desert through their "annual" burning of the plains preventing tree growth and resulting in such a thick "herbage" that what little rain fell could not be absorbed by the soil. When Kansas was first settled, he argued, the great American desert had reached practically to Topeka, but the meteorological records over twenty years had shown a significant increase in rainfall. The "civilized" settlement of Kansas, the planting of trees and tillage of the soils had increased rainfall, he argued, and would make Kansas a fertile agricultural state.[19]

Similar theories would be proposed by Colonel William Thompson, one of the first to homestead around Bismark, North Dakota. Colonel Thompson, described by the New York Daily Tribune as a "veteran boomer," proclaimed as late as 1884 that the soil in that region of the Dakotas would not allow

---

[18]F.W. Snow, "Is the rainfall of Kansas increasing?" Transactions of the Kansas Academy of Sciences 9 (1883-1884): 101-103.

[19]H.R. Hilton, "The rainfall in relation to Kansas farming," Transactions of the Kansas Academy of Sciences 7 (1879-1880): 40-41.

the rainfall to penetrate before it would evaporate. However, he contended:

> by breaking and cultivating the ground this condition of imperviousness is destroyed, and most of the rainfall runs into the ground out of the reach of the evaporating winds. This tends to localize the rainfall and to give the soil a nutritious moisture increasing the dews and having a beneficial effect on the climate.[20]

It is not surprising that Colonel Thompson had been stationed with General Custer at Fort Abraham Lincoln in 1874 and may have been involved with him in the conflicts with Hazen.[21] That such views would draw their support from a scientist whose research Hazen had cited in his battles with Custer would, no doubt, influence Hazen's desire for more accurate research.

The work done by scientists since William Herschel on the periodicity of sun spots would also catch the public eye in the late 1870s. The "boomers" of western lands had justified their claims on the unusually rainy seasons of 1872 and 1873. The subsequent dry seasons of the second half of the 1870s belied their arguments and some turned to the periodicity of sun spot activity and its alleged connection with rainfall to salvage some of their claims for the viability of the plains. While the climate was generally getting better,

---

[20] "The Dakota plains. Rainfall and tree-planting growth of Bismark," New York Daily Tribune, 8 September 1884, 2.

[21] New York Daily Tribune, 8 September 1884, 2.

the influence of sun spots would, on occasion, hide the improvement.

Others sought to find in the connection between solar phenomena and terrestrial rainfall a mechanism for long-range prediction of the weather. The increasing demands for such weather prediction both frustrated the meteorologists and encouraged much early solar physics in the United States.

General Hazen's advocacy of further research on solar physics to be used to counter the misuse of meteorology and pseudo-meteorological principles by western expansionists was welcomed by Isaac Noyes, one of his assistants at the Signal Service. Noyes actively involved himself in countering the assertions of both land "boomers" and weather prophets in a series of articles published in the Western Review of Science and industry. In February 1877 the Western Review, published in Kansas City, presented as confirmed fact the research of Schwabe and Wolf establishing an eleven-year sun spot cycle. Since sun spots appeared to be associated with variations in solar energy, the article queried if those variations were "manifested in terrestrial effects, and, if so, in what manner, and to what extent?" Research, the Western Review concluded, had demonstrated much, but was still incomplete. The investigation was, the journal maintained "in its crude, preliminary stage, where the truth is caught vaguely and by glimpse, rather than seen clearly and by a steady gaze."[22]

[22] "Sun spots and their effects," The Western Review of Science and Industry 1 (February 1877): 665-666.

Further research was needed.

In May the Western Review summarized a committee report of the British Association for the Advancement of Science which claimed that recent research had "increased the probability of a physical connection between the condition of the sun's surface and the meteorology and magnetism" of the earth.[23] The article concluded: "Whatever be the probability of the conclusions derived from these various researches they at least show the wisdom of studying together in the future these various branches of science."[24] Such advise was not lost on the Review's readers. Soon it became a medium for the theories of those advocating the settling of western plains.

By July the Western Review was reporting the local research of a Professor J.H. Tice whose storm theories, the Review claimed, were being sustained by the research of the Italian astronomer, Father Secchi. Secchi's research, the Review stated, had confirmed a "remarkable connection between the magnetism of the earth and the changes of the weather." Even where there had been no significant variations in the barometer, there appeared to be a correlation between magnetic disturbances and changes of the wind and storms.[25]

---

[23]"Meteorology and sun spots," Western Review of Science and Industry 1 (May 1877): 176.

[24]Ibid., 176.

[25]"The storms of June and July, 1877," Western Review of Science and Industry 1 (July 1877): 281.

Tice, who edited The American Meteorologist, would use that publication as a forum for research on the relation between solar and meteorological connections as well.[26]

Drawing on the work of Elias Loomis and Heinrich Schwabe, Colonel Henry Inman, another supporter of settlement in western Kansas, emphasized the relationship between solar spots and violent storms which periodically ravaged Kansas. He suggested that these periodic eras of turbulent storms resulted from changes in the earth's magnetic activity associated with the area and number of sun spots. He claimed that over the next ten years these storms would become even more violent and frequent because the sun's eleven-year cycle would reach its maximum in 1879 and the fifty-six year cycle would culminate in 1884. In spite of these allegations, he allowed that Laplace might be correct in postulating "some compensatory law which interferes at the proper time to re-adjust any impending clash."[27] However, his conclusion that this research confirmed the accepted theory that "variations in solar heat produce a similar variation in the terrestrial evaporation and an increased tendency to violent storms

---

[26]Tice's American Meteorologist, however would survive only two years. Cf. American Meteorologist (St. Louis), 1875-1877.

[27]Henry Inman, "The connection between storms and sun-spots, with record of the celebrated storms of 1600 years," The Western Review of Science and Industry 1 (September 1877): 394. In 1878 Inman began publishing the Chronoscope which presumably became a voice for his theories.

indicates that he was not familiar with Alexander's and Henry's research suggesting that increased sun spot activity meant a decrease rather than an increase in solar heat.[28]

The relationship between the sun and terrestrial meteorology was further developed in a paper read before the Kansas City Academy of Sciences in January, 1878. Colonel R.T. Van Horn, president of the society, surveyed the heat and electric theories of atmospheric winds challenging the view, which he maintained had been accepted since Halley first proposed it in 1686, that atmospheric winds were caused by the sun's heat being concentrated where its rays struck the earth vertically. He attacked this view, alleging that promoters presumed the atmosphere to be an inert fluid.

Van Horn suggested that instead a dynamic force, electricity, played an important role. Electricity or magnetism, he opined, was the primary factor driving meteorological phenomena and, while variations in the earth's magnetic field were influenced by the sun, other factors influencing changes in the magnetic fields made unreliable weather predictions based primarily on solar influences.[29]

---

[28]Ibid., 397-398.

[29]R.T. Van Horn, "About the atmosphere and its phenomena," Western Review of Science and Industry 1 (February 1878): 710-724. Van Horn utilized the view that both electricity and magnetism were fluids. Their dynamic roles as parts of the atmosphere became the basis for his attack on those who allegedly saw the atmosphere as an inert fluid. Fluid theories of heat, electricity and magnetism had been rejected by most, if not all, reputable scientists by the 1850s.

Van Horn's article received favorable comment from the local press and scientific community. The Kansas City Times praised Van Horn for demonstrating that "atmospheric currents are not and cannot be referable to the heat of the solar rays. . . ."[30] Such views suggest that the conflict between scientists using thermodynamic arguments and western expansionists adopting the doctrine that rain follows the plow may have been well developed already. One of Youman's friends wrote that he would advise him to include the article in Popular Science Monthly[31] and the Review informed its readers that Van Horn's article had received numerous favorable reviews in those journals with which it was exchanged.[32]

The scientific speculation and popular articles on the connection between solar activity and terrestrial rain fall were further incorporated into the pronouncements of "weather prophets" who were widely followed in the press during the latter part of the nineteenth century. Their claims to be able to predict the weather weeks, months, or even years in advance appear to have been significant in

---

[30]Kansas City Times, 29 March 1878, 2.

[31]H.W. Bellows to a Mr. Guffin, quoted in Western Review of Science and Industry 2 (April 1878): 62. Either Bellows never wrote to Youmans or more likely Youmans took a different view of Van Horn's paper. In either case the paper was not reprinted in Popular Science Monthly.

[32]"Editorial notes," Western Review of Science and Industry 2 (April 1878): 62.

garnering a following on the western plains. Although Noyes often disagreed with many ideas which the western scientists were presenting in the Review, he directed his papers primarily against these popular weather "prophets."

In 1877 Henry George Vennor (1841-1884) began publishing the Vennor Almanac in which he forecast the weather with a sufficient degree of success to gain quite a following.[33] After his retirement from the Canadian Geological Survey in 1880, he began corresponding with a number of American and Canadian newspapers regarding his meteorological predictions.[34] Only a few such papers appear to have taken him seriously but the fact that he received wide press coverage and was taken seriously by numerous readers indicates the importance given to his "predictions." The New York Times criticized his predictions as too general and too loose such that if he forecast a major snow storm in the second week in February, he would take credit for any storm which occurred during the month.

---

[33]W. Stewart Wallace, ed., The MacMillan dictionary of Canadian biography (Toronto: MacMillan of Canada, 1978), c.v. "Henry George Vennor." Vennor had received a college education at McGill University. From 1865 till 1880 he had been employed as a geologist on the Canadian Geological Survey. In addition to his meteorological speculations he also had some reputation as an ornithologist.

[34]Vennor claims to have quit the Canadian Geological Survey to work as an engineer "interested in phosphate mines in the Dominion." "Vennor and his system," New York Times, 25 August 1881, 5.

Vennor's creativity in explaining his failed forecasts appears to have given him as much status as his "successful" weather prophecies.[35] His reputation as a weather "prophet" was sufficiently great that there was some suggestion that he ought to replace Hazen, who had been severely criticizing Vennor's methods, at the Signal Service.[36] Vennor's assertion that weather came in two-and three-year seasonal cycles, he claimed, was derived from empirical observations rather than from theory based speculations. Some supporters inferred that these cycles were associated with both planetary motion and sun spot activity.[37]

In his articles in the Western Review of Science and Industry Noyes extolled the benefits of the "weather map" as developed by the Signal Service and attacked the "weather prophets" and Vennor in particular. The weather map showed that storm centers were associated with low pressure regions which were, Noyes claimed, the result of concentrations of

---

[35]"Wiggin's storm," New York Times, 13 December 1882, 4. The New York Times cites a letter from Vennor to the Albany Argus in which he explained the failure of his predicted January thaw by the fact that the weather had been so severe that it had pushed the thaw into February where he had predicted another thaw after a brief spell of severe weather. The accurately predicted severe weather had simply crowded out the thaw. "Mr. Vennor explains," New York Times, 6 February 1881, 7.

[36]"Vennor and his system," New York Times, 25 August 1881, 5.

[37]"Our El Mahdi," New York Times, 28 December 1883, 4.

solar heat in the atmosphere.[38] The sun, then, was the primary factor in changes in the weather. Its influence at any particular time in any particular locality would be modified by the rotation of the earth on its axis, its orbit around the sun and the change of its axis in relation to the sun.[39] The combination of these factors led to the development of "high" and "low" barometric regions which gave rise to the storms.

Anyone, who studied the daily weather maps, Noyes, argued, could fairly accurately predict the day's weather and, with somewhat less accuracy, the weather until the arrival of the next low. The public, however, was demanding that the Signal Service compete with weather prophets, such as Vennor, who predicted storms months and even years in advance. The Weather Bureau could, Noyes conceded, "guess at the weather for ten or even a hundred years ahead" but the public would not be as charitable for the errors of a government institution as they had been of Vennor.[40] While too many citizens were misled by Vennor's policies, Noyes insisted that the Signal Service would not accept his predic-

---

[38]Isaac P. Noyes "Prophecy of the weather," Kansas City Review of Science and Industry 4 (September 1880): 268.

[39]Isaac P. Noyes, "A new view of the weather question," Western Review of Science and Industry 2 (July 1878): 219.

[40]Isaac P. Noyes, "The weather prophecies(?) of Vennor," Kansas City Review of Science and Industry 4 (February 1881): 628.

tions as superior knowledge until he could predict, on first principles, where a storm center would be months in advance.

By April, 1881, the increasing demand for long-range weather prediction apparently influenced a slight modification in Noyes' position. Not only should the weather prophets use the weather map as the basis of their short-range predictions, it was time that the Signal Service began making longer range predictions of four or five days. These "sub-indications" would, of course, not have the same reliability as the daily "indications."[41] Longer range weather prediction would have to wait for such a knowledge of the weather would require a knowledge of the topography of the land, the relative distribution of land and water, of plains and mountains, and even of the environmental changes resulting from the encroachment of European settlers.[42]

In subsequent papers Noyes increasingly focused on the necessity of understanding the nature of the sun's heat source, the degree of its constancy and the role of changes in solar activity as fundamental prerequisites for predicting long-range weather changes. He challenged the view that weather was determined by electrical phenomena. Electricity had become the science of the gaps. All too often when a scientist or a public figure could not explain a phenomena,

---

[41]Isaac P. Noyes, "The storm center and weather prophets," Kansas City Review of Science and Industry 4 (April 1881): 754.

[42]Ibid., 752.

Noyes complained, they would put on a "wise look" and "credit" the cause to electricity.[43] Since the significant factor in the movement and development of storms was the barometric low pressure region, electricity did not appear to be a significant factor.

By the middle of 1882 Noyes maintained that "the sun, through his enormous heating power, is the cause, and the only cause of our daily changes. . . ."[44] The weather map had shown how the passive environmental factors on the earth, such as mountains, bodies of water and forests, affected the weather at least as much as the sun's heat. In 1883 the Signal Service began a study which ultimately "exploded" the popular allegations of a relationship between increased rainfall and railroad and telegraph lines.[45]

---

[43]Isaac P. Noyes, "Where our storms come from," Kansas City Review of Science and Industry 5 (February 1882): 639. As early as 1828 at the Scientific Congress of Berlin, Alexander von Humboldt called for a study of terrestrial magnetism. Gauss took up the study at Göttingen in 1833. By 1851 John Lamont had found a period of ten and one-third years in the range of daily magnetic declination changes. Since this corresponded with Heinrich Schwabe's announcement of a probable decennial period in sun spot activity, also published in 1851, there had subsequently been a great deal of interest in the possible connection between the two. The following year Edward Sabine announced an approximately ten year period in the number of spasmodic vibrations of the magnetic needle which Humboldt called "storms." In the same year Rudolf Wolf and Alfred Gautier made independent conclusions which agreed with Sabine.

[44]Isaac P. Noyes, "False notions in regard to the weather," Kansas City Review of Science and Industry 6 (June 1882): 91.

[45]"Report of the chief signal officer for 1883," Kansas City Review of Science and Industry 7 (December, 1883): 502; and review of the Report of the chief signal officer For 1883, by General W.B. Hazen, Kansas City Review

By the mid-1880s the Signal Service was either attacking or attempting to study most of the popular theories of meteorology.[46] However, by 1882 another Canadian "weather prophet," E. Stone Wiggins, had replaced Vennor as the prognosticator most followed in the press. Unlike Vennor, who based his predictions on everything from planetary conjunctions to sun spots to feelings in his bones[47], Wiggins claimed to have a scientific system which anyone could use. His periodic successes made him a major competitor with Vennor for coverage in the popular press. Wiggins' letter to President Arthur in November of 1882, predicting a major storm the following March and suggesting that the President order American ships into safe harbors during that period, was widely disseminated in the press.[48] Hazen publicly

---

of Science and Industry 8 (February 1885): 596-597.

[46]In 1883 for instance the Signal Service compiled a book of weather sayings based upon material it had collected. "General Hazen hunting for prognosticators," New York Times, 19 August 1882, 3 and H[enry] H[arrison] C[hase] Dunwoody, Weather proverbs (Washington: Government Printing Office, 1883). A supplement was published in 1892 "Weather proverbs. The signal service bureau making a collection of old saws," New York Times, 28 August 1892, 12. Between 1889 and 1891, the Signal Service put together a comprehensive bibliography of meteorological publications to aid it in its studies. Oliver L. Fassig, ed., Bibliography of meteorology, (Washington: Signal Office, 1889-1891).

[47]Francess G. Halpenny, gen. ed., Dictionary of Canadian biography (Toronto: University of Toronto Press, [1982]), s.v. "Henry George Vennor," by P.R. Eakins.

[48]For example "The Wiggins storm of March 9th to 11th, 1883," Kansas City Review of Science and Industry 6 (1883): 673; New York Daily Tribune, 22 December 1882, 5. New York Times, 13 December 1882, 4. When the time for the March storm approached several incidences of panic were reported including a refusal of Gloucester fishermen to take

criticized the press for publishing the letter contending that:

> All predictions of the weather to be expected a month or more in advance, whether based upon the position of the planets, or of the moon, or upon the number of sunspots, or upon any supposed law of periodicity of natural phenomena, or upon any hypothesis whatever which to-day has its advocates, are as unreliable as predictions of the time when the world will come to an end.[49]

Hazen went on to assure his readers that the Signal Service was endeavoring to keep pace with all serious advances in meteorology, while assiduously avoiding anything that suggested mere imaginings. Wiggins' apparently "successful" prediction of a February storm brought increased interest on the part of the New York Daily Tribune but by the time his prediction of a major storm failed in March the Tribune's reports had been reduced to mirthful sarcasm. As the Tribune's subsequent pronouncements about the relationship between sun spots and meteorology became far more subdued, they continued to point out the need for additional research to resolve this most important issue.[50]

---

the fleet out until after the "storm" had passed. New York Times, 22 February 1883, 1; New York Times, 7 March 1883, 5; New York Times, 8 March 1883, 1; New York Times, 9 March 1883, 1; New York Times, 10 March 1883, 1; New York Times, 11 March 1883, 1; New York Times, 13 March 1883, 5.

[49]"The storm predicted for March. What the chief signal officer says," New York Daily Tribune, 22 December 1882, 5.

[50]Both the New York Times and the New York Daily Tribune continued to cover theories on the relationship between sun spot activity and meteorological phenomena. When an M.A. Veeder corresponded with the Tribune in February 1888 pointing out the connection between recent weather fluctuations and sunspots, the papers editor commented that "The case has hardly been made out yet for those who connect

Hazen's claim that the Signal Service was keeping up with recent advances in meteorology implied research concerns in the related fields of physical astronomy. Those interests were also subtly expressed in Noyes's subsequent articles in the Kansas City Review of Science and Industry. In "Meteorology revolutionized by the weather map" Noyes maintained that the weather map developed by the Signal Service had been developed to the point where it showed the role "heat plays in the economy of nature as never before."[51] But the increasing realization of the thermodynamic aspects of meteorology only increased the importance of understanding the nature and role of solar phenomena in terrestrial meteorology.[52]

In spite of research on solar spots and meteorology going back before the middle of the nineteenth century, the question had not been settled. Few astronomers were willing to say more than the fact that heat was the most fundamental

---

solar disturbances with terrestrial storms and cold waves. But the cycle referred to by The Tribune more nearly corresponds to the 26-day period of the sun than to that of the moon." New York Daily Tribune, 22 February 1888, 7.

[51]Isaac P. Noyes, "Meteorology revolutionized by the weather map," Kansas City Review of Science and Industry 8 (May 1884): 41.

[52]While this interest often focussed on the nature of solar heat, it included concerns about the role of sun spots in potential variations in solar heat and the possible solar influences on terrestrial magnetism.

factor in the development of terrestrial meteorology and the sun was the primary source of heat. Langley and others had shown by 1884 that variations in solar heat due to sun spot activity did not change the heat received from the sun by more than 0.1 percent. Yet Benjamin Apthorp Gould continued to point out that even so slight a change, while not greatly affecting the thermodynamic processes could very well make ever so slight changes in wind patterns which did determine where the rain would fall.[53]

Astronomers had also concluded that the heat radiating directly from the sun was inadequate to maintain the temperature of the earth's atmosphere more than a few degrees above the temperature of space. The problem of the absorption of heat by the atmosphere than became a problem not only of terrestrial meteorology but of accuracy in measuring solar radiation. The nature of the problems being raised in both meteorology and astronomy encouraged overlaps in research. Nowhere were those overlaps more evident than in the work of Samuel Pierpont Langley whose research interests had drawn support from the Signal Service and would be, in large part, responsible for the founding and development of the Smithsonian Astrophysical Observatory in 1890.

---

[53]Charles A. Young, The sun, new & revised ed. (New York: D. Appleton & Co., 1898), 172-173.

CHAPTER 5

FROM HENRY DRAPER TO HARVARD'S DRAPER MEMORIAL PROGRAM: THE NEBULAR HYPOTHESIS IN THE DEVELOPMENT OF ASTROPHYSICS AT HARVARD COLLEGE OBSERVATORY.

In the spring of 1886 newspapers and journals in England and America announced the establishment at the Harvard College Observatory of the Henry Draper Memorial for the study of stellar spectra.[1] The Draper Memorial would play an important role in the establishment of organized astrophysical research at the Harvard College Observatory. Its development was also closely connected with concerns about the nebular hypothesis. The debates over the nebular hypothesis which flourished at Harvard, the agonizing reconciliation of the nebular hypothesis with religious faith developed by Benjamin Peirce and the occasional speculations of E.C. Pickering, the director of the Observatory, all influenced the development of astrophysics at Harvard. So also did the broader debate between Chauncey Wright and John Fiske over the validity of Herbert Spencer's developmental hypothesis. Wright and Fiske's periodic sparring would receive more public attention, but it was Henry Draper's interest in the nebular hypothesis, and the "empirical"

---

[1]Edward C. Pickering, "Photographic study of stellar spectra," Nature 33 (8 April 1886): 535 and "The Henry Draper Memorial," New York Herald, 21 March 1886, 11.

research program he developed to confirm it, which set the course for astrophysical research at Harvard College Observatory for the remainder of the nineteenth and the beginning of the twentieth century.[2]

Not surprisingly, historians have overlooked the role of the nebular hypothesis in the development of Henry Draper's research.[3] Most of his scientific papers address

---

[2]Chauncey Wright (1830-1875), one of the leaders of the "Metaphysical Club" beginning in the 1860s, also served as a critic for the North American Review and the Nation. Wright's influence on C.S. Peirce and other late 19th century American pragmatic philosophers is clear. But whether he would have accepted pragmatism in its mature form is still debated. He found Spencer's developmental hypothesis lacking in empirical support and dismissed it as unscientific metaphysical speculation. John Fiske (1842-1901) became a member of the Metaphysical Club while an undergraduate at Harvard. As an undergraduate he had become a disciple of Auguste Comte, later dropping Comtean philosophy for Spencer's developmentalism. When Harvard's newly elected president, Charles William Eliot, invited Fiske to give a series of lectures on Comte, he instead used the opportunity to espouse Spencer ultimately leading to critical reviews on the part of Chauncey Wright. Wright, however, reserved his most thoughtful discussions to evaluating Spencer rather than his American disciples. Edward H. Madden, Chauncey Wright and the foundations of pragmatism (Seattle, University of Washington Press, 1963) and "The coming of Darwinism," in Richard Hofstadter, Social Darwinism in American thought, revised edition (Boston: Beacon Press, 1955), 19-20.

[3]The standard works on Henry Draper are Owen Gingerich, "Henry Draper's scientific legacy," New York Academy of Science, Annals 395 (1982): 308-320; Howard Plotkin, "Henry Draper: A scientific biography" (Ph.D. diss., Johns Hopkins University, 1971); Howard Plotkin, "Henry Draper, Edward C. Pickering and the birth of American astrophysics," New York Academy of Science, Annals 395 (1982): 321-330, and Howard Plotkin, "Henry Draper, the discovery of Oxygen in the sun, and the dilemma of interpreting the solar spectrum," Journal for the History of Astronomy 8 (1977): 44-71; and E.L. Schucking, "Henry Draper: The unity of the universe," New York Academy of Science, Annals 395 (1982): 299-307.

specific issues about the physics or the chemistry of the stars and make only passing reference to the underlying motives for taking up those issues. Furthermore, Draper's research on oxygen in the sun and his subsequently more accepted research on stellar spectra were incomplete at his death. He probably did not feel justified in publishing any conclusions which he presumed could be derived from the successful results of these projects. Nevertheless, it is clear from his newspaper interviews, lectures, and articles for popular consumption that Henry Draper saw his research on nebular and stellar spectra and into the constitution of the sun as a means to gain empirical evidence to support the nebular hypothesis and, to a lesser extent, the probability of extraterrestrial life.

Henry Draper's father, John William Draper, played a major role in the early development of photography in America and had applied it among other ways to his astronomical research.[4] He had also been among the first in America to

---

[4]Charles Coulston Gillispie, ed. Dictionary of scientific biography (New York: Charles Scribner's Sons, 1973), s.v. "John William Draper," by Donald Fleming. For some of Draper's papers regarding photography and its application to astronomy see: "Remarks on the Daquerrotype process," Annals of Electricity, Magnetism and Chemistry and Guardian of Experimental Science 6 (1941): 194-210; "On the decomposition of Carbonic Acid and the Alkaline Carbonates by the light of the sun," American Philosophical Society, Proceedings 3 (1843): 111-114; "On a change produced by exposure to the beams of the sun in the properties of an elementary substance," Report of the Thirteenth Meeting of the British association for the advancement of science; held at Cork in August 1843, (London: John Murray, 1844), pt 2:9; "On the chemical action of light," Philosophical Magazine 1 (1851): 368-393; "On the of the chemical action of light," Philosophical Magazine 15 (1958): 90-93.

study the solar spectrum and made some limited, although apparently not seriously justified, priority claims to some of Bunsen's and Kirchhoff's discoveries.[5] John William Draper's interests in astronomy were broad. Whether or not they were influenced by the nebular hypothesis, it is clear that he supported Laplace's world view and the nebular hypothesis itself.[6]

Henry Draper received his undergraduate and medical training at New York's City College. It appears that he worked closely with his father while in school and acquired from him an interest in astronomy as well as the possibility of combining astronomical research and photography. Although he was not as prolific nor as wide ranging a writer as his father, Henry Draper undoubtedly drew his interest in the

---

[5]John William Draper, "Early contributions to spectrum photography and photo-chemistry," Nature 10 (30 July 1874): 243-244.

[6]In his Scientific Memoirs, John William Draper claimed that he had discovered the distinction between a continuous spectrum of solid bodies and a discontinuous spectrum of ignited gas which Huggins had subsequently used to place the nebular hypothesis on a "firm basis." Cf. John William Draper "Memoir I. Examination of the radiations of red-hot bodies. The production of light by heat," in Scientific Memoirs Being Experimental contributions to a Knowledge of Radiant Energy (London: Sampson Low, Marston, Searle & Rivington, 1878; reprint New York: Arno Press, 1973), 32 (page references are to reprint edition) and "Examinations of the radiations of red-hot bodies. The production of light by heat," American Journal of Science 2d ser., 4 (1847): 388-402.

nebular hypothesis from the intellectual milieu in which his father raised him.

Draper's interest in the nebular hypothesis was closely connected with his views of extra-terrestrial life. In a 1866 lecture entitled "Are there other inhabited worlds?," he contended that while the evolutionary development of some worlds had not yet arrived at the geological condition of the earth, "on others conspiring circumstances may have allowed life to develop even beyond our standard."[7] Certain chemicals were necessary for human life and the evolutionary development of those chemicals via the nebular hypothesis could, of course, be detected using the spectroscope.[8]

However, Draper claimed his research on solar, stellar and nebular spectra began in 1871 when, after his successful development of astronomical photography, he decided that he could combine photography with the light collecting power and equatorial movements of his recently completed 28-inch reflector to apply physics to astronomy. His early work, as did the work of many other spectroscopists of the time, focused on metallic spectra. He soon decided

---

[7]Henry Draper, "Are there other inhabited worlds?" Harper's Magazine 33 (June 1866): 54.

[8]Draper began with the argument that the fundamental prerequisites for life were air to breathe, water to drink and food to eat. Therefore heavenly bodies having the constituent materials for these substances--that is hydrogen, oxygen, carbon and possibly nitrogen--could potentially contain forms of life. Ibid., 47-48.

that "able physicists were engaged on the metallic spectra," and he would instead study the spectra of non-metals.[9]

Huggins' work on nitrogen, carbon and hydrogen and its implications for the nebular hypothesis influenced Draper's selection of those elements as the focus of his physical and astronomical observations. Huggins' studies suggested that nebulae were not condensing into stars, a basic presumption of the nebular hypothesis. Further more, it pointed out the need for greater light gathering power to determine if the elements of nebulae were nitrogen, hydrogen and other non-metallic elements or were more primary substances.[10]

The critique of the nebular hypothesis implicit in Huggins' research did not please Draper. His own research would address the very issues Huggins raised and E.C. Pickering's continuation of Draper's research at Harvard after Draper's death would lead to conflict with Huggins.[11]

---

[9]Henry Draper, "On the coincidence of the bright lines of the Oxygen spectrum with bright lines in the solar spectrum," American Journal of Science 3d ser., 18 (October 1879): 263.

[10]Huggins argued that nebulae contained few elements while stars were made up of many. Since he viewed elements as unchanging, stellar evolution via transformation of elements was unthinkable. Charles Coulston Gillispie, ed., Dictionary of scientific biography (New York: Charles Scribner's Sons, 1973), sv "William Huggins."

[11]On the tensions between Huggins and Pickering see Bessie Zaban Jones and Lyle Gifford Boyd, "The Henry Draper Memorial," in The Harvard College Observatory: The first four directorships, 1839-1919 (Cambridge, Mass.: Harvard University Press, 1971), 220-230.

Draper's application of photography to the study of stellar spectra would make more use of the light gathering capacity of telescopes and would eventually be directed towards cataloging the chemical structure of, as well as possible changes in, nebulae. But before he could study the weaker lines in nebular and stellar spectra, he needed to improve astronomical photography. Meanwhile he would turn to the study of the sun, seeking to find those "weak line" elements he would subsequently look for in nebulae and stars.

Draper's initial spectroscopic studies of the sun appear to have been a matter of choosing the most convenient star for spectroscopic studies. His announcement in 1877 that he had "discovered" oxygen in the sun led to his election to the National Academy of Sciences. More important, it was the result of theory laden research, which if correct would go far towards confirming the nebular hypothesis.[12]

Draper suggested that his "discovery" of oxygen in the sun was serendipitous. Initially, he claimed, he had been searching for nitrogen lines in the solar spectrum

---

[12]A.F. Chalmers, in What is this thing called Science? (St. Lucia, Queensland: University of Queensland Press, 1978), points out that all observations are based on theories of some sort and that those theories determine the importance of the various components of observation. Theory laden research would be that research which is so closely tied to presumed theories that the researcher observes what is not there or interprets what is there in an inconsistent manner in order to support the presumptions which originally led to the observations.

where, he suspected, there was a coincidence between the bright bands of the two spectra. Further observations had led to the conclusion that the bright bands were oxygen lines in the spectrum.[13] By his own account, his spectroscopic studies of the sun were preliminary to his planned search for non-metallic elements in stars and nebulae. He, nevertheless, quickly used his "discovery" of oxygen in the sun to draw support for the nebular hypothesis.

However, Draper's elder brother, John Christopher Draper, was also searching for oxygen in the sun and by October, 1878, claimed to have found oxygen as well as other "non metallic" substances represented "in the solar spectrum by dark lines in the same manner as metallic substances."[14] The fact that the two brothers made conflicting claims for the discovery of oxygen in the solar spectrum suggests that both had been actively searching for oxygen in the sun.[15]

---

[13]Henry Draper, "On the coincidence of the bright lines of the Oxygen spectrum with bright lines in the solar spectrum," 263.

[14]John Christopher Draper, "On the presence of dark lines in the solar spectrum which correspond closely to the lines of the spectrum of Oxygen," Nature 18 (17 October 1878): 654-657.

[15]John Christopher Draper claimed the oxygen to be in the solar envelope while Henry Draper's denial of the existence of oxygen at the higher reaches of the solar atmosphere was part of the basis for his explanation of oxygen appearing as bright lines in the solar spectrum. Neither John Christopher Draper, who was Professor of Natural History in the City College of New York, nor Henry Draper, who was professor of chemistry in the same institution, made reference to the other's works in their own research.

While John Christopher Draper did not explicitly tie his research to the nebular hypothesis, it is likely that both he and Henry were influenced by their father's advocacy of Laplace's nebular hypothesis and plurality of worlds. The discovery of the non-metal, oxygen, in the sun supported the argument that stars having oxygen were, by reason of the nebular hypothesis, more likely to have planets supporting life. Likewise, the fact that the sun contained most, if not all, the chemical constituents of the earth would go far towards supporting the nebular hypothesis.

Whether Draper's description of the events leading to his "discovery" reflected his motives at the time or the issues suggested above played an important role, it is clear that he, as well as most other spectroscopists, expected oxygen to be a major component in the solar spectrum. Many, apparently including Draper, derived their arguments from an acceptance of the nebular hypothesis.

Spectroscopists, such as Ångström and Zöllner, presumed that the absence of spectroscopic indications of oxygen and nitrogen did not mean that these elements were missing from the sun.[16] Astronomers generally assumed that the nebular hypothesis required similar constituents on all bodies of the solar system although not necessarily in the same proportion. In 1857 Henri Saint Claire Deville sug-

---

[16] W.A. Norton, "On the physical constitution of the sun," American Journal of Science 3d ser., 1 (1871): 406.

gested that the abnormal densities of some gases might be the result of dissociation resulting in mixtures of elements rather than a single elemental structure. By 1867, when the Canadian chemist, Thomas Sterry Hunt visited Deville, the two apparently speculated about using the sun as a chemical laboratory for testing dissociation under extreme temperatures and pressures using Hervé Faye's 1865 theory of the constitution of the sun and spectrum analysis.[17]

The sun, Faye maintained, was a gaseous sphere that had large convective currents resulting from surface cooling. The process of cooling at the surface would result in certain vapors having a strong affinity for oxygen and the resulting compounds sinking rapidly below the photospheric surface. At some point they would dissociate in the hotter internal regions and the lighter vapors, particularly hydrogen, would rush to the surface resulting in the tremendous hydrogen protuberances. Oxygen, as well would rise toward the surface but would be caught by other vapors and again dragged down towards the center of the sun before it could rise above the photosphere. Faye believed his theory would explain, in addition to the accepted observations of mottled or rice grain surfaces and gigantic hydrogen eruptions, why oxygen

---

[17]Henri Saint-Claire Deville, "Sur la dissociation ou decomposition Spontanee des corps sous l'influence de la chaleur," Comptes Rendus Hebdomadaires Des Séances de L'Académie des Sciences 45 (1857): 859.

had not been detected by spectrum analysis.[18]

In a lecture before the Royal Institution in May, 1867, Hunt outlined his discussion with Deville and later discussions with the British chemist Benjamin Brodie, suggesting that the chemistry of heavenly bodies might give "evidence of matter still more elemental than that revealed in the experiments of the laboratory, where we can only conjecture the compound nature of many of the so-called elementary substances."[19] By 1874 Hunt was advocating that the chemical elements of the sun and planets had evolved in accordance with "the views of Brodie, [Frank Wigglesworth] Clarke, and Lockyer, by a stoichiogenic process." This, he insisted, was but an extension of Humboldt's version of the nebular hypothesis.[20]

Considering Hunt's status in the American scientific community and Draper's interest in chemistry, Draper was, no doubt, aware of Hunt's views. Certainly he would become aware of Lockyer's exposition of chemical dissociation in the sun since Lockyer would become one of the strongest critics of his "discovery" of oxygen in the sun.

---

[18]Hervé Faye, "Sur la constitution physique du Soleil," Comptes Rendus 60 (1865): 89-96 and 138-150.

[19]Quoted in T. Sterry Hunt, "Celestial chemistry from the time of Newton," American Journal of Science 3d ser., 23 (1882): 123-133.

[20]Ibid.

Draper does not cite Faye's theory, Hunt or William Norton in his papers on oxygen in the sun, but he was undoubtedly familiar with them. He played an important role in the National Academy of Sciences and read widely. It would be surprising if he was not aware of works relevant to his own research. Furthermore, the American physicist William August Norton's 1871 article in The American Journal of Science, publicizing Faye's theory, probably reinforced it in Draper's mind about the time he began his own research. Norton spent nearly a third of his paper discussing Faye's explanation for the spectroscopists' inability to detect oxygen and nitrogen in the sun and observations by other astronomers supporting Faye's position.[21]

Draper's announcement of his "discovery" before the July, 1877, meeting of the American Philosophical Society reflected the importance of the nebular hypothesis, as well as the arguments suggested above, in his discovery. He declared that:

> from purely theoretical considerations derived from terrestrial chemistry and the nebular hypothesis, the presence of oxygen in the sun might have been strongly suspected, for this element is currently stated to form eight-ninths of the globe, one-third of the crust of the earth, and one-fifth of the air, and should therefore be a large constituent of every member of the solar system.[22]

---

[21]William Augustus Norton, "On the physical constitution of the sun," American Journal of Science 3d ser., 1 (June 1871): 403-406.

[22]Henry Draper, "Discovery of oxygen in the sun by photography, and a new theory of the solar spectrum," Journal of the Franklin Institute 104 (1877): 84.

If the nebular hypothesis is correct, then there ought to be oxygen in the sun.

The requisite change in the interpretation of the solar system would, Draper stressed, explain the presence of some of the heretofore unexplained dark lines in the solar spectrum. These lines, instead of representing unknown elements might simply be spaces between until now unrecognized bright lines in the spectrum. Equally important this interpretation would open the door to the discovery of other non-metals. This would give greater credence to the nebular hypothesis since "the discovery of oxygen and probably other non-metals in the sun gives increased strength to the nebular hypothesis, because to many persons the absence of this important group has presented a considerable difficulty."[23]

Since Draper's announcement of his discovery of oxygen in the sun required a new interpretation of the solar spectrum it was immediately controversial. The editors of the Journal of the Franklin Institute declared that Draper's paper "had been pronounced, by able judges, to be the most important contribution to solar physics since Kirchhoff's great discovery."[24]

---

[23]Ibid.

[24]Henry Draper, "Discovery of oxygen in the sun by photography, and a new theory of the solar spectrum," Journal of the Franklin Institute, 104 (1877): 81fn.

Arthur Schuster, who would become one of England's leading spectroscopists, endorsed Draper's discovery in Nature, claiming at the same time to have found dark line "compound" lines of oxygen in the solar spectrum.[25] Schuster proposed using the oxygen spectrum to determine solar temperatures. His own research had indicated that the spectrum of oxygen changed at certain temperatures and pressures. The bright line spectrum seen by Draper would be caused by oxygen at a temperature higher than that of the reversing layer. At lower temperatures and pressures one would obtain the molecular O_2 spectrum, while at higher temperatures and pressures the elemental O spectrum would be observed. Since Draper's bright line spectrum was that of elemental oxygen, he assumed that at some point higher than the photosphere, cooler temperatures would allow the formation of molecular oxygen which would be detected by the dark line spectrum caused by the reversing layer of the photosphere. Such dark lines would be extremely weak and would be broadened under the intense solar pressure. They would be difficult to measure and would probably not be

---

[25]Schuster had studied physics under Balfour Stewart and had been trained in spectroscopy by Henry Roscoe at Owens College in Manchester England. He had then gone to Heidelberg to study with Kirchhoff and received his Ph.D. in 1873. In 1897 he discovered, independently from Johannes Robert Rydberg, the relationship in spectral lines now known as the Rydberg-Schuster law. See Charles Coulston Gillispie, ed., Dictionary of scientific biography (New York: Charles Scribner's Sons, 1973), s.v. "Charles Schuster," by Robert H. Kargon.

observed unless specifically looked for. However, after three weeks of observation he believed he had sufficient evidence to support the existence of a reversed molecular oxygen spectrum in the sun.[26]

Draper's "discovery" influenced the British chemist and naturalist, Raphael Meldola, to come to theoretical considerations similar to Schuster's.[27] Many astronomers and spectroscopists, however, initially doubted the validity of Draper's discovery. The dispersion of his photographs was not great and many argued that the coincidence of bright lines was probably a chance coincidence that would be resolved when the experiment was repeated at higher dispersion rates.

---

[26]Arthur Schuster, "On the presence of oxygen in the sun," Nature 17 (20 December 1877): 148-149. Schuster, as well as several of his colleagues, utilized the term compound when referring to the molecular structure of elements. Following Deville's theory of dissociation, they argued that under intense pressure and/or high temperature compounds would break down into elements and their molecular forms. As temperature and pressure increased the substances would further breakdown into simple elements, i.e. O instead of O_2, and would at extremely high temperatures and pressures be reduced to the simplest elements, such as hydrogen, or even simpler elements unknown on earth.

[27]R. Meldola, "Oxygen in the sun," Nature 17 (27 December 1877): 161-162 and R. Meldola "On a cause for the appearance of bright lines in the solar spectrum," American Journal of Science 16 (1878): 290-300. Raphael Meldola wrote prolifically on a variety of topics ranging from chemistry, particularly chemistry related to the dye industry to natural history and evolution to conservation. In 1880 and 1884 he served terms as president of the Essex Field Club and in the mid 1890s was elected president of the chemical section of the B.A.A.S.

Acknowledging the criticisms of these spectroscopists, Draper spent two more years improving his spectroscopic pictures. In the process he improved his photographic techniques and organized a private eclipse expedition to study the nature of the solar corona with his friends Professors G.F. Barker, of the University of Pennsylvania; Henry Morton, President of the Stevens Institute, and the inventor Thomas Edison. Popular accounts in the press declared that this expedition would search for oxygen in the solar corona[28]; an "error" which Draper would subsequently dispel, declaring that most scientists knew that no oxygen existed as high as the corona.[29]

However, Draper had been searching for oxygen at the base of solar protuberances late in 1877 and early 1878. While he saw a "number of reversed lines," suggesting that he had adopted Schuster's hypothesis, his failure to find any oxygen lines "outside of the limb" led him to conclude that "the bright-line spectrum of oxygen as seen on the sun's disc must have its upper limit close to the apparent spectroscopic

---

[28]New York Times, 30 July 1878, 5.

[29]"The examiners of the sun. Prof. Henry Draper's return," New York Times, 8 August 1878, 5. In the paper he read before the Royal Society, Draper indicated that he had searched for oxygen in the chromosphere and was in the process of preparing more sophisticated equipment to continue this study. Henry Draper, "On the coincidence of the bright line of the oxygen spectrum with the bright lines in the solar spectrum," American Journal of Science 3d ser., 18 (1879): 268.

limb of the sun."[30] The elemental bright line oxygen spectrum could be explained by the existence of oxygen in the sun at such a temperature and pressure that it emitted more light than the surrounding solar masses.

By 1879 Draper was prepared to argue his discovery of solar oxygen with his European critics.[31] He had refined the oxygen line as well by developing a spark compressor and replacing his batteries with a Gramme machine providing a more uniform current. He had improved the dispersion of his oxygen spectrum photographs by four times his 1877 pictures. This had allowed him to make enlarged photographs twice the size of Ångström's chart.[32]

The critical debate appeared now to center around whether or not there were bright lines in the solar spectrum. Here Draper was supported by Lockyer, Marie Alfred Cornu and Henry Hennessy, as well as many American scientists, although Lockyer remained unconvinced that Draper's bright lines corresponded with the "bright lines" of oxygen.[33] The most

---

[30]Henry Draper, "Oxygen in the sun," Nature 17 (28 February 1878): 339-340. In the process Draper caught his right arm in the Gramme machine and was forced temporarily to terminate his experiments.

[31]Henry Draper to A.C. Ranyard, 4 April 1879 published in Astronomical Register 17 (1880): 117

[32]Henry Draper, "On the coincidence of the bright lines of the oxygen spectrum with bright lines in the solar spectrum," American Journal of Science 3d ser., 18 (1879): 267.

[33]One possible American exception was Samuel Langley who, in his published reports, preferred not to take a side in the debate. S.P. Langley, "The recent progress of solar physics," Popular Science Monthly, 16 (November 1879): 6.

vocal critic was William H. M. Christie, the Royal Astronomer, who challenged everything from the validity of Draper's theory of the bright line spectrum to the actual coincidence of the bright lines of oxygen to the bright lines of the solar spectrum.[34]

Critics of Draper's "discovery," in some cases, appeared to be more concerned about the fact that an American rather than a European had made the discovery. The American press certainly viewed Lockyer's criticism in this light. When Lockyer failed to attend Draper's lecture before the Royal Society, American newspapers pointed out the rivalry between Lockyer and Draper. The New York Times claimed that the European press had bitterly attacked Lockyer for allowing an American to be first to make such an important discovery. He had subsequently maintained that he had taken photographs at an earlier date which had shown the existence of these bright bands, implying that he deserved at least a share of the honor for the discovery. When he came to America for the 1878 solar eclipse, Draper's friends had demanded that he produce the negatives. He declined, protesting that he could not provide the pictures as he had lost them one night in the London Metropolitan Underground Railway Tunnel. This claim, the New York Times alleged, had led to the use, in American

---

[34]"Meeting of the Royal Astronomical Society, June 14, 1879," Astronomical Register 17 (1880): 158-159 & 161-162.

scientific circles, of the phrase "He's been through the tunnel" for a man who asserted more than he could prove.[35]

The hostility between Draper's supporters and Lockyer increased in 1882 when Lockyer claimed that he had successfully photographed the solar corona for the first time--a feat which Draper had accomplished during the 1878 eclipse and which had been announced while Lockyer had been his guest.[36] Certainly by the time of Draper's lecture before the Royal Society, the American public and probably a large number of American scientists presumed that Lockyer's criticisms were grounded more in personal pride than in scientific sagacity.

In spite of the reservations of Mr. Christie and Henry Huggins, who declared that he would reserve judgment although Draper had made a <u>primâ facie</u> case, most members of the Royal Society appeared convinced by Draper's presentation. The London <u>Times</u>, in reporting the meeting, concluded:

> If such evidence as Professor Draper has obtained is rejected, hardly any spectroscopic evidence can suffice to prove the existence of an element in the sun. We certainly have not stronger evidence in the case of sodium or magnesium, elements which every physicist

---

[35]"Prof. Draper's discovery. Oxygen in the sun," <u>New York Times</u>, 27 January 1879, 8.

[36]"The eclipse of the sun," <u>New York Times</u>, 19 May 1882, 1 and "The eclipse of the sun," <u>New York Times</u>, 30 May 1882, 4.

> regards as present in the sun, than Professor Draper has obtained in the case of oxygen.[37]

A similar response was provided by Hervé Faye when Draper's pictures were presented at the French Academy of Sciences.[38]

It would not be until 1887, when John Trowbridge and C.C. Hutchins remeasured spectroscopic pictures of the so-called oxygen bright lines using a spectroscope of even greater dispersion, that Draper's spectroscopic "discovery" of oxygen in the sun would finally be set aside.[39]

---

[37]"Oxygen in the sun," Times (London), 16 June 1879, 5.

[38]Comptes Rendus des Séances de l'Académie des Sciences 88 (1879): 1332-33.

[39]John Trowbridge and C.C. Hutchins, "Oxygen in the sun; contributions from the physical laboratory of Harvard University," American Journal of Science 3d ser., 34 (1887): 263-270. Trowbridge and Hutchins also discussed and set aside John Christopher Draper's claims for the dark line discovery of oxygen in the sun. However those claims were never seriously adopted by other scientists. John Christopher Draper, "On the presence of dark lines in the solar spectrum which correspond closely to the lines of the spectrum of oxygen," 654-656. "Oxygen in the sun," Nature 19 (13 February 1879): 352-353 sets forth a point by point critique of John Christopher Draper's claims.

The debate over the existence of oxygen in the sun would be raised again in the mid 1890s. C. Runge and F. Paschen made another claim for the existence of oxygen in the sun late in 1896. Their paper was critiqued by Lewis E. Jewell of Johns Hopkins University who claimed that they had detected the atmospheric lines of oxygen rather than solar lines. A debate was scheduled for the first conference of the Astronomical and Astrophysical Society of America held at the dedication of the Yerkes Observatory in October 1897. However, by the time of the conference Jewell had withdrawn his criticism. C. Runge and F. Paschen, "Oxygen in the sun," Astrophysical Journal 4 (1860): 317-319; Lewis E. Jewell, "Oxygen in the sun," Astrophysical Journal 5 (1897): 99-100 and C. Runge, "Oxygen in the sun," Publications of the Astronomical and Astrophysical Society of America 1 (1910): 14-15.

Modern discussions of Draper's solar research have suggested that his claim to have discovered oxygen in the sun was an abberation in an otherwise careful and cautious career. Within the context of scientific theories of the times his discovery appears to have been theory laden. Draper's "discovery" was colored by Faye's theory of the constitution of the sun that presumed the existence of oxygen under the solar photosphere and Draper's desire to find evidence supporting the nebular hypothesis.

Draper viewed his research on solar photography and spectroscopy as necessary preliminary research prior to subsequent research on stars and nebulae. Indeed, his solar research was carried out while he refined his instrumentation and photography to the point that he felt he could take spectroscopic pictures of the stars and of the nebulae. Thus his photographic and spectroscopic work on nebulae and stars was also closely connected to his concerns about the nebular hypothesis.[40]

However, Draper's initial publication, in 1879, suggested he was primarily concerned with astrochemistry of

---

[40]Not every one saw the progressive connection between Draper's stellar and nebular photography and his earlier work. Nor did they always view his subsequent research as important. The New York Times for instance declared that while Draper's photograph of the Orion nebula was of the "highest significance in physical astronomy" it was not on an equal ranking with "his brilliant discoveries from the study of the solar spectrum." New York Times, 20 November 1880, 3.

the stars and nebulae.[41] This may have been a strategic move resulting from Lockyer's development of the notion of elemental dissociation in the sun and stars which was then gaining notice. Lockyer, drawing from Deville's theory of dissociation, speculated that there were no elements, as we knew them on earth, in the sun. The molecular form of elements known on the earth would be dissociated into the simple elements and then into elements of lower atomic weight by the heat of the sun. Such "elements" as we might detect in the sun were in the process of forming, in the cooler portions of the sun, or decomposing in the warmer regions of the sun.[42]

Lockyer's theory would explain the dominance of hydrogen in the solar spectrum and provide a system for classifying stars. The hotter the star, the simpler its spectrum would be. Hottest stars would furnish very thick hydrogen lines. Slightly cooler stars would include a few metallic lines, while the coolest stars would show the spectra of compound metals with non-metals and of non-metals in a state of isolation. Lockyer's theory also would, by implication, result in the rejection of Draper's bright line "dis-

---

[41]Henry Draper, "On photographing the spectra of the stars and planets," American Journal of Science 3d ser., 18 (1879): 419-425.

[42]For further discussion of Deville's theory of dissociation see page 150ff.

covery" of oxygen and his entire "bright line" theory of non metallic substances which Draper claimed to exist in the sun.

Whether it was for this reason or the result of priority disputes with Lockyer, Draper expressed reservations about Lockyer's work. Lockyer had claimed credit for being the first to insert a lens between the laboratory spark, the source of the comparison spectrograph of an element, and the spectroscope. In January 1879 the New York Times reported that Draper refused to comment on Lockyer's theory but dissented from the opinion that it proved all that Lockyer claimed. The Times went on to imply that Draper held the opinion that "scientific men cannot accept a hypothesis simply because it bears the signature of a man who has made his name familiar in the literature of the spectroscope."[43]

In spite of his early reservations about Lockyer's research, Draper admitted, in a paper read before the National Academy of Sciences the following October,[44] that Lockyer's theory, if true, would make the sun and stars

---

[43]The insertion of the lens between the spark of the substance to be studied and the spectroscope allowed the spectroscopist to study the lines of various regions of the heated vapor and gave evidence that not all lines in the spectrum had the same extension. The procedure was well known by 1879 with both Lockyer and Draper claiming priority. "The new solar theory. What scientific men think of Mr. Lockyer's discovery," New York Times, 15 January 1879, 2.

[44]Henry Draper, "On photographing the spectra of the stars and planets," American Journal of Science 3d ser., 18 (1879): 419. Draper read this paper before the National Academy of Sciences October 28, 1879.

potential chemical laboratories for the study of chemistry at temperatures and pressures unavailable on earth. Nevertheless, Draper clearly did not accept Lockyer's theory. Instead of finding the most elemental condition of matter in hot stars, as Lockyer's theory required, he placed it in the gaseous nebulae as was presumed by the then accepted nebular hypothesis.[45]

While not specifically mentioning Lockyer, Draper classified the spectra of the stars he had photographed into two groups: those similar to the solar spectrum and those in which there were few spectral lines of great breadth and intensity. Presumably these latter would be Lockyer's hot stars.[46] But while hydrogen clearly was a major constituent of Vega, Draper pointed out that other lines were just as

---

[45]Ibid., 425. According to this version of the nebular hypothesis the elements evolved out of the basic stuff of the universe as a part of the evolutionary process of stars. The fact that the gaseous nebulae often showed only some of the stronger hydrogen lines suggested that this was the first element to develop.

[46]In 1867 Father Secchi had classified stellar spectra into three groups: 1. white stars showing a strong band in the green-blue--Fraunhöfer's line F--and another in the violet a little short of G; 2. Reddish stars showing bright bands in the red and orange; and 3. white stars with many fine lines similar to those in the solar system. "Spectrum analysis of the stars, by Father Secchi," Proceedings of the Franklin Institute 83 (1867): 66-67. He had subsequently expanded his classification system to four types: 1. white stars showing hydrogen lines against a continuous background; 2. yellow stars producing spectra like the sun in which iron was readily noticed; 3. orange stars with strongly marked lines both bright and dark and 4. faint red stars also displaying both bright and dark spectral lines. "Stars and suns," New York Times, 9 September 1877, 6.

conspicuous as the hydrogen lines. As a result he concluded that the evidence supporting a coincidence between simple spectra stars and hydrogen was "not complete."[47] Draper's interpretation implied that spectral types reflected most importantly an evolutionary state rather than a difference in temperature. Thus spectrum analysis could not only be used to classify the stars into types but could also provide a means of studying the evolutionary process as broadly outlined by the nebular hypothesis. Draper announced a program of photographing the spectra of the stars and research towards photographic investigation of the spectra of the gaseous nebulae as part of an attempt to confirm his assessment.[48]

Draper's research remained unpublished at his death. However, Edward C. Pickering and Charles Augustus Young organized and presented the results of his stellar spectra photographs to the American Academy of Arts and Sciences in

---

[47]Henry Draper, "On photographing the spectra of the stars and planets," 425.

[48]Ibid. Draper argued that stars evolved from nebulae where the simplest elements should be found into more complex stars during which process higher elements would evolve from the simpler elements. Therefore stars should have a greater range of elements and potentially compounds. Lockyer on the other hand argued that the elemental make up of the stars was dependent to a large extent on the temperature of the star. Extremely hot stars would have disrupted any complex forms of elements. Theoretically simple hydrogen would be the primary element of the hottest stars. Thus for Lockyer there was no correspondence between the complexity of the spectral makeup of the star and its evolutionary state; while for Draper these were intimately intertwined.

April, 1883. Their paper suggests that Draper had been attempting to arrange the stellar spectra of different stars in such a way as to show their evolutionary development. Draper appears to have concentrated his research on stars with spectra similar to the solar spectrum, stars with hydrogen bands on a continuous background and the unique star α Aquilae. The latter star had a general spectrum like α Lyrae and other stars of Secchi's class I. However, it also exhibited a multitude of very fine lines which, according to Young, Draper interpreted as signifying an evolutionary state between that of α Lyrae and that of Capella and the sun. Draper was clearly looking to use spectrum classification as a method to refine the nebular hypothesis and notions of stellar evolution.[49]

Draper's attempts to photograph gaseous nebulae and to study their spectra would reflect an interest in the very beginnings of stellar evolution. In 1873, about the same time that Draper first photographed the spectrum of α Lyrae, an article on the constitution of the nebulae, abridged from H. Schellen's Spectrum Analysis, was published in Popular Science Monthly.[50] In it Schellen had proposed a spectroscopic program to ascertain "the phases through which the sun

---

[49]Henry Draper, "Researches upon the photography of planetary and stellar spectra," Proceedings of the American Academy of Arts and Sciences 19 (1883): 256-257.

[50]H. Schellen, "The constitution of nebulae," Popular Science Monthly 3 (1873): 129-139.

and planets have passed in their process of development or transition from masses of luminous nebulae to their present condition."[51] Huggins, Schellen said, had classified some of the nebulae and he had added to Huggins classification some of the drawing by Lord Rosse. From these drawings Schellen suggested an evolutionary development based on the edifice "planned by Kant" and "erected by Laplace."[52] This evolutionary development would begin with the irregular and chaotic nebulous forms represented by the Orion nebula and the Great Magellanic clouds. Further development could be seen in the spiral or convoluted nebulae such as could be seen in the constellation Canes Venatici. Other illustrations suggested nebulae in the process of transition from spiral to annular nebulae and from annular to planetary nebulae. These, Schellen contended, were finally resolved into stellar nebula and stars.

The spectroscope would play an important role in the study of this evolutionary development since, through the spectroscope, one could determine the changes in the constitution of nebulae and stars. If the spectrum was a continuous one, then the light source would be either a solid or liquid body. If the spectrum consisted of bright lines only then the source would consist of a luminous gas. Finally, if the source produced a continuous spectrum crossed

---

[51]Ibid., 131.

[52]Ibid.

by dark lines it would indicate a source of solid or liquid incandescence surrounded by an atmosphere of vapors at a lower temperature. The position of the lines would, of course, determine at least some of the chemical constituents of the body being studied.

The outline of Draper's planned research, as presented in his November, 1880, paper on his photograph of the Orion Nebula the previous September, bears a remarkable resemblance to that suggested by Schellen. Draper declared, in direct opposition to Huggins' earlier conclusions, that "the gaseous nebulae are bodies of interest, because they may be regarded as representing an early stage in the genesis of stellar or solar systems."[53] By implication, the chemical constituents of nebulae would be the primary chemical constituents from which all other elements evolved.

Draper's choice of the Orion nebula was itself significant since more studies of that nebula had been made in the previous hundred years than of any other and the divergence of the various drawings led many astronomers to conclude that there were changes actively taking place in the Orion nebula.[54] As one of the "chaotic" nebular forms, the

---

[53]Quoted in "Late scientific fruits. Final papers read at the national academy meeting," New York Times, 20 November 1880, 3.

[54]Edward C. Pickering began his "Photographic study of the nebula of Orion" by claiming that "no portion of the heavens has been more carefully studied than that containing the nebula of Orion." In fact Edward Singleton Holden's 1878 monograph on the Orion nebula showed there had been more than 150 major studies of the Orion nebula since its discovery in 1618. Edward C. Pickering, "A photographic

Orion nebula would represent the beginning stages of the evolutionary development which Draper sought to uncover. Draper's paper, while cautioning the need for further study, pointed out that his picture showed the nebula as a "knotted structure as if a process of aggregation was going on. . . ." Draper was looking for changes in the Orion nebula which would confirm the nebular hypothesis and fully expected to find them. Only proper scientific caution restrained him from making a "statement" until he had a larger collection of negatives taken at various lengths of exposure to eliminate the possibility that the "knotted structure" was an effect of the photograph rather than a true part of the nebula. Instead, he announced that he would begin a series of photographs over the coming years which would "give the means of determining with some precision what changes, if any, are taking place."[55] Of course, any changes in the nebula

---

study of the nebula of Orion," Proceedings of the American Academy of Arts and Sciences 20 (1884-1885): 407; Edward S. Holden, "Monograph of the central parts of the nebula of Orion" in U.S. Naval Observatory, Washington Astronomical Observations for 1878 (Washington, DC: U.S. Naval Observatory, 1882), Appendix I and Edward S. Holden, "Contributions from the Lick Observatory II. The nebula of Orion," Overland Monthly 2d ser., 14 (April 1892): 401.

[55]"Late scientific fruits," 3. Draper's exhibit of his photograph of the Orion nebula was as critically received by his British counterparts as had been his initial announcement of his "discovery" of oxygen in the sun. They had been stung by his earlier discovery, which was still widely accepted, and were not interested in proclaiming the merits of another American accomplishment in their field. For a discussion of criticisms of Draper's photograph of the Orion Nebula see. Jones and Boyd, "The Henry Draper Memorial," 217.

suggesting condensation into stars would, presumably, support the nebular hypothesis.

Proving that the gaseous nebulae were condensing into stellar systems would add additional support to Draper's position in his conflict with Lockyer. In 1864 Henry Huggins had found that the spectra of nebulae consisted of three bright lines instead of the continuous colored band with dark lines such as was given by the stars. Subsequent work by Huggins and others had found that two of those three lines constituted the brightest lines of nitrogen and hydrogen. The third line did not coincide with the bright lines of any of the elements with which it was tested.[56] Some astronomers had suggested that in the process of evolving into stars, the chemical constituents of the nebular lines evolved into more complex elements. Draper appears to have been trying to use spectroscopic studies to confirm this evolutionary change of chemical elements in opposition to Lockyer's break down of chemical elements based upon temperature and pressure.

Henry Draper's death in 1882 brought an end to his research just when it was reaching the point where he could begin to address the problems of the nebular hypothesis. Clearly he was directing his research in that direction and it had led him into conflict with Lockyer who would

---

[56]H. Schellen, "The constitution of nebulae," Popular Science Monthly 3 (1873): 139. According to Schellen, the middle line of the nebular spectrum was quite close to the strongest barium line but was not coincident with it.

subsequently come to dominate astrophysical theories in the late nineteenth century. The advocacy of Draper's particular theories may have ended with his death, but the research programs continued on in his name at the Harvard College Observatory continued to reflect concerns about the nebular hypothesis.

Had it not been for the desire of his wife, Anna Palmer Draper, to complete it, Draper's research may have remained a relatively minor contribution to astronomical photography. Initially, Anna Draper had sought for some astronomer to take over Draper's work but, lacking available candidates of sufficient stature, she was eventually convinced by Edward C. Pickering, director of the Harvard College Observatory, to endow the Henry Draper Memorial at his observatory.[57]

When Pickering was called to direct the Harvard College Observatory in 1876, the position had already been rejected by Simon Newcomb, at the time one of America's best known astronomers. However, Edward Charles Pickering, the thirty-one year-old M.I.T. professor of physics, was uniquely qualified to develop a program in astrophysics at Harvard. After graduating from Harvard's Lawrence Scientific School he had been an instructor in mathematics at his alma mater for a

---

[57]For a discussion of Pickering's recruitment of the Henry Draper Memorial see Bessie Zaban Jones and Lyle Gifford Boyd, "The Henry Draper Memorial," 227-229.

year before receiving an appointment as an assistant in physics at Massachusetts Institute of Technology.

Under Pickering's guidance, astrophysical research was developed into a full fledged program at the Harvard College Observatory. Earlier astronomers at Harvard had been interested in several of the problems which astrophysics would eventually address, but none had emphasized those problems to the extent that Pickering, whose training was in physics rather than astronomy, would.

By the time of Draper's death, Pickering had already developed a program in stellar photometry and his publications had shown a particular interest in planetary nebulae,[58] which some advocates of the nebular hypothesis presumed to be an intermediate stage in the evolutionary development of stellar systems. His search for funding for astronomical research would lead him to court funds from Henry Draper's widow.

Pickering, no doubt aware of the conflicts between Draper and Lockyer and sensitive to Huggins' criticisms of his extrapolations from Draper's research, sought to divorce

---

[58]Edward C. Pickering, "Light of Webb's planetary nebula," Nature 21 (12 February 1880): 346; Edward C. Pickering, "Two new planetary nebulae," Nature 22 (5 August 1880): 327; Edward C. Pickering, "New planetary nebulae," American Journal of Science 120 (1880): 303; Edward C. Pickering, "Remarkable star spectrum; new planetary nebula," Science 2 (1881): 581; Edward C. Pickering, "New planetary nebulae," American Journal of Science 26 (1882): 302; and Edward C. Pickering, "Small planetary nebulae, discovered at the Harvard College Observatory," Sidereal Messenger 1 (1882): 139.

his research from any particular theory. The Henry Draper Memorial, as announced on March 20, 1886, concentrated on completing Draper's photographic collection of stellar spectra.[59] While the Draper Memorial sought to photograph and classify stellar spectra, it appeared that it would initially leave to others the job of interpreting what those spectra meant. In fact, Pickering announced that photographic specimens would be "gratuitously distributed from time to time" to all who would "find them of value from the scientific point of view."[60]

In spite of his apparent caution, Pickering announced the discovery of additional stars showing bright line spectra the following September. His announcement suggested the incorporation of Draper's motives for photographing stellar spectra into research program of the Henry Draper Memorial.[61] Most "ordinary stars" Pickering claimed, showed little variety in their spectra. Deviations from the typical spectra were particularly interesting and the historical record for P Cygni, one of the stars that the Draper Memorial had discovered to contain a bright line spectrum was especially instructive. P Cygni had, Pickering pointed out,

---

[59]Edward C. Pickering, "Photographic study of stellar spectra. Henry Draper Memorial," Nature 33 (8 April 1886): 535.

[60]Ibid.

[61]Edward C. Pickering, "Draper Memorial photographs of stellar spectra exhibiting bright lines," Nature 34 (9 September 1886): 439-440.

first came to the attention of astronomers in 1600 as an apparently new star. Its brightness had varied throughout the seventeenth century, finally settling down to a fifth magnitude.

The bright lines in P Cygni were apparently due to hydrogen. Although Pickering did not point it out, this discovery would conflict with Lockyer's interpretation of stellar evolution in which stars evolved from cool red giants to hot white stars to cool red stars. Hydrogen would dominate the spectrum of the hot stars where other elements would ultimately be reduced to hydrogen by the extreme heat. It would, however, still support Draper, who had implied that the evolutionary process of stars was also a process of chemical evolution beginning with the elements hydrogen and nitrogen found in nebulae. The bright line spectra of nebulae would imply that there was no heavy atmosphere absorbing the hydrogen spectrum.

By the end of the first year the Henry Draper Memorial had been greatly expanded. Unusual spectra were pointed out and a full fledged astrophysical research program was ready to be implemented. Pickering announced in the annual report that not only would a southern station be established to carry out the work of the Draper Memorial in the southern sky, an attempt would be made to "include all portions of the subject, so that the final results shall form a complete discussion of the constitution and conditions of the stars, as revealed by their spectra."[62] This program would specifically include systematic studies of stars known to be variable and stars "whose spectrum was known to be banded, to contain bright lines, or to be peculiar in other respects."[63] Henceforth, Harvard College Observatory supported a full fledged astrophysical research program.[64]

---

[62]Edward C. Pickering, "The Henry Draper Memorial," Nature 36 (12 May 1887): 31; Harvard College Observatory, First annual report of the photographic study of stellar spectra, conducted at the Harvard College Observatory (Cambridge: John Wilson and Son, University Press, 1887). Pickering's announcement in Nature was dated March 1, 1887. The announcement detailed six specific programs for the Draper Memorial: 1. Catalog of the spectra of bright stars, 2. Catalog of the spectra of faint stars, 3. the detailed study of bright star spectra, 4. the detailed study of faint stellar spectra, 5. the comparison of spectra using a standard absorption spectra, and 6. the determination of the wavelengths of lines in stellar spectra. Harvard College Observatory established a southern observatory in Peru in 1889. Harvard College Observatory, Third annual report of the photographic study of stellar spectra, conducted at the Harvard Observatory (Cambridge: John Wilson & Son, University Press, 1889).

[63]Pickering, "The Henry Draper Memorial," 34.

[64]It would appear that much of the early work of the Draper Memorial dealt with resolving technological problems associated with the photography of stellar spectra. By the first annual report, Pickering had replaced the Allen and Rowell Extra Quick photographic plates with plates produced by the M.A. Seed Company of St. Louis. Over the next year the Seed Company worked with Pickering, improving the sensitivity of their plates such that by May, 1888 the Draper photographs of stellar spectra were able to detect stars one full magnitude dimmer than they had been able to reveal a year earlier. The sensitivity of the plate was so important that Pickering standardized the quality rating of each plate. As it was taken from its box a portion of each plate was exposed to a standard light for one second. When the plate was developed a dark square, by which the sensitivity and any defects of the plate might be measured, would appear in the corner.

Another burdensome problem--that of periodic clouds blocking the stars during exposure--was eliminated by the use of a Pole-star recorder. A small telescope was placed

By the third annual report, Pickering could no longer refrain from announcing the purpose of the Draper Memorial. The research had expanded so much that the report proclaimed the field of work as then defined was "almost boundless." The areas being investigated all related "to the fundamental laws regulating the formation of the stellar system." The Henry Draper Memorial had allowed, Pickering maintained, the work to be done on such a scale that it would avoid "undesirable duplication of work."[65]

If Laplace's nebular hypothesis, or at least a version thereof, played an important role in the development of Harvard's astrophysical research program, the results derived from that program played an equally important role in

---

parallel to the earth's axis and focused on a moving photographic plate of about three feet in length. Any clouds obstructing the view would result in gaps in the line made by the pole star. On occasion a photograph, using the same plate, was taken of the stars near the pole first with and then without the spectroscopic prism in place. A comparison of these photographs tested the condition of the air, the quality of the plates and the instrument. The problem of moisture on the objectives of the photographic telescope was resolved by instituting a regular program of checking for moisture and slowly heating the objectives when it was found. Harvard College Observatory, First annual report of the photographic study of stellar spectra, conducted at the Harvard College Observatory (Cambridge: John Wilson & Son, Harvard University Press, 1887) and Harvard College Observatory, Second annual report of the photographic study of stellar spectra, conducted at the Harvard College Observatory (Cambridge: John Wilson & Son, Harvard University Press, 1888).

[65]Harvard College Observatory, Third annual report of the photographic study of stellar spectra, conducted at the Harvard Observatory (Cambridge: John Wilson & Son, Harvard University Press, 1889), 2.

the development of modern astrophysics as it relates to stellar evolution. In 1890 Harvard College Observatory published "The Draper catalogue of stellar spectra," a catalog of the spectra of 10,351 stars north of declination -25°.[66]

Largely the work of Williamina Paton Fleming, one of many women whom Pickering hired as assistants at the Harvard College Observatory, the Draper catalogue provided a far more detailed classification system than had thus far existed. The spectra were classified into seventeen divisions signified by the letters A through Q.[67] Fleming's system was admittedly an "arbitrary" classification expanding the "four types" proposed by Secchi.[68] However, Fleming's system would quickly be revised. In fact, two classification systems were

---

[66]Harvard College Observatory, The Draper catalogue of stellar spectra photographed with the 8-inch Bache telescope as a part of the Henry Draper Memorial, Annals of the Astronomical Observatory of Harvard College, vol. 27 (Cambridge: John Wilson & Son, Harvard University Press, 1890).

[67]A-D consisted of variations of Secchi's first type; E-L, Secchi's second type; M, Secchi's third type; and N, his fourth type. O was reserved for stars with predominately bright line spectra, while P was used for planetary nebulae which also gave bright line spectra. Pickering considered O and P to be a fifth class which Secchi had overlooked. Finally Q was reserved for stars with peculiar spectra which did not fit into any of the previous classifications. Significantly no class N stars appeared in the initial Draper Catalog. Review of Annals of the Astronomical Observatory of Harvard College, vol. 27, Nature 44 (28 May 1891): 89-90 and Harvard College Observatory, The Draper catalogue of stellar spectra, 3-4.

[68]Edward C. Pickering, "Preface," in The Draper catalogue of stellar spectra, iii-iv.

developed to replace Fleming's. Both played important roles in the development of astrophysics and theories of stellar evolution.

Even before the initial publication of the Draper catalogue Antonia Maury, Draper's niece, had been assigned to classifying the spectra of bright stars in the northern hemisphere. Spectroscopic pictures of these stars revealed more detailed spectra than most of those which Fleming had classified, and Maury decided to expand the number of classifications to twenty-two signified by Roman numerals.[69]

While Fleming's classification system was admittedly an arbitrary expansion of Secchi's classifications, Maury's reveals the influence of her uncle Henry Draper. Unlike Fleming, who made no attempt to identify her classification system with an evolutionary development, Maury specifically associated her division of stellar spectra into twenty-two groups with stages in stellar evolutionary development.[70]

---

[69]Classes I - VI identified spectra exhibiting strong "Orion," [helium], oxygen, nitrogen and hydrogen lines; Classes VII to XI were divisions of Secchi's type I spectra; classes XII to XVI, his type II spectra, classes IVII to XIX, his type III spectra and Class XXI his type four spectra. Class XX represented variable stars showing a "fluted" spectra while XXII was the equivalent of Fleming's type O spectra which Pickering identified as a "fifth" type. Antonia C. Maury, Spectra of bright stars photographed with the 11-inch Draper telescope as a part of the Henry Draper Memorial, Annals of the Astronomical Observatory of Harvard College, vol. 28, pt. 1 (Cambridge: John Wilson & Son, Harvard University Press, 1897) 13-49.

[70]Maury, Spectra of bright stars photographed with the 11-inch Draper Telescope, 11.

The first five groupings of Maury's system consisted of spectra where the Orion lines--later identified with helium--were prominent. Group VI designated a spectra intermediate between the "Orion stars" and Secchi's first type. The spectrum of stars of Secchi's first type made up groups VII through XI while group XII defined the transition spectrum between Secchi's first type spectrum and his second type spectrum. Groups XIII through XVI lay within Secchi's second type spectrum while groups XVI through XX made up spectra classified by Secchi as type three. Finally group XXI was equivalent to Secchi's type four and XXII to the type five suggested by Pickering. However, Maury suggested that group XXII suggested that type five stars, which exhibited a bright line spectrum similar to nebula, might form a connecting bridge between nebular spectra and the spectra of "Orion" stars.[71] Maury's classification system, in this respect, was a refinement of Draper's own classification of stellar spectra in which Draper had presumed spectral development from nebulae, with their relatively small number of spectral lines through the more complex lines found in developed stars.

Both Draper and Maury also placed great emphasis on qualitative aspects of the spectral lines. Draper had placed great import on the fact that nebular spectra exhibited only

---

[71]Ibid., 11 and Edward D. Pickering, "A fifth type of stellar spectra," Astronomische Nachrichten 127 (1891): 1-4.

a few broad intense lines. In addition, Maury noticed variations in the quality of the lines of the spectra and developed an independent group of "divisions" designated by 'a,' 'b,' and 'c' to identify the quality type of spectra exhibited. The letter 'a' was applied to spectra having wide but sharply defined lines, 'b' was applied to spectra with broad hazy lines and 'c' to spectra with narrow sharply defined lines. Lines which didn't quite fit a particular classification were identified as 'ab,' 'ac,' etc.[72]

The lines became increasingly uniform as the classification series progressed. Spectra of stars of the Orion type and Secchi's type I exhibited a predominance of the broad division 'b' lines while stars of Secchi's types two and three exhibited nothing but division 'a' lines. Again Maury suggested that here might be found a source of some of the physical aspects of evolutionary development.[73] However, division 'c' seemed to be significantly different from division 'a' and 'b.' So different that Maury speculated that the so called metallic lines which exhibited 'c' lines might not be metallic lines at all.[74] The stars exhibiting 'c' lines Maury clearly argued, were different from stars with 'a' or 'b' line spectra. Whether or not Draper directly influenced Antonia Maury, her classification

---

[72]Ibid., 2-5.

[73]Ibid., 11.

[74]Ibid., 5.

system shared his concern about qualitative aspects of spectral classification.

When Annie Jump Cannon subsequently classified the bright stars in the southern Hemisphere, it was decided to reject Maury's system. However, the classification system developed by Cannon was in many respects a combination of Maury's and Fleming's. Cannon returned to Fleming's less detailed alphabetical classification but rearranged the order of the system to one which she believed followed declining stellar temperatures. She continued to use this system in the subsequent development of the Draper catalogue.[75]

Presumably Pickering's distaste for its subjective nature led him to relegate the observation of the "quality" of the spectra from a part of the classification system to notes in the appended remarks section.[76] Cannon's classification system would be adopted by the International Astronomical Union in 1910.

Draper had argued that the evolution of the star consisted of the evolution of chemical elements. In this he

---

[75]Annie J. Cannon, Spectra of bright southern stars photographed with the 13-inch Boyden telescope as a part of the Henry Draper Memorial, Annals of the Astronomical Observatory of Harvard College, vol. 28, pt. 2 (Cambridge: Harvard College Observatory, 1901) and Annie J. Cannon and Edward C. Pickering, The Henry Draper catalogue, Annals of the Astronomical Observatory of Harvard College, vols. 91-99 (Cambridge, Harvard College Observatory, 1918-1924).

[76]Cannon and Pickering, The Henry Draper catalogue, Annals of the Astronomical Observatory of Harvard College, vol. 91, 3-4.

had opposed the British astronomer, J. Norman Lockyer, who believed that stellar evolution and changes of spectral types centered around changes in the temperature of the stars. Stars, he contended, began as swarms of meteorites. As these swarms condensed the meteoric material would be vaporized as the temperature increased. As the mass condensed it would eventually radiate heat at the same degree or greater than the condensation produced heat and would begin to cool.[77] However, the arrangements of stellar spectra developed by Maury and Cannon suggested to many astronomers that stars began as hot bodies and cooled throughout their life cycle going, in Cannon's classificatory system from O to B, A, F, G, K, M, and N. Astronomers, however, had not yet found a mechanism for determining the luminosity and presumably from that the temperature of a particular star. In 1905 Einar Hertzsprung developed a statistical method of determining absolute magnitudes based upon a star's proper motion. This statistical study showed a correlation between absolute

---

[77]J. Norman Lockyer, "The Bakerian lecture. Researches in spectrum analysis in connection with the spectrum of the sun," Philosophical Transactions 164 (1874), 492; J. Norman Lockyer, The meteoritic hypothesis. A statement of the results of a spectroscopic inquiry into the origin of cosmical systems (London and New York: MacMillan & Co., 1890), 342-354. Lockyer's views were widely spread in America in the American edition of his Elements of astronomy, which went through sixteen printings between 1870 and 1890. They were also clearly explicated in books and articles such Lockyer's Inorganic evolution as studied by spectrum analysis (London: MacMillan & Co., 1900) and his "On the chemistry of the hottest stars," Royal Society, Proceedings 61 (1897): 147-209.

magnitude and spectral type. All white stars appeared to be of about the same magnitude, but the calculated luminosity of some red stars were very bright while others were relatively dim. While he could determine the distance of only a few stars based upon their stellar parallax he was able to show that the difference between his dim red "dwarfs" and his bright red "giants" had a correlation with Maury's 'c' characteristic.[78] About the same time, Princeton's Henry Norris Russell using data which Pickering supplied from the Draper memorial program, came to a similar conclusion that there were at least two classes of "red" stars and that the "dwarfs" were in the later stages of stellar evolution.[79]

In 1912 Henrietta Swan Leavitt published the results of her study of Cepheid variables in the smaller Magellanic Cloud. She was able to establish a correlation between the period of these variables and their apparent magnitude.

---

[78]Einar Hertzsprung, "Zur strahlung der sterne," Zeitschrift Für Wissenschaftliche Photographie 3 (1905): 429.

[79]Henry Norris Russell and Arthur Hinks, "Determinations of the stellar parallax from photographs made at the Cambridge Observatory," Monthly Notices of the Royal Astronomical Society 65 (1905): 775-785 and Henry Norris Russell and Arthur R. Hinks, "The parallax of 8 stars from photographs taken at the Cambridge Observatory," Monthly Notices of the Royal Astronomical Society 67 (1906):132-135. Russell did not discuss the theory or its implications in either of these papers however. In fact there is no evidence that Russell publicly espoused the Hertzsprung-Russell diagram until 1910. Henry Norris Russell, "Determinations of stellar parallax," Astronomical Journal 26 (1910): 147-159 and Russell, "Some hints on the order of stellar evolution," Science n.s., 32 (1926): 883-884.

Presuming that these variables were all approximately the same distance--that is the stars in the smaller Magellanic Cloud were grouped together--a correlation could be made between relative distance and luminosity. The relative luminosity of other stars grouped with a Cepheid variable could then be determined.[80]

The following year, at the sixteenth annual meeting of the Astronomical and Astrophysical Society of America, held in Atlanta, Georgia, Henry Norris Russell presented a theory of stellar evolution which, while not presuming Lockyer's meteoric swarms which condensed into stars, followed his changes in temperature in the evolution of stars. Russell's work relied heavily on that of Henrietta Swan Leavitt and the Draper Memorial Program at Harvard. Equally important, Russell's theory presumed the role of condensation, emphasized in the nebular hypothesis, on the evolutionary development of stars.[81]

---

[80]Henrietta S. Leavitt, Periods of 25 variables in the small Magellanic cloud, Harvard College Observatory Circular No. 173 ([Cambridge]: Harvard College Observatory, 1912).

[81]Henry Norris Russell, "Address. Relations between the spectra and other characteristics of the stars," American Astronomical Society, Publications 3 (1918): 22-61. This lecture read at the December 1913 meeting of the American Astronomical and Astrophysical Association, was first published in Nature in 1914. Henry Norris Russell, "Relations between the spectra and other characteristics of the stars," Nature 93 (30 April 1914): 227-230, (7 May 1914): 252-258, (14 May 1914): 281-286. For a general discussion of the development of the Hertzsprung-Russell diagram see Axel V. Nielsen, "Contributions to the history of the Hertzsprung-Russell diagram," Centaurus 9 (1963): 219-253.

By the middle of the second decade of the twentieth century, the Henry Draper Memorial program at Harvard College Observatory had become the center of a developing astrophysical research program. The theories which astrophysicists developed from the data which the program provided resembled more closely the early groping of Norman Lockyer instead of those of Henry Draper. Nevertheless, those theories still focused on the nature and evolution of stars and stellar systems and proposed models which in large part may be viewed as more sophisticated versions of Laplace's nebular hypothesis.

Since the middle of the nineteenth century, Harvard savants had grappled with the philosophical, religious and scientific implications of the nebular hypothesis. The Draper Memorial program, arising out of these concerns, provided observational data for astrophysicists who put forth empirically based explanations to questions originally formulated in philosophical, religious and theoretical contexts. The spectroscope did not lead to these questions, it only provided new and, at least for the scientists using spectroscopic research, more satisfying answers. While Hertzsprung and Russell may not have been aware of the specific concerns of Henry Draper, Benjamin Peirce and others associated with the development of astronomy and astrophysics at Harvard, the astrophysical theories which they developed out of Harvard's astrophysical research owed their existence to the philosophical, religious, and scientific questions of these and other men and women prior to the institutionalization of the science of astrophysics.

CHAPTER 6

FROM THE WEATHER BUREAU TO THE SMITHSONIAN: SAMUEL PIERPONT LANGLEY AND THE DEVELOPMENT OF THE SMITHSONIAN ASTROPHYSICAL OBSERVATORY

Two factors play important roles in Langley's development of the Smithsonian Astrophysical Observatory in 1891. First, his conviction that the ultimate meaning of solar research lay in the measurable influence of the sun's heat on terrestrial meteorology defined Langley's astrophysical research. Second, Langley turned to the government as the one source of funding.

During the 1870s and early 1880s the debates about the viability of agriculture on the western plains had encouraged the development of solar physics research in the Army Signal Service. The precise role which those debates played in the development of Samuel Pierpont Langley's research is less clear. Langley's published lectures and papers, while individually consistent, appear full of contradictions when taken as a whole. Langley borrowed from Plato's myth of the cave to point out his distrust of the senses,[1] yet he criticized _á priori_ ideas.[2] He indicated a

---

[1]S.P. Langley, "Sunlight and the Earth's atmosphere," _Nature_ 32 (7 May 1885): 18.

[2]Langley cited Berkeley's contributions to optics in a "New theory of vision" as "an exceptional if not a unique instance of a great physical generalization reached by _a priori_ reasoning." S.P. Langley, "The history of a doctrine," _American Journal of Science_ 3d ser., 37 (1889): 6.

profound distrust of scientific "truths" and encouraged at most their provisional acceptance, but he considered himself a member of "the orthodox scientific church."[3] He would explain erroneous popular views of science as the result of improper training but would welcome a paper containing "folly" at conferences on the ground that the members should not determine which papers contained wisdom or folly "when it is chiefly because we are ignorant that we are here?"[4] Langley claimed a reverence for pure science yet he almost always sided with applied science over pure science.

Reconciliation of all the apparent philosophical and intellectual contradictions in Langley's publications is beyond the scope of this dissertation. The development of Langley's research on the sun, radiant heat and terrestrial meteorology, however, illustrates important aspects of the rise of American astrophysics. At the superficial level, Langley's attempt at applying solar physics to solving the practical problems of weather prediction might be seen within the context of a scientist looking for financial support for pure science. Langley was among the first to utilize his observatory to provide time standards to the railroads. Certainly the income from the sale of time signals, purported

---

[3]Ibid., 19.

[4]Quoted in "Notes and news," Science, 10 (August 12, 1887): 81.

to be several thousand dollars annually,[5] went a long way in providing research funds for the Allegheny Observatory.

Langley focused on the relationship between his research and terrestrial meteorology in his successful application to the Army Signal Service for financial support for his Mt. Whitney expedition in 1881. He was clearly familiar with the rising demands of western farmers for some reliable means of long range weather prediction and he regularly held forth the hope that his research would satisfy those demands. His success in garnering financial support for his research from wealthy patrons suggests his capacity for connecting his scientific research with the interests of those less familiar with abstract scientific concepts.

However, to view Langley's explicit connection of the practical applications of solar physics to weather prediction as a mechanism for obtaining fiduciary support for his research is to overlook both Langley's understanding of the nature of scientific practice and the zeitgeist which provided the context for that understanding. Most, if not all, of Langley's seeming anomalous views can be reconciled by placing them within the context of scientific pragmatism. Certainly the environment of pragmatic philosophy provides an

---

[5]Ian R. Bartky, "Naval Observatory time dissemination before the wireless," in Sky with ocean joined: Proceedings of the sesquicentennial symposia of the U.S. Naval Observatory, December 5 and 8, 1980, ed. Steven J. Dick and Leroy E. Doggett (Washington, DC: U.S. Naval Observatory, 1983), 6.

important backdrop for understanding Langley's interest in the relationship between solar heat and terrestrial meteorology.

Pragmatism has traditionally been traced to C.S. Peirce's 1878 paper "How to Make Our Ideas Clear"[6] in which he maintained that the clearness of ideas was dependent upon the enunciation of the consequences of those ideas. Even earlier Chauncey Wright, secretary of the American Academy of Arts and Sciences, declared that the importance of abstract scientific ideas lay in the working out of their practical consequences. While Langley did not become a member of the American Academy, to which Peirce also belonged, until the 1880s, Peirce was widely read. Langley's associations made it practically impossible for him not to be aware of Peirce's works. C.S. Peirce's father, Benjamin Peirce, was a mathematician-astronomer at Harvard College while Langley was at the Observatory there. The possible influence of Chauncey Wright on Langley, however, appears to be more direct. In his "A physical theory of the universe," published in the April 1864 issue of North American Review, Wright criticizes Spencer's interpretation of the nebular hypothesis, suggesting that a dynamical theory of heat should require physicists to study the consequences of the heat released from the sun:

> The history of its heat would become involved with the grander phenomena of the weather,--phenomena that may be regarded as typical of that cosmical weather, concerning

---

[6]C.S. Peirce, "How to make our ideas clear," Popular Science Monthly, 12 (1878): 286-302.

> the laws of which we must inquire in considering what becomes of the sun's heat.[7]

Langley's own writings in the 1870s focused on the clarifying of terminology and the working out the practical measurable consequences of solar phenomena, methods and ideas which subsequently became the hallmarks of pragmatism.[8]

No where did Langley more clearly enunciate his views of the pragmatic nature of science than in his public lectures. In an 1875 lecture on the origin of solar heat, published in the New York Daily Tribune, Langley argued that scientific thought was not different from common thought except that it was more vigorous in "seeking to verify every idea by pursuing it to some consequence, the truth or falsity of which can be demonstrated by actual measurement and by

---

[7]Chauncey Wright, "A physical theory of the universe," in Philosophical discussions, (New York: Burt Franklin, 1971 reprint, 1877), 23. Wright's role in the development of pragmatism is still actively debated. Cf. Barbara MacKinnon, "Chauncey Wright (1830-75)," in American philosophy. A historical anthology, ed. Barbara Mackinnon (Albany: State University of New York Press, 1985), 115; Edward H. Madden, "The logic of science and cosmology," in Chauncey Wright and the foundations of pragmatism, (Seattle: University of Washington Press, 1963), 73-94; and Elizabeth Flower and Murray G. Murphey, "The evolutionary controversy," in A history of Philosophy in America, 2 vols. (New York: G.P. Putnam's Sons, 1977), 2:525-553.

[8]In 1882 Langley and C.S. Peirce served together on the National Academy of Science Committee to seek support for an eclipse expedition to the Caroline Islands in 1883. Clearly, by this time, there can be no doubt regarding potential discussions of common ideas. Frederick W. True, A history of the first half-century of the National Academy of Sciences 1863-1913 (Washington, DC: n.p., 1913), 63.

facts and figures."[9] A careful study of Langley's research suggests that Langley sought to verify his studies on the nature of the sun by finding the consequences of his theories in measurable patterns of terrestrial meteorology.

According to Langley, his initial research on the sun while at the Allegheny observatory had been a revision of Joseph Henry's study of solar heat with the thermopile. He may have been led to Henry's works through their mutual admiration of Macedonio Melloni.[10] He was undoubtedly familiar with Stephen Alexander's explicit connections between solar and terrestrial phenomena. Whether or not he was attempting to confirm those connections, his subsequent

---

[9]S.P. Langley, "Sources of solar heat. A lecture by Prof. S.P. Langley," New York Daily Tribune, 10 March 1875, 2. In "The laws of nature," Science 15 (1902): 921-27, Langley went so far as to argue that the laws of nature were creations of individual human minds to give order to sensations and to provide a sense of security in dealing with nature. That need for security was further manifested by man's inexorable drive to describe these laws as manifestations of the mind of a supreme being.

[10]In a speech before the American Association for the Advancement of Science in 1879, Langley found the root of modern interest in solar physics in Melloni's La Thermochrose. Nine years later, in another speech before the same body, Langley used Melloni as an example of how presumed scientific knowledge is but a manifestation of personal truths. S.P. Langley, "Address of Prof. Samuel P. Langley, vice-president, section A, before the American association for the advancement of science," Proceedings of the American association for the advancement of science; twenty-eighth meeting held at Saratoga Springs, N.Y., August, 1879 (Salem: Published by the Permanent Secretary, 1880), 51-63 and Langley, "The history of a doctrine," Proceedings of the American association for the advancement of science, thirty-seventh meeting, held at Cleveland, August, 1888, (Salem: Published by the Permanent Secretary, 1889), 1-23.

papers presented the view that the ultimate meaning of solar research lay in the promise of measuring and predicting the terrestrial consequences of that activity.

Langley was undoubtedly influenced by the work of the French physicist Hervé Faye as well. Faye had been involved in a controversy with British scientists over the constitution of sun spots for several years. Drawing on an analogy from James Pollard Espy's theory of storms, he published his cyclonic theory of sun spots in 1872. Joseph Henry's support for Espy in his conflict with James Redfield had focused on the role of solar heat in the creation of terrestrial storms. Lacking the ability to make accurate measurements or even detailed observations in the sun, Langley readily turned to Faye's terrestrial analogies to provide models for his solar observations. Among Langley's early papers, several announced observational support for Faye's theories.[11]

While not all of Langley's papers focused on the relationship between solar heat and meteorology, clearly that became the direction of his research at an early stage. However, like the subsequent pragmatic philosophers, Langley

---

[11]S.P. Langley, "On the minute structure of the solar photosphere," American Journal of Science 3d ser., 7 (1874): 87-101; S.P. Langley, "The solar photosphere," In Proceedings of the American association for the advancement of science. Twenty-second meeting held at Portland, Maine, August, 1873 (Salem: Published by the Permanent Secretary, 1874), 161-173. Faye's influence on Langley's early work can also be found in his public lectures. Cf. "Sources of solar heat. A lecture by Prof. S.P. Langley," New York Daily Tribune, 10 March 1875, 2.

first sought to clarify the confusing observational terminologies used by solar physicists in the second half of the nineteenth century. Among his early publications in the American Journal of Science, Langley's "On the minute structure of the solar photosphere," published in 1874, ostensibly sought to standardize and define the various terminologies used by different observers in describing the phenomena they observed. His choice of analogies to terrestrial phenomena and in particular to terrestrial meteorology is also reflected in his concluding support for Hervé Faye's cyclonic theory of the movements in the sun's photosphere.[12] In another paper, published a year later in the same journal, Langley admitted the danger of "pushing too far, conclusions drawn from terrestrial analogy," but he continued to underscore the usefulness of terrestrial meteorology for providing explanatory models for solar phenomena.[13] However, the problem lay not so much in the analogies as in the inability to measure the consequences of the corresponding events in the sun.

While one could not directly measure the sun's heat at its source, one could measure the heat received by the earth. Langley assumed that once the heat received by the

---

[12] S.P. Langley, "On the minute structure of the solar photosphere," 99.

[13] S.P. Langley, "On the comparison of certain theories of solar structure with observation," American Journal of Science 3d ser., 9 (1875): 196.

earth was properly measured one would be able to predict future weather patterns based on changes in the amount and nature of that reception.[14] It was but a small step from this position to one in which the ultimate understanding of changes in the sun lay in an understanding and measurement of the consequence of those changes on the earth. For Langley the former position was a manifestation of the latter. Humans were "creatures" of the sun, dependent upon it for their very existence. A temporary interruption of solar heat would lead to the immediate extinction of life on earth. The very meaning of solar research lay, therefore, in its ability to predict how minute changes in the sun would affect life on the earth.[15] For Langley the distinction between pure and applied science was blurred. Applied science was but that branch of pure science which worked out the necessary consequences which gave meaning to the abstract principles and theories making up scientific knowledge.

Detecting the ramifications of the solar heat received by the earth, however, was virtually impossible with the technology of the 1870s. Langley's problems lay, then, not in his presumption of the importance of the connection

---

[14]S.P. Langley, Researches on solar heat and its absorption by the Earth's atmosphere. A report of the Mount Whitney expedition, Professional Papers Signal Service, No. 15, (Washington, DC: Government Printing Office, 1884) and S.P. Langley, "On the amount of atmospheric absorption," American Journal of Science 3d ser., 28 (1884): 179.

[15]"Sources of solar heat. A lecture by Prof. S.P. Langley," New York Daily Tribune, 10 March 1875, 2.

between changes in solar heat and terrestrial meteorology, but in the complexities of determining precisely what were the consequences and in measuring the effects of those changes. For the next ten to fifteen years, his research addressed the technological and theoretical issues which hindered those crucial measurements.

For years physicists and astronomers had estimated the solar constant, the amount of heat received normally per unit area at the outer layer of the earth's atmosphere.[16] Claude Pouillet had established a value of 1.78 calories/cm^2/second, while in 1875 Jules Louis Gabriel Violle estimated it to be 2.54 calories/cm^2/second based on his measurements at the base and top of Mount Blanc. André Crova, at Montpelier, had estimated its value at 2 calories/cm^2/second. This broad range of estimates as to the amount of solar energy impinging on the surface of the earth's atmosphere made any attempt to measure the consequences of that heat on the earth futile. Astronomers had also observed that the light of the sun was partially

---

[16]In the cgs system the solar constant is measured by the units ergs/cm^2/second or, utilizing the mechanical equivalent of heat--4.18×10^7 ergs = 1 calorie--calories/cm^2/second. Early astronomers and physicists measuring the solar constant would utilize that number to estimate the heat of the sun. Others, such as Langley, would utilize values of solar heat derived from spectroscopic research in an attempt to derive a solar constant value from measurements at the sun and at the surface of the earth. Modern measurements presume that the solar constant varies over time but is approximately $1.37 \pm 0.02 \times 10^6$ ergs/cm^2/sec.

absorbed by the solar atmosphere. Laplace had determined that the solar atmosphere absorbed about 11/12 of the sun's radiation, and Secchi had found an absorption rate of 78 percent.

Earlier scientists had not, Langley observed, attempted to draw conclusions from their measurements of this solar absorption regarding its effect on terrestrial temperature. He argued that solar absorption was not as high as Laplace and Secchi had calculated. His own research indicated that the solar atmosphere, like the terrestrial atmosphere, selectively absorbed the energy radiated at the surface. In order to determine the temperature of the sun, one would first have to map both the earth's and the sun's absorption spectrum. Even with the limited mapping of such spectrums as had been done, Langley argued that the solar constant approached three calories/cm^2/second.[17]

Significantly he pointed out that "with a slight change in the depth and absorptive power of this atmosphere, fluctuations in terrestrial temperature will ensue, very great in comparison with any actually observed within historic periods. . . ." Furthermore, he argued that it

---

[17]S.P. Langley, "The selective absorption of solar energy," American Journal of Science 3d ser., 25 (1883): 182. Langley initially set his estimate at 2.84 calories. In subsequent publications he raised the estimate to 3.0 calories. He again lowered his estimate to about 2.5 calories sometime after 1900.

could be shown that the solar atmosphere was not in a "strictly stable condition."[18]

Langley had found in his study of the sun a portent of change which could be theoretically measured on the earth. Utilizing his estimation that the solar atmosphere absorbed about 50 per cent of the sun's radiation, he argued that an increase or decrease of 25 per cent in the solar atmosphere would result in a corresponding decline or increase in the earths average temperature of about 100° F.

Father Secchi had concluded that differences in the measurements of heat at different parts of the sun's disc made in 1852 at Rome and in 1873 and 1874 at Allegheny must be explained by a change in solar absorption. Langley differed, agreeing with Faye that no evidence of variation had been established. Any such shift in absorption by the solar atmosphere would have the necessary consequence of a change in terrestrial temperatures. Langley went on to contend that although Hermann von Helmholtz and John Ericsson had established the probability of a constancy of solar energy within the solar envelope during measurable history, there was no reason to proscribe great cyclical changes corresponding to past geological changes in the earth.[19]

---

[18]S.P. Langley, "The solar atmosphere, an introduction to an account of researches made at the Allegheny Observatory," American Journal of Science 3d ser., 10 (1875): 490.

[19]Ibid., 497.

Having established, at least in his own mind, that the sun's atmosphere did not change its rate of absorption frequently and that studies over periods greater than one life would be necessary to confirm any influence of such a change on terrestrial temperatures, Langley turned to the possible effects of sun spots on the earth's weather. Joseph Henry and Stephen Alexander's research in the 1840s had determined that less heat was radiated from sun spots than from surrounding regions of the sun.[20] Should that difference be significant, it could be a factor causing changes in terrestrial weather patterns during the sun spot cycle. In 1876 Langley concluded that changes in the emission of radiant energy due to the sun spot cycle would not result in significant change in the amount of solar energy received by the earth.[21]

If the amount of energy radiated by the sun and received by the earth remained approximately the same, changes in the nature of that energy might still have consequences on the earth. Langley decided to focus on the relationship between wavelength and the energy received by the earth. However, to detect the energy of a homogeneous

---

[20]S.P. Langley, "Measurement of the direct effect of sun-spots on terrestrial climates," Monthly Notices of the Royal Astronomical Society 37 (1876): 5-6.

[21]Langley calculated that the change in the earth's mean temperature over the sun spot cycle would be greater than .05°C but less than .3°C. S.P. Langley, "Measurement of the direct effect of sun-spots on terrestrial climates," 11.

wavelength would require the development of new and more delicate instrumentation. That instrumentation, as Langley subsequently pointed out, would have to do more than detect the weak spectrum in the infrared; in order to be useful it would have to measure the energy. Between 1878 and 1881, Langley worked on developing the bolometer. For the next twenty years, many of Langley's publications dealt primarily with improvements to his instrumentation.[22]

Langley's inclusion in the Signal Service's Pikes Peak expedition in 1878 and the Signal Service's funding for Langley's Mount Whitney expedition in 1882 reflected both Langley's focus on the potential measurable consequences of theories of the sun and the growing demand to find ways of utilizing theories of solar physics in the day to day meteorological predictions of the Signal Service. The Mount Whitney expedition first sought to develop a means of measur-

---

[22]S.P. Langley, "A proposed new method in spectrum analysis," American Journal of Science 3d ser., 14 (1877): 140-146; S.P. Langley, "New solar photographs," Popular Science Monthly 12 (April 1878): 748; S.P. Langley, "The spectroscope in solar work," Scientific American 53 (19 October 1878): 242-243; S.P. Langley, "The Bolometer," Proceedings American Metrological Society 2 (1881): 184-190; S.P. Langley, "Experimental determination of wave-lengths in the invisible prismatic spectrum," American Journal of Science 3d ser., 27 (1884): 169-188; S.P. Langley, "Note on the transmission of light by wire gauze screens," American Journal of Science 3d ser., 30 (1885): 210-212; S.P. Langley, "Note on the optical properties of rock-salt," American Journal of Science 3d ser., 30 (1885): 477-481; S.P. Langley, "The Bolometer," American Journal of Science 4th ser., 5 (1898): 241-245 and "Mr. Langley's recent progress in Bolometer work at the Smithsonian Astrophysical Observatory," Astronomy and Astrophysics 13 (January 1894): 41-44.

ing the solar constant by measuring the selective absorption of solar heat. Second, Langley sought to determine the extent to which the earth's atmospheric temperature was due to direct radiant heat of the sun and the extent to which it was due to the absorption, storage and re-radiation of heat in the earth's atmosphere.[23]

Langley's explicit connection between his research on the sun and terrestrial meteorology led by 1878 to a loose alliance with the meteorological research of the U.S. Army Signal Service.[24] However, by the mid-1880s, the Signal Service's scientific research program, as well as all

---

[23]Langley's calculation of a solar constant of nearly 3.0 calories/cm^2/minute was based on the formula of Pierre Bouguer and Johann Heinrich Lambert. That formula assumes the average transparency for the atmosphere is sufficiently close to the transparency for each section. As Langley's assistant at the Smithsonian Astrophysical Observatory, Charles Greeley Abbott, pointed out in 1905, that formula did not accurately measure the decreased transmission of the atmosphere between the top of Mount Whitney and the observing station at the base of the mountain. Abbott records that Langley, quoting the saying "What has posterity done for us, that we should care so much for the opinion of posterity," refused his suggestion to publish a correction. As a result the correction was not published until after Langley's death in 1906. In fact, sometime after 1900 Langley appears to have modified his estimate of the solar constant while still accepting the utilization of Bouguer's formula. Charles Greeley Abbott, The sun and the welfare of man (Washington DC: Smithsonian Institution, 1929), 22; S.P. Langley, "On the amount of atmospheric absorption," American Journal of Science 3d ser., 28 (1884): 163-180; S.P. Langley, "The 'solar constant' and related problems," The Astrophysical Journal 17 (1903): 89-99; and S.P. Langley, "On a possible variation of the solar radiation and its probable effect on terrestrial temperatures," Astrophysical Journal 19 (1904): 305-321.

[24]See pp. 110-111.

scientific research carried on by the government, was under attack. In 1884 Congress set up a joint commission to "consider the present organization of the Signal Service, Geological Survey, Coast and Geodetic Survey, and the Hydrographic Office of the Navy Department."[25] The Commissions duties included resolving conflicts between the various geological surveys, determining the duties of the Signal Service, and, in general, attempting to define the role of

---

[25]Joint commission to consider the present organization of the Signal Service, Geological Survey, Coast and Geodetic Survey, and the Hydrographic Office of the Navy Department, with a view to secure greater efficiency and economy of administration of the public service in said bureaus, authorized by the sundry civil act approved July 7, 1884, and continued by the sundry civil act approved March 3, 1885, Testimony, 49th Cong., 1st Sess., March 16, 1886, Senate Miscellaneous Document No. 82 (Washington, DC: Government Printing Office, 1907), hereafter referred to as Allison Commission, Testimony.

The classic source for the history of the Allison Commission is A. Hunter Dupree, "The Allison Commission and the Department of Science, 1884-1886," 215-231 in Science in the federal government: A history of policies and activities to 1940 (Cambridge, Mass.: Harvard University Press, Belknap Press, 1957). The Allison Commission was a joint Congressional commission formed to study and make recommendations regarding the relationship between government and science. Dupree briefly mentions the concerns about research in the Signal Service in his discussion of the Allison Commission. He further discusses meteorological research of the Signal Service in pages 187-192. This dissertation will focus on different aspects of the Allison Commission report than did Dupree.

In addition to Allison (Iowa), the initial members of the commission were Senators Eugene Hale (Maine) and George H. Pendleton (Ohio) and Congressmen Robert Lowry (New York), Hilary A. Herbert (Alabama) and Theodore Lyman (Massachusetts). In 1885 Senator John T. Morgan (Alabama) and Congressmen John T. Wait (Connecticut) replaced Pendleton and Lyman who had not been re-elected. A survey of the papers of Senator William B. Allison for the period 1883-1886 in the archives of the Iowa State Historical Society revealed no material relating to the Allison Commission.

the federal government in scientific activity.

Of the 1084 pages of testimony taken by the Allison Commission, 414 pages dealt with problems of the Signal Service, but virtually none of it discussed the new research being done at the Signal Service. Instead, much of the debate was over its administration and particularly whether the duties of the Signal Service should be carried out under military or civilian administration. It may be significant, however, that many of the figures attacking General William B. Hazen's administration of the Signal Service had also been involved in the earlier conflicts about the viability of the high western plains.

Congressman Theodore Lyman of Massachusetts, the one member of the commission with a scientific background, turned to the National Academy of Sciences for their recommendations on the organization of science under the federal government. The committee formed by the National Academy of Sciences was chaired by General M.C. Meigs and included Langley, as well as leading astronomers and scientists whose names would subsequently be associated with astrophysical research.[26]

---

[26]The committee appointed by the National Academy of Sciences to aid the Allison Commission included, in addition to General Meigs, William H. Brewer, Cyrus Comstock, S.P. Langley, Simon Newcomb, Professor E.C. Pickering, Professor W.P. Trowbridge, Francis A. Walker, and C.A. Young. Newcomb and Comstock, on the orders of the Secretary of the Navy and the Secretary of War, their superiors, resigned shortly after their appointments. Langley, Pickering and Young were already leading figures in research fields relating to astrophysics. Trowbridge had made contributions to the discussions in America about the nebular hypothesis.

Lyman asked the committee to advise the commission on the organization of government science in Europe, how the government could best organize its scientific branches, and what changes or additions to government science would be desirable.[27]

The National Academy of Sciences committee reported in favor of a cabinet level Federal Department of Science. Alternatively, the committee suggested the consolidation of government science into four bureaus: the Coast and Interior Survey, the Geological Survey, a Meteorological Bureau, and, since the committee had focused on the scientific and predictive divisions of the Signal Service work, a "physical observatory to investigate the laws of solar and terrestrial radiation and the application to meteorology with such other investigation in exact science as the Government might assign to it." The sundry investigations which might be assigned included the establishment of standard weights and measures, as well as electrical research.

While the physical observatory reflected the interests of a majority of the members of the committee, its inclusion appeared initially to be a catch all for all research not specifically assigned to the other departments. In fact the separate division for an astrophysical

---

[27]W.B. Hazen to Maj. Gen. M.C. Meigs, Sept. 17, 1884 in Allison Commission, Testimony, 12.

observatory may have been a reaction to General Hazen's testimony to the National Academy of Science's committee.

In 1881 the National Academy of Sciences had appointed, at General Hazen's request, a Committee on Questions of Meteorological Science and Its Applications.[28] Langley, as one of the committee members, may have used the committee to obtain support for his Mt. Whitney expedition. However, there is no indication of any formal activity by the committee before it was disbanded in 1884. In spite of Hazen's strong support for Langley's research in 1882, he only weakly acknowledged the scientific research of the Signal Service in his testimony. As a result he may have offended those whose astrophysical research was closely connected with his service. The inclusion of electrical research under the "physical observatory" makes more sense when one considers the perceived relationship between sun spots and electrical storms.[29] General Hazen's testimony to

---

[28]The committee, chaired by Simon Newcomb, included Loomis, Wolcott Gibbs, H.A. Newton, William Ferrel, Charles A. Schott, Ogdon N. Rood, C.A. Young and Langley. Frederick W. True. _A history of the first half-century of the National Academy of Science, 1863-1913_ (Washington, DC: n.p., 1913), 290.

[29]The relationship between electricity, the sun and the weather played an important role in research into atmospheric electricity. Views on the subject were sufficiently established that at a September 1884 electrical conference, held in connection with the Philadelphia Electrical Exhibition, Professor Cleveland Abbe presented a paper and led a discussion on the possible relations between atmospheric electricity, earth-currents and the weather. "Discussions at the electrical conference," _Popular Science Monthly_ 26 (1884-1885): 286.

the Commission pointed out that the Signal Service had just begun research on the relationship between the earth's electrical currents and meteorological phenomena.

General Hazen's letter, informing the Academy of Science's committee of the work of the Signal Service, complained that while government sponsored meteorology in Europe dealt primarily with climatology, in America "the specific object for which appropriations have annually been made has been the immediate and current benefits arising from the "observation and report of storms."[30] As a result, little had been "left for climatological work or research."[31] In spite of these reservations, Hazen asserted that European meteorological research took so long to be published that it was of no value to the public when it finally reached them. He concluded: "I doubt if there is anything in any of these services that could be advantageously incorporated into our own."[32]

Two months later, in his written testimony before the Allison Commission, Hazen again sounded very cautious about the original research being done in the Signal Service. The Study Room, under the direction of Cleveland Abbe, had been organized shortly after Hazen had taken over the Signal

---

[30]W.B. Hazen to Maj. Gen. M.C. Meigs, Sept. 17, 1884, in Allison Commission, Testimony, 12.

[31]Ibid.

[32]Ibid.

Service. However, its primary duties were to create special reports and information demanded by others in the Signal Service. The Study Room's duties included the study of atmospheric electricity and solar radiation, in addition to developing standard instruments, preparing tables for reducing meteorological observations and preparing meteorological textbooks and training meteorological classes at Fort Myer. In spite of the value of this research, Hazen concluded that, excepting its connection to the predictive work of the Signal Service, everything done in the Signal Service's Study Room "could as well be done under any other Bureau."[33]

By 1885, then, General Hazen appears to have been backing away from supporting the astrophysical research of Langley and others. Langley, on the other hand, appears to have taken the lead in defending the government's role in scientific activity. At the Ann Arbor meeting of the American Association for the Advancement of Science, Langley proposed a series of resolutions defending the work of the Coast Survey against a Treasury Department report which alleged that the value of the Survey's scientific work was "meager."[34] Langley's resolutions, praising the high caliber of the Coast Survey research and calling for evaluations by "scientific men" of the scientific work performed in

---

[33]Ibid., 19-20.

[34]"Official science at Washington," Popular Science Monthly 27 (1885): 845.

government, passed unanimously.[35] However, it was attacked by the editors of Popular Science Monthly, who, with Louis Agassiz, opposed government funding of science[36] and who felt that Langley and the scientific community showed more concern for establishing their professional interests as an integral part of the government than they did for the proper role of government.[37]

Agassiz had argued that the role of government did not include funding scientific activity. Such funding he felt, should be left to the private sector. Langley, on the other hand, argued that the issue was really an interpretation of the Constitution. Those who held that the federal government's powers were limited to those explicitly granted by the constitution would limit the role of science to the functions of the patent office or other "economic" institu-

---

[35]Ibid.

[36]On Agassiz's position regarding government funding of science see "Science and the state," Popular Science Monthly 29 (1886): 414; and Dupree, Science in the federal government, 220-224.

[37]"Official science at Washington," 844-847. The breadth of Popular Science Monthly's attack on Langley's resolutions goes beyond the scope of this dissertation. However the editors advocated that the government should take a laissez faire attitude regarding scientific activity arguing that even Government use of the National Academy of Sciences and the Smithsonian Institution lessened the government's ability to perform its constitutional duties and discouraged private institutions from taking up research in competition with government funded institutions. "Science and the state," 412-415.

tions indispensable for federal operations.[38] Presumably, those who found the role of government limited only by expressed constitutional prohibitions would welcome government's role in funding scientific research, at least where that activity would potentially benefit the nation.

The Smithsonian, Langley would suggest, provided a means of circumventing the constitutional limitations of the strict constructionists.[39] Once the government accepted responsibility for Smithson's bequest, it could and did provide funding for obligations it imposed on the Smithsonian beyond those which could be provided by Smithson's bequest.[40] One needed to get the government to request information, not settle constitutional debates.

Langley's support for government science and Hazen's apparent abandonment of scientific research in the Signal Service in 1884 can, in part, explain the National Academy of Science Committee's support for removal of the Weather Bureau from the Signal Service and the explicit separate establish-

---

[38]S.P. Langley, "The scientific work of the government," MacClure's Magazine 35 (1904): 81.

[39]Ever since the debate between the Federalist followers of Alexander Hamilton and the Republican followers of Thomas Jefferson periodic debates took place over the powers of the Federal government. Strict constructionists argued that the powers of the Federal government were limited to those specifically enumerated by the Constitution. Others argued that the constitution granted inferred or implied powers not specifically granted but necessary to carry out functions which the Constitution delegated to the Federal government.

[40]Langley, "Scientific work of the government," 92.

ment of a physical observatory to carry on the scientific research which Langley and others had begun with the support of the Signal Service.

They also suggest some of the motivation for Langley's acceptance of assistant secretary of the Smithsonian Institution in 1887 on the condition that an observatory would be provided for him to continue his astrophysical research.[41] Langley maintained the nominal directorship of the Allegheny Observatory until the opening of the Smithsonian Astrophysical Observatory in 1891.[42] The new observatory was created primarily to investigate "what the sun is, how it effects terrestrial climate and life, and how it may best be studied for the purposes of the meteorologist." Specifically, the observatory was to study solar light, heat and radiant energy in general and their relations to terrestrial physics--a continuation of Langley's earlier work at the Allegheny Observatory.[43]

In many respects the observatory's work was a continuation of the sort of research sponsored by the Signal Service Weather Bureau in the early 1880s and proposed by the

---

[41]"The Smithsonian Astrophysical Observatory," The Sidereal Messenger 10 (1891): 272.

[42]"New director of Allegheny Observatory," The Sidereal Messenger 10 (1891): 297 and "The Smithsonian Astrophysical Observatory," The Sidereal Messenger 10 (1891): 272.

[43]"The Smithsonian Astrophysical Observatory," The Sidereal Messenger 10 (1891): 272, 273.

National Academy of Science's report to the Allison Commission in 1885. However, under the Smithsonian Institution the observatory would not be a branch of the government. In fact, the government initially was to only provide a site for the observatory and $10,000 for its maintenance.[44] The equipment and observatory itself were to be financed by a $5,000 grant from Alexander Graham Bell and a matching bequest from the late Dr. J.H. Kidder, formerly Curator of Exchanges at the Smithsonian Institution.[45] However, Congress failed to designate a location for the observatory. Langley, in his annual reports and other papers, continued to complain about problems associated with the "provisional" site as late as 1903.[46]

---

[44]"The Smithsonian Astrophysical Observatory," Nature 44 (16 July 1891): 254-255.

[45]"The Smithsonian Institution," Nature 45 (14 January 1892): 262 and "Mr. Langley's recent progress in Bolometer work at the Smithsonian Astrophysical Observatory," Astronomy and Astrophysics 13 (1894): 43-44.

[46]"Smithsonian Astrophysical Observatory, in Smithsonian Institution Annual Report for 1893 (Washington, DC: Government Printing Office, 1894), 60; "Smithsonian Astrophysical Observatory, in Smithsonian Institution Annual Report for 1896 (Washington, DC: Government Printing Office, 1897), 68; "Smithsonian Astrophysical Observatory, in Smithsonian Institution Annual Report for 1898 (Washington, DC: Government Printing Office, 1899), 69; "Smithsonian Astrophysical Observatory, in Smithsonian Institution Annual Report for 1902 (Washington, DC: Government Printing Office, 1903), 85; "The Smithsonian report for year ending 1892," Nature 48 (22 June 1893): 184; "The Smithsonian Institution report," Nature 49 (22 February 1894): 397-399; "Smithsonian investigations," Nature 53 (5 March 1896): 429; S.P. Langley, "The Bolometer," Nature 57 (28 April 1898): 621-622; and "Smithsonian report on scientific work," Nature 68 (7 May 1903): 20-22.

Langley utilized grants and bequests to improve the connection between the Smithsonian Institution and meteorologists. When Thomas George Hodgkins donated $200,000 to the Smithsonian in 1892, the interest from $100,000 was designated for studies on the properties of atmospheric air considered in its broadest relationship to all branches of science.[47]

While Langley saw the Smithsonian as a mechanism to circumvent potential constitutional problems for government funding of science,[48] the Astrophysical Observatory, in particular, became the center for the continuation of his own astrophysical research.

For the first few years, research at the Smithsonian Astrophysical Observatory focused on improving Langley's bolometer in preparation for what Langley called the first stage of a long labor--the accurate mapping of the infra-red spectrum.[49] With Langley's first bolometer in 1882 it had

---

[47]Smithsonian Institution Annual Report for 1892 (Washington, DC: Government Printing Office, 1894), 60; "The Smithsonian report for year ending 1892," Nature 48 (22 June 1893): 184.

[48]Langley appears to have viewed private foundations which supported scientific activity as a threat to the Smithsonian's continued existence. He is alleged to have charged C.D. Walcott, then director of the U.S. Geological Survey and influential in brokering the initial $10,000,000 Carnegie endowment for science, with having "ruined the Smithsonian Institution." He still recognized the important role of the Carnegie Foundation and turned to it for support. Charles Greeley Abbot, The sun and the welfare of man (Washington DC, Smithsonian Institution, 1929), 21.

[49]S.P. Langley, "On recent researches in the Infrared spectrum," Nature 51 (1 November 1894): 15-16.

taken two observers two years to map twenty spectral lines.[50] By 1894 both observers had been replaced by a photographic plate. A beam of light was directed onto the plate by a galvanometer and the plate was moved through the spectrum by a clock drive. Workers could deduce the amount of heat and the position in the spectrum from the curve drawn on the photographic plate.[51]

With these improvements, Langley could record more infra-red spectrum lines in a single afternoon than he had previously recorded in a year's work.[52] In 1896 and 1897 mechanisms to automatically control the temperature of the bolometer were devised and the instrument was placed in a chamber with temperature controlled to 0.1° Centigrade, thereby reducing drift to a minimum.[53] By 1897 Langley's

---

[50]Ibid., 13. According to the process used in 1882 one observer read the position of the bolometer in the spectrum while the second observer read the deflection on a galvanometer scale.

[51]Ibid., 14-15.

[52]"Smithsonian Astrophysical Observatory, in Smithsonian Institution Annual Report for 1893, 60; "Smithsonian Astrophysical Observatory, in Smithsonian Institution Annual Report for 1894 (Washington, DC: Government Printing Office, 1894), 75; "Mr. Langley's recent progress in Bolometer work at the Smithsonian Astrophysical Observatory," 41-44.

[53]"Smithsonian Astrophysical Observatory, in Smithsonian Institution Annual Report for 1897 (Washington, DC: Government Printing Office, 1898), 66; "Smithsonian Astrophysical Observatory, in Smithsonian Institution Annual Report for 1900 (Washington, DC: Government Printing Office, 1901), 99; S.P. Langley, "The Bolometer," American Journal of Science 4th ser., 5 (1898): 241-245; and G[eorge] E[llery] H[ale], Review of Annals of the astrophysical observatory of the Smithsonian Institution, vol. 1, by S.P. Langley, In Astrophysical Journal 13 (1901): 281.

"spectrobolometer" could reputedly measure temperature differences as small as 0.000001 degree centigrade. Langley quickly completed his study of about 200 spectrum lines in solar infra-red radiation.[54] However, the study was not published until 1901.[55] By that time the study had been expanded to 600 spectrum lines[56] and Langley had moved on to stage two of his research--giving meaning to those newly mapped infrared spectrum lines.[57] Langley, true to his pragmatic view of science, sought to find the meaning of the lines, not in an explanation of the lines themselves, but in the consequences of those lines for man's life on earth.[58]

54"Smithsonian Astrophysical Observatory, in Smithsonian Institution Annual Report for 1897, 66 and "Notes," Nature, 57 (7 April 1898): 544. While public pronouncements failed to clearly specify, it is quite clear that variations of 0.000001° C did not refer to the degree of accuracy in measuring solar heat. Rather they referred to the sensitivity of the spectrobolometer itself.

55S.P. Langley, The absorption lines in the infra-red spectrum of the sun, Annals of the astrophysical observatory of the Smithsonian Institution, vol. 1 (Washington, DC: Smithsonian Institution, 1901), 7-216.

56"Recent studies of infra-red region of solar spectrum," Nature 63 (15 November 1900): 68.

57S.P. Langley, "On recent researches in the infra-red spectrum," 15-16.

58Langley's most complete expression of his pragmatic philosophy of science can be found in "The laws of nature," read March 10, 1902, before the Philosophical Society of Washington, DC Cf. S.P. Langley, "The laws of nature," Science 15 (1902): 921-927.

Langley's early publications derived from these studies concentrated on potential seasonal variations in the Telluric lines which might prove useful in weather prediction.[59] Langley predicted that the study of these lines might result in crop predictions analogous to the Weather Bureau's weather predictions.[60]

Either Langley decided that such predictions proved unfeasible or he perceived new lines of research focusing on the portent of changes in the sun for weather prediction held greater significance, for, after 1901, research at the Smithsonian Astrophysical Observatory again concentrated on finding variations in the solar constant.[61] Perhaps the change in the emphasis was the result of additional improvements to the bolometer which had increased its precision.[62] For whatever reason, Langley returned to the study of the solar constant which he had abandoned for some twenty

---

[59]"Recent studies of infra-red region of solar spectrum," 68; S.P. Langley, "Sur les derniers résultats obtenus dans l'étude de la partie infra-rouge de spectre Solaire," Comptes Rendus 131 (1900): 734-736; "Paris, academy of sciences, November 5," Nature, 63 (15 November 1900): 75.

[60]S.P. Langley, "The new spectrum," American Journal of Science 11 (1901): 413.

[61]Smithsonian Institution Annual Report for 1902, 85 and "The solar constant," Nature 67 (2 April 1903): 522.

[62]C.G. Abbot's improvement's to the bolometer's galvanometer after 1901 had resulted in the ability to measure temperature differences as small as .00000001° Centigrade. Smithsonian Institution Annual Report for 1902, 85 and "Smithsonian report on scientific work," Nature, 68 (7 May 1903): 20-22.

years.[63] It did not take long for Langley to again point out the importance of this study for weather prediction.

The temporary structure for the Smithsonian Astrophysical Observatory was notably inadequate for the study of the solar constant. Not only did traffic vibrations interfere with the accuracy of the bolometer, the low level of the observatory, Washington weather and urban pollution combined to interfere with regular measurements of solar radiation. Having apparently failed in his attempts to draw on government support, Langley, in 1902, turned to the Carnegie Foundation, recommending that Carnegie provide $500,000 to fund a solar research in South America throughout an eleven-year sun spot cycle.[64] As always, his argument for the observatory hinged on advancing solar physics to the point where it could predict long-range weather patterns. By the end of the year, Langley and Professor George Ellery Hale were corresponding about the development of a mountain solar observatory in the western United States[65] and Langley was

---

[63]Langley's assistant, Charles Greeley Abbot, suggests that Langley was reluctant to remeasure, or at least revise, his early value of the solar constant. It may be that Abbot, who had day to day charge of the Observatory, rather than Langley, instituted the new study. Charles Greeley Abbot, The sun and the welfare of man, 14.

[64]S.P. Langley to Hon. Charles D. Walcott, Secretary of the Carnegie Institution, 28 February 1902, quoted in "A sub-tropical solar physics observatory," Nature, 67 (1 January 1903): 207.

[65]"A sub-tropical solar physics observatory," 207 and George E. Hale, "The solar observatory of the Carnegie Institution of Washington," Astrophysical Journal 21 (1905): 155.

announcing the Smithsonian's plan to study the solar constant and how its measurement was affected by absorption within the sun's envelope and the earth's atmosphere. A focal point of this study would be twofold: searching for variations in the solar constant and determining its correlation with terrestrial meteorology.[66]

Around March 26, 1903, observers noted a rapid decline of approximately 10% in solar radiation measurements at the Smithsonian Astrophysical Observatory. While much of this decline was due to decreased transmission through the earth's atmosphere,[67] Langley publicly speculated about a correlation between these measurements and the relatively cool summer.[68] By mid-1904, Langley had found an average decline in the mean temperature recorded throughout the world to be about 2° C with the maximum decline being in those

---

[66]S.P. Langley, "The 'solar constant' and related problems," Astrophysical Journal 17 (1903): 98-99.

[67]C.G. Abbot, who had day to day charge of the Smithsonian Astrophysical Observatory at this time, and others subsequently found the most significant cause for the decline in the transmission of solar radiation through the earth's atmosphere to be the result of significant volcanic activity beginning in 1902. While Langley admitted the possibility, he would conclude only that nothing certain was known. C.G. Abbot, "Recent studies on the solar constant of radiation," Monthly Weather Review 31 (December 1903): 590; Henry J. Cox, "Recent studies of the solar constant," Popular Astronomy 13 (1906): 148; S.P. Langley, "On a possible variation of the solar radiation and its probable effect on terrestrial temperatures," Astrophysical Journal 19 (1904): 312-314.

[68]S.P. Langley, "Variation of atmospheric absorption," Nature 69 (5 November 1903): 5.

regions removed from large bodies of water. Theoretically a 10% decline in the solar constant, he maintained, would have resulted in a reduction in mean temperature of no more than 7.5° C.

Having found a correlation between temperature and the solar radiation measured at the earth's surface, Langley was determined to find a source for at least some of the reduction in the sun itself. He noted that there had been an increase in sun spot activity beginning on March 21.[69] In mid-1904 he announced in Nature a study of the absorption of solar radiation in the sun's envelope independent of changes in the earth's atmosphere and that he had found that the absorption had decreased in the previous six months. Studies of solar radiation through the earth's atmosphere also showed an increase in solar radiation during the same period.[70]

Langley's argument, that the decline in solar radiation received at the earth's surface in 1903 was due, in part, to a decline in solar radiation through the sun's envelope, appears not to have convinced many astrophysicists. They were convinced, however, of the need for further study of the solar constant. George Ellery Hale cooperated with Langley in developing the research program at the Mt. Wilson

---

[69]S.P. Langley, "On a possible variation of the solar radiation and its probable effect on terrestrial temperatures," 315.

[70]S.P. Langley, "Variations of atmospheric absorption," Nature 70 (30 June 1904): 198.

Solar Observatory and meteorologists would combine with some astrophysicists in one apparent answer to Langley's and others' calls for international cooperation in research on the solar constant and changes in the weather.[71]

Langley's death, on February 22, 1906, brought to an end his astrophysical labors before what he called the second stage--the interpretation of solar radiation--could get off the ground. Astrophysicists and others commemorating his career have stressed his role as an experimental astrophysicist and historians have subsequently largely ignored the theoretical issues and pragmatic philosophy which motivated his research.[72] Hale would continue Langley's solar research at Mt. Wilson, but papers coming from that

---

[71]S.P. Langley, "On a possible variation of the solar radiation and its probable effect on terrestrial temperatures," 320-321; "International co-operation in solar research," _Astrophysical Journal_ 20 (1904): 301-305; George E. Hale, "Co-operation in solar research," _Astrophysical Journal_ 20 (1904): 306-312; "Solar and terrestrial changes," _Nature_ 72 (3 August 1905): 332-333.

[72]J. Gordon Vaeth, _Langley: Man of science and flight_ (New York, Ronald Press Co., 1966); C.D. Walcott, "Biographical memoir of Samuel Pierpont Langley," _Biographical Memoirs of the National Academy of Sciences_ 7 (1917): 247-268; Bessie Zaban Jones, _Lighthouse of the skies, the Smithsonian Astrophysical Observatory: Background and history 1864-1955_ (Washington, Smithsonian Institution, 1965); Paul H. Oehser, "Samuel Pierpont Langley," in _Sons of science: The story of the Smithsonian Institution and its leaders_ (New York: H. Shuman, 1949), 110-140; Paul H. Oehser, "Samuel Pierpont Langley: Astrophysics and flying machines," in _The Smithsonian Institution_ (New York: Praeger, 1970), 49-53; and Charles Coulston Gillispie, ed., _Dictionary of scientific biography_ (New York: Charles Scribner's Sons, 1973), s.v. "Samuel Pierpont Langley," by Don F. Moyer.

institution generally ignored the implications of their research for meteorology.[73] The development, in 1904, of an international commission to study the relationship between solar radiation and terrestrial meteorology had included world renowned astrophysicists, such as Langley, Norman Lockyer, George Ellery Hale and Knut Ångström, but it found the most significant lacunae in research to be in meteorological fields.[74]

Within the United States, researchers at the Smithsonian Astrophysical Observatory continued their work on solar radiation and terrestrial meteorology well into the twentieth century, but they remained virtually alone among American astrophysical institutions in emphasizing the application of astrophysical research to terrestrial issues. Many of the papers resulting from research at the Smithsonian Astrophysical Observatory, published outside the Smithsonian, can be found in meteorological journals, rather than in

---

[73]A survey of the first ten volumes of Mount Wilson Observatory Contributions, published between February, 1905, and October, 1920, found no discussion of the meteorological implications of the solar physics research at the observatory.

[74]"International co-operation in solar research," 301-305; George E. Hale, "Co-operation in solar research," 306-312; "solar and terrestrial changes," Nature 72 (3 August 1905): 332-333.

astrophysical publications.[75] The connection between solar physics and terrestrial meteorology had been a driving force in the work of Langley and many of his assistants, but even before 1900 most leading American astrophysicists had abandoned the explicit enunciation of that association.

---

[75]C.G. Abbot, "The relation of the sunspot cycle to meteorology," Monthly Weather Review 30 (1902): 178-181; Frederick Eugene Fowle, "Atmospheric transparency for radiation," Monthly Weather Review 42 (1914): 2-4; C.G. Abbot, "Work of the Smithsonian Astrophysical Observatory at Caloma, Chile," Monthly Weather Review 47 (1919): 1-3; "Measurements of the solar constant of radiation," Monthly Weather Review 47 (1919): 85-87; and "Discrepancies between Ångström and Smithsonian instruments," Monthly Weather Review 48 (1920): 147-149.

CHAPTER 7

THE ESTABLISHMENT OF ASTROPHYSICS: JAMES EDWARD KEELER'S SPEECH AT THE DEDICATION OF THE YERKES ASTROPHYSICAL OBSERVATORY

Astrophysics as a scientific field appears to have been well established prior to the opening of the Yerkes Observatory in 1897. The astrophysical research at Harvard College Observatory funded by the Draper Memorial had been progressing for over ten years and the Smithsonian Astrophysical Observatory had been operating for nearly seven. James Edward Keeler and W.W. Campbell had carried on significant research programs at the Lick Observatory in California and George Ellery Hale had made a name for himself with his private telescope in Chicago. The Astrophysical Journal, published since 1894, was beginning to provide some measure of standardization for astrophysical measurements.[1]

For many, if not most people who thought about it, astrophysics continued to be closely connected with both the nebular hypothesis and meteorological issues associated with solar heat. Such scientifically acceptable "speculation"

---

[1]While the standards for printing maps of the spectra set by the editorial staff of the Astrophysical Journal when the publication began in 1895 did not meet with immediate universal approval, it did provide the foundation for subsequent debates about such standards. "On the mode of printing tables of wavelengths," Astrophysical Journal 4 (1896): 306-308; "On the mode of printing maps of spectra," Astrophysical Journal 5 (1897): 216-217 and "On the mode of printing maps of spectra and tables of wave-lengths," Astrophysical Journal 6 (1897): 55-56 & 155-146.

into the physical condition of the sun and stars remained the domain of diverse groups of researchers. Astrophysicists, seeking to define their field, found it increasingly important to differentiate themselves from others utilizing research on the sun and stars. How did an astrophysicist differ from a geologist or astronomer theorizing about the formation of the earth and solar system? What separated him from the meteorologist developing models about the effects of the sun on rainfall? In giving meaning to his studies, where did he diverge from the philosopher-theologian drawing on scientific research to speculate about the relationship between the mind of God and the universe? If astrophysics was to be a separate field, the astrophysicist's work would have to be different from the overlapping fields, out of which his field had been born.

The distinction between the new astronomy and the old and the diverse group of physicists, geologists, meteorologists and astronomers who claimed to be practitioners of the new astronomy clouded any definition of the new astronomy and, at least for some, the standards of scholarship acceptable in the new field. For many the "new astronomy" appeared to be an extension of the "new physics." John Trowbridge's 1884 college preparatory textbook by that title had focused on the role of the conservation of energy as one of the principle laws of physics.[2] This theme fitted

---

[2]John Trowbridge, <u>The new physics. A manual of experimental study for high schools and preparatory schools for college</u> (New York: D. Appleton & Co. 1884).

well with those who saw the study of the interrelationship between solar heat and terrestrial meteorology as a part of the new field. But it also became the theme of much of the popular speculative writings. The absence of clear cut standards of scholarship led amateurs and popularizers to stake claims to the new field. As one amateur "new astronomer," Rev. James W. Hanna, brashly put it in justifying his article in the Kansas City Review of Science and Industry:

> We hear of the 'New Astronomy' on every breeze. Can we have a finger in the matter? The advice of Mrs. Jack Means is "While you're gitten, git a plenty." The builders of the new had better gather it all in. So here is our offering."[3]

Many astronomers were concerned that there were too many unqualified "fingers" in the "new astronomy" pie. The public contributions to the "new astronomy" of the untrained, self-trained and poorly trained would invariably lead to misunderstandings which professional scientists would have to correct. It was not just the Vennors and the Wiggins, referring vaguely to astronomy and astrophysics in their predictions about the weather, but also the amateur scientists, whose training would allow them to blend astronomical fact with mathematical and physical fiction, who would have to be countered. In many cases, scientists warned, no one but the specialist would be able to separate

[3]Rev. James W. Hanna, "Solar dynamics -- Some new astronomy," Kansas City Review of Science and Industry 8 (October 1884): 308.

the mixture of astronomical truth from pseudo science. Even astronomers with little or no training in physics could potentially misunderstand or misconstrue the findings of the new astronomy.

H.S. Pritchett, director of the Morrison Observatory pointed out the problem in an 1884 review of Thomas Bassnett's <u>true theory of the sun . . .</u>:

> It is somewhat interesting to note the number of persons in the United States who feel competent to give the "only true" theory of the physical universe. . . . [T]hese theories, often contradict the best known laws of mathematics and physics, and go directly against the teaching and experience of the Masters in Science. In America there is perhaps, a larger class of men who feel competent to propound a theory of the universe, who are ignorant of the principles of mathematics and physics necessary for such discussions, then elsewhere.[4]

H.S.S. Smith, professor of astronomy and physics at the University of Kansas, complained that the attempts to popularize the new astronomy had led to

> A class of writers who, without the training or knowledge necessary to the task, aim to produce explanations and theories in provinces, where, as it might almost be said, "angels fear to tread."[5]

In reviewing one such book, Smith went on to allege that the "departure from accuracy of statement is frequently so slight that only one conversant with the subject would notice the

---

[4]H.S. P[ritchett], Review of <u>The true theory of the sun, showing the common origin of the solar spots and corona, and of atmospheric storms and cyclones</u>, by Thomas Bassnett, <u>Kansas City Review of Science and Industry</u> 8 (July 1884): 142-143.

[5]H.S.S. S[mith], Review of <u>The sun: Its constitution; its phenomena; its condition</u>, by Nathan T. Carr, <u>Kansas City Review of Science and Industry</u> 7 (December 1883): 504.

delinquency." As a result, those not "familiar with the minutiae of solar research" would be bound to receive "hurtful impressions."[6] Such works by unqualified scholars, Pritchett argued, would be useless to the student and "valuable only as a curiosity" to the scientific man.[7]

The problems which Smith and Pritchett raised only increased over the next ten years as numerous works on solar physics and the "new astronomy" were written by amateur scientists and popularizers trying to resolve all the anomalies of the nebular hypothesis or the connection between solar heat and terrestrial meteorology.

The popular press did little to alleviate the concerns of astrophysicists about their field of study. As early as 1881 the New York Tribune had editorialized that the new astronomy, with its theories about the relation between the planets, sun spots and terrestrial phenomena, was bringing the principles of astrology under a scientific domain.[8] While few popular articles focused on this aspect of the new science, astrophysicists were, no doubt, concerned not only with the validity of pronouncements being made in the name of the new astronomy, but with the breadth of research which was popularly perceived to be a part of the field. If the "new

---

[6]Ibid.

[7]H.S. P[ritchett], review of The true theory of the sun, by Thomas Bassnett, 143.

[8]"Astrology and astronomy," New York Daily Tribune, 17 April 1881, 9.

astronomy" was to be allied with the "new physics" then some means would have to be taken to exclude the numerous popular treatises announcing "the only true theory of the physical universe" which explained heavenly phenomena based upon some interpretation of the conservation of energy.[9]

John Hume Kedzie's book may be taken as a case in point. Kedzie, a Chicago "educationist" who had served at least one session in the Illinois legislature[10], published Speculations. Solar heat, gravitation and sun spots to reconcile the apparent limitations on the life of the sun with the presumed age of the universe. He saw the solar system as a form of perpetual motion machine operating in strict conformance with the conservation of energy in a closed system. According to Kedzie, the sun emitted energy in the form of heat and light waves. As these waves of heat and light left the region of the sun the wavelengths were imperceptibly transformed until they became gravitational waves which travelled toward the sun. At the photosphere these gravitational waves came in contact with carbon like substances. Just as electricity and carbon resulted in the

---

[9]J[ohn] H[ume] Kedzie, Speculations. Solar heat, gravitation and sunspots (Chicago, S.C. Griggs & Co., 1886); Rev. James W. Hanna, Revised astronomy; or theoretical astronomy from a new base (Chicago: Fleming, c1891); William M[cKendree] Bryant, The world-energy and its self-conservation (Chicago: S.C. Griggs & Co., 1890) and I.W. Heysinger, The source and mode of solar energy (Philadelphia: J.B. Lippincott Co., 1895).

[10]W. Stewart Wallace, ed., Dictionary of North American authors (n.p.: Gale Research Co., 1968), s.v. "John Hume Kedzie.

arc light so gravity waves and carbon in the sun resulted in the transformation back into waves of heat and light.[11]

Speculations. Solar heat, gravitation and sun spots was precisely the sort of popular publication which H.S.S. Smith had warned about. Kedzie portrayed himself not as a scientist but as a layman. He described his work as "not in the least technical" and easily "read in a day or less." Yet reviewers who appeared to have little or no scientific training often described Kedzie's theory, as did the New York Times review, as "ingenious" and "complex" and "by no means to be readily understood."[12]

Kedzie clearly sought to have his work judged by the scientific community. He sent a copy to some fellows of the American Association for the Advancement of Science requesting their examination and opinions. Whatever their actual reception, Kedzie reported that they had "been received with such unexpected, if not undeserved, favor" that he was sending copies to all of the fellows "on the same terms."[13]

---

[11]Kedzie, Speculations. Solar heat, gravitation and sunspots.

[12]Review of Speculations. Solar heat, gravitation, and sunspots, by J.H. Kedzie, In New York Times, 5 July 1886, 3.

[13]J.H. Kedzie to Secretary, Smithsonian Institution, 5 February 1887, Incoming Correspondence, Box 7, Folder 7, p. 159, Record Unit 30, Office of the Secretary, 1882-1890, Smithsonian Institution Archives, Washington, D.C. Kedzie noted that the book could be read "in a day or less." However Samuel Pierpont Langley noted at the bottom of the letter that he could not advise Prof. Baird to devote "'a day or less' to reading this book."

The few "scientists" who bothered to publicly review Kedzie's work as often as not gave the opinion that Kedzie's errors were ones to be expected in the field of astronomy. Edwin S. Crawley, a mathematician at the University of Pennsylvania, for instance, pointed out that Kedzie's theory directly conflicted with the theory of gravitation. It implied that gravity was based upon the absorption of gravitational waves and hence the force of gravity would be proportional to surface area rather than the mass of astronomical bodies.[14]

In spite of his critique of Kedzie's theory, Crawley praised Speculations. Solar Heat, Gravity and sun spots for "having presented a clear view of the nature of the problem to be solved." "This," he continued,

> is always an essential introduction to the right understanding and correct solution of every problem, yet in how many cases do men of science, men who ought to know better, go to work without having taken properly this preliminary step. Particularly is this the case in a field like that of astronomy where it is almost impossible to restrain the imagination from soaring far beyond the legitimate pale of fact and where it is so much more difficult to prove or to refute the results of even fanciful speculation because it is impossible to reproduce the conditions in terrestrial experimentation.[15]

Even a few astronomers found positive insights in Kedzie's book. The Irish astronomer, W.H.S. Monck, went so far as to argue that the essence of Kedzie's theory could be

---

[14]Edwin S. Crawley, "Criticism of a new theory of solar heat and gravitation," Sidereal Messenger 7 (1888): 328-329.

[15]Ibid., 333.

salvaged if the energy which returned to the sun was not denoted as gravity.[16]

The popular and religious press was even more supportive of Kedzie's theory. Professor William G. Frost of Oberlin College proclaimed that:

> "The revolution in the popular conceptions of the universe which Kedzie's hypothesis is destined to produce, is almost inconceivable. . . . His teaching is sustained by many a suggestive quotation from great scientists, including Newton himself, who seem to have wandered all around this great discovery without exactly hitting it."[17]

Similar supportive declarations were reprinted from numerous religious and popular papers such as Bibliotheca Sacra, Lutheran Quarterly, the Chicago Tribune, and the Boston Sunday Times. The Philadelphia Times went so far as to proclaim that "Mr. Kedzie's theory is the only one so far advanced that seems at all satisfactory."[18]

That speculations such as Kedzie presented in his work could go through a second edition by 1891 and receive

---

[16]W.H.S. Monck, "Mr. Kedzie's theory of solar heat," Sidereal Messenger 7 (1888): 440-442.

[17]Advertising sheet for Solar heat, gravitation, and sunspots in Incoming Correspondence, Box 7, Folder 7, p. 160, Record Unit 30, Office of the Secretary, 1882-1890, Smithsonian Institution Archives, Washington, D.C. William G. Frost, Professor of Greek and Theology at Oberlin College, subsequently became President of Berea College in Kentucky. He had no formal training in science and is best known, perhaps for his role in the abolitionist movement and subsequently in favor of black civil rights. His interest in science was primarily from the perspective of reconciling science and theology. William G. Frost, For the mountains. An autobiography (New York: Fleming H. Revell Co., 1937), 44.

[18]Ibid.

such widespread popular support even among "scientists" not trained in physics and astronomy was undoubtedly cause for concern. When the Smithsonian Institution received a complementary copy of Kedzie's work with a request for an opinion on it, S.P. Langley told Secretary Baird the book was not worth his time. No doubt, he expressed similar opinions to his former assistant James Edward Keeler if they discussed the matter. Rather than critique Kedzie's book Langley forwarded a copy of Professor F.H. Bigelow's article on the solar corona[19] which, if accepted by Kedzie, should have corrected at least some of his misconceptions about the sun. Yet Kedzie eagerly wrote back disputing Bigelow's description of the make up of the corona and offering to provide Bigelow with a copy of his book on solar heat.[20]

Other writers were equally problematic in their use of the "principles" of physics. I.W. Heysinger, apparently a Philadelphia homeopath, argued that the true source of solar energy was in the "potential energy" of space, rather than the sun. All planets, he argued, had an opposite electrical polarity from the sun and acted as induction machines generating electricity from the attenuated aqueous vapor of space through their rotation on their axis. This electricity

---

[19]Presumably Frank Hagar Bigelow, The solar corona discussed by spherical harmonics (Washington DC: Smithsonian Institution, 1889).

[20]J.H. Kedzie to Prof. S.P. Langley, 16 November 1889, Incoming Correspondence, Box 7, Folder 7, p. 161, Record Unit 30, Office of the Secretary, 1882-1890, Smithsonian Institution Archives, Washington, D.C.

flowed in a constant current to the sun where it was transformed into solar light and heat.[21]

Heysinger's work, which went through two editions, pointedly sought to show how his interpretation of the new astronomy fit with a biblical cosmology. Both Heysinger's and Kedzie's theories eliminated the nebular hypothesis. Their comparatively wide acceptance within the religious press may reflect a growing tension between the main stream of astrophysics, which accepted the nebular hypothesis, and the more conservative elements of the religious community for which the heavens still declared the glory of God. The fact that they cited the authorities of the new astronomy in support of their theories must have been problematic for Langley, Keeler and others who were trying to define the new field of astrophysics.

Kedzie, Heysinger and others who used conservation of energy and concepts of electromagnetism to explain their "true theories of the heavens" clearly identified their works as a part of the "new astronomy" as opposed to the "old astronomy" and they, like Reverend Hanna, were determined to have their "finger in the pie." These speculative works clearly presented a problem to astrophysicists trying to define a new field of research which had arisen in response to debates about the nebular hypothesis and speculations

---

[21] I.W. Heysinger, The source and mode of solar energy (Philadelphia: J.B. Lippincott Co., 1895).

regarding the relationship between solar heat and terrestrial meteorology.

In addition to problems from speculators, quacks and popularizers utilizing the new field of astrophysics as a forum to express their own theories, astrophysicists had to be concerned about the rise of public expectations for results promised for meteorology and weather prediction. In 1884, Samuel Pierpont Langley had defined the field as the study of the sun, moon, and stars "for what they are in themselves, and in relation to ourselves." The field had begun, he wrote, with the study of the external features of the sun. This had led to the discovery of the effects of the sun on the daily life of earth's inhabitants. These discoveries were bringing "results of the most practical and important kind, which a generation ago were unguessed at."[22]

Langley wrote his definition of the new field as part of a work pleading for increased support for astrophysical research.[23] The promise of practical benefits had not been fulfilled by the mid-1890s and many astrophysicists, including Charles Augustus Young, were warning that any connection between sun spots and terrestrial meteorology was at best

---

[22]Samuel Pierpont Langley, The new astronomy (Boston: Houghton Mifflin & Co., 1891), 3-4. Cf. Samuel P. Langley, "The new astronomy," Century Magazine 28 (1884): 712-726, 922-936; 29 (1884): 224-241, 700-721 & 33 (1887): 339-355, 586-598.

[23]Samuel Pierpont Langley, The new astronomy, [iii].

immeasurable.[24] The claims for the practical application of the new science, while still advocated by Langley's assistants at the Smithsonian Observatory[25], had not panned out and most astrophysicists appeared reluctant to support those claims. Some, no doubt, were concerned that such claims might bring a backlash from supporters disenchanted with the failure of astrophysicists to deliver the practical results long claimed for the new astronomy.

---

[24]Charles A. Young, Elements of astronomy, rev. ed. (Boston: Ginn & Co., 1897), 132 and C.A. Young, The sun, new & rev. ed. (New York: D. Appleton & Co., 1898), 177.

[25]Charles Greeley Abbot, Langley's successor as director of the Smithsonian Astrophysical Observatory continued research on the relationship between solar radiation and terrestrial meteorology through the 1950s. However, his research was generally considered to be a branch of meteorology rather than astrophysics. C.G. Abbot, "Measurement of the solar constant of Radiation," Monthly Weather Review 47 (1919): 85-87; C.G. Abbot, The dependence of terrestrial temperatures on the variations of the sun's radiation (Washington, DC: Smithsonian Institution, 1936); C.G. Abbot, Further evidence on the dependence of terrestrial temperatures on the variations of the sun's radiation (Washington, DC: Smithsonian Institution, 1936); C.G. Abbot, Important interferences with normals in weather records associated with sunspot frequency (Washington, DC: Smithsonian Institution, 1952); C.G. Abbot, Sixty-year weather forecasts, Smithsonian Miscellaneous Collections, vol. 128, no. 3 (Washington, DC: Smithsonian Institution, 1955); C.G. Abbot, Periodic solar variation, Smithsonian Miscellaneous Collections, vol. 128, no. 4 (Washington, DC: Smithsonian Institution, 1955).

Frank Washington Very, another of Langley's assistants at the Allegheny Observatory, also continued research in a similar vein before drifting into attempts to scientifically justify Swedenborgianism. Frank W. Very, "On the need of adjustment of the data of terrestrial meteorology and of solar radiation, and on the best value of the solar constant," Astrophysical Journal 34 (1911): 371-387, and Frank W. Very, An epitome of Swedenborg's science (Boston: The Four Seas Co., 1927).

Finally, the name, the "new astronomy," often used for astrophysics created some tension in that it implied, at least to some, that traditional astronomical programs were out of date and no longer useful. Edward Singleton Holden, the director of the Lick Observatory, took pains to avoid this, devoting an article in Century Magazine to showing that "the old astronomy is not idle; that it has its new side; and that its energies are addressed to the solution of tremendous problems of the highest significance." The role of the new astronomy, Holden argued, was "to trace the life-history of an individual star," while that of the old was to "show how all these single stars are bound together to make a universe."[26] Yet even Holden would profess on occasion that advocates of the old astronomy too often saw their role as looking at the heavens and discovering new stars.[27]

Some self-trained astronomers, such as Edgar L. Larkin, director of the Windsor Observatory, had gained publicly respected positions. Many of their publications continued traditional astronomy's pronouncement that the heavens declared the glory of God and an implied alliance between astrophysics and theological orthodoxy strengthened by the debates over the astrophysical versus the geological

---

[26]Edward S. Holden, "Sidereal astronomy. Old and new," Century 14 (1886): 788.

[27]Edward S. Holden, Handbook of the Lick Observatory (Mt. Hamilton: Lick Observatory, 1888, 38 and Agnes Marie Clark, review of Handbook of the Lick Observatory by Edward S. Holden, Nature 38 (30 August 1888), 410.

determinations of the age of the earth. On the other hand, support for Kedzie, Hanna and others by members of the religious community created tensions for professional astrophysicists who sought to shield their research from theological debate.

Furthermore, the public could not easily distinguish the professional from the amateur John Hume Kedzie or the astronomer Edgar L. Larkin from trained astrophysicists, such as James Keeler or George Ellery Hale, merely by reading the popular and semi-popular treatises on the "new astronomy" which were overrunning the presses.

By 1897 it was apparent that many self-proclaimed "laymen," who had general notions of the principles of the new astronomy, considered themselves and were considered by the public to be on nearly equal, if not equal, ground with trained physicists and astronomers.[28]

The problem was not that books like Kedzie's, Larkin's or Hanna's were being published, but that the public

---

[28]As late as 1911, Edgar L. Larkin's interpretation of the Orion Nebula, for instance, appears to have influenced Seventh-Day Adventist interpretations of their "prophet's," Ellen G. White, vision's regarding eschatology and the second coming. Although by that time, Larkin was the director of the Mt. Lowe Railway Observatory, an observatory solely built for public use, Adventists either did not compare his writings with more main stream scientists or did not differentiate the validity of his views from professional astrophysicists. Cf. Merton E. Sprengel and Dowell E. Martz, "Orion revisited," Review and Herald, 25 March 1976, 4-7; 1 April 1976, 9-11; and 8 April 1976, 6-8; and Lucas Reed, "The open space in Orion," in Astronomy and the Bible (Mountain View, CA: Pacific Press Publishing Co., 1919), 236-257.

could not distinguish between those representing the valid research of scientists, like Langley and Abbot, and those resulting from the speculations and hopes of scientists, visionaries, theologians and quacks, who sought to explain anomalies, justify theologies or provide the scientific equivalent of patent medicines to farmers seeking methods to control the weather.

That all of these groups found resources in the new astronomy was problematic for those seeking to develop astrophysics as a scientific specialty. Would the failure of researchers at the Smithsonian Astrophysical Observatory to develop long-range weather prediction be the fault of astrophysics, or of an understanding of meteorology? Equally important were the problems raised by the close connection in many minds between religion and astronomy. Should astrophysicists be responsible for reconciling the changes in their understanding of the heavens with the discrepancies these would raise about the nature and role of God in the universe?

For many physicists involved in astronomical research, James Peirce's "ideas in the mind of God" pushed astrophysics too close to theology. Many were undoubtedly concerned that if astrophysics staked a claim in the science versus religion debates, it might lose the popular support it had engendered. On the other hand, were the debate solely between religious communities, then astrophysics could remain outside the discussion.

The nature of the root questions about the nebular hypothesis and about the relationship between solar heat and terrestrial meteorology, which had given rise to astrophysical research, now made it increasingly difficult to distinguish astrophysical research from philosophical speculation and, in the case of weather prediction, from chicanery.

The dedication of the Yerkes Observatory in 1897 provided an opportunity to finally define for the American public what astrophysics was and what it was not. The Yerkes Observatory garnered widespread public attention in housing the world's largest telescope, but it was also the first major American observatory specifically designed for astrophysical research.

In his dedicatory speech, James Edward Keeler, Langley's successor as director of the Allegheny Observatory, addressed the problem of defining astrophysics. Astrophysics, he admitted, was a new field created out of the overlapping fields of astronomy, physics and chemistry. Its recent development into a professional science had occurred so recently, he argued, that even the name "astrophysics" could only be found in the very latest dictionaries.[29]

Keeler's speech can be seen as the announcement of the birth of a new scientific field of research. It provided a clear definition of what was astrophysical research and

---

[29]James E. Keeler, "The importance of astrophysical research and the relation of astrophysics to other physical sciences," Astrophysical Journal 6 (November 1897): 271-272.

what was chaff which had attached itself to astrophysics. At the same time, he removed the science of astrophysics from the cultural context of its birth. As assistant at the Allegheny Observatory under Langley's directorship, Keeler must have been intimately aware of the relationship between Langley's work and hoped for improvements in long-range weather forecasting. However, he separated astrophysics from any hoped for public benefits:

> The study of astrophysics does not at present seem to have a very direct bearing on the practical affairs of everyday life.[30]

Even the potential of utilizing the sun as a source of mechanical power, he believed, would more likely come from the application of present knowledge than from the discoveries of astrophysics. Keeler clearly sought to remove astrophysics from any potential disrepute which might come from faddist weather prophets basing their predictions on purported astrophysical principles.

In America he was largely successful. By 1903 an American commenting on the state of astrophysics would attest that:

> It is an amazing thing that the enormous utility of recent work on the sun's connection with the conditions which bring famine or plenty to India, for instance, is lost sight of by almost all astronomers. Astronomers and astrophysicists, even, are apt to look at it in its purely scientific interest, as if it had none other than what it might share with the discovery of the motion of a

---

[30]Ibid, 273.

nebula.[31]

The dramatic shift from the emphasis on the pragmatic application of astrophysical research, held by many astronomers and physicists in the late nineteenth century, to the sublimation of that aspect of their science by many of the same astrophysicists by the end of the century might be viewed as one aspect of the professionalization of astrophysics. As a profession, astrophysics had to be removed from those aspects of every day life which had earlier excited so much popular interest.

But Keeler went even further. The importance of astrophysics lay not in its practicality, but in its role in science. Even if practical applications could be found it would provide no more a justification for astrophysics than the applications to surveying, time standards and navigation provided for the old astronomy. The justification for astrophysics, as well as astronomy, was that "they enable us better to understand the universe of which we form a part, and that they elevate the thoughts and ennoble the minds of men."[32]

Keeler agreed that others might have different views as to what was of value in the new science. But he opined that the important advances in astrophysics were the result

---

[31]"A sub-tropical solar physics observatory," Nature 67 (1 January 1903): 207.

[32]James E. Keeler, "The importance of astrophysical research and the relation of astrophysics to other physical sciences," 273.

of the introduction of photography to astrophysical research and, of course, the results derived from the use of the spectroscope. Photography gave a permanent record of an observation. It also increased the light-gathering capability by building up light emissions over time. From this record Keeler argued, astrophysicists had been able to show that bright stars, faint stars, and nebulosity were physically related.

The spectroscope was equally important. Its use as a precision instrument had resulted from new designs resulting from Lord Rayleigh's and Professor Wadsworth's work on resolving power.[33] But its importance lay in the information which the astrophysicist could provide to allied sciences. The spectroscope provided accurate information regarding motion of heavenly bodies in the line of sight. It had been useful in helping physicists determine the speed of light. Spectroscopy also provided chemists with additional information. Astrophysicists had first discovered helium in the sun. It had subsequently been detected in stars and nebulae and had finally been "run to earth."[34] Even more important, because the spectrum of some elements changed under different conditions of temperature and pressure, the physicist and chemist could study those changes in their laboratories and the results could be used to provide information regarding

---

[33]Ibid., 279.

[34]Ibid., 281.

the temperature and pressure of stars. On the other hand, because the conditions on many stars were not replicable on earth, the astrophysicist could provide physicists and chemists with a means of extending their terrestrial laboratory experiments.[35]

Keeler's views regarding the importance of astrophysics ignored, or at least did not focus on, the questions which influenced the rise of astrophysical research. He was obviously concerned with separating astrophysics from popular notions correlating solar physics with the future of long-range weather forecasting. He was equally concerned with separating the new science from the theological and intrascientific debates over the age and origin of the universe. No doubt he was familiar with the recent resurgence, in 1893, of that debate which drew on the conclusions of numerous American geologists and the American Astronomers, Simon Newcomb and S.P. Langley.[36] The divisiveness of that debate between Huxley and Lord Kelvin in Europe undoubtedly encouraged an attempt to remove similar concerns from the realm of American astrophysical research. His view of the importance of astrophysics reflected a perspective

---

[35]Ibid.

[36]Joe Burchfield, Lord Kelvin and the age of the Earth (New York: Science History Publications, 1975), 107; George P. Merrill, The first one hundred years of American geology (New Haven: Yale University Press, 1924), 648-662; Clarence King, "The age of the Earth," American Journal of Science 3d ser., 45 (1893): 1-20; and Simon Newcomb, Popular astronomy (New York: Harper, 1878), 505-511.

which focused on the internal development of science. A science which did not reflect the cultural environment in which it took place could effectively separate itself from the analogous pseudo-scientific ideas which arose in the same environment.

But some explanation for the beginnings of astrophysics had to be given. Since astrophysics was providing answers to issues rising out of its sister sciences of astronomy, physics and chemistry, its importance lay in the mechanism for providing astrophysics' unique answers. In focusing on astrophysics' unique mechanisms, Keeler implicitly found a beginning for astrophysics in its instrumentation--a view which most subsequent writers have followed.

Conclusion

Through out much of the nineteenth century, the nebular hypothesis played an important role in American thought. Ideas associated with the nebular hypothesis found expression in the publications of American writers, such as Ralph Waldo Emerson and Edgar Allen Poe. Those ideas also became the focal point of philosophers and religious thinkers, such as Tayler Lewis and Laurens Hickok. The broader issues relating the nebular hypothesis to religious and philosophical thought found expression in and provided motivation for the early scientific concerns of Stephen Alexander, Benjamin Peirce and others. The concerns about the validity of the nebular hypothesis among astronomers who influenced the development of research at the Harvard College Observatory reflected the broader interests of the culture and encouraged the development of the Henry Draper Memorial research program.

During the second half of the nineteenth century, pragmatic speculations regarding the relationship between solar radiation and terrestrial meteorology became widespread. Both land speculators and their opponents used arguments derived from these speculations to justify their positions on the settlement of the western plains. Weather prophets utilized the new theories to give respectability to their forecasts and meteorologists called on further research to improve their arguments against the widely followed prophecies. Some scientists turned to solar physics to provide answers to the relationship between weather and extraterrestrial phenomena. Astrophysicists, such as Samuel Pierpont Langley, put forward the promise of practical results to encourage support for solar physics which ultimately led to the development of the Smithsonian Astrophysical Observatory.

James Edward Keeler's focus on instrumentation in portraying the roots of American astrophysics reflects the culmination of a shift in perspective. He found protection for the reputation of astrophysical research in the internal development of the science separated from the cultural factors which he felt threatened the legitimacy of his field.

The cultural interest in the nebular hypothesis, as well as the hoped for ability to predict, if not control, the weather, were, no doubt, as extraneous to astrophysics as the ideas of extraterrestrial life then being encouraged by research at the Lowell Observatory. Such ideas, Keeler declared, were not the responsibility of science. Astrophysics would not long suffer from "outside misapprehension," Keeler explained. Sustained effort in the real field of astrophysics, would be "sufficient safeguard against the intrusion of triflers into its workshops."[37]

Keeler would also invert the relationship between the popular interest in astrophysical research and the rise of the science. Astrophysics had not developed because of popular support. Rather, interest in astrophysics was derived from an intellectual excitement which the new field had created. Keeler's explanation drew analogy from popular interest in art. Astrophysics, he claimed, had painted a picture of the stars "in the brigher colors" while the old astronomy, with its dull focus on the measurement of celes-

---

[37]Keeler, "The importance of astrophysical research and the relation of astrophysics to other physical sciences," 276. Keeler's viewpoint on the issue of extraterrestrial life is somewhat surprising and apparently reflects Keeler's matured opinion. He clearly did not hold research on extraterrestrial life with such disdain in his earlier work. In an 1882 interview, reported in the New York Times, Keeler expressed the opinion that the importance of C.A. Young's spectroscopic studies showing evidence of water on Venus lay in the fact that they provided support for the potential of life on that planet. In fact Keeler had asserted "No astronomer would care to say that Venus is not inhabited." New York Times, 9 December 1882, 1.

tial motions, had garnered "little enthusiasm in the popular mind."[38] Popular interest in the new astronomy not only did not arise from a hoped for practical application but from its appeal to the popular imagination. The new astronomy portrayed stars not just as moving dots on the heavenly canopy but as suns "pouring out floods of light and heat . . . , torn by conflicting currents and fiery eruptions, shrouded in absorbing vapors or perhaps in vast masses of flame."[39] The intellectual excitement of the new astrophysical discoveries resulting from the invention of the spectroscope, Keeler declared, had inflamed the popular imagination and garnered support for the new science, allowing the astrophysicist to cultivate his field of research.

Astrophysics, Keeler claimed, painted these new and vivid colors through its use of instrumentation unavailable to the old astronomy. Implicitly, then, astrophysics did not arise out of a cultural interest in a set of problems which astrophysics might answer. Rather, astrophysics was born of the spectroscope and the development of astronomical photography. Popular interest, in Keeler's reconstruction of the development of astrophysics, was the result of, not a factor in, the development of astrophysical research.

---

[38]Keeler, "The importance of astrophysical research and the relation of astrophysics to other physical sciences," 275.

[39]Ibid.

In Keeler's mind, it was not cultural interest in astrophysical questions which gave rise to astrophysical research. Yet astrophysical research as a "pure science" would be possible only in a culture where such interest developed.[40] This Keeler declared was both a boon and a bain. It was a boon in that the awakening of popular interest provided support for astrophysical research. It was a bain in that the public could not really understand the significance of the most important astrophysical discoveries. It was the responsibility, therefore, of the astrophysicist, not to explain these discoveries, but to express public admiration for them. In a sense, the professional astrophysicist was to be a priest for the science. He would do the research, make the discoveries and then tell the public which "discoveries" were worthy of admiration.

Keeler's view of astrophysics focused on the results of astrophysical research and on the instrumentation which gave rise to those results. It specifically ignored the broader root questions which had encouraged the development and use of that instrumentation. Subsequent writers have largely followed Keeler's analysis in describing the beginnings of American astrophysics. In so doing, they have removed the development of the science from its cultural context. There can be no question that the development of instrumentation during the second half of the nineteenth

---

[40]Ibid.

century provided an important part in the story of the beginnings of astrophysics. That story has been at least partially told elsewhere.[41]

True astrophysics could not have developed in the way that it did without the spectroscope and the development of astronomical photography. But Keeler's explanation, subsequently adopted by historians addressing the beginning of astrophysics, provided only half the story. Astrophysical research also was the result of a desire to find answers to a group of questions. Those questions had a cultural context. One group of questions, arising out of an intense debate throughout the nineteenth century over the nebular hypothesis and appropriate mechanisms for proving or disproving an astronomical theory, had profound implications for religion and could possibly destroy the perception of astronomy as a safe haven for the devout. The second group of questions, equally profound, arose from a desire to develop some mechanism for control or prediction of long-range weather patterns. Such prediction would have been a boon to western farmers. Without placing the beginnings of American astrophysics in its cultural context, without recognizing the

---

[41]A.J. Meadows, "The origins of astrophysics," in <u>Astrophysics and twentieth-century astronomy to 1950</u>, ed. Owen Gingerich, pt. A, vol. 4: <u>The general history of astronomy</u>, (Cambridge, Mass.: Cambridge University Press, 1984), 3-15; Dieter B. Herrmann, "The origins of astrophysics," in <u>The history of astronomy from Herschel to Hertzsprung</u> (Cambridge: Cambridge University Press, 1984), 69-112 and Giorgio Abetti, "The birth of astrophysics," in <u>The history of astronomy</u> (London: Sidgwick & Jackson, 1954), 181-206.

interest in a set of questions which astrophysicicists initially claimed to be able to answer, historians have provided only an incomplete interpretation of the birth of American astrophysics.

Both sets of questions, when inadequately or speculatively addressed, potentially threatened popular support for the fledgling science of astrophysics. It should not be surprising that Keeler sought to dislodge them from the framework of astrophysical research. However, the birth of American astrophysics can not be completely understood without placing it in the context of nineteenth-century philosophical and theological interest in the nebular hypothesis and the relationship between the sun and the weather.

BIBLIOGRAPHY

Abbe, Cleveland. "The meteorological work of the U.S. Signal Service, 1870-1891." U.S. Department of Agriculture, Weather Bureau, Bulletin 11 (1894): 282-283.

Abbot, C[harles] G[reeley]. The dependence of terrestrial temperatures on the variations of the sun's radiation. Washington, DC: Smithsonian Institution, 1936.

________. "Discrepancies between Ångström and Smithsonian instruments." Monthly Weather Review 48 (1920): 147-149.

________. Further evidence on the dependence of terrestrial temperatures on the variations of the sun's radiation. Washington, DC: Smithsonian Institution, 1936.

________. Important interferences with normals in weather records associated with sunspot frequency. Washington, DC: Smithsonian Institution, 1952.

________. "Measurements of the solar constant of radiation." Monthly Weather Review 47 (1919): 85-87.

________. Periodic solar variation. Smithsonian Miscellaneous Collections, vol. 128, no. 4. Washington, DC: Smithsonian Institution, 1955.

________. "The relation of the sunspot cycle to meteorology." Monthly Weather Review 30 (1902): 178-181.

________. "Recent Bolographic results from the Astrophysical Observatory at Washington." Astrophysical Journal 8 (1898): 250-252.

________. "Recent studies on the solar constant of radiation." Monthly Weather Review 31 (December 1903): 587-592.

________. Sixty-year weather forecasts. Smithsonian Miscellaneous Collections, vol. 128, no. 3. Washington, DC: Government Printing Office, 1955.

________. The sun and the welfare of man. Washington DC: Smithsonian Institution, 1929.

________. "Work of the Smithsonian Astrophysical Observatory at Caloma, Chile." Monthly Weather Review 47 (1919): 1-3.

Abbot, Francis Ellingwood. Review of The principles of biology, by Herbert Spencer. In North American Review 107 (1868): 377-422.

Abetti, Giorgio. "The birth of astrophysics." Chap. in The history of astronomy. London: Sidgwick & Jackson, 1954.

Alexander, Stephen. "Miscellaneous contributions to astronomical science." American Philosophical Society Proceedings 4 (1847):219-229.

________. "On the origin of the forms and the present condition of some of the clusters of stars and several of the nebulae." Astronomical Journal 2 (1852): 95-96, 97-103, 105-111, 113-115, 126-128, 140-142, 148-152, 158-160.

________. "On the origin of the forms and present state of some of the clusters of stars." In Proceedings of the American association for the advancement of science; Sixth meeting held at Albany (N.Y.), August 1851. Washington, DC: S.F. Baird, 1852, 128-129.

________. "On the physical phenomena which accompany solar eclipses." American Philosophical Society Proceedings (1843): 183-211.

________. Statement and exposition of certain harmonies of the solar system. Smithsonian Contributions to Knowledge, vol. 21, pt. 1. Washington, DC: Smithsonian Institution, 1875.

Alexander, Stephen and Joseph Henry. "Experiments relative to the spots on the sun." American Philosophical Society Proceedings 4 (1847): 173-176.

Archibald, E. Douglas. "Conservation of solar energy." Nature 25 (30 March 1882): 504.

"Astrology and astronomy." New York Daily Tribune, 17 April 1881, 9.

"Astronomy of Laplace." American Quarterly Review 7 (June 1830): 255-279.

"Astrophysical work at the Smithsonian Institution." Nature 71 (20 April 1905): 592.

Aughey, Samuel G. Sketches of the physical geography and geology of Nebraska. Omaha, NE: n.p., 1880.

Aughey, Samuel G. and Charles Dana Wilber. Agriculture beyond the 100th meridian or a review of the U.S. Public Land Commission. Lincoln, NE: University Press, 1880.

Bannister, Robert C. Social Darwinism: Science and myth in Anglo-American social thought Philadelphia: Temple University Press, 1979.

Bartky, Ian R. "Naval Observatory time dissemination before the wireless." In Sky with ocean joined: Proceedings of the sesquicentennial symposia of the U.S. Naval Observatory, December 5 and 8, 1980, Edited by Steven J. Dick and Leroy E. Doggett, 1-28. Washington, DC: U.S. Naval Observatory, 1983.

Benedict, W.R. "Theism and evolution." Andover Review 6 (1886): 336-350, 601-622.

Bigelow, Frank Hagar. The solar corona discussed by spherical harmonics. Washington, DC: Smithsonian Institution, 1889.

Blau, Joseph. "Laurens P. Hickok: The orthodoxy of reason." Chap. in Men and movements in American philosophy. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1952.

Bond, Frederick Drew. "Poe as an evolutionist." Popular Science Monthly 71 (1907): 267-274.

Bowden, Martin. "The great American desert and the American frontier 1800-1882: Popular images of the plains." In Anonymous Americans: Explorations in nineteenth century social history, ed. Tamera K. Hareven, 48-79. Englewood Cliffs, NJ: Prentice Hall, 1971.

________. "Desert wheat belt, plains corn belt. Environmental cognition and behavior of settlers in the plains margin, 1850-99." In Images of the Plains. The role of human nature in settlement, ed. Brian W. Blouet and Merlin P. Lawson, 189-201. Lincoln: University of Nebraska Press, 1975.

[Bowditch, Nathaniel]. "Modern astronomy." North American Review 20 (April 1825): 309-366.

Bryant, William M[cKendree]. The world-energy and its self-conservation. Chicago: S.C. Griggs & Co., 1890.

Burchfield, Joe D. Lord Kelvin and the age of the earth. New York: Science History Publications, 1975.

Cannon, Annie J. Spectra of bright southern stars photographed with the 13-inch Boyden telescope as a part of the Henry Draper Memorial. Annals of the Astronomical Observatory of Harvard College, vol. 28, pt. 2. Cambridge: Harvard College Observatory, 1901.

Cannon, Annie J. and Edward C. Pickering. The Henry Draper catalogue. Annals of the Astronomical Observatory of Harvard College, vols. 91-99. Cambridge, Harvard College Observatory, 1918-1924.

Cassirer, Ernst. Kant's life and thought. New Haven and London: Yale University Press, 1981.

Challis, James. "On the source and maintenance of the sun's heat." Philosophical Magazine 4th ser., 26 (1863): 460-467.

Chalmers, A. F. What is this thing called science? St. Lucia, Q. : University of Queensland Press, 1978.

Chalmers, Thomas. Discourses on the Christian revelation, viewed in connexion [sic] with the modern astronomy, 2 vols. Edinburgh: Thomas Constable & Co., 1854.

________. On the power, wisdom, and goodness of God as manifested in the adaption of external nature to the moral and intellectual constitution of man. London: William Pickering, 1835.

Chase, Pliny Earle. "On the nebular hypothesis." Philosophical Magazine 1 (1876): 315-319, 507-510; 2 (1876): 29-36, 198-202; 3 (1877): 203-211; 4 (1877): 291-298; 5 (1878): 292-297, 362-367; 6 (1878): 128-132 and 448-454.

Clark, Harry Hayden. "Emerson and science." Philological Quarterly 10 (July 1931): 225-260.

Conner, Frederick William. "Emerson: The beginnings." Chap. in Cosmic optimism: A study of the interpretation of evolution by American poets from Emerson to Robinson. Gainesville: University of Florida Press, 1949.

________. "Poe & John Nichol. Notes on a source of Eureka." In All these to teach: Essays in honor of C.A. Robertson, ed. Robert A. Bryan et. al., 190-208. Gainesville: University of Florida Press, 1965.

Corwin, Edward S. "The impact of the idea of evolution on the American political and constitutional tradition." In Evolutionary thought in America, ed. Stow Persons, 240-265. New Haven: Yale University Press, 1950

Cox, Henry J. "Recent studies of the solar constant." Popular Astronomy 13 (1906): 147-149.

Crawley, Edwin S. "Criticism of a new theory of solar heat and gravitation." Sidereal Messenger 7 (1888): 328-329.

Croll, James. "Age of the sun in relation to evolution." Nature 17 (10 January 1878): 206-207.

"The Dakota plains. Rainfall and tree-planting growth of Bismark." New York Daily Tribune, 8 September 1884, 2.

Davidson, Edward H. Poe: A critical study. Cambridge: Harvard University Press, 1957.

Dana, James D. Review of The six days of creation, by Tayler Lewis." Bibliotheca Sacra 13 (1856): 80-129, 631-656; 14 (1857): 388-413, 461-524.

Deville, Henri Saint-Claire. "Sur la dissociation ou decomposition spontanee des corps sous l'influence de la chaleur." Comptes Rendus Hebdomadaires Des Séances de L'Académie des Sciences 45 (1857): 859-861.

"Discussions at the electrical conference." Popular Science Monthly 26 (1884-1885): 286.

"Prof. Draper on absence of carbon in stellar universe." New York Times, 12 August 1881, 4.

"H. Draper on photographs of Orion." New York Times, 20 November 1880, 2.

"Prof. Draper's discovery. Oxygen in the sun." New York Times, 27 January 1879, 8.

"Professor Draper's paper." New York Daily Tribune, 1 September 1879, 4.

"Draper's photograph of Arion [sic]." New York Times, 13 December 1880, 4.

"Draper's photographs of Orion, &c." New York Times, 2 April 1882, 9.

"Prof. H. Draper's views on N. Lockyer's discovery as to the composition of the elements." New York Times, 15 January 1879, 2.

"Prof. H. Draper, discovery of oxygen in the sun reasserted." New York Times, 27 January 1879, 8.

Draper, Henry. "American contributions to spectrum-analysis." Journal of Science 2 (1865): 395-401.

________. "Astronomical observations on the atmosphere of the Rocky Mountains, made at elevations of from 4,500 to 11,000 feet, in Utah and Wyoming territories and Colorado." American Journal of Science 3d ser., 13 (1877): 89-94.

________. "On the coincidence of the bright lines of the oxygen spectrum with bright lines in the solar spectrum." American Journal of Science 3d ser., 18 (1879): 277.

________. "Discovery of oxygen in the sun by photography, and a new theory of the solar spectrum." Journal of the Franklin Institute 104 (1877): 81-88.

________. "Are there other inhabited worlds?" Harper's Magazine 33 (June 1866): 45-54.

________. "Oxygen in the sun." Nature 17 (28 February 1878): 339-340.

________. "On photographing the spectra of the stars and planets." American Journal of Science 3d ser., 18 (1879): 419-425.

________. "Researches upon the photography of planetary and stellar spectra." Proceedings of the American Academy of Arts and Sciences 19 (1883): 256-257.

________. "Silver glass telescopes and celestial photography in America." Journal of Science 1 (1864): 381-387.

Draper, John Christopher Draper. "On the presence of dark lines in the solar spectrum which correspond cloesely to the lines of the spectrum of oxygen." Nature 18 (17 October 1878): 654-657

Draper, John William. "On a change produced by exposure to the beams of the sun in the properties of an elementary substance." In Report of the thirteenth meeting of the British association for the advancement of science; held at Cork in August 1843, pt 2:9. London: John Murray, 1844.

________. "On the chemical action of light." Philosophical Magazine 1 (1851): 368-393.

________. "Early contributions to spectrum photography and photo-chemistry." Nature 10 (30 July 1874): 243-244.

________. "On the decomposition of carbonic acid and the alkaline carbonates by the light of the sun." American Philosophical Society, Proceedings 3 (1843): 111-114.

________. "Memoir I. Examination of the radiations of red-hot bodies. The production of light by heat." In Scientific Memoirs Being Experimental Contributions to a Knowledge of Radiant Energy, pp. 1-51. London: Sampson Low, Marston, Searle & Rivington, 1878; reprint, New York: Arno Press, 1973.

________. "Examinations of the radiations of red-hot bodies. The production of light by heat." American Journal of Science 2d ser., 4 (1847): 388-402.

________. "On the interference spectrum, and the absorption of the tithonic rays." Philosophical Magazine 3d ser., 26 (1845): 465-478.

________. "On the of the chemical action of light." Philosophical Magazine 15 (1858): 90-93.

________. "On the nature of flame, and on the condition of the sun's surface." Philosophical Magazine 15 (1858): 90-93.

________. "On a new form of spectrometer, and on the distribution of the intensity of light in the spectrum." American Journal of Science 3d ser., 18 (1879): 30-34.

________. "On the phosphorograph of a solar spectrum, and on the lines of its infra-red region." American Journal of Science 3d ser., 21 (1881): 171-182.

________. "Remarks on the Daquerrotype process." Annals of Electricity, Magnetism and Chemistry and Guardian of Experimental Science 6 (1941): 194-210.

________. "Researches in actino-chemistry. Memoir first: On the distribution of heat in the spectrum." American Journal of Science 3d ser., 4 (1872): 161-175.

________. "Researches in actino-chemistry. Memoir second: On the distribution of chemical force in the spectrum." American Journal of Science 3d ser., 5 (1873): 25-38 & 91-98.

Driver, S.E. "The cosmogony of Genesis: A defense and a critique." Andover Review 8 (December 1887): 639-649.

Dunwoody, H[enry] H[arrison] C[hase]. Weather proverbs. Washington, DC: Government Printing Office, 1883.

Dupree, A. Hunter. Science in the federal government: A history of policies and activities to 1940. Cambridge, MA: Harvard University Press, Belknap Press, 1957.

"The eclipse of the sun," New York Times, 19 May 1882, 1.

"The eclipse of the sun." New York Times, 30 May 1882, 4.

"Editorial notes." Western Review of Science and Industry 2 (April 1878): 62.

"Our El Mahdi." New York Times, 28 December 1883, 4.

"Electricity in solar rays." American Journal of Science 17 (1830): 389-390.

Elliott, Clark A. Biographical dictionary of American science: the seventeenth through the nineteenth centuries. S.v. "Pliny Earl Chase."

Emerson, Ralph Waldo. "Astronomy." In Young Emerson speaks: Unpublished discourses on many subjects, 170-179. Edited by Arthur Cushman McGiffert, Jr. Boston: Houghton Mifflin Co., 1938.

________. The complete works of Ralph Waldo Emerson, 10 vols. Edited by E.W. Emerson. Centenary Edition. Boston: Houghton Mifflin Co., 1903-1921.

________. Journals of Ralph Waldo Emerson. Edited by Edward Waldo Emerson and Waldo Emerson Forbes. Boston and New York: Houghton Mifflin Co., 1909-1914.

________. The letters of Ralph Waldo Emerson, 6 vols. Edited by Ralph L. Rusk. New York: Columbia University Press, 1939.

________. "Perpetual forces." North American Review 125 (1877): 271-282.

Encyclopedia Brittanica. 9th ed. S.v. "Meteorology."

Encyclopedia Brittanica. 5th ed. S.v. "Physical geography, by Thomas Stewart Traill.

"The examiners of the sun. Prof. Henry Draper's return." New York Times, 8 August 1878, 5.

"An expedition for solar research." Astrophysical Journal 19 (1904): 385-386.

Fassig, Oliver L., ed. Bibliography of meteorology. Washington, DC: Signal Office, 1889-1891.

Faye, Hervé. "Sur l'atmosphere du Soleil." Comptes Rendus 49 (1859): 564-571, 594-600.

________. "Sur la constitution physique du Soleil." Comptes Rendus 60 (1865): 89-96; 138-150.

Fiske, John. The miscellaneous writings of John Fiske. Vol. 2, Outlines of cosmic philosophy. Boston and New York: Houghton Mifflin & Co., 1902.

FitzGerald, Geo. "Dr. Siemens' solar hypothesis." Nature 26 (25 May 1882): 80.

Flaugergues, Honoré. "Observations sur la chaleur que produisaient les rayons du soleil pendant l'éclipse de cet astre, le 7 Septembre 1820, faites à l'observatoire de Viviers." Journal de Physique 92 (1821): 435-444.

Flower, Elizabeth and Murphey, Murray G. "The evolutionary controversy." Chap. in A history of philosophy in America. New York: G.P. Putnam's Sons, 1977.

Forrey, Samuel. Meteorology: Comprising a description of the atmosphere and its phenomena, the laws of climate in general, and especially the climatic features peculiar to the region of the United States; with some remarks on the climates of the ancient world, as based on fossil geology. New York: H.G. Langley, 1842.

________. "Researches in elucidation of the distribution of heat over the globe, and especially of the climatic features peculiar to the region of the United States." American Journal of Science 47 (1844): 18-50, 221-241.

Fourier, Baron [Jean Babtiste Joseph]. "General remarks on the temperature of the terrestrial globe and the planetary spaces." American Journal of Science 32 (1837): 1-20.

Fowle, Frederick Eugene. "Atmospheric transparency for radiation." Monthly Weather Review 42 (1914): 2-4.

Frost, William Goodell. For the mountains. An autobiography. New York: Fleming H. Revell Co., 1937.

Gilfillan, George. "John Pringle Nichol." In A gallery of literary portraits, 2:254-255. Edinburgh: n.p., 1855.

Gillispie, Charles Coulston ed. Dictionary of scientific biography. New York: Charles Scribner's Sons, 1973. S.v. "John William Draper," by Donald Fleming; "William Huggins," by Herbert Dingle; "Jonathan Homer Lane," by Nathan Reingold; "Samuel Pierpont Langley," by Don F. Moyer; "Elias Loomis," by Gisela Kutzbach; "Constantine Samuel Rafinesque" by Joseph Ewan; "Charles Schuster," by Robert H. Kargon; and "(Pietro) Angelo Secchi," by Giorgio Abetti.

Gilman, Daniel C. The life of James Dwight Dana. New York and London: Harper & Bros., 1899.

Gingerich, Owen. "Henry Draper's scientific legacy." New York Academy of Science, Annals 395 (1982): 308-320.

Goldfarb, Stephen. "Science and democracy: A history of the Cincinnati Observatory, 1842-1872." Ohio History 78 (1969): 172-178.

Goetzmann, William H. Exploration and empire. New York: Alfred A. Knopf, 1966.

Goode, G. Brown. "The Smithsonian Institution." Nature 53 (23 January 1896): 281-285.

H[ale], G[eorge] E[llery]. "Astronomical and physical conference at the Harvard College Observatory." Astrophysical Journal 8 (1898): 54-55.

________. "The Harvard conference." Astrophysical Journal 8 (1898): 193-198.

________. Review of Annals of the Astrophysical Observatory of the Smithsonian Institution, vol. 1, by S.P. Langley. In Astrophysical Journal 13 (1901): 280-288.

Hale, George E. "Co-operation in solar research." Astrophysical Journal 20 (1904): 306-312.

________. "The Solar Observatory of the Carnegie Institution of Washington." Astrophysical Journal 21 (1905): 151-172.

Halpenny, Francess G. gen. ed. Dictionary of Canadian biography. Toronto: University of Toronto Press, [1982]. S.v. "Henry George Vennor," by P.R. Eakins.

Hanna, Rev. James W. Revised astronomy; or theoretical astronomy from a new base. Chicago: Fleming, c1891.

________. "Solar dynamics -- Some new astronomy." Kansas City Review of Science and Industry 8 (October 1884): 308-327.

Harris, George. Review of The destiny of man viewed in the light of his origin, by John Fiske. In Andover Review 3 (January 1885): 82-85.

________. Review of The idea of God as affected by modern knowledge, by John Fiske. In Andover Review 5 (1886): 98-102.

Harvard College Observatory. The Draper catalogue of stellar spectra photographed with the 8-inch Bache telescope as a part of the Henry Draper Memorial. Annals of the Astronomical Observatory of Harvard College, vol. 27. Cambridge: John Wilson & Son, Harvard University Press, 1890.

________. First annual report of the photographic study of stellar spectra, conducted at the Harvard College Observatory. Cambridge: John Wilson & Son, Harvard University Press, 1887.

________. Second annual report of the photographic study of stellar spectra, conducted at the Harvard College Observatory. Cambridge: John Wilson & Son, Harvard University Press, 1888.

________. Third annual report of the photographic study of stellar spectra, conducted at the Harvard College Observatory. Cambridge: John Wilson & Son, Harvard University Press, 1889.

________. Fourth annual report of the photographic study of stellar spectra, conducted at the Harvard College Observatory. Cambridge: John Wilson & Son, Harvard University Press, 1900.

Hayden, Ferdinand V. Preliminary report of the United States geological survey of Wyoming and portions of contiguous territories. 42d Cong., 2d Sess. House Executive Document, vol. 15, No. 325 (1871), 6-8.

"General Hazen hunting for prognosticators." New York Times, 19 August 1882, 3.

Hazen, W[illiam] B[abcock]. Our barren lands: The interior of the United States west of the 100th meridian and east of the Sierra Nevadas. Cincinnati: R. Clarke, printers, 1875.

________. Some corrections of "Life on the plains". Saint Paul, Minn.: Ramaley & Cunningham, 1875.

________."The great middle region of the United States, and its limited space of arable land." North American Review 120 (1875): 1-34.

________. "Worthless railroad land. The Northern Pacific Railroad country--Views of Maj.-Gen. Hazen." New-York Daily Tribune, 7 February 1874, 2.

Helmholtz, Hermann. "On the application of the law of the conservation of force to organic nature." Proceedings of the Royal Institution 3 (1858-62): 347-357.

________. "On the interaction of natural forces." Philosophical Magazine 4th ser., 11 (1856): 489-518.

"The Henry Draper Memorial." New York Herald, 21 March 1886, 11.

"Henry Draper star spectra." New York Times, 29 October 1879, 8.

Henry, Joseph. "On the application of the telegraph to the premonition of weather changes." Proceedings of the American Academy of Arts & Sciences 4 (1859): 271-275.

________."Experiments relative to spots on the sun." Walker's Electrical Magazine 2 (1846): 321-324.

________. "On the heat of the solar spots." In Report of the fifteenth meeting of the British association for the advancement of science; held at Cambridge in June 1845. London: John Murray, 1846, pt 2:215-217.

________. Scientific writings of Joseph Henry, 2 vols. Washington, DC: Smithsonian Institution, 1886.

________. "Suggestions as to the establishment of a physical observatory." In Smithsonian Annual Report for 1870, 141-142. Washington, DC: Smithsonian Institution, 1871.

________. "Systematic meteorology in the United States." In Smithsonian Annual Report for 1865, 52. Washington, DC: Smithsonian Institution, 1866.

Herrmann, Dieter B. "The origins of astrophysics." Chap. in The history of astronomy from Herschel to Hertzsprung. Cambridge: Cambridge University Press, 1984.

Hertzsprung, Einar. "Zur strahlung der sterne." Zeitschrift Für Wissenschaftliche Photographie 3 (1905): 429-442.

Heysinger, I.W. The source and mode of solar energy. Philadelphia: J.B. Lippincott Co., 1895.

Hickok, Laurens P. Rational cosmology; or, The eternal principles and the necessary laws of the universe, 2d ed. New York: D. Appleton & Co., 1859.

________. Rational psychology; or, The subjective idea and the objective law of all intelligence. Schenectady, New York: G.Y. Van Debogert, 1854.

Hilton, H.R. "The rainfall in relation to Kansas farming." Transactions of the Kansas Academy of Sciences 7 (1879-1880): 37-46.

Hinrichs, Gustavus. "Introduction to the mathematical principles of the nebular theory, or planetology." American Journal of Science 2d ser., 39 (1865): 46-58, 134-150, 276-286.

Hoagland, Clayton. "The universe of Eureka: A comparison of the theories of Eddington and Poe." Southern Literary Messenger 1 (May 1939): 307-313.

Hoffman, Daniel. "The mind of God, or 'What I here propound is true.'" Chap. in Poe Poe Poe Poe Poe Poe Poe. Garden City, New York: Doubleday & Co., Inc., 1972.

Hofstadter, Richard. Social Darwinism in American thought, rev. ed. Boston: The Beacon Press, 1955.

Holden, Edward S. "Contributions from the Lick Observatory II. The nebula of Orion." Overland Monthly, 2d ser., 14 (April 1892): 401-404.

________. Handbook of the Lick Observatory. Mt. Hamilton: Lick Observatory, 1888.

Holden, Edward Singleton. "Monograph of the central parts of the nebula of Orion." In U.S. Naval Observatory. Washington Observations, 1878, appendix I. Washington, DC: U.S. Naval Observatory, 1878.

________. "On the proper motion of the Trifid nebula: M. 20 = H. v; 10, 11, 12 = h. 1991, 3718 = G.C. 4355." American Journal of Science 3d ser., 14 (1877): 433-458.

________. "Recent progress in astronomy." North American Review 131 (1880): 375-383.

________. "Sidereal astronomy. Old and new." Century 14 (1886): 601-609 & 780-788.

Holman, Harriet. "Hog, Bacon, Ram, and other 'savans' in Eureka: Notes toward decoding Poe's encyclopedic satire." Poe Newsletter 2 (1969): 49-55.

Holyoke, Edward Augustus. "An estimate of the heat and cold of the American atmosphere beyond the European, in the same parallel of latitude; with some thoughts on the causes of the excess." Memoirs of the American Academy of Science, 2 (1793): 65-88.

Humphreys, W.J. "Biographical memoir of Cleveland Abbe." Biographical Memoirs of the National Academy of Sciences 8 (1919): 469-508.

Hunt, T. Sterry. "Celestial chemistry from the time of Newton." American Journal of Science 3d ser., 23 (1882): 123-133.

________. "On the conservation of solar energy." Nature 25 (27 April 1882): 602-603.

Inman, Col. Henry. "The connection between storms and sunspots, with record of the celebrated storms of 1600 years." Western Review of Science and Industry 1 (September 1877): 393-398.

"International co-operation in solar research." Astrophysical Journal 20 (1904): 301-305.

Irons, James Campbell. Autobiographical sketch of James Croll LL.D., F.R.S., etc. with memoir of his life and work. London: Edward Stanford, 1896.

Jackson, John Brinkerhoff. "The plains." In American space. The centennial years, 1865-1876, 166-180. New York: W.W. Norton & Co., 1972.

Jaki, Stanley L. Planets and planetarians: A history of theories of the origin of planetary systems. New York: John Wiley & Sons, 1978.

James, Frank A.J.L. "Thermodynamics and source of solar heat, 1846-1862." British Journal for the History of Science 15 (1982): 157-181.

Jefferson, Thomas. Notes on the state of Virginia. Richmond, Va.: J.W. Randolph, 1853.

Jewell, Lewis E. "Oxygen in the sun." Astrophysical Journal 5 (1897): 99-100.

Johnson, F.H. "Mechanical evolution." Andover Review 1 (June 1884): 631-649.

________. "Theistic evolution." Andover Review 1 (April 1884): 363-381.

________. "Revelation as a factor in evolution." Andover Review 5 (January 1886): 19-37.

Joint commission to consider the present organization of the Signal Service, Geological Survey, Coast and Geodetic Survey, and the Hydrographic Office of the Navy Department, with a view to secure greater efficiency and economy of administration of the public service in said bureaus, authorized by the sundry civil act approved July 7, 1884, and continued by the sundry civil act approved March 3, 1885. Testimony, 49th Cong., 1st Sess., March 16, 1886. Senate Miscellaneous Document No. 82. Washington, DC: Government Printing Office, 1907.

Jones, Bessie Zaban. Lighthouse of the skies, the Smithsonian Astrophysical Observatory: Background and history 1864-1955. Washington, DC: Smithsonian Institution, 1965.

Jones, Bessie Zaban and Boyd, Lyle Gifford. The Harvard College Observatory: The first four directorships, 1839-1919. Cambridge, MA: Harvard University Press, Belknap Press, 1971.

Juettner, Otto. "Daniel Vaughn." In Daniel Drake and his followers: Historical and biographical sketches, 300-304. Cincinnati: Harvey Publishing Co., 1909.

Kant, Immanuel. Allgemeine naturgeschichte. Edited by F. Krafft. Munich: Kindler, 1971.

________. Universal natural history and theory of the heavens. Edited by Stanley L. Jaki. Edinburgh: Scottish Academic Press, 1981.

Kedzie, J[ohn] H[ume]. Speculations. solar heat, gravitation and sunspots. Chicago: S.C. Griggs & Co., 1886.

Keeler, James E. "The importance of astrophysical research and the relation of astrophysics to other physical sciences." The Astrophysical Journal 6 (November 1897): 271-288.

King, Clarence. "The age of the earth." American Journal of Science 3d ser., 45 (1893): 1-20

Kirkwood, Daniel. "Does the motion of the inner satellite of Mars disprove the nebular hypothesis?" American Journal of Science 3d ser., 14 (1877): 327-328.

________. "On the nebular hypothesis." American Journal of Science 2d ser., 30 (1860): 161-181.

________. "On the testimony of the spectroscope to the truth of the nebular hypothesis." American Journal of Science 3d ser., 2 (1871): 155-156.

Kroeker, Marvin E. "The arid lands controversy." Chap. in Great plains command: William B. Hazen in the frontier west. Norman, Okla.: University of Oklahoma Press, 1976.

________. "Deceit about the garden: Hazen, Custer and the arid lands controversy." North Dakota Quarterly 38 (Summer 1970): 5-21.

Lane, J.H. "On the theoretical temperature of the sun; under the hypothesis of a gaseous mass maintaining its volume by its internal heat, and depending on the laws of gases as known to terrestrial experiment." American Journal of Science 2d ser., 50 (1870): 57-74.

________. "Report of J. H. Lane, Esq." In Report of the of superintendent the United States Coast Survey showing the progress of the survey during the year 1869, 167-169. Washington, DC: Government Printing Office, 1872.

"Mr. Langley's recent progress in Bolometer work at the Smithsonian Astrophysical Observatory." Astronomy and Astrophysics 13 (1894): 41-44.

Langley, S.P. "Experimental determination of wave-lengths in the invisible prismatic spectrum." American Journal of Science 3d ser., 27 (1884): 169-188.

________. "The history of a doctrine." Proceedings of the American association for the advancement of science, thirty-seventh meeting, held at Cleveland, August, 1888. Salem: Published by the Permanent Secretary, 1889, 1-23.

________. The absorption lines in the infra-red spectrum of the sun, Annals of the Astrophysical Observatory of the Smithsonian Institution, vol. 1, 7-216. Washington, DC: Smithsonian Institution, 1901.

________. "The actinic balance." American Journal of Science 3d ser., 21 (1881): 187-198.

________. "Address of Prof. Samuel P. Langley, vice-president, section A, before the American association for the advancement of science." Proceedings of the American association for the advancement of science; twenty-eighth meeting held at Saratoga Springs, N.Y., August, 1879. Salem: Published by the Permanent Secretary, 1880, 51-63.

________. "On the amount of astmospheric absorption." American Journal of Science 3d ser., 28 (1884): 163-180.

________. "Annals of the astrophysical observatory of the Smithsonian Institution, Volume 1." Monthly Weather Review 30 (May 1902): 258-260.

________. "The Bolometer." American Journal of Science 4th ser., 5 (1898): 241-245.

________. "The Bolometer." Nature 57 (28 April 1898): 620-622.

________. "The Bolometer." Proceedings American Metrological Society 2 (1881): 184-190.

________. "On the comparison of certain theories of solar structure with observation." American Journal of Science 3d ser., 9 (1875): 192-198.

________. "Energy and vision." American Journal of Science 3d ser., 36 (1888): 359-379.

________. "On hitherto unrecognized wave-lengths." American Journal of Science 3d ser., 32 (1886): 83-106.

________. "The invisible solar and Lunar spectrum." American Journal of Science 3d ser., 36 (1888): 397-411.

________. "On the Janssen solar photograph and optical studies." American Journal of Science 3d ser., 15 (1878): 297-301.

________. "The laws of nature." Science 15 (1902): 921-927.

________. "Measurement of the direct effect of sun-spots on terrestrial climates." Monthly Notices of the Royal Astronomical Society, 37 (1876): 5-11.

________. "On the minute structure of the solar photosphere." American Journal of Science 3d ser., 7 (1874): 87-101.

________. "The Mount Whitney expedition." Nature 26 (3 August 1882): 314-317.

________. The new astronomy. Boston: Houghton Mifflin & Co., 1891.

________. "The new astronomy." Century Magazine 28 (1884): 712-726, 922-936; 29 (1884): 224-241, 700-721 and 33 (1887): 339-355, 586-598.

________. "The new spectrum." American Journal of Science 11 (1901): 403-413.

________. "Note on the optical properties of rock-salt." American Journal of Science 3d ser., 30 (1885): 477-481.

________. "Note on the transmission of light by wire gauze screens." American Journal of Science 3d ser., 30 (1885): 210-212.

________. "Observations on invisible heat-spectra and the recognition of hitherto unmeasured wave-lengths made at the Allegheny Observatory." American Journal of Science 3d ser., 31 (1886): 1-12.

________. "Observations on Mt. Whitney." Science, 13 (February 1882): 132-136.

________. "On a possible variation of the solar radiation and its probable effect on terrestrial temperatures." Astrophysical Journal 19 (1904): 305-321.

________. "A proposed new method in spectrum analysis." American Journal of Science 3d ser., 14 (1877): 140-146.

________. "The recent progress of solar physics." Popular Science Monthly 16 (November 1879): 1-7.

________. "On recent researches in the Infra-red spectrum." Nature 51 (1 November 1894): 12-16.

________. Researches on solar heat and its absorption by the earth's atmosphere. A report of the Mount Whitney expedition, Professional Papers Signal Service, No. 15. Washington, DC: Government Printing Office, 1884.

________. "The scientific work of the government." MacClure's Magazine 35 (1904): 81-92.

________. "The selective absorption of solar energy." American Journal of Science 3d ser., 25 (1883): 153-183.

________. "The solar atmosphere, an introduction to an account of researches made at the Allegheny Observatory." American Journal of Science 3d ser., 10 (1875): 489-497.

________. "The 'solar constant' and related problems." <u>The Astrophysical Journal</u> 17 (1903): 89-99.

________. "New solar photographs." <u>Popular Science Monthly</u> 12 (April 1878): 748.

________. "The solar photosphere." In <u>Proceedings of the American association for the advancement of science. Twenty-second meeting held at Portland, Maine, August, 1873</u>. Salem: Published by the Permanent Secretary, 1874, 161-173.

________. "Sources of solar heat. A lecture by Prof. S.P. Langley." <u>New York Daily Tribune</u>, 10 March 1875, 2.

________. "The spectroscope in solar work." <u>Scientific American</u> 53 (1878): 242-243.

________. "Sunlight and skylight at high altitudes." <u>Nature</u> 26 (12 October 1882): 586-589.

________. "Sunlight and the earth's atmosphere." <u>Nature</u> 32 (7 May 1885): 17-20 & 32 (14 May 1885): 40-43.

________. "Sur les derniers résultats obtenus dans l'étude de la partie infra-rouge de spectre Solaire." <u>Comptes Rendus</u> 131 (1900): 734-736.

________. "The total solar eclipse as observed by the Smithsonian Expedition." <u>Nature</u> 62 (12 July 1900): 246-248.

________. "Variation of atmospheric absorption." <u>Nature</u> 69 (5 November 1903): 5.

________. "Variations of atmospheric absorption." <u>Nature</u> 70 (30 June 1904): 198.

Langley, S.P. and F.W. Very. "On the cheapest form of light." <u>American Journal of Science</u> 3d ser., 40 (1890): 97-113.

Laplace, P.S. <u>Exposition du système du monde</u>. Paris: Impr. du Cercle-Social, [1796].

________. <u>Mécanique céleste</u>, translated with commentary by Nathaniel Bowditch. Boston: Hillard, Grey, Little & Wilkins, 1829-1839.

________. <u>The system of the world</u>, 5th ed., translated by Henry H. Harte. Dublin: University Press, 1830.

________. <u>The system of the world</u>, translated by J. Pond. London: R. Phillips, 1809.

Lardner, Dionysius. "The stellar universe." In _Popular lectures on science and art; delivered in the chief cities and towns in the United States_, pp. 395-396. New York: Greeley & McElrath, 1846.

"The late Daniel Vaughan." _Popular Science Monthly_ 15 (1879): 127-129.

"Late scientific fruits. Final papers read at the national academy meeting." _New York Times_, 20 November 1880, 3.

Leavitt, Henrietta S. _Periods of 25 variables in the small Magellanic cloud_. Harvard College Observatory Circular No. 173. [Cambridge]: Harvard College Observatory, 1912.

"Lecture before the Royal Society: Prof. H. Draper, "The sun." _New York Times_, 29 June 1879, 8.

Lewis, Tayler. _The Bible and science; or, the world-problem_. Schenectady, N.Y.: G.Y. Van Debogert, 1856.

________. _Natural religion the remains of primitive revelation. A discourse, pronounced at Burlington, before the literary societies of the University of Vermont, August 6th, 1839_. New York: University of New York Press, 1839.

________. _Plato contra atheos. Plato against the atheists; or, the tenth book of the dialogue on laws, accompanied with critical notes, and followed by extended dissertations on some of the main points of the Platonic philosophy and theology, especially as compared with the holy scriptures_. New York: Harper & Brothers, 1845.

________. _The six days of creation; or the scriptural cosmology, with the ancient idea of time-worlds, in distinction from worlds in space_. Schenectady, N.Y.: G.Y. Van Debogert, 1855.

"Literary notices." _New York Observer_ 18 (1840): 158.

Lockyer, J. Norman. "The Bakerian lecture. Researches in spectrum analysis in connection with the spectrum of the sun." _Philosophical Transactions_ 164 (1874): 479-494.

________. "On the chemistry of the hottest stars." _Royal Society, Proceedings_ 61 (1897): 147-209.

________. _The chemistry of the sun_. London and New York: MacMillan & Co., 1887.

________. Elements of astronomy. New York: D. Appleton & Co., 1888.

________. Inorganic evolution as studied by spectrum analysis. London: MacMillan & Co., 1900.

________. The meteoritic hypothesis. A statement of the results of a spectroscopic inquiry into the origin of cosmical systems. London and New York: MacMillan & Co., 1890.

________. Studies in spectrum analysis. London: C.K. Paul & Co., 1878.

Lockyer, J. Norman and Hunter, W.W. "Sun-spots and famines." 19th Century 2 (1877): 583-604.

Loomis, Elias. "The Auroral-Borealis, or polar light; its phenomena and laws." In Smithsonian Report for 1865, 208-248. Washington, DC: Smithsonian Institution, 1866.

________. "Comparison of the mean daily range of the magnetic declination, with the number of Auroras observed each year, and the extent of the black spots on the surface of the sun." American Journal of Science 2d ser., 50 (1870): 153-172; 55 (1873): 245-260.

________. "Contributions to meteorology, being results derived from an examination of the United States weather maps and from other sources." American Journal of Science 3d ser., 11 (1876): 1-17.

________. "On electrical currents circulating near the earth's surface, and their connection with the phenomena of the Aurora Polaris." American Journal of Science 2d ser., 34 (1862): 34-46.

________. "The great auroral exhibition of 28th Aug. to 4th Sept. 1859; and the geographical distribution of auroras and thunder-storms." American Journal of Science 2d ser., 30 (1860): 79-100, 339-361; 32 (1861): 71-84, 318-335.

________. "Notices of auroras extracted from the meteorological journal of Rev. Ezra Stiles, S.T.D., formerly president of Yale College; to which are added, notices of a few other auroras recorded by other observers, at New Haven, Conn." Transactions of the Connecticut Academy of Arts and Sciences 1 (1866-71): 155-172.

________. "On the physical condition of the sun's surface, and on the motion of the solar spots." In Proceedings of the American association for the advancement of science. Fifteenth meeting, held at Buffalo, N.Y. August, 1866. Edited by Joseph Lovering, 1-5. Cambridge, Mass.: Cambridge Press, 1867.

Lovering, Joseph. "Obituary memoir of Joseph Henry." Smithsonian Miscellaneous Collections 21 (1881): 427-439.

________. "On the application of mathematical analysis to researches in the physical science." Cambridge Miscellany of Mathematics, Physics, and Astronomy no. 3 (October 1842): 128-129.

Lyell, Charles. Principles of geology: Or the modern changes of the earth and its inhabitants considered as illustrations of geology. New York: D. Appleton & Co., 1857.

McCulloch, Hugh. Men and measures of half a century. New York: Charles Scribner's Sons, 1889.

MacKinnon, Barbara. American philosophy. A historical anthology. Albany, N.Y.: State University of New York Press, 1985.

Madden, Edward H. Chauncey Wright and the foundations of pragmatism. Seattle: University of Washington Press, 1963.

"Magnetic influence of the solar beam." American Journal of Science 18 (1830): 181.

"Magnetic influence of the violet ray." American Journal of Science 18 (1830): 171-172.

Malone, Dumas, ed. Dictionary of American biography. New York: Charles Scribner's Sons, 1943. S.v. "Pliny Earl Chase," by Marjory Hendricks Davis; "Laurens Perseus Hickok;" and "Ormsby MacKnight Mitchel" by Jermain G. Porter.

Maury, Antonia C. Spectra of bright stars photographed with the 11-inch Draper telescope as a part of the Henry Draper Memorial. Annals of the Astronomical Observatory of Harvard College, vol. 28, pt. 1. Cambridge: John Wilson & Son, Harvard University Press, 1897.

Maury, T.B. "Poetry and philosophy of Indian summer." Harper's Monthly Magazine 48 (December 1875): 89-98.

________. "The telegraph and the storm: The United States Signal Service." Harper's New Monthly Magazine 42 (July 1871): 398-418.

"Measurements of solar radiation." Nature 64 (8 August 1901): 352-353.

"Meeting of the Royal Astronomical Society, June 14, 1879." Astronomical Register 17 (1880): 153-164.

Memorial of Joseph Henry. Smithsonian Miscellaneous Collections, vol. 21, no. 2. Washington, DC: Smithsonian Institution, 1881.

"Meteorology and sun spots." Western Review of Science and Industry 1 (May 1877): 176.

Meadows, A.J. "The origins of astrophysics." In Astrophysics and twentieth-century astronomy to 1950, ed. Owen Gingerich, pt. A, 4:3-15. The general history of astronomy. Cambridge, Mass.: Cambridge University Press, 1984.

Meadows, A.J. and J.E. Kennedy. "The origin of solar-terrestrial studies." Vistas in Astronomy 25 (1982): 419-426.

Meech, L.W. "On the computation of the sun's daily intensity at the exterior surface of the earth, and secular changes of heat." American Journal of Science 2d ser., 10 (1850): 49-55.

________. "On the relative intensity of the heat and light of the sun at different latitudes of the earth." In Smithsonian Annual Report for 1870, 141-142. Washington, DC: Smithsonian Institution, 1858.

Meldola, R. "On a cause for the appearance of bright lines in the solar spectrum." American Journal of Science 3d ser., 16 (1878): 290-300.

________. "Oxygen in the sun." Nature 17 (27 December 1877): 161-162.

Merrill, George P. The first one hundred years of American geology. New Haven: Yale University Press, 1924.

Mitchel, O.M. The astronomy of the Bible. New York: Blakeman & Mason, 1863.

"On the mode of printing maps of spectra and tables of wave-lengths." Astrophysical Journal 6 (1897): 55-56, 155-146.

"On the mode of printing maps of spectra." Astrophysical Journal 5 (1897): 216-217.

"On the mode of printing tables of wavelengths." Astrophysical Journal 4 (1896): 306-308

Monck, W.H.S. "Mr. Kedzie's theory of solar heat." Sidereal Messenger 7 (1888): 440-442.

Morris, Charles. "On the conservation of solar energy." Nature 25 (27 April 1882): 601-602.

Mount Wilson Observatory. Mount Wilson Observatory Contributions. vols. 1-10, 1905-1920.

National cyclopaedia of American biography. New York: James T. White & Co., 1929. S.v. "James Pollard Espy" and "Laurens Perseus Hickok," by Ernest Sutherland Bates.

Nelson, Roland W. "The definitive edition of Edgar Allan Poe's Eureka: A Prose Poem." Ph.D. diss., Bowling Green State University, 1974.

"New director of Allegheny Observatory." The Sidereal Messenger 10 (1891): 297.

"The new solar theory. What scientific men think of Mr. Lockyer's discovery." New York Times, 15 January 1879, 2.

"The new spectrum." New York Tribune, 20 April 1901, 3.

Newcomb, Simon. "Evolution and theology. A rejoinder." North American Review 128 (1879): 647-663.

________. Popular astronomy. New York: Harper, 1878.

________. "Recent astronomical progress." North American Review 123 (1876): 86-112.

________. The reminiscences of an astronomer. Boston and New York: Houghton Mifflin & Co., 1903.

________. "Review of Croll's Climate and time with especial reference to the physical theories of climate maintained therein." American Journal of Science 3d ser., 11 (1876): 263-273.

________. "On some points in climatology, a rejoinder to Mr. Croll." American Journal of Science 3d ser., 27 (1884): 21-26.

Newcomb, Simon, Noah Porter, Joseph Cook, James Freeman Clarke, and James McCosh. "Law and design in nature." North American Review 128 (1879): 537-562.

Newton, H.A. "Memoir of Elias Loomis, 1811-1889." Biographical Memoirs of the National Academy of Sciences 3 (1895): 213-252.

"Prof. Nichol's lecture of Friday last." New York Herald, 1 February 1948, 4.

N[ichol], J.P. "State of discovery and speculation concern ing the nebulae." London and Westminster Review 3 (1836) 390-409.

Nichol, John Pringle. Contemplations on the solar system, 3d ed. Edinburgh: John Johnstone, 1847.

________. Cyclopaedia of the physical sciences, comprising acoustics, astronomy, dynamics, electricity, heat, hydrodynamics, magnetism, philosophy of mathematics, meteorology, optics, pneumatics, statics, &c. &c. 3d ed. London: Charles Griffin & Co., 1868.

________. The planetary system: Its order, and physical structure. London: H. Bailliere, 1850.

________. Thoughts on some important points relating to the system of the world, 1st American ed. Boston and Cambridge: James Munroe & Co., 1848.

________. Views of the architecture of the heavens. Edinburgh: William Tait, 1837.

________. Views of the architecture of the heavens. 2d ed. New York: Dayton & Newman, 1842.

________. Views of astronomy. Seven lectures delivered before the Mercantile Library Association of New York in the months of January and February, 1848. New York: Greeley & McElrath, 1848.

Nielsen, Axel V. "Contributions to the history of the Hertzsprung-Russell diagram." Centaurus 9 (1963): 219-253.

Nordstedt, George. "Poe and Einstein." Open Court 44 (March 1930): 173-180.

"The Northwest. General G.A. Custer in reply to General Hazen." Minneapolis Tribune, April 17, 1874, 4.

Norton, William Augustus. "On the physical constitution of the sun." American Journal of Science 3d ser., 1 (1871): 395-407.

"Notes and news." Science 10 (1887): 80-81.

Noyes, Isaac P. "Detail remarks on the weather." Western Review of Science and Industry 3 (June 1879): 94-96.

________. "Evidence from the weather map." Western Review of Science and Industry 3 (February 1880) 589-597.

________. "False notions in regard to the weather." Kansas City Review of Science and Industry 6 (June 1882): 87-92.

________. "Meteorological discoveries." Kansas City Review of Science and Industry 8 (August 1884): 196-204.

________. "Meteorological factors and phenomena." Kansas City Review of Science and Industry 6 (November 1882): 421-428.

________. "Meteorology revolutionized by the weather map." Kansas City Review of Science and Industry 8 (May 1884): 40-46.

________. "A new view of the weather question." Western Review of Science and Industry 2 (July 1878): 218-229, 279-292.

________. "The passing of 'low' barometer." Kansas City Review of Science and Industry 5 (June 1881) 102-107.

________. "Prophecy of the weather." Kansas City Review of Science and Industry 4 (September 1880): 268-274.

________. "The storm center and weather prophets." Kansas City Review of Science and Industry 4 (April 1881): 750-755.

________. "Tornadoes." Kansas City Review of Science and Industry 4 (August 1880): 208-216.

________. "The weather map and the official weather indications." Kansas City Review of Science and Industry 5 (August 1881): 245.

________. "The weather prophecies(?) of Vennor." Kansas City Review of Science and Industry 4 (February 1881): 627-631.

________. "The weather-prophet farce." Kansas City Review of Science and Industry 5 (October 1881): 350-355.

________. "Where our storms come from." Kansas City Review of Science and Industry 5 (February 1882): 639-642.

Numbers, Ronald L. "The American Kepler: Daniel Kirkwood and his analogy." Journal for the History of Astronomy 4 (1973): 13-21.

________. Creation by natural law: Laplace's nebular hypothesis in American thought. Seattle and London: University of Washington Press, 1977.

Oettingen, A.J. von. Foreword to Allgemeine naturgeschichte und theorie des himmels by Immanuel Kant. Edited by A.J. von Oettingen. Leipzsig: Wilhelm Engelmann, 1898.

Oehser, Paul H. "Samuel Pierpont Langley." In Sons of science: The story of the Smithsonian Institution and its leaders, 110-140. New York: H. Schuman, 1949.

________. "Samuel Pierpont Langley: Astrophysics and flying machines." In The Smithsonian Institution, 49-53. New York: Praeger, 1970.

"Official science at Washington." Popular Science Monthly 27 (1885): 844-847.

Opie, John. "The environment and the frontier." In American frontier and western issues. A Historiographical review, ed. Roger L. Nichols, 7-26. New York: Greenwood Press, 1986.

Orr, Isaac. "An essay on the formation of the universe." American Journal of Science 6 (1823): 128-149.

Ostrum, John Ward, ed. Letters of Edgar Allan Poe, 6 vols. New York: Gordian Press, 1966.

"Oxygen in the sun. Times (London), 16 June 1879, 5.

"Oxygen in the sun." Nature 19 (13 February 1879): 352-353.

Pannekoek, A. A history of astronomy. London: George Allen & Unwin, Ltd., 1961.

"Paris, academy of sciences, November 5." Nature 63 (15 November 1900): 75.

[Peirce, Benjamin]. "Bowditch's translation of the Mécanique Céleste." North American Review 48 (January 1839): 173-177.

Peirce, Benjamin. "On the connection of comets with the solar system." In Proceedings American association for the advancement of science. Second meeting, held at Cambridge, August, 1849. Boston: Henry Flanders & Co., 1850, 188-122.

________. "On the constitution of Saturn's ring." American Journal of Science 2d ser., 12 (1851): 106-108.

________. Ideality in the physical sciences, ed. James Mills Peirce. Boston: Little, Brown & Co., 1881.

Peirce, C.S. "How to make our ideas clear." Popular Science Monthly 12 (1878): 286-302.

"The philosophy of Herbert Spencer." North American Review 100 (1865): 423-476.

Physicus [George John Romanes]. A candid examination of theism. Boston: Houghton, Osgood & Co., 1878.

Pickering, Edward C. "Draper Memorial photographs of stellar spectra exhibiting bright lines." Nature 34 (9 September 1886): 439-440.

________. "A fifth type of stellar spectra." Astronomische Nachrichten 127 (1891): 1-4.

________. "The Henry Draper Memorial." Nature 36 (12 May 1887): 31-34.

________. "Light of Webb's planetary nebula." Nature 21 (12 February 1880): 346.

________. "New planetary nebulae." American Journal of Science 3d ser., 20 (1880): 303.

________. "New planetary nebulae." American Journal of Science 3d ser., 26 (1882): 302.

________. "Two new planetary nebulae." Nature 22 (5 August 1880): 327.

________. "Photographic study of stellar spectra. Henry Draper Memorial." Nature 33 (8 April 1886): 535.

________. "A photographic study of the nebula of Orion." Proceedings of the American Academy of Arts and Science 20 (1884-1885): 407-416.

________. "Remarkable star spectrum; new planetary nebula." Science 2 (1881): 581.

________. "Small planetary nebulae, discovered at the Harvard College Observatory." Sidereal Messenger 1 (1882): 139.

Plotkin, Howard. "Henry Draper: A scientific biography." Ph.D. diss.. Johns Hopkins University, 1971.

________. "Henry Draper, Edward C. Pickering and the birth of American astrophysics." Annals of the New York Academy of Science 395 (1982): 321-330.

________. "Henry Draper, the discovery of oxygen in the sun, and the dilemma of interpreting the solar spectrum." Journal for the History of Astronomy 8 (1977): 44-71.

Poe, Edgar Allan. "Eureka: A prose poem." In The complete works of Edgar Allan Poe, 2d ed., edited by James A. Harrison, 16:179-316. New York: AMS Press, 1979.

Poisson, [Simon Denis]. "Memoir upon the temperature of the solid parts of the globe, of the atmosphere, and of those regions of space traversed by the earth." American Journal of Science 34 (1838): 57-69.

Pouillet, Claude M. "Memoire sur la chaleur solaire, sur les pouvoirs rayonnants et absorbants de l'air atomospherique, et sur la temperature de l'espace." Comptes Rendus 7 (1838): 24-65.

P[ritchett], H.S. Review of The true theory of the sun, showing the common origin of the solar spots and corona, and of atmospheric storms and cyclones, by Thomas Bassnett. Kansas City Review of Science and Industry 8 (July 1884): 142-143.

Quinn, Arthur Hobson. Edgar Allan Poe: A critical biography. New York and London: D. Appleton-Century Co., 1941.

Rafinesque, C.S. Celestial wonders and philosophy, or the structure of the visible heavens with hints on their celestial religion and theory of futurity. Philadelphia: Printed for the Central University of Illinois, 1838.

________. Genius and spirit of the Hebrew Bible. Including the Biblic philosophy of celestial wisdom, religion and theology, astronomy and realization, ontology and mythology, chronometry and mathematics. Philadelphia: Printed for the Eleutheriun of Knowledge, 1838.

________. "Some essential views of geology, by Dr. Hibbert and Rafinesque." Atlantic Journal and Friend of Knowledge 1 (Autumn 1833): 191-195.

________. The world, or instability: A poem in twenty parts with notes and illustrations. Philadelphia: J. Dobson, 1836.

Rankine, W.J.M. "On the dynamical theory of heat." Philosophical Magazine 4th ser., 27 (1864): 194-196.

________. "On the reconcentration of the mechanical energy of the universe." Philosophical Magazine 4th ser., 4 (1852): 358-360.

Ratner, Sidney. "Evolution and the rise of the scientific spirit in America." Philosophy of Science 3 (1936): 104-122.

"Recent studies of infra-red region of solar spectrum." Nature 63 (15 November 1900): 68.

Reed, Lucas. "The open space in Orion." Chap. in Astronomy and the Bible. Mountain View, Calif.: Pacific Press Publishing Co., 1919.

Reingold, Nathan, ed. The papers of Joseph Henry, vol. 1-5. Washington, DC: Smithsonian Institution Press, 1972-1985.

"Report by Professor S.P. Langley of observations at Oakland, Kentucky." In United States Coast Survey observations of total eclipse of Aug. 7, 1869, 21-22. Washington, DC: Coast Survey, 1870.

"Report of the chief signal officer for 1883." Kansas City Review of Science and Industry 7 (December 1883): 501-502.

Report of the commissioner of the general land office for the Year 1867. Washington, DC: Government Printing Office, 1867.

Review of Annals of the astronomical observatory of Harvard College, vol. 27. Nature 44 (28 May 1891): 89-90.

"Review of The correlation and conservation of forces: A series of expositions, by Prof. Grove, Prof. Helmholtz, Dr. Mayer, Dr. Faraday, Prof. Liebig, and Dr. Carpenter; with an introduction and brief biographical notices of the chief promoters of the new views, by Edward L. Youmans. In North American Review 100 (1865): 619-622.

Review of Outlines of cosmic philosophy, based on the doctrine of evolution, with criticisms on the positive philosophy, by John Fiske. In North American Review 120 (January 1875): 200-204.

Review of <u>Report of the chief signal officer For 1883</u> by General W.B. Hazen. <u>Kansas City Review of Science and Industry</u> 8 (February 1885): 596-597.

Review of <u>Speculations. Solar heat, gravitation, and sunspots</u>, by J.H. Kedzie. <u>New York Times</u>, 5 July 1886, 3.

"Review of <u>Views of the architecture of the heavens. In a series of letters to a lady</u>, by J.P. Nichol. <u>New York Evangelist</u> 11 (1840): 158.

Runge, C. "Oxygen in the sun." <u>Publications of the Astronomical and Astrophysical Society of America</u> 1 (1910): 14-15.

Runge, C. and F. Paschen. "Oxygen in the sun." <u>Astrophysical Journal</u> 4 (1860): 317-319.

Rusk, R.L. <u>The life of Ralph Waldo Emerson</u>. New York: Charles Scribner's Sons, 1949.

Russell, Henry Norris. "Address. Relations between the spectra and other characteristics of the stars." <u>American Astronomical Society, Publications</u> 3 (1918): 22-61.

________. "Determinations of stellar parallax." <u>Astronomical Journal</u> 26 (1910): 147-159

________. "Relations between the spectra and other characteristics of the stars." <u>Nature</u> 93 (30 April 1914): 227-230; 93 (7 May 1914): 252-258, & 93 (14 May 1914): 281-286.

________. "Some hints on the order of stellar evolution." <u>Science</u> n.s., 32 (1926): 883-884.

Russell, Henry Norris and Hinks, Arthur. "Determinations of the stellar parallax from photographs made at the Cambridge Observatory." <u>Monthly Notices of the Royal Astronomical Society</u> 65 (1905): 775-785.

Russell, Henry Norris and Arthur R. Hinks. "The parallax of 8 stars from photographs taken at the Cambridge Observatory." <u>Monthly Notices of the Royal Astronomical Society</u> 67 (1906):132-135.

Schellen, H. "The constitution of nebulae." <u>Popular Science Monthly</u> 3 (1873): 129-139.

Schneider, Herbert W. <u>A history of American philosophy</u>, 2nd ed. New York and London: Columbia University Press, 1963.

Schucking, E.L. "Henry Draper: The unity of the universe." Annals of the New York Academy of Science 395 (1982): 299-307.

Schuster, Arthur. "On the presence of oxygen in the sun." Nature 17 (20 December 1877): 148-149.

"Science and the state." Popular Science Monthly 29 (1886): 412-415.

"Science for the people. Oxygen in the sun." New York Daily Tribune, 5 July 1879, 6.

"Science for the people. photograph of a nebula." New York Daily Tribune, 6 February 1881, 9.

Siemens, C[harles] W[illiam]. "On the conservation of solar energy." Nature 25 (9 March 1882): 440-444.

Siemens, Charles Wiliam. "Conservation of solar energy." Nature 25 (30 March 1882): 504-505.

________. "On the conservation of solar energy." Nature 25 (27 April 1882): 603.

________. "Some of the questions involved in solar physics." Nature 28 (3 May 1883): 19-21.

"Sketch of General Albert J. Myer." Popular Science Monthly 18 (1880): 408-411.

Smith, Henry Nash. "The garden and the desert." Chap. in Virgin land: The American West as symbol and myth. Cambridge, Mass.: Harvard University Press, 1970.

________. "Rain follows the plow: The notion of increased rainfall for the Great Plains, 1844-1880." Huntington Library Quarterly 10 (February, 1847): 169-193.

S[mith], H.S.S. Review of The sun: Its constitution; Its phenomena; its condition, by Nathan T. Carr. Kansas City Review of Science and Industry 7 (December 1883): 504.

"The Smithsonian Astrophysical Observatory." Nature 44 (16 July 1891): 254-255.

"The Smithsonian Astrophysical Observatory." The Sidereal Messenger 10 (1891): 271-273.

"Smithsonian Astrophysical Observatory." In Smithsonian Institution Annual Report for 1893, 60. Washington, DC: Government Printing Office, 1894.

"Smithsonian Astrophysical Observatory." In Smithsonian Institution Annual Report for 1894, 75. Washington, DC: Government Printing Office, 1894.

"Smithsonian Astrophysical Observatory." In Smithsonian Institution Annual Report for 1896, 68. Washington, DC: Government Printing Office, 1897.

"Smithsonian Astrophysical Observatory." In Smithsonian Institution Annual Report for 1897, 66. Washington, DC: Government Printing Office, 1898.

"Smithsonian Astrophysical Observatory." In Smithsonian Institution Annual Report for 1898, 69. Washington, DC: Government Printing Office, 1899.

"Smithsonian Astrophysical Observatory." In Smithsonian Institution Annual Report for 1900, 99. Washington, DC: Government Printing Office, 1901.

"Smithsonian Astrophysical Observatory." In Smithsonian Institution Annual Report for 1902, 85. Washington, DC: Government Printing Office, 1903.

Smithsonian Institution Annual Report for 1892. Washington, DC: Government Printing Office, 1894.

"The Smithsonian Institution report." Nature 49 (22 February 1894): 397-399.

"The Smithsonian Institution." Nature 45 (14 January 1892): 261-262.

"Smithsonian investigations." Nature 53 (5 March 1896): 428-429.

"The Smithsonian report for year ending 1892." Nature 48 (22 June 1893): 184.

"Smithsonian report on scientific work." Nature 68 (7 May 1903): 20-22.

Snow, F.W. "Is the rainfall of Kansas increasing?" Transactions of the Kansas Academy of Sciences 9 (1883-1884): 101-103.

"The solar constant." Nature 67 (2 April 1905): 522.

"The solar system." Southern Review n.s., 6 (July 1869): 203-223.

"Solar and terrestrial changes." Nature 72 (3 August 1905): 332-333.

"Solar work at the Smithsonian Astrophysical Observatory." Nature 70 (1904): 39.

"Spectrum analysis of the stars, by Father Secchi." Proceedings of the Franklin Institute 83 (1867): 66-67.

Spencer, Herbert. Essays: Scientific, political and speculative. New York: D. Appleton & Co., 1864.

________. Illustrations of universal progress. New York: D. Appleton & Co., 1864.

________. "Recent astronomy and the nebular hypothesis." Westminster Review 70 (1858): 185-225.

________. Recent discussions in science, philosophy and morals, new and enlarged edition. New York: D. Appleton & Co., 1873.

Sprengel, Merton E. and Martz, Dowell E. "Orion revisited." Review and Herald, 25 March 1976, pp. 4-7; 1 April 1976, pp. 9-11; & 8 April 1976, pp. 6-8

Stallo, Johann Bernhardt. The concepts and theories of modern physics. New York: D. Appleton & Co., 1882.

"Stars and suns." New York Times, 9 September 1877, 6.

"The storm predicted for March. What the chief signal officer says." New York Daily Tribune, 22 December 1882, 5.

"The storms of June and July, 1877." Western Review of Science and Industry 1 (July 1877): 272-282.

"A sub-tropical solar physics observatory." Nature 67 (1 January 1903): 207.

"Sun spots and their effects." The Western Review of Science and Industry 1 (February, 1877): 663-666.

"The sun's composition." Journal of the Franklin Institute 108 (1879): 100-104.

"Sun's eclipse: Draper expedition for observation." New York Times, 14 July 1878, 6.

"Sun's eclipse: Draper's expedition instruments." New York Times, 26 July 1878, 5.

"Sun's eclipse: Professor Draper's work; Prof. Watson's discovery of Vulcan." New York Times, 8 August 1878, 5.

"The sun. Professor Young's second lecture. A mass of information crowded into a talk of an hour and a half." New York Daily Tribune, 10 January, 1883, 2.

"Sun: Total eclipse examined by a photography of the spectrum of the corona: Prof. Draper's precedence over Mr. Lockyer." New York Times, 30 May 1882, 4.

"Terrestrial temperatures and the solar radiation." Nature 74 (31 May 1906): 112.

Thomson, Sir William. "On the age of the sun's heat." MacMillan's Magazine 5 (1862): 288-293.

________. "Inaugural address before the British association at Edinburgh, August 2d." American Journal of Science 102 (1871): 269-294.

________. "Physical considerations regarding the possible age of the sun's heat." In Report of the thirty-first meeting of the British association for the advancement of science; held at Manchester in September, 1861. London: John Murray, 1862, 27-28.

________. "On the sun's heat." Proceedings of the Royal Institution 12 (1889): 1-21.

Torrey, Henry A.P. Review of Scientific theism, by Francis Ellingwood Abbot. Andover Review 5 (May 1886): 550-554.

Trouvelot, L. "On the veiled solar spots." American Journal of Science 3d ser., 11 (1876): 169-176.

Trowbridge, David. "On the nebular hypothesis." American Journal of Science 2d ser., 38 (1864) 344-360; 39 (1865): 25-43.

Trowbridge, John. The new physics. A manual of experimental study for high schools and preparatory schools for college. New York: D. Appleton & Co., 1884.

Trowbridge, John and C.C. Hutchins. "Oxygen in the sun; contributions from the physical laboratory of Harvard University." American Journal of Science 3d ser., 34 (1887): 263-270.

True, Frederick W. A history of the first half-century of the National Academy of Sciences 1863-1913. Washington, DC: n.p., 1913.

Vaeth, Gordon. Langley: Man of science and flight. New York, Ronald Press Co., 1966.

Van Horn, R.T. "About the atmosphere and its phenomena." Western Review of Science and Industry 1 (February 1878): 710-724.

"Professor Vaughan on the origin of asteroids." Popular Science Monthly 15 (1879): 570-571.

Vaughan, Daniel. "On the light of suns, meteors, and temporary stars." In Report of the twenty-seventh meeting of the British association for the advancement of science; held at Dublin in August and September, 1857, 42-44. London: John Murray, 1858.

________. "The origin of worlds." Popular Science Monthly 15 (1879): 1-10.

________. Phenomena of the material world. Cincinnati: Longley Brothers, 1856.

________. "On the phenomena which may be traced to the presence of a medium pervading all space." Philosophical Magazine 21 (1861): 507-515.

________. "Researches in meteoric astronomy." In Report of the twenty-fourth meeting of the British association for the advancement of science; held at Liverpool in September, 1854, 26-27. London: John Murray, 1855.

________. "Secular variations in lunar and terrestrial motion from the influence of tidal action." In Report of the twenty-seventh meeting of the British association for the advancement of science; held at Dublin in August and September, 1857, 40-42. London: John Murray, 1858.

________. "On the solar spots and the variable stars." Philosophical Magazine 15 (1858): 359-362.

"Vennor and his system." New York Times, 25 August 1881, 5.

"Mr. Vennor explains." New York Times, 6 February 1881, 7.

Very, Frank W. An epitome of Swedenborg's science. Boston: The Four Seas Co., 1927.

________. "On the need of adjustment of the data of terrestrial meteorology and of solar radiation, and on the best value of the solar constant." Astrophysical Journal 34 (1911): 371-387.

________. "Solar constant." Monthly Weather Review 29 (1901): 304.

Volney, C.F. View of the climate and soil of the United States of America. London: J. Johnson, 1804.

Walcott, Charles D. "Biographical memoir of Samuel Pierpont Langley 1834-1906." Biographical Memoirs of the National Academy of Sciences 7 (1912): 247-268.

Waldo, F. "Some remarks on theoretical meteorology in the United States, 1855 to 1890." Bulletin. Weather Bureau, United States Department of Agriculture 2 (1895): pt 2:323-324.

Walker, Sears C. "Examination of Kirkwood's analogy." In Proceedings American association for the advancement of science. Second meeting, held at Cambridge, August, 1849. Boston: Henry Flanders & Co., 1850, 212-219.

Wallace, W. Stewart, ed. Dictionary of North American authors deceased before 1950. N.p.: Gale Research Co., 1968. S.v. "John Hume Kedzie."

________. The MacMillan dictionary of Canadian biography. Toronto: MacMillan of Canada, 1978. S.v. "Henry George Vennor."

"Weather proverbs. The signal service bureau making a collection of old saws." New York Times, 28 August 1892, 12.

Webb, Walter Prescott. The great plains. Boston: Ginn & Co, 1931.

Webster, Noah. "On the supposed change in the temperature of winter." Memoirs of the Connecticut Academy of Arts and Sciences 1 (1810): 1-67.

________. A collection of papers on political, literary, and moral subjects. New York: Webster & Clark, 1843.

Whewell, William. Astronomy and general physics considered with reference to natural theology, 5th ed. London: William Pickering, 1836.

"Wiggin's storm." New York Times, 13 December 1882, 4.

"The Wiggins storm of March 9th to 11th, 1883." Kansas City Review of Science and Industry 6 (March 1883): 673.

Wilber, Charles Dana. The great valleys and prairies of Nebraska and the Northwest. Omaha, NE: n.p., 1881.

Williams, Samuel. The natural and civil history of Vermont. Walpole, N.H.: Isaiah Thomas & David Carlisle, 1794.

Williams, William Mattieu. The fuel of the sun. London: Simpkin Marshall & Co., 1878.

Wright, Chauncey. Philosophical discussions. New York: Burt Franklin, 1971 reprint of 1877.

[Wright, Chauncey]. "A physical theory of the universe." North American Review 99 (1864): 1-33.

________. Review of The origin of the stars, and the causes of their motions and their light, by Jacob Ennis. In North American Review 104 (April 1867): 618-626.

Youmans, Edward L., ed. The correlation and conservation of forces: A series of expositions, by Prof. Grove, Prof. Helmholtz, Dr. Mayer, Dr. Faraday, Prof. Liebig and Dr. Carpenter. New York: D. Appleton & Co., 1865.

________. "Spencer's evolution philosophy." North American Review 129 (1879): 389-403.

Young, Charles A. Elements of astronomy, rev. ed. Boston: Ginn & Co., 1897.

________. "Memoir of Stephen Alexander, 1806-1883." Biographical Memoirs of the National Academy of Sciences 2 (1886): 249-259.

________. The sun, new & rev. ed. New York: D. Appleton & Co., 1898.

ACKNOWLEDGEMENTS

No dissertation is written without the explicit help of many people. I cannot possibly thank all who have had a role in finishing this dissertation. Without the patient support and counsel of my wife, Xiaohua Yang, who agreed to marry me even though I had not yet finished the promised dissertation, this work would never have been completed. Professor Alan Marcus, of the Center for the Historical Studies of Technology and Science, in arranging a contract to organize the Maytag archives, provided the economic support necessary for this dissertation to gestate. His occasional counsel also aided in directing me out of discouraging periods of inaction. My rambling conversations with him about my dissertation undoubtedly made far more sense to me than they did to him, but they helped bring together ideas with which I had been struggling.

While I remain responsible for the historical viewpoint taken in this dissertation, much credit must go to Professor Robert Schofield for directing my intellectual and historiographic development. To assert that he has been patient with me would be an understatement. The thoughtful suggestions, as well as the stern lectures, he provided as he coaxed each chapter to completion, have been crucial to the finishing of the dissertation. More than he is aware, his suggestions and notes provided this dissertation with insights and direction. Few students are so honored to be directed by a professor capable of being both patient and supportive and yet firmly molding their intellectual development. He deserves credit for the development of my historical skills to the point where they are today. Whatever may be my intellectual and professional weaknesses, they are the result of my failings as a student rather than of his failure as a mentor.

Many others deserve acknowledgement. Professor Gail Evans provided useful content and grammatical suggestions. Professor LeeAnn Willson pointed out differences between the terminology used by 19th century astrophysicists and their modern equivalents. The librarians at Robert and Ellen Sorge Parks Library at Iowa State University and at the University of Kansas libraries provided invaluable assistance. My parents who continued to encourage me while concerned that I had become a professional student; and friends who cheerfully answered the late night calls from a frustrated scholar, have all played a significant role in seeing this dissertation to a completion. To each I express my sincere appreciation.