**SCIENTIFIC CONTROVERSY AND THE NEW ASTRONOMY:
THE INTELLECTUAL AND SOCIAL CONTEXTS OF THE HEVELIUS-HOOKE DISPUTE**

by

Voula Saridakis

Thesis submitted to the faculty of the
Virginia Polytechnic Institute and State University
in partial fulfillment of the requirements for the degree of

MASTER OF SCIENCE

in

Science and Technology Studies

APPROVED:

Mordechai Feingold, Chair

Joseph Pitt

Albert Moyer

Roger Ariew

May 1993
Blacksburg, Virginia

Scientific Controversy and the New Astronomy:
The Intellectual and Social Contexts of the Hevelius-Hooke Dispute

by
Voula Saridakis
Science and Technology Studies
Chair: Mordechai Feingold

(Abstract)

During the seventeenth century, science, and especially astronomy, underwent significant changes in which the emphasis on instrumentation shifted from a more qualitative approach to precise quantitative measurement. These changes were further encouraged by the formation of scientific societies, such as the Royal Society in London and the Royal Academy of Sciences in Paris, where members worked together as a collective to validate knowledge. Because members could freely dissent within the community, a prescribed behavior for participants in disputes was proposed, although seldom followed. Furthermore, disputes were not influenced by intellectual issues alone -- social factors also guided and influenced the course of controversies.

This study is an analysis of one scientific controversy in which the participants deviated from the prescribed code

of behavior in scientific disputes, and, although the controversy was guided primarily by social factors, intellectual factors ultimately determined its outcome. In the Introduction, I discuss two sociological theses (Merton, Shapin and Schaffer) which are relevant to scientific controversies. In Chapter 1, I describe the changing nature of astronomy and instrumentation in the seventeenth century with special emphasis on micrometers and telescopic sights. In Chapter 2, I explore the nature of scientific controversy vis-à-vis the Royal Society, and two particular controversies which did not deviate from the expected rules of behavior. A descriptive account of the Hevelius-Hooke dispute follows in Chapter 3, and in Chapter 4, I provide concluding remarks on the dispute. Finally, in the Conclusion, I discuss the intellectual and social contexts of the Hevelius-Hooke dispute.

Acknowledgments

This thesis would not have been possible if it were not for the large group of people who brought it all together. First, and foremost, I want to thank my committee chair, Moti Feingold, who pulled this topic (and me on bad days) together. His patience in reading and rereading my earlier drafts, and guidance in this thesis, deserve a sincere thanks. I would also like to express gratitude to the other members of the committee, Bert Moyer, Joe Pitt, and Roger Ariew, who have provided me with guidance and assistance as well.

Those who were not directly involved but helped in the preparation of this thesis: Richard Burian, who provided many helpful hints during the proposal, and actual, defense; Richard Hirsh, whose "Hirsh Method" of writing helped get this thesis underway; Glenn Bugh in the History Department, ~~with~~ to whom I complained in both English and Greek; Lucy Cox at the Inter-Library Loan office, who went to the ends of the earth (almost) to find my requests; Carolyn Furrow, the best, and most helpful secretary on this side of the planet; and Charlotte Webb, Anne Fitzpatrick, Moira Rogers, and other STS graduate students who, in one way or another, kept me from going crazy.

Finally, my deepest and most sincere appreciation goes out to those who are a part of my life. I want to thank Joe Malgioglio from the bottom of my heart for being by my side these last two years, and especially these last few hectic months, and helping me realize that there is more to life than academics. There are no words to express the gratitude I feel for my family: my father Agisilaos, my mother Sophia,

my sister Angie, and my brother Mike, have supported me through all my endeavors, pushed me to do my best, and always provided a shoulder for me to lean on. I will love them always.

TABLE OF CONTENTS

List of Figures

Introduction
The Intellectual and Social Contexts of Scientific Controversies

The Intellectual and Social Contexts of Disputes

Scientific controversies and priority disputes became fairly common during the latter half of the seventeenth century. Although their study "constitutes an interesting problem for further research," few historians or sociologists of science have dealt adequately with this topic.[1] Despite this neglect, the study of controversy in science affords both historians and sociologists two important advantages. First, controversies "often involve disagreements over the reality of entities or propriety of practices whose existence or value are subsequently taken to be unproblematic or settled."[2] To use H. M. Collins' metaphor, "institutionalized beliefs about the natural world are like the ship in the bottle;" scientific controversies allow us to see that the

---

[1] Robert K. Merton, Science, Technology and Society in Seventeenth Century England (New York: Howard Fertig, 1970), 169; cited in Rob Iliffe, "'In the Warehouse': Privacy, Property and Priority in the Early Royal Society," History of Science 30 (1992): 29.

[2] Steven Shapin and Simon Schaffer, Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life (Princeton: Princeton University Press, 1985), 7.

ship was first "a pile of sticks and string, and that it was once outside the bottle."[3] The second advantage is that during a controversy, the participants "attempt to deconstruct the taken-for-granted quality of their antagonists' preferred beliefs and practices."[4] One must be careful, however, not to depend on only one side of the controversy. Therefore, it is important to "note the constructive and deconstructive strategies employed by both sides to the controversy."[5]

In addition to identifying the intellectual changes and developments in science, the study of scientific controversies also entails the social interactions of the participants.[6] Scientific disputes "become an integral part of the social relations between scientists," often guiding their actions and behavior, before, during, or after, the controversy.[7] Although social factors play an important part in scientific controversy, the intellectual factors are more

---

[3] Collins, "The Seven Sexes"; cited in Shapin and Schaffer, Leviathan, 7.

[4] Shapin and Schaffer, Leviathan, 7.

[5] Ibid.

[6] H. Tristram Engelhardt, Jr., and Arthur L. Caplan, "Introduction: Patterns of Controversy and Closure: the Interplay of Knowledge, Values, and Political Forces," in Scientific Controversies: Case Studies in the Resolution and Closure of Disputes in Science and Technology, eds. H. Tristram Engelhardt and Arthur L. Caplan (Cambridge: Cambridge University Press, 1987), 1; and Ronald N. Giere, "Controversies Involving Science and Technology: a Theoretical Perspective," in Ibid., 127.

[7] Robert K. Merton, "Priorities in Scientific Discovery," in The Sociology of Science: Theoretical and Empirical Investigations ed. Norman W. Storer (Chicago: Chicago University Press, 1973), 289. Although Merton refers specifically to priority disputes, I will extend this view to all types of controversies.

likely to determine the outcome.[8] The non-intellectual, or social, factors which could affect both the course and outcome of disputes include: personality traits, institutional pressures, hostility between scientists across national lines, and "chance" events such as the death of one of the participants.[9]

Since scientific controversies usually entail a difference of belief and the social interactions of participants, they are characterized by the fact that they are both public (everyone knows about them, not just the immediate participants involved) and continuous (they last for a specific length of time). Therefore, although they may begin with only two individuals, they ultimately become a "community activity."[10] This "community activity," or "collective performance," offered scientists in the seventeenth century the chance to correct "the natural workings of the 'idols': the faultiness, the idiosyncrasy, or the bias of any individual's judgment and observational ability."[11] Shapin and Schaffer essentially argue that the collective worked together to filter out deviant errors and behavior.

---

[8] Ernan McMullin, "Scientific Controversy and its Termination," in Engelhardt and Caplan, Scientific Controversies, 88.

[9] Ibid., 60.

[10] Ibid., 51-52.

[11] Shapin and Schaffer, Leviathan, 78. Shapin and Schaffer argue that it was Robert Boyle's (1627-1691) social technology which made "the production of knowledge visible as a collective enterprise." (Ibid.)

One indication that individual personality traits do not necessarily effect scientific controversies, is the participation of scientists who can be characterized as modest, or reluctant to enter controversies:

> [These scientists] want to see 'fair play,' to see that behavior conforms to the rules of the game. The very fact of their entering the fray goes to show that science is a social institution with a distinctive body of norms exerting moral authority and that these norms are invoked particularly when it is felt that they are being violated.[12]

With pressure from scientific norms (especially originality), even the meek and unaggressive scientists assert their claims, and press for their own priorities in science.[13]

Merton claims that originality is responsible for the advancement of knowledge and the progress of science. Those who make original contributions to knowledge are the ones who are rewarded for having best fulfilled their roles and making the institution of science work efficiently.[14] Therefore, these rewards provide an incentive for the pursuit of science.[15] Specifically in the seventeenth century, the "idea of progress became widely current," as a result of the developments in science and technology (instrumentation).[16]

Yet the emphasis on originality and the desire to demonstrate new "truths", could lead to what was perceived to

---

[12] Ibid., 292-293.
[13] Ibid., 293, 294.
[14] Ibid., 293, 323.
[15] Merton, "Some Social and Cultural Factors in Scientific Advance," in Science, 236.
[16] Ibid., 232, 236.

be deviant behavior such as fraud and plagiarism. Competition in science, which is "intensified by the great emphasis on original and significant discoveries," may provide an incentive "for eclipsing rivals by illicit or dubious means," in which case the deviant response is "libel and slander rather than theft."[17] If the standards which govern "conflictful interaction" deteriorate, a controversy develops which is "reinforced by group loyalties," and "there develops an atmosphere of thoroughgoing hostility and mutual distrust."[18]

Individualism, Witnesses, and the Rules of Etiquette

The idea of a collective community of scientists who believed in the progress of science through original work, (especially through experiment), became a characteristic of the Royal Society of London. Founded in 1660 and formally chartered in 1662, this body "presented their own community as an ideal society where dispute could occur safely and where subversive errors were quickly corrected."[19] The Royal Society members had more power and authority than individuals, and "matters of fact" (objects of perceptual

---

[17] Merton, "Priorities," 311-312.
[18] Ibid., 314.
[19] Shapin and Schaffer, Leviathan, 298.

experience) were only generated when "the community freely displayed its joint assent."[20]

Members did not tolerate individualism, but especially "radical individualism -- the state in which each individual set himself up as the ultimate judge of knowledge," and true knowledge was only considered objective when it was "produced by the collective, and agreed to voluntarily by those who comprised the collective."[21] Furthermore, the experimental philosophers were opposed to tyranny and dogmatism, and believed that "no isolated powerful individual authority should impose belief;" knowledge came from nature, not from "privileged persons."[22]

According to Shapin and Schaffer, a strong emphasis was placed on the collective act of witnessing the production of knowledge, an important practice if knowledge were to be empirically-based.[23] The testimony of eye-witnesses was effective if two specific conditions were met. First, "the witnessing experience had to be accessible" to the collective, otherwise it would not be able to confirm the experience. Second, "witnesses had to be reliable and their testimony had to be creditable," a condition that was usually

---

[20] Ibid.
[21] Ibid., 78.
[22] Ibid., 298. Boyle asserted that "intellectual sects which based their power on the authority of individuals, were vulnerable to attack by the experimental or communal philosophers"; yet others used Boyle's authority and power for their own "claims to success" (Ibid., 322).
[23] Ibid., 56. Shapin and Schaffer specifically label it "virtual witnessing" (see Ibid., 60).

met only if one was part of the collective.[24] During scientific controversies, witnesses became allies, usually siding with one of the participants.

Instruments, which became an integral part of seventeenth century science, were, for the most part, developed to assist the senses which, experimental philosophers like Boyle and Robert Hooke (1635-1702), argued were limited "in their ability to discern and constitute" objects of perceptual experience.[25] An instrument's capacity to produce "matters of fact" depended on the instrument's "physical integrity", and more specifically, on collective agreement.[26] Not only was the "physical integrity. . . vital to the perceived integrity of the knowledge the [instrument] helped to produce," if the instrument lacked physical integrity, critics used it as a strategy to "attack adherents' claims and to substitute alternative accounts."[27]

Since members argued that disputes could occur safely in the collective, Boyle suggested several rules of etiquette in scientific controversy that had to be observed.[28] Essentially, he argued that "disputes should be about findings and not about persons," and that although it was proper to criticize one's inaccuracies, it was "most improper

---

[24] Ibid., 336.
[25] Ibid., 36.
[26] Ibid., 29.
[27] Ibid., 30.
[28] Some of these "rules" are in his Proëmial Essay (1661).

to attack [one's] character."[29] One had to be careful not to cross this personal boundary because doing so would risk "making foes out of mere dissenters,"[30] something which happened frequently in scientific disputes involving Hooke. Boyle further argued that those who could contribute "matters of fact", no matter how deceived they were, had to be "treated as possible converts to the experimental form of life," otherwise their harsh treatment would alienate them from the cause and the community:[31]

> And as for the (very much too common) practice of many, who write, as if they thought railing at a man's person, or wrangling about his words, necessary to the confutation of his opinions; besides that I think such a quarrelsome and injurious way of writing does very much misbecome both a philosopher and a Christian, methinks it is as unwise as it is provoking. For if I civilly endeavour to reason a man out of his opinions, I make myself but one work to do, namely, to convince his understanding; but, if in a bitter or exasperating way I oppose his errors, I increase the difficulties I would surmount, and have as well his affections against me as his judgment: and it is very uneasy to make a proselyte of him, that is not only a dissenter from us, but an enemy to us.[32]

Boyle perceptively noted that this proper protocol had commonly been violated, but unfortunately, the scientist's character continued to be the subject of personal attacks in subsequent disputes.

---

[29] Shapin and Schaffer, Leviathan, 73. Boyle stated, "For I love to speak of persons with civility, though of things with freedom." (Ibid.)
[30] Ibid.
[31] Ibid.
[32] Boyle, Proëmial Essay, 312; cited in Ibid., 73-74.

When the Royal Society initially formed, the collective prescribed a set of rules for the conduct of its members, specifically in eye-witnessing experiences, generating "matters of fact", and advancing science by adhering to its norms (especially originality and the disinterested pursuit of truth). However, as I shall demonstrate through the controversy which developed between Johannes Hevelius (1611-1687) (Figure 1a) and Hooke in the late 1660's, the dispute deviated from the course of what was considered proper behavior. The standards which governed this controversy deteriorated, resulting in the influence of group loyalty and an atmosphere of hostility and mistrust.

Furthermore, this case study, although guided, for the most part, by social factors, had intellectual factors influence its outcome. Of paramount importance in this case study is the issue of accuracy versus precision -- a topic which becomes clearer as the events of the dispute unfold. High accuracy occurs when there are few "systematic" errors such as errors in the calibration of instruments, human errors, errors caused by varying experimental conditions, and imperfect techniques. High precision occurs when there are few "random" errors such as errors of judgment, fluctuating conditions, and small disturbances.[33] Hevelius' measurements were accurate because of the techniques he employed in

---

[33] Yardley Beers, Introduction to the Theory of Error (Reading, MA: Addison-Wesley Publishing Company, Inc., 1957), 4-5.

filtering out certain types of "systematic" errors such as those listed above. Hooke, on the other hand, was concerned with the precision of telescopic sights -- obtaining precise measurements by filtering out small, "random" errors.

The social factors which influenced this controversy included the personal interactions of all the participants involved, pressure from the Royal Society to eye-witness claims made by the participants, hostility among the participants across national lines, and the "chance" event of Hevelius' death. Despite the complex personal relationships among the participants, however, open sights were ultimately abandoned for intellectual reasons.

Chapter 1
The Changing Nature of Astronomy and Instrumentation in the Seventeenth Century

Science and Instrumentation in the Seventeenth Century

During the seventeenth century, natural philosophy became "mathematicised" in the sense that it became clear that nature could only be understood in mathematical terms, and as natural philosophers realized that their unaided senses could not always discern the complexity of nature. Therefore, before the mathematical method could be applied, nature had to be carefully "investigated before it could be read and explained."[34]

Even before the classical period, astronomical instruments and mathematical models were used to unlock the secrets of the heavens. In the fourth-century B.C. and Hellenistic period, astronomers attempted to explain the physical construction of the heavens using these models, and Hellenistic astronomers, in particular, used simple instruments such as dioptras and armillary astrolabes to assist in their observations.

---

[34] Anthony Turner, Early Scientific Instruments: Europe 1400-1800 (London: Sotheby's Publications, 1987), 87.

By the later medieval period, uses for the astrolabe diversified as single-latitude astrolabes, universal astrolabes, and astrolabe-quadrants, became important calculating devices for both astronomy and geometry.[35] As the instrument-making trade grew, armillary spheres, cross-staffs, and other instruments were introduced into common astronomical practice. In the sixteenth century, until the time of Tycho Brahe's (1546-1601) work, mathematical models remained an important component of astronomy, despite the decline of innovation in instrumentation.[36] In the second half of the sixteenth century, Tycho became increasingly unsatisfied with several instruments because, as interest in measurement increased, these instruments could not keep up with his expectations of accuracy. As a result, Tycho organized the construction of a collection of instruments which were eventually used to make more precise observations.

The next great development in astronomical instrumentation was the telescope (or "spy glass" as it was initially called) in the early seventeenth century. The identity of the "inventor" of the telescope is debated, but actual recorded history of the telescope began with Hans Lipperhey's petitioning for a patent in 1608. Unfortunately for him, the States-General in the Netherlands, who was appointed to examine his instrument, rejected his claim on

---

[35] Ibid., 16.
[36] Ibid., 57.

the grounds that the telescope "could not be kept a secret" because it was easy to duplicate.[37]

Since all that was needed in order to construct a telescope were two lenses and a tube, many individuals succeeded in constructing such instruments; chief among them was Galileo Galilei (1564-1642). He first heard about the invention in the summer of 1609, and he immediately constructed a telescope with a magnification power of about 3 -- the usual power of telescopes that were being built at the time. Initially, he demonstrated military uses for the telescope, but eventually turned it towards the heavens. He changed the telescope in two fundamental ways to accommodate his curiosity and need for observation. He constructed the telescope with higher magnification, and supplied "his objectives with aperture stops. . . . In making these two improvements, he transformed the telescope from a gadget into a scientific instrument."[38] As such, the telescope became a more dependable instrument.

The invention of the microscope, which inevitably followed the telescope's, also "took place in a non-learned, everyday context."[39] Both the telescope and the microscope had extraordinary consequences in the seventeenth century, because they not only revealed

---

[37] Albert Van Helden, "The Invention of the Telescope," Transactions of the American Philosophical Society 67, no. 4 (1977): 21.
[38] Ibid., 26.
[39] Turner, Early, 122.

> new worlds and new phenomena, they also revealed the power of a new "instrumental" way of approaching and scrutinizing nature. Instruments hitherto had either been tools for specialized purposes in everyday life, or, in the context of learning, they had been used for measuring or simulating (or both) a given phenomenon. They had not, however, in any case been instruments of discovery in the way that the [telescope and microscope] were, and in a way that other new instruments invented in the seventeenth century were intended to be. The example of the telescope and microscope, combining perhaps with the growth in importance of an empirical approach, led to an expectation of discovery and so a willingness to create the instruments by which discovery could take place.[40]

Several of these more prominent instruments of discovery included the barometer and air-pump (whose inventions resulted from the investigation of vacuums), the thermometer, and what was to become another important instrument for astronomers, the pendulum clock.[41]

In these later instruments, quantification was of paramount importance, because without measurement, there could be no discovery. This makes them different than the telescope or microscope which were "exploratory instruments without the capacity to measure."[42] Astronomers, in particular, considered it a disadvantage that telescopes alone could not calculate the particular measurements which

---

40 Ibid.

41 By the middle of the century, Christian Huygens (1629-1695) adapted his new pendulum clocks so that they could indicate minutes "with greater accuracy than the old clocks could indicate hours" [H.C. King, The History of the Telescope, (London: Charles Griffin and Company Limited, 1955), 104].

42 Ibid.

interested them the most: the altitude of a celestial object, its diameter or distance from another celestial object, and the time it was either observed, or crossed the meridian. The first two measurements were easily gauged using traditional instruments such as the quadrant, sextant, and cross-staff, but the telescope was not powerful enough to replace the more traditional measuring instruments. Only when the telescope was eventually combined with traditional measuring instruments (in addition to new measuring devices), were "the great advances in practical astronomy in the later decades of the seventeenth century. . . achieved."[43] However, before discussing the application of the telescope to measuring instruments, and the measuring devices which enhanced this discovery, it is first necessary to explore the development of the telescope in the seventeenth century.

The Development of the Telescope

Following the invention of the Galilean, or "Dutch" telescope, Johannes Kepler (1571-1630) decided to change the

[43] Ibid. Observational instruments did not immediately "produce the sharp improvement in the accuracy" of certain measurements, but rather raised new questions that needed to be answered (Curtis Wilson, "Predictive Astronomy in the Century after Kepler," in Planetary Astronomy from the Renaissance to the Rise of Astrophysics. Part A: Tycho Brahe to Newton, eds. Réné Taton and Curtis Wilson, The General History of Astronomy Series, ed. Michael Hoskin, Vol. 2 (Cambridge: Cambridge University Press, 1989), 162.

component lenses within the tube; instead of using a convex objective lens and a concave eyelens, he employed two convex lenses. The Keplerian, or "astronomical" telescope, offered both advantages and disadvantages compared with the Galilean model, depending on whether or not one was an astronomer. In the Galilean telescope, the combination of a convex and concave lens produced an upright image, and the focal length of the objective lens, which gathered light, was the distance between that lens and the observer's eye. However, the image produced by the "astronomical" telescope was inverted, and the focal length was between the object glass and some point within the tube. Those using the telescope for terrestrial purposes, disliked the "astronomical" telescope because it inverted their images. Astronomers, however, found that this was not a major obstacle because inverted celestial objects did not significantly interfere with their observations.[44]

Astronomers found that the most substantial advantage afforded by the "astronomical" telescope, was its wider field of view. Because astronomers could see a larger area of the sky with one glance through the Keplerian model, the "astronomical" telescope "had almost entirely displaced Galileo's instrument for serious astronomical work."[45]

---

[44] See King, History, 36, 45 for a more detailed explanation of the differences between the two types of telescopes.

[45] Turner, Early, 91. See also Albert Van Helden, "The 'Astronomical Telescope', 1611-1650," Annali dell' Istituto e Museo di Storia della Scienza di Firenze 1 (1976): 34. It was on the "astronomical" telescope that subsequent telescopic sights and measuring devices were mounted.

Seventeenth century telescopes possessed several problems which seemed impossible to correct. The two most pressing were spherical and chromatic aberration. Spherical aberration, which was caused by an inaccurate geometrical shape of the lens, resulted in an image which was distorted along the edges, and focused in the center. Chromatic aberration, which affected all parts of the object (image), produced diffused and highly colored edges, and the colors increased with the use of additional lenses.[46] A third problem which usually interfered with observation was "uninvited", or stray, light which tended to "creep" into telescopes.[47] Of the three difficulties associated with telescopes, chromatic aberration was the "chief motivating source behind most changes," and astronomers tried to solve this dilemma by building longer telescopes.[48] Consequently, between 1635 and 1660, the magnification on the most powerful telescopes increased from approximately 30 to 150, and telescopes increased in length from about 2 to 10 meters.[49]

---

[46] Phyllis Allen, "Problems Connected with the Development of the Telescope (1609-1687)," Isis 34 (1943): 303.

[47] Ibid., 310.

[48] Ibid., 303-304. See also J.A. Bennett, The Divided Circle: A History of Instruments for Astronomy, Navigation and Surveying (Oxford: Phaidon, 1987), 66; and King, History, 50. Astronomers believed that the colors would diminish if light traveled a longer distance between the object glass and eyelens.

[49] Maria Luisa Righini Bonelli and Albert Van Helden, "Divini and Campani: A Forgotten Chapter in the History of the Academia del Cimento," Annali dell' Istituto e Museo di Storia della Scienza di Firenze 6 (1981): 15.

The longer the telescopes became, the more difficult they were to use. Therefore, astronomers eventually developed the "aerial" (or long-focus) telescope which consisted of a long, solid, wooden tube, usually constructed by sections, and held up by a series of wooden planks, cables, pullies, and a mast (Figure 1b).[50] Even the "aerial" telescope, however, was riddled with difficulties such as the bending and warping of the tube, the difficulty in aligning the lenses, and the shaking of the whole apparatus with the slightest breeze. As a result, further modifications were implemented, and Huygens, in the 1650's, developed the "air" telescope. The "air" telescope differed from the "aerial" in that it was a tubeless telescope in which the object glass component and eyelens component were connected by a cord which aligned and adjusted the two lenses (Figure 2).[51] Unfortunately, even the "air" telescope did not solve all problems as it was difficult to manipulate the cords and properly align the lenses.[52] Towards the end of the century, the problems associated with long refractors prompted astronomers to search for other possibilities and, consequently, Isaac Newton (1642-1727) produced the reflecting telescope in 1668. But although the substitution of lenses by mirrors seemed to remove certain obstacles which

---

[50] Silvio Bedini, "The Aerial Telescope," Technology and Culture 8 (1967): 398. Hevelius' "aerial" telescope was 150 feet in length.
[51] Turner, Early, 92.
[52] Bedini, "Aerial," 399.

affected lenses, astronomers discovered that there were still problems in casting and shaping mirrors. Therefore, the reflector did not become popular until the 1720's and 1730's, when the methods for producing mirrors were considerably improved.[53]

As the popularity of telescopes increased, astronomers became more interested in the competitive telescope-making field as well as the craftsmen who made the lenses. Telescopes were considered both precious and expensive commodities because of their usefulness -- there was an increasing demand for them -- and because of the difficulty in constructing good lenses.[54] It was the Italian craftsmen and scientists who "dominated the art of telescope-making" throughout the seventeenth century, especially Francesco Fontana (c.1590-1656), Eustachio Divini (1610-1685) and Giuseppe Campani (1635-1715).[55] Fontana and Divini were the "first of a new breed of professional opticians" who differed from the "unlettered craftsmen who worked in the shadow of scientists and [were] all but invisible."[56] They became part of the "scientific circles, [as] they were well informed, if

---

[53] Turner, Early, 93.

[54] Takehiko Hashimoto, "Huygens, Dioptrics, and the Improvement of the Telescope," Historia Scientiarium. International Journal of the History of Science Society in Japan 37 (1989): 60-61.

[55] Bonelli and Van Helden, "Divini," 4-5.

[56] Ibid., 8.

not learned, experimented with new methods and optical systems, made their own observations, and published books."[57]

By 1665, Campani, the youngest of the three, surpassed Divini's capabilities, and "became known as Europe's finest telescope maker, and virtually every discovery made in the heavens, from 1664 to the end of the century, was made with a Campani telescope."[58] Campani's success was due mainly to his invention of a lathe which ground and polished lenses from glass blanks, thereby by-passing the initial process of casting lenses in molds. Using this method, he produced lenses of greater clarity and focus (and less chromatic aberration) than his predecessors.[59]

Features of Seventeenth-Century Astronomical Instruments

At the same time that telescopes were being developed in the seventeenth century, traditional measuring instruments, such as quadrants, sextants, and octants, were also undergoing change. Two particular features were apparent in the design of most of these instruments. First, the "builder aimed to support his limb on a construction of radial bars that resembled the spokes of a wheel to which the limb formed

[57] Ibid.
[58] Ibid., 4, 39; and Bedini, "Aerial," 397.
[59] Bedini, "Aerial," 397.

a segment."[60] Hevelius' instruments, for example, had more support than Tycho's, because these radial bars lessened the chances of instrument distortion. Second, the builders concentrated mostly on the vertical rigidity of the arc, barely paying attention to the firmness of the lateral plane.[61] The most probable reason for this was because of the manner in which these instruments were positioned. The limb and radial bars of a quadrant, for example, were perpendicular, not parallel, with the ground. Therefore, builders were more concerned with the vertical, as opposed to the horizontal, rigidity of the instrument, since any changes in its verticality would result in exaggerated errors of measurement.[62]

The Application of Telescopes to Precision Astronomy

The development of telescopes and traditional measuring instruments occurred at the same time, yet even as early as Galileo, astronomers considered quantifying telescopes, or

---

[60] Allan Chapman, "The Design and Accuracy of Some Observatory Instruments of the Seventeenth Century," Annals of Science 40 (1983): 468.

[61] Ibid.

[62] Both these features eventually disappeared in eighteenth century instruments. By 1725, George Graham (c.1674-1751) replaced radial spokes with "a rigid trellis of bars," and this design was incorporated by others such as J. Sisson and John Bird (1709-1776). Since limbs became an integral part of the trellis, the whole instrument had "less freedom to move and distort." (Ibid.) (Figure 3)

somehow increasing the accuracy of measuring instruments. By 1640, William Gascoigne (c.1620-1644) had discovered the two methods of applying telescopes to precision astronomy. First, a smaller sized telescope, or "telescopic sight", could be attached to a graduated arc (i.e., a quadrant or sextant). In this case, it was necessary to place cross-hairs in the focal plane of the object glass. Since the cross-hairs were in the same plane as the celestial object under observation, they were simultaneously in focus with the object.[63] The second method of using telescopes to measure small angular separations, was to use them alone, without affixing them to graduated arcs. Two conditions had to be met in order for this method to work: the two objects being measured had to be simultaneously visible in the field of view, and the telescope had to be fitted with a micrometer -- a mechanism which enabled one to measure small angles.[64]

Cross-hairs and micrometers differed significantly. Cross-hairs were two hairs that intersected each other at right angles, and were used to locate the center of the field of view. Consequently, they acted as sights which aligned the telescope with the target object.[65] The micrometer was used for different purposes altogether. It was a small measuring device, made up of several component parts which

---

[63] Abraham Wolf, A History of Science, Technology, and Philosophy in the 16th & 17th Centuries (New York: The Macmillan Company, 1935): 167.
[64] Ibid., 168.
[65] Bennett, Divided, 64.

worked together to measure small angular distances and apparent diameters of planets.[66] The hairs (or metal bars) were parallel to each other, and there was some type of internal scale which measured angular widths.

As with telescopes and graduated measuring instruments, the use of telescopic sights on measuring instruments generated its own set of problems. First, by using telescopes to distinguish angular measurement, defects were magnified as a whole.[67] Furthermore, the use of these measuring instruments meant that errors in the division of the instrument, the wearing down of component parts, and the warping of the whole frame affected measurement.[68] Therefore, there were limitations to the accuracy one could achieve using these instruments. Nevertheless, some of the difficulties in graduation were partly improved by constructing instruments of larger radii, ensuring that the divisions would be more visible. Moreover, telescopes increased the magnification, and improved the definition, of the target objects.[69]

The Micrometer

Gascoigne has been credited as the inventor of the micrometer, a device he had developed by 1640. Born about

---

66 Ibid.

67 King, History, 102.

68 Ibid.

69 Ibid.

1620, he died in 1644 when he fell with other loyal subjects of Charles I at the battle of Marston Moor.[70] Yet the role he played in precision astronomy was profound. He has the distinction of being the first to discover the improvement afforded to observational astronomy by the use of cross-hairs,[71] a discovery which led to his invention of the micrometer.

In common to both cross-hairs and micrometers was the reticule, which "constituted the starting point of the true telescopic sights and filar micrometer."[72] Gascoigne came up with the idea when he observed spider hairs in his telescope. His cross-hairs (and other early cross-hairs) were usually made of hair or textile thread because they were both economical and readily available.[73] Furthermore, as Gascoigne undoubtedly discovered, cross-hairs had to be visible enough to accurately center a celestial object, but at the same time, "the thickness could not be so great that they covered the object viewed, nor could the illumination be so strong so as to make faint objects invisible."[74] Because the hairs were

---

[70] S.J. Rigaud, ed., Correspondence of Scientific Men of the Seventeenth Century, Vol. 1 (Hildesheim, Germany: Georg Olms Verlagsbuchandlung, 1965), 34.
[71] Allan Chapman, Dividing the Circle: The Development of Critical Angular Measurement in Astronomy, 1500-1850 (London: Ellis Horwood Limited, 1990), 36.
[72] Ibid.
[73] Jon Darius and P.K. Thomas, "Crosswires in a Guiding Eyepiece," Journal of Physics, E: Scientific Instruments 14 (1981): 761.
[74] Ibid.

sharply defined (they were in the same focal plane as the objects he viewed), he began using them in his observations.[75]

The evidence for Gascoigne's accidental discovery, came chiefly from two letters he sent William Oughtred (1575-1660). In the first, dated December 2, 1640, Gascoigne informed Oughtred that he had "either found out, or stumbled" onto an invention

> whereby the distance between any the least stars, visible only by a perspective glass, may be readily given. . . to a second; affording the diminutions and augmentations of the planets strangely precise, as also their centres. . . and the inclination of one star to another of a well known site; and able to bring sufficient aid to your aged eyes to find all requisites. . . . It is a novelty, capable of such frequent use, that before it travel to other able judges, may I receive that favour it shall undergo your experiment and censure?[76]

Eventually, cross-hairs led to the development of the micrometer, and in a subsequent letter, he revealed a detailed description and diagram of this new mechanical device:

> I believe also by a ruler with a hair in it, moving upon the centre of a circular instrument graduated with transversal lines

---

[75] This occurred either in late 1638 or early 1639 [Randall Brooks, "The Development of Micrometers in the Seventeenth, Eighteenth and Nineteenth Centuries," Journal for the History of Astronomy 22 (1991): 129-130; and Turner, Early, 133]. He only used "astronomical" telescopes, as they were the only type of telescopes that could clearly focus the image of the hairs within the tube [See Albert Van Helden, Measuring the Universe: Cosmic Dimensions from Aristarchus to Halley (Chicago: University of Chicago Press, 1985), 119; Van Helden, "Astronomical," 29; and Bennett, Divided, 64].

[76] (Emphasis mine) Rigaud, Correspondence, Vol. 1, 33-34. Note the emphasis Gascoigne places on the preciseness of his invention.

> and two glasses. . . we might in a few nights find the true meridian and pole [of two objects].[77]

Gascoigne also suggested that if it was too dark to see the hairs, one could shine a candle through the side of the tube, without the light interfering with the instrument. Finally, he told Oughtred that he had shown his "internal" scale "and its use in a glass," to others who, apparently, were surprised, because they thought that all possible means for taking measurements had been exhausted (one particular friend of his could not keep the invention a secret).[78]

Before continuing with the development of the micrometer, it is first necessary to describe what the "micrometer" consisted of during these early stages. Initially, the micrometer referred to two separate instruments which, eventually, merged: a pair of reticule wires or plates, and screws with turning parts that were used to measure small distances.[79] Micrometers were usually small (only about six inches in length), and they were easily inserted into the tubes of "astronomical" telescopes, near the eyelens.

According to descriptions in his correspondence, Gascoigne's micrometer consisted of

> two thin pieces of metal [which] were mounted parallel to each other on a screw of fine

---

[77] February, 1641, Ibid., 51.
[78] Ibid., 54.
[79] Chapman, Dividing, 40-41.

> pitch fixed so that the knife-edges were always equidistant from the optical axis of the telescope. The two blades could be opened by means of the screw, the number of revolutions needed to attain a required opening being shown on a scale and fractions of a revolution on a dial divided into a hundred parts.[80]

Unlike subsequent micrometers, Gascoigne used two screws on both sides of his device -- each screw moved its own reticule either towards or away from the center axis in the field of view.[81]

Despite Gascoigne's premature death, a small group of his friends, including Oughtred and Richard Towneley (1629-1707) knew about his invention through his correspondence with them. However, they did not immediately publicize his work and inventions. In fact, the invention of the micrometer did not appear in print until 1659, when Christian Huygens published his System Saturnia, in which he described a variation of Gascoigne's micrometer which he had developed. Instead of knife-edge metal pieces, Huygens used a "virgula" -- a solid metal bar which covered the target object, or aligned with the edges of two objects (Figure 4).[82]

Following Huygens' publication, other micrometers were designed by various individuals: Cornelio Malvasia suggested "a grid network of squares formed by fine silver wire," Eustachio Divini redesigned a micrometer of "cross wires in

---

[80] Turner, Early, 133.

[81] Chapman, Dividing, 41; and Brooks, "Development," 131.

[82] Turner, Early, 133.

focus," and Robert Hooke developed his hair micrometer which used hairs in place of thin metal bars (Figure 4).[83] Even Towneley, who initially learned about Gascoigne's micrometer from his correspondence, constructed his own micrometer, which most closely resembled Gascoigne's micrometer (Figure 5). The only substantial difference between the two, was that Towneley used only one screw that moved one reticule, while the other remained fixed.[84]

It was not until the late 1660's, after the French had developed Huygens' micrometer, and as a result of a priority dispute, that the "world of science" finally "learn[ed] about Gascoigne's micrometer."[85] Adrien Auzout (1622-1691) first disclosed his micrometer invention in a letter to Henry Oldenburg (c.1618-1677), dated December 18, 1666, in which he informed Oldenburg of his novel use of a filar micrometer:

> I applyd myself the last Summer to take ye Diameters of ye Sun, ye Moon, and ye other Planets, by a method, yt one M. [Jean] Picard [1620-1682] and I have, wch I believe to be ye best of all those, yt have been practis'd hitherto, seeing we can take ye Diameters to seconds, being able to divide one foot into 24000 or 30000. parts, scarce failing as much as in one only part, so yt we can in a manner be <u>assured</u>, not to deceive ourselves in 3. or 4. seconds.[86]

---

83 Ibid.

84 Brooks, "Development," 131.

85 Van Helden, <u>Measuring</u>, 119; and Turner, <u>Early</u>, 133.

86 A. Rupert Hall and Marie Boas Hall, eds. <u>The Correspondence of Henry Oldenburg</u>, Vol. 3, 1666-1667 (Madison, WI: The University of Wisconsin Press, 1965), 297.

When this letter was read before the Royal Society, Hooke and Christopher Wren (1632-1723) declared that Picard and Auzout's method was not new since both Hooke and Wren were already aware of these methods. However, Hooke and Wren did not seem to inform the French of their own methods for measuring the apparent diameters of planets. Therefore, the French did not learn about the work of the English until an extract of Auzout's letter was published in the Philosophical Transactions of the Royal Society.[87] Auzout's letter "stimulated Towneley to send in an account of Gascoigne's device as improved by Towneley himself."[88] That same year, Auzout developed a micrometer which was a variation of Pierre Petit's. It consisted of a series of parallel hairs (or tapered metal bars) on two grids (one fixed, the other moving) (Figure 6).[89] After Auzout's letter, the use of micrometers for measuring small angular distances became widely known, and many astronomers attempted to design their own devices.[90]

Before the introduction of the micrometer, telescopes could neither measure the apparent diameters of celestial

[87] Ibid., 299 ff. 6. The extract was published in Vol. 1, no. 21, 373.
[88] Turner, Early, 133. This was an extract of Towneley's letter to William Croune (1633-1684), written sometime before May 6, 1667. It was published in Vol. 2, no. 25, 457-458 of the Philosophical Transactions.
[89] Auzout's micrometer became standard in France, but did not "catch so quick in England" [Robert McKeon, "Les Debuts de l'Astronomie de Precision I. Histoire de la Realisation du Micrometre Astronomique," Physis: Rivista di Storia delle Scienza, 13 (1971): 284]. This was not surprising considering the English favored Gascoigne's priority over the micrometer's invention.
[90] Brooks, "Development," 131, 135.

bodies, nor could they measure the angle subtended by two objects.[91] Before Gascoigne's invention came into use, "the measuring of apparent diameters [specifically], was much more an art than a science," as measurements did not necessarily become more accurate with time (accuracy depended on the observer's abilities).[92] Therefore, the micrometer eventually changed the methods of observation.

Telescopic sights

As micrometers profoundly affected precision astronomy, so, too, did telescopic sights. The improvement afforded by these sights "probably [constituted] the single most important technical advance in precision instrumentation in the seventeenth century."[93] Although open sights on traditional measuring instruments had been popular until the seventeenth century, these instruments were limited by the resolving power of the human eye, which is only about one minute of arc.[94] Besides the increase in precision afforded by telescopic lenses, they also provided a more precise line of collimation (the alignment of cross-hairs with the optical

---

[91] See Chapman, Dividing, 42.

[92] Van Helden, Measuring, 121. John Flamsteed (1646-1719) was so impressed with the micrometer, that he informed William Molyneux (1656-1698) that the "first valuable measurements he had ever made were taken when he was given a micrometer" [Lesley Murdin, Under Newton's Shadow: Astronomical Practices in the Seventeenth Century (Bristol: Adam Hilger Limited, 1985), 121].

[93] Bennett, Divided, 63.

[94] Turner, Early, 87-88.

center of the target object, or objects).[95] This method ensured that the image in the field of view and the cross-hairs, were both in focus, and superimposed one over the other. Aligning the line of collimation was more of a problem for naked-eye sights because there were no cross-hairs to superimpose over the image; the eye had to alternately focus between the near sights and the distant objects, making human error more probable.[96]

Gascoigne was the first to successfully combine telescopic sights with traditional measuring instruments, something he performed around the same year he invented the micrometer (~1640).[97] Even though Gascoigne was the first to successfully make the substitution, Jean-Baptiste Morin (1583-1656), suggested replacing open sights with telescopic sights as early as 1634.[98] Morin was not successful, however, because he used a Galilean, rather than an "astronomical", telescope, and it was necessary to use two convex lenses in

---

95 Wolf, History, 167-168.

96 Ibid. See also Robert McKeon, "Le renouvellement de l'astronomie de précision de Tycho Brahe à Jean Picard. Des pinnules aux appareils de visée optique," in Jean Picard et les Débuts de l'Astronomie de Précision au XVI Siècle, ed. Guy Picolet (Paris: Centre National de la Recherché Scientifique, 1987), 121-122.

97 J.G. Simms, William Molyneux of Dublin: A Life of the Seventeenth-Century Political Writer & Scientist, ed. P.H. Kelly (Dublin: Irish Academic Press, 1982), 61; Robert McKeon, "Les Debuts de l'Astronomie de Precision II. Histoire de l'acquisition des instruments d'astronomie et de géodesie munis d'appareils de visée optique," Physis: Rivista di Storia delle Scienza 14 (1972): 227; and Chapman, Dividing, 40.

98 Maurice Daumas, Scientific Instruments of the Seventeenth and Eighteenth Centuries, trans. and ed. Mary Holbrook (New York: Praeger Publishers, 1972), 49; Turner, Early, 137; and McKeon, "Les Debuts (Part II)," 224.

order to have a common focus between the object glass and eyelens.[99]

Like the micrometer, the diffusion of Gascoigne's discovery of telescopic sights was limited -- perhaps even more so since he did not share the results of his discovery with Oughtred. Consequently, Gascoigne's invention was temporarily forgotten until 1665 when Towneley reintroduced Gascoigne's work.[100] There is evidence, however, that Hooke and Wren began experimenting with telescopic sights as early as 1665.[101] Telescopic sights were also independently rediscovered by Divini, who in a letter dated July 15, 1663, described a measuring instrument mounted with telescopic sights, of his own design, which he constructed for a friend looking for an instrument to aid him in his surveying.[102]

The manner in which the discovery of telescopic sights paralleled the invention of the micrometer is most apparent when another priority dispute between the French and the English erupted in the late 1660's. As in the case of the micrometer, Auzout and Picard "formally introduced" their

---

[99] Daumas, Scientific, 49. As in the case of micrometers, only "astronomical" telescopes could be used on measuring instruments. Inevitably, the Galilean telescope was abandoned in precision astronomy.
[100] McKeon, "Les Debuts (Part II)," 228.
[101] Ibid., 229; and Chapman, Dividing, 37. Hooke claimed as much in his Animadversions on the First Part of the 'Machina Coelestis' of. . . Johannes Hevelius (London, 1674).
[102] Ibid. It is interesting to note the progression of discovery, from micrometer to telescopic sights, that is evident in both Gascoigne and Divini's cases. The application of telescopic lenses on astronomical measuring instruments immediately followed the micrometer's invention. (Ibid.)

independent discovery of telescopic sights on graduated measuring instruments to scientific circles in 1666-7, in an effort to improve the limited accuracy of open sights.[103]

In 1717, William Derham (1657-1735) responded to the Frenchmen's claims of having discovered telescopic sights, in the Philosophical Transactions. Derham felt he was "Duty bound, to do that young but ingenious Gentleman, Mr. Gascoigne, the Justice, to assert his invention to him."[104] Towneley, he claimed, sufficiently proved that the invention of the micrometer was Gascoigne's and not Auzout or Picard's.[105] Derham added that "Gascoigne was the first that measured the Diameters of the Planets, &c. by a Micrometer," and "that he was the first that applied Telescopick Sights to Astronomical Instruments."[106] Derham referred to two letters written by Gascoigne to William Crabtree (1610-1644), wherein Gascoigne described his micrometer and the application of "measuring glasses" to both quadrants and sextants.[107] Derham then concluded:

> To these I could have added other Passages of the like Nature: but these may be sufficient, to shew that Mr. Gascoigne, as early as 1640,

---

[103] Eric G. Forbes, Greenwich Observatory, Vol. 1, Origins and Early History (1675-1835) (London: Taylor and Francis Limited, 1975), 6; and John W. Olmsted, "The 'Application' of Telescopes to Astronomical Instruments, 1667-1669," Isis 40 (1949): 224-225.

[104] Philosophical Transactions, Vol. 30, no. 352, 603.

[105] Auzout, Picard, and others conceded that Gascoigne was the first to invent the micrometer. Derham, who provided similar proof for telescopic sights, likewise convinced others that Gascoigne had discovered them.

[106] Philosophical Transactions, Vol. 30, no. 352, 604.

[107] 25 January 1641 and 24 December 1641, Ibid., 604-605.

> made use of Telescopes on Quadrants and Sextants, as well as in his Invention of the Micrometer. . . . It is very manifest, that long before the French Gentleman's Claims, our Countryman Mr. Gascoigne had made use of those Sights in his Astronomical Instruments; particularly in two or more sorts of Micrometers (as I plainly find) and in his Quadrant and Sextant. And had it pleased God to have given him a longer Life, we might have expected greater things from his pregnant and sagacious wit.[108]

After the introduction of cross-hairs, micrometers, and telescopic sights, all of which may be credited to Gascoigne, the precision of measurements in observation increased from approximately one minute, to one second, of arc.[109] Precise measurements of apparent diameters of planets, orbital calculations of comets, and the angles subtended by two celestial objects, were considered reliable only if one used either a micrometer-mounted telescope, or a measuring instrument with telescopic lenses and cross-hairs.[110] Telescopically-mounted measuring instruments were even more precise when micrometers were added, a method which was eventually employed by Flamsteed, Cassini, and Picard among others.[111]

---

108 Unfortunately, he died prematurely (Ibid., 609-610). Hooke also vehemently supported his "countryman's" invention when Cassini claimed that the French had invented telescopic sights and micrometers before the English ["Dr. Hook's Answer to some particular Claims of Mons. Cassini's, in his Original and Progress of Astronomy," in Robert Hooke, Philosophical Experiments and Observations, ed. W. Derham (London, 1726; reprint, London: Frank Cass and Company Limited, 1967), 391].

109 Olmsted, "Application," 215.

110 Eric G. Forbes, "The Comet of 1680-1681," in Standing on the Shoulders of Giants: A Longer View of Newton and Halley, ed. Norman J.W. Thrower (Berkeley, CA: University of California Press, 1990), 319.

111 Ibid.

In both the case of the micrometer and telescopic sights, the instruments were independently discovered by the French (Picard, Auzout) and the English (Gascoigne, Wren, Hooke) (Figure 7). Since each side knew it had arrived at the discoveries independently, and because "the institutionalized stakes of reputation are high and the joy of discovery immense,"[112] controversies across national lines inevitably developed. As there was more of an emphasis on the development of instrumentation in the second half of the seventeenth century, there were more chances that other instruments would be independently discovered, thereby creating an unstable environment which usually led to controversy, a topic which will be explored in the next chapter.[113]

---

112 Merton, "Priorities," 314.

113 Although the independent discovery of inventions can lead to controversy, it is not the only cause of disputes.

Chapter 2
Controversies in the Second Half of the Seventeenth Century

The Role of the Royal Society in Controversies

After The Royal Society of London for Improving Natural Knowledge chartered in 1662,[114] Thomas Sprat listed its three goals: collecting data, deciding upon "matters of fact" by relying on the authority of numbers, and "establishing conclusion," a task in which the members were "not yet very daring."[115] One of the most essential factors for the success of the Royal Society, was the communication and diffusion of knowledge, a process which ensured that information was not only gathered, but also shared with the rest of the scientific community.[116] Members knew that letters to Henry Oldenburg, the secretary of the Royal Society, became public property since they were eventually published as articles in

---

[114] Marie Boas Hall, "The Royal Society's Role in the Diffusion of Information in the Seventeenth Century," Notes and Records of the Royal Society of London 29 (1975): 173.

[115] Thomas Sprat, History of the Royal Society, eds. J.I. Cope and H.W. Jones (St. Louis: Washington University Press, 1958), 95-100, 107; cited in Noriss S. Hetherington, "The Hevelius-Auzout Controversy," Notes and Records of the Royal Society of London 27 (1972): 103.

[116] M.B. Hall, "Royal Society's Role," 175. It was only through this time-consuming correspondence that scientists were aware of what was going on elsewhere in scientific circles (Ibid., 176).

the Philosophical Transactions.[117] Therefore, scientists always wrote to Oldenburg when they wanted "to be sure that the attention of the Society as a whole was drawn to their news."[118]

From the years 1662 to 1677, Oldenburg became involved with a variety of duties:

> [He] acted as a centre for the communication of scientific news, searching out new sources of information, encouraging men everywhere to make their work public, acting as intermediary between scientists and, through the Philosophical Transactions, providing a medium for the publication of short scientific papers.[119]

Since scientists knew that letters to Oldenburg became public property, they realized that, in scientific matters, they wrote to Oldenburg in his capacity as secretary of the Royal Society, and not as a personal friend.[120] As the intermediary through which this correspondence passed, and as the editor and publisher of the Philosophical Transactions, Oldenburg was in a strong, yet at the same time, "vulnerable", position, because "in relaying discoveries and inventions to the public, he constantly ran the risk of being accused of lack of patriotism by members like Wallis [and Hooke] who

---

117 Hall and Hall, Correspondence, Vol. 2 (1663-1665), xxii.

118 Marie Boas Hall, "Oldenburg and the Art of Scientific Communication," British Journal for the History of Science 2 (1965): 285.

119 Ibid., 277. The Philosophical Transactions was founded in 1665, and was Oldenburg's own "private venture" as he conceived, edited, and published it alone. It became especially popular with those who could not regularly attend Royal Society meetings, and those abroad (M.B. Hall, "Royal Society's Role," 184-186).

120 Iliffe, "In the Warehouse," 39.

were extremely sensitive to issues of international property rights.[121]

Oldenburg believed that the best way to advance science, was by having scientists compare and criticize each other's ideas, and correspondence afforded that opportunity because stating one's views publicly let others know what that particular individual was doing.[122] Oldenburg attempted the following method: first, "let X know Y's scientific ideas, but also let X know what Y thought of X's ideas" -- this would provoke X to answer Y's criticisms, and to further develop X's own theory (the Royal Society would act as the adjudicator in the exchange of correspondence between the two).[123] Although this was the usual format for the exchange of information, the tactics used in resolving disputes usually depended upon the social status of participants and the "contingent local situation of the debate."[124]

The specific task of the Royal Society in disputes, was to "police behaviour" over discoveries and new inventions, thereby forestalling, "in principle," ungentlemanly behavior between members of the Royal Society -- though controversies continuously occurred.[125] Furthermore, the social status and

---

[121] Ibid., 34-35. Even Boyle had warned him in 1667 to "proceed carefully in order to avoid charges of plagiarism." Personal disputes could be avoided if Oldenburg was more cautious endorsing letters (Ibid., 35).
[122] Hall and Hall, Correspondence, Vol. 2, xxii-xxiii; and M.B. Hall, "Oldenburg," 287.
[123] Ibid. (for both sources)
[124] Iliffe, "In the Warehouse," 53.
[125] Ibid., 36.

manners of the participants mattered as much as any feature of a discovery or invention.[126] The Royal Society's function as adjudicator, was a distinction recognized not only by its members in England, but also foreigners (whether they were members or not) who regarded the Royal Society "as the ultimate tribunal for arbitration in matters of scientific achievement and the co-ordinator of all news in the world of science."[127]

Ultimately, the Royal Society acted as arbiter, not only for English, but also for international, priority disputes and controversies. In fact, the number of international controversies equaled, if not surpassed, the number of English controversies. National priority claims became a central issue in any dispute which crossed international boundaries:

> In a world made up of national states, each with its own share of ethnocentrism, the new discovery redounds to the credit of the discoverer not as an individual only, but also as a national. From at least the seventeenth century, Britons, Frenchmen, Germans, Dutchmen, and Italians have urged their country's claims to priority.[128]

Ideally, the Royal Society needed to demonstrate a non-partisan attitude in all international controversies, but by

---

[126] "Questions concerning honour revolved around disputes about truth" (Ibid., 54).

[127] M.B. Hall, "Royal Society's Role," 184.

[128] Merton, "Priorities," 296. One is reminded of the two priority disputes over the micrometer and telescopic sights which transpired between the English and French in the 1660's.

the mid-1660's, even Oldenburg sided with the English, and was eager to defend English priorities.[129]

The Hooke-Auzout Controversy over the Lens-Grinding Machine

In the early 1660's, Oldenburg successfully served as the intermediary in a minor controversy between Robert Hooke and Adrien Auzout over the issue concerning a lens-grinding machine of Hooke's design.[130] The prescribed practice of methodically replying to another's criticisms worked especially well in this particular controversy, and the whole matter was so politely handled, that even Hooke was not offended.[131] In order to understand Hooke's emphasis on the capabilities of his lens-grinding machine, it is necessary to briefly explain his relationship with mechanical devices, and his attitude in disputes.

Since he was a child, Hooke was always involved in instrument-making, eventually adding to, or modifying, instruments (he was the first to use hairs in a micrometer).[132] Moreover, he was a "propagandist of new

---

[129] Hall and Hall, Correspondence, Vol. 3, xxv; and M.B. Hall, "Oldenburg," 287. This does not necessarily suggest that Oldenburg always sided with the English, regardless of the issues concerned.

[130] Hall and Hall, Correspondence, Vol. 2, xxii.

[131] M.B. Hall, "Oldenburg," 287. However, it helped that Oldenburg was on Hooke's side.

[132] Richard S. Westfall, "Robert Hooke," Dictionary of Scientific Biography, Vol. 6, ed. Charles Coulston Gillispie (New York: Charles Scribner's Sons, 1970), 487.

instrumentation," and had a "commitment to progress through [this] instrumentation."[133] The "one crucial theme" which affected almost every single controversy he became involved with, was "his lifelong stress on the value of instruments in enhancing human ability to observe and measure natural phenomena."[134] Without instruments to extend the human senses and measure phenomena, the progress of science would slow down and, ultimately, scientists would exhaust all possible means of observation.

More than any other individual during the seventeenth century, Hooke's irascible and jealous temperament contributed extensively to his behavior towards others.[135] He was described as the "universal claimant" because, unlike other scientists, "there was scarcely a discovery in his time which he did not conceive himself to claim."[136] Not surprisingly, Hooke became involved in endless priority disputes and controversies. His custom in such cases was to "defend his interests," and insist "that his originality be publicly acknowledged."[137]

---

133 Michael Hunter and Simon Schaffer, "Introduction," in Robert Hooke: New Studies, eds. Michael Hunter and Simon Schaffer (Woodbridge, Suffolk: The Boydell Press, 1989), 8, 19.
134 Ibid., 17.
135 Béziat contends that his attitude had something to do with his childhood infirmities and the fact that he was a hunchback [L.C. Béziat, "La vie et les travaux de Jean Hévélius," Bulletino di Bibliographia e di Storia delle Scieze Matematiche e Fisiche 8 (1875): 613].
136 Merton, "Priority Disputes," 287.
137 Steven Shapin, "Who was Robert Hooke?" in Hunter and Schaffer, Robert Hooke, 274-275. Although this response did not greatly differ from the response of other participants in controversies, his irascible behavior almost always accompanied his rebuttals. In Hooke's heated

In the Preface to his Micrographia (1665), Hooke described a lens-grinding machine which could create a lens of any size radius -- a machine which he never built. Several months after the publication of his book, Auzout, in an epistle against Guiseppe Campani, mentioned his own arguments against Hooke's proposal, claiming that Hooke should not promote a machine without trying it first. In addition to the difficulties involved in trying to perfect such a machine, Auzout doubted its ability to make lenses with focal lengths of 1,000 or even 10,000 feet, and he emphasized how one could not even find a good lens of 30 feet focal length.[138]

Hooke responded to Auzout's objections, point by point, in an agreeable manner.[139] When Oldenburg acquainted Auzout with Hooke's reply, Auzout responded with a new letter to Oldenburg in which he, too, addressed each of Hooke's counter-arguments. He insisted that the only effective way Hooke can respond to Auzout's objections, was by building the machine:

---

controversy with Christian Huygens, Huygens eventually lost faith in the Royal Society's ability to successfully adjudicate the matter, and he stopped acting like a gentleman when he realized Hooke never did, nor intended to (Iliffe, "In the Warehouse," 44).

[138] Hall and Hall, Correspondence, Vol. 2, 388-389 n. 2. Auzout stated, "I was most surprised. . . to see that he had published it on [the basis of] a simple theory, without having tried it in great or in small" (Ibid., 389 n. 3). Hooke responded, "It was not meer Theory I propounded, but somewhat of History and matter of Fact," as he claimed that he had tried building this machine (Hooke to Oldenburg, ? May 1665, Hall and Hall, Correspondence, Vol. 2, 383).

[139] See Hooke to Oldenburg, ? May 1665, Hall and Hall, Correspondence, Vol. 2, 383-389.

> I think that when doubts are raised about the working of a machine it is not enough to try to reply to the reasons given for these doubts; since the question is whether it works or not, the only thing to do to silence those who raise doubts is to show them how it can be made to work, and any other reply is in danger of being useless. . . . He will please forgive me then if I continue to doubt the worth of his machine in spite of his reply and if I wait until he has made it work before retracting what I said in my comments.[140]

The cordial exchange between Hooke and Auzout is apparent in Oldenburg's reply to Auzout in which he once more listed Hooke's further reflections upon Auzout's arguments:

> Mr. Hooke salutes you, and affirms that he is very particularly obliged to you for your conduct towards him, in the letter you addressed to me. Surely, Sir, it is indeed the <u>right way to manage a correspondence between two worthy men and fine minds, when each expresses to the other his thoughts and discoveries in a frank and polite way, without offence given or taken</u>, so that their minds may reciprocally stimulate each other and learn from each other, to the further progress of knowledge.[141]

Oldenburg, who translated Auzout's last letter for Hooke's benefit, included certain marginal comments on Hooke's behalf, which were "evidently intended to urge Hooke to a further reply."[142] Hooke, however, terminated the exchange and the Hooke-Auzout controversy ended on friendly terms.[143]

---

140 22 June 1665, Ibid., 419.

141 (Emphasis mine) 23 July 1665, Ibid., 441-442. Here, it is most apparent that Oldenburg's prescribed behavior for replying to another's criticisms worked.

142 Auzout to Oldenburg, 12 August 1665, Ibid., 468-474.

143 As Hooke never built this machine, Auzout's position was considered "sounder" (Ibid., Vol. 3, xxiv). Hooke and Auzout eventually became

The Hevelius-Auzout Controversy over Cometary Positions

The diffusion of scientific information through the Royal Society would not have been complete without the input from foreign scientists. Oldenburg realized the importance of initiating correspondence with scientists in other countries, and sometimes, he undertook such correspondence without the approval of the Society's Council.[144] Oldenburg hoped to initiate a correspondence with Johannes Hevelius of Danzig -- one of Europe's most renowned and revered astronomers since the 1630's, and the Council instructed him to initiate such an exchange in February 1663.[145]

In his first letter, Oldenburg informed Hevelius of the goals of the Society and the importance of correspondence:

> For it is now our business. . . to attract to the same purposes men from all parts of the world who are famous for their learning, and to exhort those already engaged upon them to unwearied efforts. Indeed, friendship among learned men is a great aid to the investigation and elucidation of the truth; if such friendship could be spread through the whole world of learning, and established among those whose minds are unfettered and above partisan zeal, because of their devotion to truth and human welfare, philosophy would be raised to its greatest heights. This our

involved in the priority disputes over telescopic sights and micrometers mentioned in the first chapter.

[144] M.B. Hall, "Oldenburg," 285.

[145] Ibid., 286. Oldenburg had already prepared the letter which he sent only after the Council granted him permission to do so.

> Fellows are striving for with all their might and for that reason they are developing a wider correspondence with those who philosophize truly, that is, (in my opinion) with those who read the Book of the World. . . . I think that you, famous Sir, will not by any means stand aside from this work and that. . . you will soar into the heavens and bring us news thence now and again; for by this the science of the stars continually advances.[146]

In return for Hevelius' information on observations, and copies of his books, Oldenburg sent him news of English observations and current news about science in general.[147] Oldenburg's correspondence with Hevelius lasted for fourteen years, until Oldenburg's death in 1677, and it comprised of over 100 letters, resulting in a growing friendship between the two.

As in the case of the Hooke-Auzout controversy, the exchange of letters concerning the Hevelius-Auzout controversy filtered through Oldenburg, although in both cases, the French exchanged correspondence with non-Englishmen as well. The prescribed behavior for replying to another's criticisms also worked in this case, despite a brief period of animosity between the participants.[148] The controversy began when Hevelius rejected, in his Prodromus Cometicus (1665), the Auzout/Cassini hypothesis that the comet of 1665 "moved in a circle, and hence was a regularly

---

[146] 18 February 1663, Hall and Hall, Correspondence, Vol. 2, 27-28. See also M.B. Hall, "Royal Society's Role," 180.
[147] M.B. Hall, "Royal Society's Role," 180-181.
[148] M.B. Hall, "Oldenburg," 287.

recurrent star."[149] The major issue that became the center of the controversy, was that Hevelius claimed he had seen the comet on February 18 in a position not consistent with Auzout and Cassini's orbit, and Auzout charged that Hevelius' observations were false, since he and others had seen the comet 1°17′ distant from Hevelius' position.[150]

The first intimation that Hevelius positioned the comet in an unlikely location, was made by Pierre Petit (c.1594-1677) who "found it odd that [Hevelius] has made [the comet] pass through a point where it never was," and that he observed it only in February, whereas others observed it in March as well.[151] Auzout felt inclined to inform the public of Hevelius' errors, by printing his letter to Petit on the matter -- although Auzout was confident that the Royal Society, which possessed a copy of Hevelius' Prodromus, would soon pass judgment on the matter.[152]

As arbiter, the Royal Society hoped that by comparing Auzout and Hevelius' observations with those made by Englishmen, it would be in a better position to "discern where the mistake lies; and having discern'd it, will certainly be found highly impartial and ingenuous in giving

---

149 Hall and Hall, Correspondence, Vol. 2, 407-408 n. 8.
150 Ibid. See also Hetherington, "Hevelius-Auzout," 104.
151 Oldenburg to Robert Boyle, c. 16-18 June 1665, Hall and Hall, Correspondence, Vol. 2, 406.
152 Auzout to Oldenburg, 22 June 1665, Ibid., 425-426.

their sense of the same."[153] By June 28, 1665, astronomers in the Royal Society endorsed Auzout's position.[154] Taking all possibilities into account, the astronomers attempted to explain the discrepancies in the cometary positions observed by the participants of the controversy, by hypothesizing that there were two comets instead of one.[155]

When the anonymous group of Royal Society astronomers finally made their report, they concluded that whatever Hevelius had seen on the night of February 18, it was not the comet observed by Auzout and Cassini, and that Auzout's observations had been more reliable than Hevelius'.[156] The group found

> such an unanimous consent in what has been just now declared, & the Controversie being about Matter of fact, wherein Authority, Number, and Reputation must cast the Ballance Mons. Hevelius, who is as well known for his Ingenuity, as Learning, will joyn and acquiesce in that Sentiment.[157]

Oldenburg informed Hevelius of the Society's decision in a letter dated January 24, 1666:

---

[153] Philosophical Transactions, Vol. 1, no. 3, 36-40. See also Hetherington, "Hevelius-Auzout," 104-105.

[154] Thomas Birch, The History of the Royal Society of London: For Improving of Natural Knowledge from its First Rise, Vol. 2, The Sources of Science Series, ed. Harry Woolf, no. 44 (New York: Johnson Reprint Corporation, 1968), 63.

[155] "All the astronomers known to me believe that there were not one but two comets. . ." (Oldenburg to Robert Moray (c.1608-1673), 12 October 1665, Hall and Hall Correspondence, Vol. 2, 568). See also Iliffe, "In the Warehouse," 40.

[156] Hall and Hall, Correspondence, Vol. 2, xxii-xxiii; and Hetherington, "Hevelius-Auzout," 105.

[157] Philosophical Transactions, Vol. 1, no. 9, 150-151; also cited in Hetherington, "Hevelius-Auzout," 105.

> These astronomers were unanimous in their opinion that the comet did not by any means draw near to [your proposed position]. . . . Although what I here write to you is not the decision of the Society proper (for indeed its meetings have not yet been resumed), it is that of those men to whom the Society would have delegated the consideration of this case, without any shadow of doubt, if it had been in session, and from whose judgment in matters of this kind it would hardly have dissented.[158]

Despite Oldenburg's tact, however, Hevelius was disappointed that he was "condemned without a hearing by some of the chief astronomers of the Royal Society," especially since he was ready to vindicate himself against Auzout.[159]

In his final letter on the matter, Auzout claimed that he had little time to consider the dispute any further, and he had put it aside because of his "lack of interest,"[160] a disinclination which became manifest when he refused to respond. After this became evident, Oldenburg had to make excuses for Auzout's lack of enthusiasm in the matter, and he informed Hevelius that Auzout delayed because of his "poor health" and "press of business" in forming the Royal Academy of Sciences in Paris.[161] Hevelius did not appreciate Auzout's indifference, but he conceded to the judgment of the Royal Society.[162]

---

158 8 May 1666, Hall and Hall, Correspondence, Vol. 3, 30.
159 Hevelius to Oldenburg, 23 June 1666, Ibid., 171-172.
160 Auzout to Oldenburg, 18 December 1666, Ibid., 298.
161 31 January 1668, Ibid., Vol. 4, 137. This was a long delay!
162 Hevelius to Oldenburg, 3 June 1668, Ibid., 447-448.

The controversies between Hooke and Auzout, and Hevelius and Auzout, demonstrate the role of Oldenburg and the Royal Society in the diffusion of scientific knowledge by comparing and contrasting differing opinions, and provide examples of disputes which did not deviate from the prescribed rules of behavior. However, both controversies also prepared the background for the controversy which developed between Hevelius and Hooke over the relative merits of telescopic versus naked-eye sights. In his dispute with Auzout, Hooke emphasized the ability of a machine to extend human capabilities, an aspect of instrumentation that was necessary to discover the secrets of nature. Hooke's strong belief in the power of instruments carried over into the controversy with Hevelius. Hevelius, in his dispute with Auzout, conceded to the final decision of the Royal Society, regardless of their judgment against him. Therefore, he trusted its authority, and viewed it as a collective in which disputes could be properly adjudicated. When the controversy erupted between Hevelius and Hooke, Hevelius pleaded his case with the Royal Society once more, in the hopes that it would resolve the issue.

Chapter 3
The Controversy Between Hevelius and Hooke

Astronomical Measurement before the Controversy

As uses for the telescope expanded during the seventeenth century, astronomers realized that telescopes could not only be used for observational purposes, but also for measurement. Tycho Brahe was one of the first astronomers in Europe to use astronomical measuring instruments to observe celestial phenomena, improving his observations by constructing larger instruments, such as a 19-foot quadrant.[163] In obtaining his measurements, however, Tycho used a pointer to measure angles, minutes, and seconds of arc. By 1631, Pierre Vernier (1584-1638) improved upon Tycho's method by replacing the pointer with a small scale which was placed edge to edge with the larger scale on which the measurements were read.[164] By the early 1640's, William Gascoigne had discovered not only the micrometer, but also the advantages of using telescopic sights on graduated measuring instruments, two inventions which were not fully

---

[163] King, History, 16.
[164] Ibid., 94.

exploited until the late 1660's by several French astronomers including Adrien Auzout and Jean Picard.

By the middle of the seventeenth century, astronomers used their various measuring instruments to compute the precise positions of comets, and Mercury and Venus' transits across the sun. They slowly realized the great potential of replacing the more traditional open sights with telescopic sights on measuring instruments, such as quadrants and sextants, because, as Picard and John Flamsteed believed, "telescopes mounted on astronomical measuring instruments, [improved] the accuracy of positional astronomy."[165]

Although most European astronomers used sextants and quadrants mounted with telescopic sights by the late seventeenth century, the preference for telescopic sights over open sights became a controversial issue in the late 1660's when certain individuals challenged each other's position. This "watershed in positional astronomy"[166] involved Robert Hooke in London, and Johannes Hevelius in Danzig, with the latter remaining a staunch supporter of open sights.

Hevelius' instruments and observatory were considered by many the most spectacular astronomical apparatus in Europe. His quadrants and sextants "consisted of a grid of bars and

---

165 A. Pannekoek, A History of Astronomy (London: Allen and Unwin, 1961); cited in Albert Van Helden, "The Telescope in the Seventeenth Century," Isis 65 (1974): 55.
166 Van Helden, "Telescope," 55.

transversals, which had two sights fixed on one side of the instrument, a plumb-line suspended over the degree graduations, . . . an alidade [which pivoted] around the centre of the arc of the circle, . . . [and] a system of cords, pullies and counterpoises [which facilitated] moving the alidade over the graduations" (Figure 8).[167]

For measuring purposes, Hevelius used the Vernier system, which was more precise than Tycho's method,[168] circular transversals for dividing his instruments into minutes, and an external micrometer screw for dividing them into seconds.[169] Another important characteristic of his instruments was that they were heavily ornamented with figurines and other decorations, a style representative of "an industry which was not yet able to detach itself from the ancient tradition of the craftsman as an artist."[170] Hevelius believed that certain other instruments, such as the pendulum clock and the micrometer, were no more than "adjuncts" to his quadrants and sextants.[171]

Despite Hevelius' opposition to the use of telescopic sights on measuring instruments, he nevertheless, used telescopes, and was one of the first to do so.[172] He used a

---

167 Daumas, Scientific, 48-49.
168 Bennett, Divided, 66.
169 Béziat, "La vie," 534-535.
170 Daumas, Scientific, 48.
171 Chapman, Dividing, 33.
172 Silvio Bedini, "The Tube of Long Vision; The Physical Characteristics of the Early Seventeenth Century Telescope," Physis: Rivista di Storia delle Scienza 13 (1971): 203.

Galilean telescope for his first major work, Selenographia (1647), but substituted the Galilean, for the "astronomical" (or Keplerian) telescope, thereafter.[173] It is important to note, however, that Hevelius' use of telescopes was limited to observations of the moon (Galilean telescope) and the sun ("astronomical" telescope), and that these telescopes differed from the astronomical measuring instruments which lacked lenses. Instead of using lenses, he used naked-eye sights, or pinnules, which helped him pin-point particular celestial objects without bringing their images closer (Figure 9).

Hevelius housed all his instruments in his own observatory which he finished building in 1657. "Stellaburgum", as it was called, was built over the roofs of three adjoining houses, and was considered the finest observatory in Europe until "1671 and 1676 when the French and English national observatories were established in Paris and Greenwich" (Figure 10).[174]

Tycho and Hevelius shared both differences and similarities between their work and instruments, that were significant to the subsequent controversy between Hevelius

---

[173] Van Helden, "Astronomical," 35.

[174] Ivan Volkoff, Ernest Franzgrote, and A. Dean Larsen, Johannes Hevelius and his Catalog of Stars (Provo, Utah: Brigham Young University Press, 1971), 23.

and Hooke.[175] The major differences between Tycho and Hevelius' instruments resulted from the increase in the accuracy of measurement between the two. Like Tycho, Hevelius believed that larger instruments helped him obtain a higher degree of precision in his measurements. Consequently, he changed the radii of his quadrants and sextants to six, or even nine, feet. He also limited his use of wood, as he relied more on metals, especially brass, assuming that metals were less vulnerable to bending and warping than wood.[176] Finally, unlike Tycho, Hevelius used an "external" micrometer,[177] and other mechanical aids, to help track celestial objects with more precision.[178] Despite these differences, they shared one crucial similarity: Hevelius continued using the same open sights as Tycho had used half a century before. These open sights consisted of cylindrical fore-sights and double-slit nearsights which were "sometimes mounted on an alidade, sometimes moving independently on the limb."[179]

Besides the Tychonic slits, Hevelius and Tycho shared several expectations which were manifest in their work. In

---

[175] Hooke eventually used these similarities and differences against Hevelius when he published his Animadversions on the First Part of the 'Machina Coelestis' of. . . Johannes Hevelius (London, 1674).

[176] Béziat, "La vie," 591.

[177] It was not, in reality, an external micrometer, but a threaded screw used for finely adjusting the alidade across the degree scale (Daumas, Scientific, 49).

[178] Chapman, Dividing, 32.

[179] Bennett, Divided, 66-67; see also J. A. Bennett, "Hooke's Instruments for Astronomy and Navigation," in Hunter and Schaffer, Robert Hooke, 22.

addition to their belief that larger instruments yielded more accurate measurements, the two also argued that accuracy depended on prolonged, meticulous observations, carried out with diligence and patience. Furthermore, both believed in the law of averages. The "secret" to Tycho's precision and consistency involved his combining "of measurements from many instruments over a long period of time, [making prolonged observation paramount to his success] . . . . He was the first to see that it was also necessary to take long series of observations so that random, instrumental and human error can be averaged out."[180] By filtering out these "systematic" errors, Tycho's computations were highly accurate. Hevelius continued this Tychonic tradition, acquiring a great multitude of observations from his different instruments so that he could filter out any errors which might accrue.[181]

Despite the progress in instrumentation between Hevelius and Tycho, Hevelius remained, for the most part, an adherent of the Tychonic tradition. Perhaps what best characterizes him as the last astronomer of the Tychonic school, was his continued advocacy of open sights even though he was aware of

---

[180] Walter G. Wesley, "The Accuracy of Tycho Brahe's Instruments," Journal for the History of Astronomy 9 (1978): 51-52.

[181] Chapman, Dividing, 82. One other similarity, which was manifest in Hevelius' Machina coelestis, was that it was modeled after Tycho's Astronomiae instauratae mechanica (1598), in which Tycho described his instruments, the use of pinnules (open sights), divisions, and his observatory "Stjerneborg", on the isle of Hveen. See Tycho Brahe, Tycho Brahe's Description of his Instruments and Scientific Work as Given in Astronomiae instauratae mechanica, trans. and ed. Hans Ræder, Ellis Strömgren, and Bengt Strömgren (Copenhagen: I Kommission Hos Ejnar Munksgaard, 1946) for the comparison.

the opinions of others who depended on lenses for taking measurements. His deep-rooted conviction to defend open-sights is demonstrated by the sentiment found in his works -- "I prefer the unaided eye" (Figure 11).[182]

"War of Words" before the Controversy

Despite the fact that correspondence between Hevelius and Hooke concerning telescopic sights began as early as the mid-1660's, their "disagreement [was] so politely handled on both sides that it cannot, at this point, be called a controversy."[183] Curiously, Hevelius and Hooke never met face to face, nor did they ever write directly to each other, as all their correspondence passed through Henry Oldenburg. This is even more unusual considering that Hevelius corresponded with (or met) almost every other individual who was involved with the dispute between open and telescopic sights, such as Edmond Halley (1656-1742), John Flamsteed, and John Wallis (1616-1703).

In 1661, Hooke published a work entitled "Discourse of a new instrument to make more accurate observations in

---

182 "Praestat nudo oculo", from the celestial map in Firmamentum Sobiescianum, Gedani, 1690 (Volkoff et al., Hevelius, 46). See also J.D. North, "Johannes Hevelius," Dictionary of Scientific Biography, Vol. 6, 364.

183 Hall and Hall, Correspondence, Vol. 5 (1668-1669), xxiv.

astronomy, than ever were yet made" (1661).[184] No copies survived, but John Aubrey (1626-1697) described Hooke's instrument as one which "performs more, and more exact, then all the chargeable apparatus of the noble Tycho Brahe or the present Hevelius of Dantzick."[185] Therefore, Hooke was concerned with the precision of astronomical instruments as far back as 1661, although he did not communicate this to the other members of the Royal Society until 1665.

The first evidence of an exchange between Hevelius and Hooke on the issue of sights occurred in 1665. The same year, Oldenburg informed Auzout that Hooke believed only a "few persons can distinguish an angle less than one minute, although some by practice can train themselves to see a lesser one."[186] The only other evidence of correspondence during this year, is in Hooke's Animadversions on the first part of the 'Machina Coelestis' of. . . Johannes Hevelius (1674). Hooke stated that he had communicated with Hevelius (through Oldenburg) on the limitations of naked-eye sights, and "also the way of making instruments of much less bulk, to do ten times more then 'twas possible to do with the largest instruments made the common way."[187] Hooke reprinted

---

[184] G. Keynes, A Bibliography of Robert Hooke (Oxford, 1966), 12; cited in Bennett, "Hooke's Instruments," in Hunter and Schaffer, Robert Hooke, 21-22.

[185] Ibid., 22.

[186] Hooke eventually dropped the claim that one can train himself to distinguish less than one minute of arc. 23 July 1665, Hall and Hall, Correspondence, Vol. 2, 443.

[187] R. Hooke, Animadversions on the first part of the 'Machina Coelestis' of. . . Johannes Hevelius, ed. R.T. Gunther, Early Science in

Hevelius' reply against telescopic sights, indicating that Hevelius responded that same year.[188]

Hevelius and Hooke picked up their correspondence again in 1667 as each furnished his own arguments towards telescopic sights. Hooke indicated to Oldenburg that he hoped Hevelius would consider using telescopic sights on his measuring instruments. He was even willing to help Hevelius learn more about telescopic sights, if necessary.[189] Hevelius was "grateful" for Hooke's advice, and requested that the latter send him "a full description of those telescopic sights fitted to sextants for measuring stellar distances; for [he relies] on his confidence that they are indeed useful in observation."[190] Hevelius' letter was read at the Royal Society meeting on November 4, 1667, and Oldenburg communicated Hevelius' request for a description of telescopic sights and the date in which Hevelius would publish the Cometographia (1668). The Royal Society wanted to comply with Hevelius' request for the information, and desired "Mr. Hooke to be mindful of his promise" to send Hevelius the information he requested.[191]

Oxford, Vol. 8, The Cutler Lectures of Robert Hooke (London: Dawsons of Pall Mall, 1931; reprint, Winchester: Warren and Son Limited, 1968), 41.
[188] Ibid. The only indication that there was an exchange between the two in 1665, is in Hooke's Animadversions (Hooke does not give an exact date for either letter). I have not been able to locate these letters in any other sources.
[189] c. 20 February 1667, Hall and Hall, Correspondence, Vol. 3 (1666-1667), 348.
[190] Hevelius to Oldenburg, 11 October 1667, Hall and Hall, Correspondence, Vol. 3, 519.
[191] Birch, History, Vol. 2, 209.

Correspondence between Hevelius and Hooke became more frequent in the following year. Oldenburg informed Hevelius of Hooke's opinion of telescopic sights, quoting Hooke verbatim:

> Telescopic sights so greatly surpass those commonly used in instruments of all kinds, whether quadrants, sextants, or levels, especially for any kind of celestial observation, that with them an instrument of one span radius can be made much more accurate than another of sixty-foot radius, however good, having common sights. . . . As for dividing this quadrant[192] into degrees, minutes, and seconds, no doubt the very skillful Mr. Hevelius knows many good ways of doing this. If he would like to know those I have discovered, I will gladly impart them to him at a word.[193]

For his part, Hevelius thanked Hooke by sending him a copy of Cometographia, and requested Oldenburg to find some good, inexpensive lenses for him. Responding to Hooke's arguments for telescopic sights, Hevelius argued that telescopic sights are not as reliable as open sights, even if the sights are properly mounted. Furthermore, telescopes may shift because they are not "firmly fixed. . . even if they are calibrated with all diligence."[194] Hevelius asked that someone provide him with proof that telescopic sights are more accurate, by presenting him with eight observations of distances (observed using telescopic sights) between certain stars which he

---

[192] Hooke is referring to a quadrant which he himself designed.

[193] 11 May 1668, Hall and Hall, Correspondence, Vol. 4, 396-397.

[194] Hevelius to Oldenburg, 3 June 1668, Ibid., 447-448. This letter was read to the Royal Society on June 25, 1668 (Birch, History, Vol. 2, 301).

named.[195] Hooke replied to Hevelius' latest contention by arguing that "the naked eye is barely able to distinguish with confidence an angle of one whole minute, whereas the eye furnished with a telescope can easily distinguish any number of seconds of arc, or even a single second."[196]

Cometographia and Ensuing Correspondence

In this second stage of correspondence, the disagreement between Hevelius and Hooke became a controversy characteristic of the late seventeenth century.[197] Hevelius' Cometographia, a text concerning the history and physical constitution of comets, and especially the comet of 1652, was published in the fall of 1668.[198] Oldenburg distributed several copies to certain members, including Hooke, during the Royal Society meeting on October 22, who were asked to "peruse it, and bring in their thoughts upon it."[199] Several days later, Oldenburg reported to Hevelius that, after having read Cometographia, Hooke

---

[195] Hooke never took up Hevelius' challenge to find the eight distances he listed. However, Flamsteed eventually made these eight observations and compared them to Hevelius' figures (Béziat, "La vie," 617).
[196] Oldenburg to Hevelius, 30 July 1668, Hall and Hall, Correspondence, Vol. 4, 579. Hooke ignored his previous claim that one can train himself to discern less than a minute of arc (See n. 186).
[197] See Ibid., Vol. 2, xxii-xxiii.
[198] North, "Hevelius," 362.
[199] Birch, History, Vol. 2, 313.

> persists in his view that [telescopic sights] are (your doubts notwithstanding) ten, twenty, thirty, or forty times more accurate than the common sights. . . . And he adds that it is not possible by means of the common sights to effect observations of the fixed stars or the moon to a higher precision than that of a whole minute, whereas with the instrument he now uses he can make some reliable observations down to a second of arc.[200]

In his next two letters to Oldenburg, Hevelius emphasized again the difficulties associated with telescopic sights. Although he was not distraught with Hooke at this point, the quarrel was augmented by Hevelius' deep convictions that telescopic sights are inferior to open sights. First, Hevelius countered Hooke's arguments by claiming that smaller telescopic instruments cannot be used more accurately or confidently than larger instruments affixed with common sights. Second, he also argued that large telescopes "show things more distinctly" than their smaller counterparts. Third, small telescopes cannot be "pointed precisely" to any object "because of the reduced distance between the sights."[201] Fourth, small telescopes reveal the rays of stars in the same manner as naked-eye observation. Lastly, lenses have to be removed frequently and cleaned, especially in the winter, as they are susceptible to dirt and filminess caused by the "vapor emitted from the mouth and eyes because of the very intense

[200] (Emphasis mine) 28 October 1668, Hall and Hall, Correspondence, Vol. 5 (1668-1669), 115. Notice how Hooke turned the issue from one of accuracy, into one of precision.
[201] 19 November 1668, Ibid., 186.

cold."[202] Hevelius also signified that he "cannot persuade himself that the distances between the stars can be more accurately observed with those little instruments. . . than with great instruments furnished with common sights according to [his] own system."[203]

This latest series of letters, which followed the publication of the Cometographia, indicated Hevelius and Hooke's increasing concern to defend their respective positions. Furthermore, Oldenburg disclosed to Hevelius that Hooke, who had promised to send Hevelius telescopic lenses, delayed sending them "because of what [Hooke] himself observed in some of [Hevelius'] letters, that that kind of sight did not please [Hevelius] very well, and partly because Mr. Hooke is busy (besides other things) in rebuilding the houses of the restored city of London."[204] Although Hooke may have, indeed, been otherwise engaged, the letter indicated Hooke's awareness of Hevelius' dissatisfaction with telescopic sights, and Hooke's own indifference towards Hevelius' request. By 1671, Hevelius also showed signs of irritability towards Hooke, as Hooke had still not sent him the "instrument for measuring small distances, promised long

---

[202] This last argument appears somewhat specious. The nature and validity of Hevelius' objections will be discussed later. This list of objections appears in his letter dated 19 November 1668 (see n. 201).
[203] Hevelius to Oldenburg, 11 December 1668, Hall and Hall, Correspondence, Vol. 5, 244.
[204] 2 August 1669, Ibid., Vol. 6 (1669-1670), 170.

ago,"[205] and Hevelius expressed to Oldenburg his impatience with the man who was "all words and no deeds."[206]

In the early 1670's, Flamsteed was involuntarily drawn into the debate since he, too, advocated the use of telescopic sights. Flamsteed and Hooke were never intimate friends, however, and for the most part, they never got along well. Although Flamsteed believed Hevelius possessed good instruments and, for a time, argued Hevelius was one to be "emulated,"[207] he claimed telescopic sights provided him with more accurate observations than Tycho Brahe's use of plain sights.[208]

In Flamsteed's first letter concerning Hevelius' use of open sights, he indicated that he still had a high opinion of Hevelius even though the latter remained a staunch advocate of open sights:

> As for Monsr: Hevelius I esteeme him a person of that candor and ingenuite, as not to [grow] angry at any one, who shall civilly and without gall, informe him of his errors: & hee cannot be so disintelligent as not [to] perceive, that his friends noteing them in a civil, may prevent his enemies from commenting on them in a detracting way. . . .[209]

---

205 Hevelius first made this request in 1668! Oldenburg stated, "I see that you take it ill" that Hooke has delayed sending this instrument [9 November 1671, Ibid., Vol. 8 (1671-1672), 353].

206 This phrase will have a direct bearing on the controversy in the future.

207 24 November 1669, Flamsteed to William Brounker (c.1620-1684), in Rigaud, Correspondence, Vol. 2, 81.

208 Murdin, Under, 124. Flamsteed eventually equated the accuracy of Hevelius' observations with Tycho's use of open sights.

209 Flamsteed to Oldenburg, 18 February 1671, Hall and Hall, Correspondence, Vol. 7 (1670-1671), 465. Flamsteed may be referring to Hooke in this passage, but he is also referring to himself, as he

Flamsteed directly addressed Hevelius' use of open sights in a subsequent letter to Oldenburg. News of Hevelius' plans for a new star catalog had already reached the Royal Society by the early 1670's.[210] Flamsteed, who was interested in Hevelius' catalog, eagerly awaited its publication, because Tycho's catalog, which was the star catalog currently being used, contained many errors resulting from his use of open sights. Flamsteed contended that if Hevelius used open sights, then "it will be difficult to judge whether [he and others] ought to make use of Tychoes Catalogues or [Hevelius'] when they come forth."[211] Flamsteed repeated this concern in an epistle to Cassini where he stated:

> I hear that the famous Johannes Hevelius has promised [to effect] this reformation,[212] but as he is reported to measure the intervals between the fixed stars with open sights only, devoid of lenses, mounted upon his instruments, one can expect no greater precision than we find in Tycho, especially as we know how hard it is to line up open-sighted instruments on the fixed stars in the darkness of the night, particularly on the smaller stars.[213]

Flamsteed did not appear overly concerned with Hevelius' use of open sights at this time. However, following the

---

imparted to John Collins (1625-1683) in another letter, "I shall write to [Hevelius] and inform him civilly of his errors myself" (1 August 1671, Rigaud, Correspondence, Vol. 2, 120).

[210] Hevelius' star catalog, which would eventually supersede Tycho's, was not published until 1690, three years after his death.

[211] Flamsteed to Oldenburg, 16 November 1672, Hall and Hall, Correspondence, Vol. 9 (1672-1673), 327.

[212] i.e., construct a more accurate star catalog

[213] (Emphasis mine) Again, there is an emphasis on "precision" (see n. 200 above). 5 September 1673, Hall and Hall, Correspondence, Vol. 10 (1673-1674), 194.

publication of Hevelius' Machina coelestis pars prior (1673), Hevelius' explicit views could no longer be taken lightly by those who vindicated the use of telescopic sights.

From *Machina coelestis pars prior* to *Animadversions*

In 1673, Hevelius published his next great work entitled, Part I of the 'Machina Coelestis' of Johannes Hevelius, containing Organographia, or an accurate depiction and description of all the astronomical instruments with which the author has so far explored and measured the stars.[214] The bulk of the book detailed the various astronomical instruments Hevelius used throughout his lifetime, including quadrants, sextants, octants, and telescopes. He dedicated two chapters (14 and 15, respectively) to his chief mechanical perfections, including his sights and the divisions of his instruments,[215] and described his great observatory, "Stellaburgum", in the last remaining chapters of the text. Some of his more prominent measuring instruments included: a brass quadrant (3 feet in radius), with a wooden base (Figure 12a); a brass sextant (3 feet in radius), with a wooden base, requiring two observers

[214] Translated from the Latin by Oldenburg in a letter to Cassini, 11 September 1673, Ibid., Vol. 10, 188. This work was reviewed in the Philosophical Transactions of the Royal Society, 22 December 1673, Vol. 8, no. 99, 6171-6172.
[215] Béziat, "La vie," 534.

(Figure 12b); a brass sextant (4 feet in radius), with a moving alidade; a wooden quadrant which rested on a vertical axis, and was controlled by cables, pullies, and counterweights (these made moving the instrument easy); a large wooden sextant (6 feet in radius), which had supports for both the observer's arms; a large wooden octant (8 feet in radius), with two distinct arcs (making it, in reality, two separate instruments of the same radius and in the same plane), and having two mobile sights but no alidades; several smaller brass quadrants of 1, 1 1/2, and 2 feet radii (Figure 12c); a large azimuthal quadrant (4 feet in radius) that Hevelius claimed was so easy to use and dependable, that after thirty years of use, he has not been able to detect any changes (Figure 8); and finally, a brass sextant (6 feet in radius) designed to be used by two observers to measure certain angular measurements between stars (each observer would find one of the two stars) (Figure 12d).[216]

Hevelius' telescopes were as grand as his measuring instruments. His objective when constructing his own telescopes was to simplify previous telescopes which he considered cumbersome and unwieldy.[217] His largest telescope (described in the Machina coelestis) was 150 feet in length

---

[216] Jean Baptiste Delambre, Histoire de l'Astronomie Moderne, Vol. 2, The Sources of Science Series, ed. Harry Woolf, no. 25 (New York: Johnson Reprint Corporation, 1969), 459-460. See also Volkoff et al, Hevelius, 24, 28.

[217] C. Leeson Prince, The Illustrated Account given by Hevelius in his 'Machina Coelestis' of the Method of Mounting his Telescopes and Erecting an Observatory (Lewes, England: Sussex Advertiser, 1882), 40.

(Figure 1). It was suspended on a 90-foot mast which was deeply set into the ground and held down by four large cables which would protect it during a storm.[218] The telescope's tube was sectional, and the entire instrument was controlled by a system of ropes and pullies,[219] in much the same way as he controlled his measuring instruments. There were several problems with this instrument that made it impractical. First, one could only observe on the best nights since the slightest breeze caused the whole apparatus to shake. Second, since the planks which supported the sectional tubes were made of wood, they were subject to bending or warping. Finally, because the telescope was so long, Hevelius found it difficult to constantly align the lenses. Because of these impracticalities, Hevelius rarely used this particular telescope.[220]

Of principle concern to the adherents of telescopic sights, was Hevelius' chapter on open sights in which Hevelius explicated his reasons for using open sights, and his disregard for telescopic sights and lenses. He stressed several specific reasons for not using telescopic sights (lenses).[221] First, he argued that lenses cannot be

---

[218] Ibid., 50

[219] King, History, 51-52.

[220] Ibid., 53.

[221] All his arguments are listed between pages 293-300, but especially 296 (Chapter 14: "De Instrumentorum Pinnacidiis, sive Dioptris", Machina coelestis pars prior, Gedani, 1673). See also Béziat, "La vie," 611-612; Chapman, Dividing, 38; and King, History, 100.

maintained in the same position. Consequently, an observer could not acquire the same exact measurements each time he used telescopic sights. On the other hand, with his open sights he can repeat the same observation with equal precision each time because he did not have to worry about shifting telescopes caused by their lack of being "firmly fixed."[222] Besides, lenses could easily break, especially in cold weather. Furthermore, winter weather could fog up or dull lenses when the breath of an observer came into contact with them.[223] As a result, the lenses needed to be frequently removed and cleaned (which, undoubtedly, interrupted the observer from his work).

Hevelius leveled objections, not only against lenses, but also cross-hairs which were usually affixed to telescopic sights used for measurement. Essentially, he used the same arguments for cross-hairs that he had used for lenses, namely, that cross-hairs could easily break or become disarranged, and it was tedious to constantly repair them. He therefore viewed them as contraptions that get in the way of an observer and his primary instruments.[224] In this respect, Hevelius also argued that cross-hairs (their intersection) have a tendency to conceal small stars so that an observer has difficulty distinguishing them. One final

222 Hevelius had previously stressed this objection to Oldenburg (see text marked by n. 194).
223 Again, he mentioned this in a letter to Oldenburg (see n. 202).
224 Hevelius could not tolerate anything coming between his eyes and the celestial objects he measured.

objection Hevelius raised, dealt with the specific position of the cross-hairs. Although Hevelius never doubted that telescopic sights helped one see more distinctly, cross-hairs caused inaccuracies because of their close proximity to the observer's eyes (a few inches). Therefore, telescopic sights were less effective than open sights in which the sights themselves were six, or even eight feet from the eyes. Hevelius believed that the proximity between the eye and cross-hairs in telescopic lenses could not ensure an accurate line of collimation.[225] Although Hevelius took all his objections seriously, some were easily refuted.[226]

With the publication of the Machina coelestis pars prior, the controversy became a personal battleground. Hevelius, in listing his favorite savants in the text, failed to mention Hooke's name,[227] nor did he send Hooke a copy of the book.[228] The nature of Hevelius' arguments, coupled with these personal slights against Hooke, were enough to prompt Hooke to retaliate.

---

225 See Chapman, Dividing, 38. A line of collimation (perfect alignment) "was obtained when the two or more cross-hairs in the focal plane of the eyelenses exactly fitted over those in the focus of the object glasses" (Allen, "Problems," 309).
226 Hooke, Flamsteed, and Molyneux eventually refuted these objections.
227 Béziat, "La vie," 613-614.
228 Hevelius to Oldenburg, 13 August 1673, Hall and Hall, Correspondence, Vol. 10, 142. See also J.A. Scott, The Mathematical Works of John Wallis, D.D., F.R.S. (1616-1703), 2nd ed. (New York: Chelsea Publishing Company, 1981), 127. Oldenburg distributed the copies Hevelius sent him on November 20, 1673 (Birch, History, Vol. 3, 110).

Hooke began his vindication of telescopic sights by carrying out experiments during meetings of the Royal Society, where he could convince others to comply with his views. On January 15, 1674, he performed an experiment with a ruler "to shew, that [one] cannot by the naked eye make any astronomical or other observation to a greater exactness than that of a minute, by reason, that whatever object appears under a less angle, is not distinguishable by the naked eye,"[229] and he repeated the experiment a week later.[230] For his part, Hevelius marshaled further arguments against the use of telescopic sights. He contended that large instruments, such as quadrants and sextants, cannot be shifted or inverted so as to test telescopic sights, and besides, no one had proved to him that telescopic sights were useful for observation. Hevelius also argued, for the first time, how one with good eyesight and experience can observe with the same accuracy as another using telescopic sights. Finally, he restated his argument for the repeatability of observations, a procedure which was impossible to carry out using telescopic sights.[231]

In addition to the arguments specifically against telescopic sights, Hevelius also argued how other astronomers

---

229 Birch, History, Vol. 3, 120. Also in R. T. Gunther, ed., Early Science in Oxford, Vol. 7, The Life and Work of Robert Hooke (Part II), by Robert Hooke, (London: Dawsons of Pall Mall, 1930; reprint, Winchester: Warren and Son Limited, 1968), 416.

230 22 January 1674, Birch, History, 121. Also in Gunther, Early, Vol. 7, 417.

231 ?16 March 1674, Hall and Hall, Correspondence, Vol. 10, 520-522.

(including Flamsteed) "have already pronounced their verdict on [his] observations before they have seen them, examined them or known anything at all of them."[232] Hevelius asked his critics to at least "suspend judgment" until after they had gained the necessary experience acquired only through years of observation; only then could they sufficiently address these issues.[233] And, he pointed out, it is only fair that one of the adherents of telescopic sights construct a complete star catalog using telescopic sights alone, so that it could be compared with one in which open sights had been used. Hevelius was certain that no one wanted to carry out this task, and that individuals the likes of Hooke were deluding themselves with the notion that it was enough to assess one's accuracy on the basis of a handful of telescopic observations.[234]

Hooke retorted forcefully with his, Animadversions on the first part of the "Machina Coelestis" of the honourable, learned, and deservedly famous astronomer Johannes Hevelius.[235] There are two distinct themes in the text -- Hooke's vindication of telescopic sights, and his description

---

[232] Ibid., 521.
[233] Hevelius tried to draw attention to the fact that Hooke was not, by profession, an astronomer, nor had he carried out long, consistent observations.
[234] This letter was read during the Royal Society meeting on April 23, 1674 (Birch, Vol. 3, 133). Also in Gunther, Vol. 7, 421. During the same meeting, Hooke produced a quadrant with telescopic sights "wherein appeared the pre-eminence of such sights above the common dioptra's." (Ibid.)
[235] Hooke read this work to members of the Royal Society on December 11, 1673, although it was not published until 1674.

of an equatorial quadrant and its graduation into degrees (Figure 13).[236] Hooke explained his reasons for attacking Hevelius' position:

> Nor should I have published these my thoughts, had I not thought them so highly decryed by a person of so great authority, fearing that hereby other observators might have been deterr'd from making any use of them, and so the further progress of astronomy might have been hindered.[237]

He stated how he can do more with an instrument of one foot radius, than Hevelius can with an instrument of 10, 20, 30, even 60 feet in radius, fitted with open sights. He furthermore claimed (as he did many times) that "observations made with common sights. . . are no ways capable of certainty to a minute or two."[238] Elsewhere, Hooke stated that if two stars are not separated by at least a minute of arc, they would appear as one to those using open sights.[239] Hooke directly addressed Hevelius' argument that lenses and cross-hairs can easily be broken, by affirming that the "plumb-line of any of [Hevelius'] instruments may be broken, or his sights bended."[240] Similarly, Hooke contended that Hevelius'

---

236 Chapman, Dividing, 45.
237 Hooke, Animadversions, in Gunther, Early, Vol. 8, 79. He admitted to writing the work as a response against Hevelius' objections.
238 Ibid.
239 Hevelius never denied that telescopic sights helped discern objects, but he held onto his belief that experience and good eyesight prevailed over telescopic sights (King, History, 100).
240 Ibid., 43. Although Hooke's objection was as ostensible as Hevelius', Hooke attempted to draw attention to Hevelius' arguments (see text marked by n. 224).

instruments are subject to shrinking, swelling, bending, and warping.[241]

Hooke further argued that with his instruments, only one observer was necessary; Hevelius required two observers, and Tycho Brahe required four.[242] It became absolutely necessary, he stated, that there was a "unanimous concurrence" in their measurements. If even one failed to give an accurate measurement, the entire observation became uncertain and useless.[243] Hooke's main objection to open sights was his conviction that "'tis impossible with Sights made after Ticho's or Hevelius his way, to distinguish any distance in the Heavens <u>less than half a minute, or thirty Seconds, and hardly one of a hundred can distinguish a minute</u>."[244] Hooke's main contention, therefore, rested on the inability of the human eye to distinguish below a certain angle of measurement, and the telescope's power to increase magnification.

Like Hevelius, Hooke listed his objections, point by point, in his text. However, Hooke also carried his censure a step further by attacking not only open sights and all they

---

[241] Hooke, <u>An Attempt to Prove the Motion of the Earth from Observations</u> (London, 1674). Published in his <u>Cutlerian Lectures</u> (see Gunther, <u>Early</u>, Vol. 8, 16). Flamsteed believed that making larger instruments, such as Tycho and Hevelius', was detrimental, as the instruments inevitably bended or warped with time. Bending even one quarter of an inch could result in an error of minutes (Forbes, <u>Greenwich</u>, 232).

[242] Not all of Hevelius' instruments required two observers (see text marked by n. 216).

[243] Hooke, <u>Animadversions</u>, in Gunther, <u>Early</u>, Vol. 8, 91.

[244] (Emphasis mine) Ibid., 43. See also Volkoff et al, <u>Hevelius</u>, 36.

encompassed, but also denunciating Hevelius' work, methods, and observations in general. He accused Hevelius of being "circumspect, to find out the inconveniences and difficulties that do acrew to the best observators, even with the best instruments. . . . [Hevelius] would have done himself and the learned World a much greater piece of service" had he improved his instruments by adding telescopic sights.[245]

The greatest insult to Hevelius was the likeness of his work and his instruments to Tycho Brahe's.[246] Hooke argued that because Tycho's instruments were as large as Hevelius', the measurements of the former were just as accurate.[247] Hooke even claimed that Tycho's large wooden quadrant and mural quadrant were generally better than Hevelius' instruments, and his mural quadrant recorded more accurate measurements than Hevelius' instruments,[248] and that the sights and way of division, were the same as well -- further implying the lack of improvement between Tycho and Hevelius. Such blunt denigration of Hevelius' work and instruments by suggesting that they had not improved over Tycho's, obviously provoked Hevelius further, and the controversy intensified.

In the second half of his Animadversions, Hooke described an equatorial quadrant of his own design that

---

245 Hooke, Animadversions, in Gunther, Early, Vol. 8, 38.
246 Ibid.
247 Actually, Hevelius' measurements were more accurate than Tycho's (Béziat, "La vie," 591).
248 Hooke, Animadversions, in Gunther, Early, Vol. 8, 70.

greatly surpassed any instrument of Hevelius' making (Figure 13). Hooke claimed that his instrument could superimpose two separate images, thereby allowing one observer to take both sights at once. He also described a tangent screw that finely adjusted measurements, and introduced a clockwork drive that could be controlled by a conical pendulum clock, making the instrument follow a particular star's movement across the sky without adjustment by the observer.[249] Although Hooke's instrument was never built, these important innovations eventually influenced instrumentation in astronomy.[250]

Hevelius was greatly incensed by Hooke's verbal attacks, but he had friends who supported his cause like Wallis, who, in a letter to Oldenburg, defended Hevelius against Hooke.[251] Hooke, Wallis believed, was too "harsh" with Hevelius, and Wallis was incensed that Hooke had published a statement of his against Hevelius in the Animadversions.[252]

Hevelius' reaction to Hooke's mockery of his observations and instruments was to write an impassioned letter to Oldenburg informing him how he had suffered at Hooke's insults. Hevelius began by declaring that Hooke

---

[249] See Bennett, Divided, 68-69.
[250] Bennett, "Hooke's Instruments," in Hunter and Schaffer, Robert Hooke, 29.
[251] Wallis and Hevelius had been friends since the 1630's when Hevelius met Wallis in England!
[252] 11 January 1675, Hall and Hall, Correspondence, Vol. 11 (1674-1675), 157.

should not have written in English, as he wasted both Hevelius and his friends' time in trying to translate it.[253] He also expressed his extreme distaste for disputes:

> You may believe, my friend, that I approach this little job with extreme reluctance: not because I am unsure whether I have untied [Hooke's] Gordion knots or laid myself open to those darts he has been pleased so often to hurl at me, which I can certainly dodge -- by no means! But because my mind (as, I judge, is proper in a candid and warm-hearted man) wholly abhors such things, especially disputes with others and contentions in mere idle words against a Fellow of the illustrious Royal Society.[254]

Hevelius further claimed that he had never tried to force his opinion on anyone, nor had he compromised anyone else's views:

> I have urged no one to be my partisan, nor have I made efforts to persuade anyone to relinquish his own point of view, which he might think the nearer to the truth; much less have I so conducted myself as to presume to play the rôle of dictator to free minds. In my little works I never, by any means, tried to lay down laws for anyone or for posterity as though they should follow in every detail in my footsteps, or as though that business was to be done thus and not otherwise. . . .[255]

Hevelius proceeded to describe Hooke as a "busybody", who "labors in vain with words and deeds," and is interested only in what others are doing, but never improving his own work.[256] Although Hooke continuously boasted about his precision,

---

[253] Hevelius may have also been sensitive to the fact that by publishing in the vernacular, it could be read by a wider audience.
[254] (Emphasis mine) 21 August 1675, Hall and Hall, Correspondence, Vol. 11, 467.
[255] Ibid.
[256] (Emphasis mine) Ibid., 468.

Hevelius claimed he never saw any proof that telescopic sights yield more accurate measurements than open sights. On a more personal level, Hevelius directly addressed Hooke's impertinence:

> Moreover, it is equally unworthy continually to tear to pieces, despise and scorn (for whatever reason) the labours [in observing] performed and at present still to be performed by others (with the best intentions). . . . For it is obvious to all, the distinguished Hooke has always (in all my writings, letters and conversations), whenever his name has been mentioned, been treated by me honourably and from a sincere heart, as is due to any man of such ability; when it was necessary, I replied to everything modestly and without any <u>personal attacks or stinging remarks</u>, as you, honourable friend, know best of all. . . . Yet how Mr. Hooke has treated me before (not his followers alone but the whole learned world may be read at length in almost every page of his <u>Animadversions</u> where he reviles my observations and small labours, slights them and makes them of no account, and myself he everywhere slanders, mocks and uses scornfully. . . . The best of the joke is, that while he almost assaults me with his praises [yet] as is evident from the rest of his phraseology on various pages he more and more mocks and wounds me, as [will appear] at greater length in its proper place. . . . It almost seems as though he meant in these pages as it were to revenge himself upon me and give vent to his anger. . . .[257]

The animosity between Hevelius and Hooke was final, and the two never communicated on friendly terms again.

Several Royal Society members, including Oldenburg and Flamsteed, became increasingly concerned with the quarrel between Hevelius and Hooke. They appeared to be in a dilemma

---

[257] (Emphasis mine) Ibid., 469-471.

as they advocated Hooke's position, but at the same time, did not wish to alienate one of Europe's greatest astronomers. Furthermore, Hooke's snide comparisons between Tycho and Hevelius, and his sarcastic attitude towards Hevelius' work, only served to further frustrate these Royal Society members. Oldenburg was especially incensed as he had developed a personal friendship with Hevelius since the beginning of their correspondence.[258] Oldenburg reassured Hevelius that he wanted to protect him and his work from "ill-wishers".[259] Furthermore, Hooke's contentions had not "weakened the Royal Society's regard for Hevelius."[260]

Flamsteed, Halley, and the Fire of 1679

When Flamsteed was appointed Astronomer Royal in 1675, he became interested once more in the polemical exchange

[258] By the year of Oldenburg's death (1677), the two had been corresponding with each other for fourteen years. According to Hall and Hall's Correspondence alone, Oldenburg sent fifty-six letters to Hevelius, and Hevelius replied with fifty-three. Therefore, they exchanged, on average, four letters a year, although there were more letters exchanged in the first couple of years of their correspondence.

[259] 15 May 1676, Hall and Hall, Correspondence, Vol. 12 (1675-1676), (London: Taylor and Francis), 295.

[260] 27 January 1677, Ibid., Vol. 13 (1676-1677), (London: Taylor and Francis), 197. There was a significant difference between writing to Oldenburg on a personal level, and writing to him as secretary of the Royal Society. In later years, when Hooke claimed Oldenburg "had written to Mr. Hevelius more and different things, than he had been directed to do by the Royal Society," Hevelius replied with a letter in which he denied Hooke's claim (in other words, Oldenburg always wrote to him in his capacity as secretary of the Royal Society) [17 April 1686, Birch, History, Vol. 4, 504]. However, Oldenburg continued to side with Hevelius in the dispute.

between Hevelius and Hooke.[261] Although Hevelius was aware of Flamsteed's preference for telescopic sights, he respected Flamsteed as he was a "proper" astronomer who regularly performed observations, and did not mock the work of others.[262] Nevertheless, even though Flamsteed respected the labors of Hevelius, he felt it was important to point out Hevelius' errors which were due to his use of open sights. As Flamsteed was aware of Hooke's acrimonious attacks on Hevelius' work, he offered "to defend Hevelius against Hooke's criticisms, expecting that the latter will accomplish little by way of solid observation."[263] Despite Flamsteed's support, however, Hevelius' reply was somewhat indignant as he protested that not all the discrepancies in his measurements were due to observational errors.[264] By January 1677, Hevelius' bitterness towards Hooke's publication, together with Flamsteed's strictures, led the Royal Society to assure Hevelius "that what Mr. Hooke had published against

---

[261] Béziat, "La vie," 614. Flamsteed, in his new, more powerful position, no doubt felt that it was necessary to vindicate the precision of telescopic sights.

[262] Hevelius to Oldenburg, 11 March 1676, Hall and Hall, Correspondence, Vol. 12, 216.

[263] 20 July 1676, Ibid., Vol. 13, 20 n.1. The editors have paraphrased the letter from the Royal Society MS. F.1, no. 118.

[264] 23 December 1676, paraphrased in Eric G. Forbes, "Early Astronomical Researches of John Flamsteed," Journal for the History of Astronomy 7 (1976): 133-134. Forbes recalculated the figures for the comet of 1680, the object of Hevelius and Flamsteed's disagreement, and compared them with Flamsteed's figures. Hevelius was sixty times more accurate than Flamsteed (Ibid., 134)!

him, was done without any approbation or countenance from the Society."[265]

Flamsteed's criticisms against Hevelius resulted in a temporary rift between the two as Hevelius was convinced that Flamsteed and Hooke had joined forces against him:

> Anybody who reads your letter will undoubtedly say that you are doing with all your strength and in fact with better weapons the same thing as Hooke is trying to prove with lesser ones: namely, that I cannot measure distances with my sextants, that is with the naked eye, except to three or at most to two whole minutes. . . . I well know now that I shall labour in vain with you, since I perceive abundantly clearly that you will never agree with me whatever other argument I produce and however many more observations I adduce to the contrary.[266]

Flamsteed's previous support was not enough to placate Hevelius who asked Flamsteed to allow him to his own opinion, as he would allow Flamsteed to his.[267] On April 5, 1677, Flamsteed asked Oldenburg to inform Hevelius that he had not joined forces with Hooke,[268] and Oldenburg ultimately was able to reassure Hevelius on the verity of the fact.[269] Hevelius

---

[265] 25 January 1677, Birch, History, Vol. 3, 332. This decision came three years after Hooke's publication!

[266] Hevelius to Flamsteed, 23 December 1676, Forbes, Early, 133-134; Forbes, Gresham, 37.

[267] 23 December 1676, in The Gresham Lectures of John Flamsteed, ed. Eric G. Forbes (London: Mansell Information/Publishing Limited, 1975), 37; and ?2 January 1677 in E.F. MacPike, Hevelius, Flamsteed, and Halley: Three Contemporary Astronomers and Their Mutual Relations (London: Taylor and Francis, Ltd., 1937), 80-81.

[268] Hall and Hall, Correspondence, Vol. 13, 236.

[269] 19 April 1677, Ibid., 247-248. Hooke and Flamsteed's adverse attitudes towards each other are evinced by two entries in Hooke's diary. When Hooke showed his quadrant design to Flamsteed in 1674, Flamsteed was not impressed, and Hooke wrote for May 28, "I shewd Flamsteed my quadrant. He is a conceited cocks comb" (p. 105). In the

responded that he was not offended, and that he only wished that Flamsteed not debase all his observations without having seen them first.[270] For his part, though Flamsteed remained a firm advocate of telescopic sights, he continued to respect Hevelius' work and the merit of his observations.

The incident between Hevelius and Flamsteed intensified Hevelius' concern because Flamsteed's views had to be taken more seriously than those of Hooke. As Oldenburg, Flamsteed, and certain other members of the Royal Society became increasingly uncomfortable with the situation, young Edmond Halley decided to visit Hevelius in Danzig in 1679.[271] Hevelius was delighted that an English astronomer and a proponent of telescopic sights would finally come to observe for himself the accuracy of Hevelius' instruments.[272] When he arrived in mid-May, Halley and Hevelius carried out a series of observations together, with Halley using telescopic sights he had brought along with him, whereas Hevelius used his own open-sight instruments. Halley sent several letters to

---

entry for November 27, 1677, the year of Oldenburg's letter, Hooke recorded, "At Tompions. Flamsteed an Ignorant impudent Asse" (p. 330) [both in The Diary of Robert Hooke eds. H.W. Robinson and W. Adams (London, 1935); cited in Bennett, Divided, 69].

270 28 November 1677, Correspondence, Vol. 13, 363. Oldenburg never had a chance to read this letter as he died before it reached England.

271 MacPike suggests that Flamsteed gave Halley the idea for the visit since there was no reason for Halley to be interested in old-fashioned instruments (MacPike, Hevelius, 85). Nevertheless, young Halley could not turn down the opportunity to meet the great European astronomer, and become familiar with his instruments which were slowly being phased out.

272 See "Memoir" by ?Martin Folkes and "Éloge de M. Halley" by D'Ortous de Mairan, both in E. Halley, Correspondence and Papers of Edmond Halley, ed. E.F. MacPike (New York: Arno Press, 1975), 3-4, 18.

Flamsteed and to the Royal Society, where he expressed his astonishment at the accuracy of Hevelius' measurements which were repeated several times.[273]

At Hevelius' request, Halley left behind a testimonial letter which attested his high esteem for the accuracy of Hevelius' open-sight observations.[274] In the letter, Halley stated that he was "abundantly satisfied of the use and certainty of [Hevelius'] instruments and observations,"[275] and he wondered why he ever doubted the accuracy of observations by open sights, readily "offer[ing] himself a voluntary witness, (of the almost-incredible certainty of those his instruments) against all who shall for the future call [Hevelius'] observations in question."[276]

On September 26, 1679, while Hevelius was away from his observatory, a servant of his, in a fit of vengeance, started a fire which eventually burned down the observatory, many of Hevelius' manuscripts, and all of his instruments.[277] Capellus, a kinsman of Hevelius, informed Hevelius' brother of the losses which were incurred: almost all of his unbound books (including Machina coelestis), all of his instruments,

---

[273] Halley to Flamsteed, 7 June 1679, MacPike, Correspondence, 43; Birch, Vol. 3, 488; and Folkes, "Memoir," in MacPike, Correspondence, 4. Although there were discrepancies between the two sets of observations, Halley wrote that the differences were negligible (Simms, Molyneux, 25).

[274] Dated 8/18 July 1679. Halley eventually regretted leaving written testimony behind.

[275] Translated from the Latin by Wallis in Philosophical Transactions, Vol. 15, no. 175, 1168-1169.

[276] Ibid.

[277] Béziat, "La vie," 623.

seven buildings, and most of the household items. Fortunately, Kepler's manuscripts, Hevelius' correspondence, and most of his bound books (including his star catalog) were saved.[278] Despite Hevelius' devastation at the damage caused by the fire, he immediately began rebuilding his observatory and replacing it with new instruments.[279]

Annus climactericus, the Death of Hevelius, and Thereafter

In the years following the fire, Hevelius' correspondence with members of the Royal Society much decreased -- to a large part because of Oldenburg's death in 1677. Nevertheless, Hevelius kept himself busy by rebuilding his observatory, and in 1685, he published what was to become his last work, the Annus climactericus.[280] Since this was a history of the controversy between Hevelius and Hooke, it brought attention to the old quarrel once more. The title referred to the year 1679, a year which began so auspiciously for Hevelius with Halley's visit, but ended tragically with the fire.[281] With great detail, Hevelius recounted Halley's visit, and reprinted twenty-seven letters from individuals

[278] Capellus, "A Letter on the Hevelian Conflagration. . . ." (1679), in MacPike, Hevelius, 109-110.

[279] Volkoff et al., Hevelius, 4. Unfortunately, the newer instruments did not compare with the previous ones.

[280] His great star catalog and other works were published posthumously, in 1690, by his wife Elizabetha.

[281] Béziat, "La vie," 632. See also Volkoff et al., Hevelius, 56.

who were personally familiar with Hevelius' observations and instruments.[282]

Hevelius' text was ready by April 1684, when he sent the Royal Society and several of its members, including Francis Aston (c.1645-1715) (then secretary of the Royal Society), Wallis, Halley, and Flamsteed, copies of the book.[283] In reply to a letter by Aston, dated February 9, 1685, in which Aston claimed that the Annus climactericus had been "well-received," Hevelius stated that he was pleased that his work "was received by all friends, as well as by the Royal Society, with such serene countenance."[284]

Later that year, Wallis provided a favorable review of the Annus climactericus in the Philosophical Transactions[285] -- a review which worried many astronomers, including Flamsteed.[286] In a letter to Richard Towneley, Flamsteed suggested he ignore the review because Wallis did not understand the particular advantages of telescopic sights.[287] William Molyneux presented the work to the Dublin

---

282 Béziat, "La vie," 617.

283 Hevelius to Aston, April 1684, referred to in a letter from H.P. Kraus, Rare Books and Manuscripts. Hooke, of course, was not sent a copy.

284 29 December 1685, Ibid. Hevelius' reply came ten months after Aston's letter. This is a further indication of Hevelius' diminishing exchange of correspondence with Royal Society members (correspondence between Oldenburg and Hevelius had been much more frequent).

285 Vol. 15, no. 175. See also Volkoff et al., Hevelius, 45; and Simms, Molyneux, 25.

286 MacPike, Hevelius, 94. Wallis clearly indicated in his review that he favored Hevelius, and that Hooke had been unfair in his attacks against Hevelius. See also Simms, Molyneux, 25.

287 15 March 1686, MacPike, Hevelius, 95.

Philosophical Society on November 9, 1685, and on November 16, he "read a large discourse containing his thoughts of Hevelius's Annus climactericus," in which he demonstrated that the differences between Halley and Hevelius' observations in Danzig, differed significantly.[288] Molyneux also composed a review of his own which he sent to Halley on December 2, 1685, where he gave a more objective, academic statement than Wallis.[289]

After the publication of the Annus climactericus, Halley and Molyneux exchanged a series of letters which dealt with Hevelius' seeming misrepresentation of Halley. The latter became infuriated with Hevelius for three reasons: first, Hevelius claimed that Halley brought with him to Danzig a sextant, whereas Halley actually took a less-precise quadrant;[290] second, Hevelius mentioned that Halley had been sent to St. Helena by the Royal Society, whereas Halley went "on his own initiative and at his own expense";[291] third, Hevelius claimed that Halley was sent by the Royal Society to examine Hevelius' instruments, but the Royal Society had little to do with Halley's decision to visit Hevelius in Danzig.

---

[288] British Library, Add. MS. 4811, ff. 5v-7; cited in Simms, Molyneux, 25. The differences between measurements were as much as eight minutes!
[289] Ibid., 97. The letter was read during the Royal Society meeting on December 9, 1685 (Birch, History, Vol. 4, 450). Wallis' personal friendship with Hevelius contributed to his personal review of the work.
[290] This suggested that Hevelius' measurements were as precise as those taken with a sextant, a more precise instrument than a quadrant.
[291] Simms, Molyneux, 26-27.

Molyneux was also displeased with Hevelius because he believed Hevelius had misled him by insisting that Halley had used a sextant.[292] Halley gave vent to his anger by informing Molyneux of their "common concern to vindicate the truth from the aspersions of an old peevish gentleman."[293] In a subsequent letter to Molyneux, Halley stated:

> As to Mr. Hevelius we heare as yet no farther from him, and I am very unwilling to let my indignation loose upon him, but will unless I see some publick notice taken elsewhere, let [our objections against him] sleep till after his death if I chance to outlive him, for I would not hasten his departure by exposing him and his observations as I could do and truly as I think he deserves I should.[294]

Molyneux replied that Halley should speak up before Hevelius' death so that he would have the opportunity to exonerate himself.[295] However, there is no indication that Halley ever publicly expressed his displeasure towards Hevelius. Hevelius' resentment towards Hooke lasted until his death, which was hastened through the combination of Hooke's attacks, the fire in 1679, and various other concerns that troubled the last devotee of the Tychonic programme.[296]

Molyneux, who was familiar with the controversy, published his Dioptrica nova in 1692, a work which mainly

---

[292] Birch, History, Vol. 4, 477; and Simms, Molyneux, 26.
[293] 27 March 1686, MacPike, Correspondence, 60.
[294] 27 May 1686, Ibid., 65.
[295] 19 June 1686, Simms, Molyneux, 27. Also in K. Theodore Hoppen, ed. The Papers of the Dublin Philosophical Society 1683-1708 (Dublin: Irish Manuscripts Commission, 1983, text-fiche), 1140.
[296] Béziat, "La vie," 636.

focused on optics, but in which there is also found a brief discussion of the controversy. A staunch adherent of telescopic sights, Molyneux, like Halley and Flamsteed, expounded his own set of counter-arguments against Hevelius' objections as specified in the Machina coelestis. Overall, he believed that Hevelius' instruments had one major flaw that rendered them imperfect -- namely, that they lacked telescopic sights.[297] Molyneux agreed with Hooke that the eye can rarely distinguish below a minute or half a minute of arc, and that telescopes remove the glaring light which surrounds celestial objects. However, he also argued that Hevelius' exceptional powers as an observer, afforded him with more precise measurements.[298] Despite his admiration for Hevelius' accomplishments, Molyneux still believed that open sights had their limitations:

> But when the eye is armed with a Telescope, it may discern an angle less than a Second. The Telescope that magnifies distinctly the appearance of body, magnifies also distinctly the appearance of extension, space, and motion through space.[299]

Molyneux leveled his most serious criticism against Hevelius' claim that the shortness of the line of collimation, which was only between the eye and cross-hairs,

---

[297] Unlike Hooke, Molyneux argued that although Hevelius' instruments had their "open-sight" flaw, he stressed that these instruments were not "useless, faulty, or no better than Ticho's. . ." [Dioptrica nova, London, 1692), 228].
[298] Ibid., 229.
[299] Molyneux used an analogy where he claimed that it would be similar to watching the movement of a minute-hand through a microscope (Ibid., 244).

interfered with observation. Molyneux believed that this was proof enough that Hevelius did not comprehend the nature of telescopic sights. The line of collimation was not between the eye and cross-hairs, but "between the object-glass and cross-hairs."[300] Therefore, the cross-hairs were automatically superimposed onto the image of the object under observation. Despite initial problems with collimating telescopic sights, this method was eventually mastered,[301] and astronomers, such as Flamsteed, agreed that the collimation afforded a greater degree of precision in measurement and simplicity in usage.[302]

Molyneux responded to Hevelius' other objections against cross-hairs and lenses, which he felt were unjustified as well. To Hevelius' argument that cross-hairs covered small stars, Molyneux retorted that one could reasonably use the finest silk-worm thread, or the "finest needle," both of which could "bisect the smallest stars."[303] Furthermore, Hevelius' assertion that cross-hairs are invisible on dark nights was refuted by Molyneux who pointed out that one could use a small light, such as a candle, to illuminate the cross-hairs.[304] Finally, Molyneux argued that Hevelius' claim that

---

300 Ibid., 231. Molyneux proposed this same argument on two previous occasions -- in a letter to Halley, 8 April 1686 (Birch, History, Vol. 4, 477), and in a letter to Aston, 15 March 1684, read during the Royal Society meeting on 26 March 1684 (Birch, History, Vol. 4, 272).

301 Chapman, Dividing, 39.

302 Murdin, Under, 124.

303 Molyneux, Dioptrica nova, 245.

304 Ibid.

lenses fog up on cold nights because of the observer's exhalations was unjustified:

> 'Tis true, the breath of the observer, if puft into the telescope, will sully the eye-glass; but how easily is this avoided? Who is it goes purposely to make a speaking-Trumpet of a Telescope?[305]

In addressing Hevelius' objections of telescopic sights, Molyneux managed to remain the most objective in his counter-arguments, providing justification without the sarcasm which was characteristic to Hooke's attacks.

Hooke faced a disastrous fate following Hevelius' death. His quarrels with others (especially Newton), and the death of his niece-mistress in 1687, turned him into "a recluse and a cynic."[306] Nevertheless, Hooke continued to lecture against open sights. On December 6, 1693, he read a discourse on telescopes in which he mentioned how he "had improved and recommended the Use of Telescope Sights for Astronomical Instruments, in his Animadversions on Hevelius's Machina Coelestis."[307] In 1705, three years after Hooke's death, Richard Waller, then secretary of the Royal Society, published a collection of Hooke's papers and discourses which included a letter by Hooke in which he vindicated himself

---

305 Ibid.

306 Westfall, "Hooke," 487.

307 "Mr. Waller's Observations upon Dr. Hook's Discourses, concerning Telescopes and Microscopes," in Hooke, Philosophical, 272.

against the charges of Wallis and Molyneux that he had been too harsh with Hevelius.[308]

Hooke recounted the specific objections Molyneux leveled against him in a letter read during a Royal Society meeting.[309] First, they argued that what Hooke (and others) claimed is true about Hevelius' work and instruments, was "an Event highly deplorable, not only [for Hevelius], but the whole Republica Literaria". Hooke replied that if what he had said was true and certain, then the "Republica Literaria" would be better off knowing it. Second, they argued Hooke had doubted Hevelius' observations because Hevelius did not use telescopic sights or Hooke's method of dividing instruments (and also that Hooke described an instrument which was never constructed). In reply, Hooke accused Hevelius of being the "first Agressor in Print."[310] Hooke added that discovering and communicating the truth, outweighed the consequences of diminishing the importance of another's work. Hevelius had done the same with Tycho's

---

308 Found in "The Life of Dr. Robert Hooke," in Robert Hooke, The Posthumous Works of Robert Hooke Containing his Cutlerian Lectures, and other Discourses, Read at the Meetings of the Illustrious Royal Society, ed. Richard Waller, 2nd ed. (London: Frank Cass and Company Limited, 1971), Waller does not give a date for the letter. However, it had to have been written some time after Wallis' review of the Annus climactericus (1685).

309 The only letter by Molyneux between 1685 and 1687 that could be the letter referred to here, is one he sent to Halley and read on December 9, 1685 (see n. 289). This is unlikely, however, since there is no mention of Hooke at all in the letter (Birch, History, Vol. 4, 450). If Birch's volumes are accurate, then the letter had to have been read after Hevelius' death.

310 (i.e., with his Machina coelestis) (Hooke, Posthumous Works, xvi). This particular objection seems somewhat immature.

measurements and instruments, yet "Hevelius would be thought highly to value Ticho Brahe, and not to have made any Reflections upon him."[311] Moreover, Hooke argued that it was always considered proper to criticize others when necessary. Citing from his own Animadversions, Hooke asserted that he never intended to personally castigate Hevelius and his work, but Hevelius had exhausted the possibilities afforded by open sights:

> He had gone as far as was possible for humane Industry to go with Instruments of that kind, which were as compleat and exact as Instruments with plain Sights could be made; and that he had calculated with all imaginable care and skill, and deliver'd them with the like Candor and Integrity: But yet that it was my Opinion, that this ought not to discourage others from making use of Telescope-sights, and to make better Observations with Instruments by that means more exact.[312]

Hooke continued by complaining that he was misrepresented when others asserted that he claimed telescopic sights yielded measurements which were 60 times more precise than those using open sights.[313] Lastly, they

---

[311] Ibid., xvii.

[312] Ibid. Hooke did not have to worry about others using open sights.

[313] I have come across, not only primary sources, but secondary sources that make this claim. Hooke was justified in making this objection despite the fact that he claimed that his instrument (the one described in his Animadversions) is "40 times better then what is now made and described by Hevelius. . ." (Animadversions, in Gunther, Early, Vol. 8, 73). Those who have inaccurately suggested that Hooke claimed telescopic sights were 60 times more accurate than open sights, might have been confused with Hooke's claim that he could "do more with a Quadrant, Sextant or Octant, of 1 foot Radius, furnished with Telescopical Sights and Screws, then can possibly be done with any other Instruments, furnished only with Common Sights, though 10, 20, 30, nay threescore [60] foot Radius (emphasis mine, Ibid., 79). Therefore, he

argued that Hooke never took up Hevelius' challenge to carry out measurements using telescopic sights.[314] Hooke responded that the Fire of London prevented him from carrying out the particular measurements Hevelius had specified, although Hevelius' "unkind Reception" of previous observations by Hooke, "was enough to deter" Hooke from complying with Hevelius' wishes.[315] Although Hooke replied to each complaint in his letter, his sarcastic and acrimonious personality could not be separated from his views. In the final years of his life, he wrote several small papers, but he remained bed-ridden, and died on March 3, 1702.[316]

After Hevelius' death in 1687, no one else continued to advocate the use of open sights with the same fervor as Hevelius, nor was anyone qualified to continue this tradition. Astronomers realized that the nature of astronomy had changed to such a degree that to emulate Hevelius' methods was to go back on progress itself.

---

was discussing the dimensions of instruments, not the increase in precision between the two types of sights!

314 Hooke, Posthumous Works, xvi.

315 Ibid., xviii.

316 Westfall, "Hooke," 487.

Chapter 4

Evaluation and Interpretation of the Controversy

Reasons for Hevelius' Continued Advocacy of Open Sights

Despite the claims others made about the advantages afforded by telescopic sights, Hevelius remained a staunch supporter of open sights, and there are several reasons for this. First, he did not want to add telescopic sights to the instruments he had been using for decades.[317] He spent many years in constructing and perfecting instruments that he thought were especially useful for making astronomical measurements and he had grown accustomed to using these instruments. Adding telescopic sights would force him to relearn how to use instruments which he doubted were superior to his own. John Wallis pointed out the futility of changing Hevelius' instruments when he stated:

> For we are to consider, that his Instruments were made, (with great cost & care,) & he a diligent Observer with them, and by long practise (for it is not to be gained presently) expert in the manage of them, long before these Telescopic Sights were thought of. And I do not know, whether, in such Instruments so well fixed, & so charyly

[317] Volkoff et al., Hevelius, 46.

> preserved hitherto, it were advisable to alter them. For, beside ye expense of time, & losse of so many good Observations which might be made while that must be doing, he might possibly spoil a good Instrument instead of making it better. . . . And we know, yt, in travailing, when a man hath once made a choise of a good Rode, though perhaps not absolutely ye best, he may sooner come to his journies end by keeping steady to that, than by often shifting of Rodes in hope to find a better. And so here; a diligent use of good Instruments, though perhaps not absolutely the best possible, doth more advance ye work, than spending the time in projecting or making better Instruments with making little or no use of them.[318]

Wallis' main contention, that Hevelius is better off using instruments he is familiar with, was shared by William Molyneux who agreed that, over the years, Hevelius had become accustomed to his instruments.[319]

Furthermore, Hevelius argued that no one had proven their advantages over plain sights. He believed that only those who had carried out decades of observation using telescopic sights, could successfully evaluate their relative merits.[320] Nevertheless, he did not dispute the theoretical advantages of telescopes (namely, that they brought objects

---

318 12 January 1674, Hall and Hall, Correspondence, Vol. 10, 432-433.

319 Molyneux, Dioptrica nova, 244-245.

320 "When at some time we shall have observations from both parties continued over a space of 20 or 30 years, that is to say those taken from the heavens with telescopic sights and those taken only with our [naked] sights, the matter will be much clearer" (Hevelius to Oldenburg, ?16 March 1674, Hall and Hall, Correspondence, Vol. 10, 521-522). The advantages in Hevelius' statement are clear -- telescopic sights, although initially discovered in the 1640's, were not commonly used until the late 1660's. Since they had only been fully exploited for several years when he made this suggestion, it would take another ten or twenty years for someone to carry out prolonged observations using telescopic sights.

closer to the eye); although he argued that they were not superior to open sights in terms of practical measurement.[321]

Hevelius also ran the risk of calling the accuracy of all his observations into question, by acknowledging the superiority of telescopic sights.[322] If he abandoned plain sights, he sustained (perhaps correctly) that his contemporaries would question the accuracy of all the observations and measurements he had previously obtained.[323]

Finally, Hooke's reactions to Hevelius' continued use and support of open sights, contributed to Hevelius' continued advocacy of them. First, Hevelius believed that Hooke did not have enough years of experience, using telescopic sights, to draw any conclusions.[324] However, Hooke's lack of experience was only the first among Hevelius' criticisms against Hooke in his advocacy of the superiority of open sights. He also argued that Hooke was "all words and no action:"[325]

> I am a citizen of the free republic of letters, and a member of that illustrious Royal Society whose motto is Nullius in verba. Accordingly, whoever exercises his right and attempts the task in his own style and relies on the bare word of no one, let him but make

---

[321] Volkoff et al., Hevelius, 46.

[322] Hooke, Posthumous, xv.

[323] Molyneux agreed with this particularity: "The accuracy of all [Hevelius'] former labors did depend" on his use of instruments which he was most familiar with (see text marked by n. 322). Molyneux, Dioptrica nova, 244-245.

[324] See n. 323.

[325] This only added to Hevelius' certainty that no one could prove the supremacy of telescopic sights.

> it his business to furnish not phrases only but also the facts he has promised, in the manner to which his duty binds him.[326]

Hooke's overall behavior towards Hevelius, his work, and his instruments, was more than enough to persuade Hevelius to retain his use of open sights.[327]

Hevelius' Supporters

During the controversy, several individuals supported Hevelius. Most of these appear in his Annus climactericus, where letters from Wallis, Ismael Boulliaud (1605-1694), Titus Livius Burattini, and Bernhard Fullenius, "who were in a position to judge for themselves the care and precision that he brought to all his observations," were quoted at length.[328]

John Wallis supported Hevelius from the very beginning of the dispute. In 1667, he wrote to Oldenburg that he "assents" to Hevelius' use of "Quadrants, sextants, & such other instruments (for places & distances) much before those of the Telescope; though this allso be in its kind an

---

326 Hevelius to Oldenburg, 21 August 1675, Hall and Hall, Correspondence, Vol. 11, 473.

327 "Vilifying [Hevelius'] instruments, and slighting his performances with them as no better than those in the Age before him, did but exasperate the noble old man, and made him adhere more obstinately to his former practice" (Molyneux, Dioptrica nova, 230-231). See also King, History, 101-102.

328 Béziat, "La vie," 617.

Excellent Instrument, where those can not be used."[329] However, even though he supported Hevelius' use of open sights, Wallis clearly did not believe that they were always the most appropriate sights to use.

After the publication of the Machina coelestis, Wallis argued that there was "no reason to be displeased" with Hevelius' continued use of open sights on measuring instruments, and that it is better for Hevelius to continue using the instruments he was most familiar with.[330] However, Wallis also stated that Hevelius' instruments were not the best instruments to use, and that it would have been improper to discourage others from using telescopic sights.[331]

But although Wallis did not necessarily advocate the use of open sights under all circumstances, he nevertheless, defended Hevelius against Hooke. Following the publication of Hooke's Animadversions, Wallis claimed that Hooke's attacks had been too personal:

> I have now read ye whole of Mr. Hooke's against Hevelius, which I think bears a little too hard upon him. Hee might have published his own way to as good advantage as he pleased, without so frequent Reflections on Hevelius, as he hath at every turn. For Hevelius hath deserved well.[332]

---

329 19 January 1667, Hall and Hall, Correspondence, Vol. 3, 313.

330 Wallis to Oldenburg, 12 January 1674, Ibid., Vol. 10, 432-433 (see passage marked by n. 318).

331 "But those who. . . think Telescopick sights so much better: As I would not blame [Hevelius] for making ye best use he can of what he hath; so neither would I discourage them from doing better." (Ibid.)

332 Wallis to Oldenburg, 11 January 1675, Ibid., Vol. 11, 154-155.

Furthermore, Wallis provided the best support Hevelius could have asked for by publishing a favorable review of the Annus climactericus in the Philosophical Transactions.

Wallis began his review by denouncing Hooke's behavior, and arguing that Hooke had self-serving reasons for his attacks of Hevelius:

> Mr. Hook published his Animadversions. . . with much more of bitterness and boasting (as this Author thinks, and others also whom [Hevelius] cites,) then there was reason for. Which he thinks was done out of design to disparage Him, his Instruments, and his Observations (unsight and unseen,) and to prepossess others with mean and slight thoughts of them, (even before they were yet published;) and a high opinion of himself who (with so little charge and so small Instruments) could do things so much more accurate than had hitherto ever been done, by any: thus seeking to raise his own reputation by disparaging what is done by others, in things wherein himself doth nothing.[333]

Although he castigated Hooke for his unprofessional attitude, Wallis recognized the perils involved when either party deviated from the proper behavior expected of participants in disputes, and he stressed the importance of experience:

> For there be advantages, and disadvantages, in both ways; which may, by sharp words, be aggravated to a great hight; while yet, whether of the two, upon the whole matter, is

---

[333] Philosophical Transactions, Vol. 15, no. 175, 1165. Wallis added that Hooke had the nerve to censure others when he, himself, has nothing to boast about: "Thinking that it more becomes learned Men, not to boast of what they can, or will, or mean to do, but rather to let the world know what they have done. And when Mr. Hook hath performed things so much more accurate, it will then be time to tell the World what they are" (Ibid., 1172).

> to be preferred, cannot be otherwise determined than by experience.[334]

Even as late as 1686, Wallis indicated that Hevelius' "instruments and observations [are not] so contemptible. . . as [Hooke] seems to represent them," and that furthermore, "Hevelius with his plain sights can distinguish to a small part of a minute, notwithstanding what hath been said to the contrary."[335]

Ismael Boulliaud (Bullialdus) supported Hevelius' adherence to open sights, and in a letter to Oldenburg, Boulliaud declared that he was "of the same opinion" as Hevelius.[336] Boulliaud listed two specific reasons for his shared support of open sights:

> [Hevelius] also wrote to me about his opinion about using a telescope instead of [open] sights for observation; in this I am of his opinion, the more because it is as it were impossible that there should be no refraction, which will make the object [appear to be] remote from the place where the eye alone, unaided by the telescope, will see it. For the rest, those with sharp sight have only to make a telescope to observe that it is only

---

334 Ibid. He also stated that Hooke was never able to provide that experience and prove to Hevelius that telescopic sights were more accurate.

335 Wallis to Oldenburg, 12 February 1686; cited in Scott, Mathematical, 128-129.

336 6 February 1675, Birch, History, Vol. 3, 181. The letter was read to the Royal Society on February 11, 1675. Others, like G. W. Leibniz (1646-1716), became aware of Boulliaud's open support of Hevelius' views, even though they were not familiar with the specifics of the controversy: "I have seen Hooke's attack on Hevelius' apparatus; I am not sufficiently versed in astronomical observation to dare put my oar in. Mr. Boulliaud seems to stand by Hevelius; Cassini and Picard think that telescopes are not to be neglected" (Leibniz to Oldenburg, 20 March 1675, Hall and Hall, Correspondence, Vol. 11, 242).

useful to those who blink (myops and dim-sighted people).[337]

Although the nature of Wallis and Boulliaud's support differed somewhat,[338] it is important to emphasize the personal interactions between the two with Hevelius, that may have biased their opinions. Unlike most of the adherents of telescopic sights, Boulliaud and Wallis were personally acquainted with Hevelius for decades before the controversy erupted. Hevelius met both men when he traveled around Europe in the early 1630's -- Wallis in 1630, and Boulliaud in 1631.[339] Furthermore, they were closer to Hevelius in age than other adherents of telescopic sights.[340] At the time of their meeting, Hevelius was nineteen, Wallis was about fourteen, and Boulliaud twenty-six. Consequently, Wallis and Boulliaud were more familiar with open sights, as telescopic sights were not commonly used before 1660. This does not necessarily suggest that they supported Hevelius solely because they had similar experiences with astronomical instruments, but it does suggest that Wallis and Boulliaud

---

[337] Boulliaud to Oldenburg, mid-April 1675, Ibid., Vol. 11, 277.
[338] Wallis, unlike Boulliaud, did not necessarily believe that open sights yielded more precise measurements. He argued, instead, that the accuracy in Hevelius' measurements were due to Hevelius' powers as an observer.
[339] Béziat, "La vie," 500.
[340] G-D Cassini, Picard, and Auzout, other adherents of telescopic sights who were born in the 1620's, were exceptions. Nevertheless, Wallis and Boulliaud were still older (even if only by several years).

were more tolerant because they were familiar with open sights.[341]

After Hevelius became acquainted with Wallis, they initiated a correspondence in the mid-seventeenth century. As in the case of Oldenburg, the correspondence between Wallis and Hevelius helped forge a friendship between the two that had matured by the beginning of the dispute. Furthermore, Wallis and Hooke disliked and mistrusted each other. Both of these factors influenced Wallis' biased support of Hevelius, and castigation of Hooke.

Boulliaud's adherence to the use of open sights may also have been affected by his personal friendship with Hevelius. The two kept up an active correspondence after Hevelius' visit, and Boulliaud never ceased to admire Hevelius' work.[342] Furthermore, Boulliaud traveled to Danzig in 1661, to renew his friendship with Hevelius and to learn more about the nature of Hevelius' "instruments and methods of observation."[343] This suggests that Boulliaud was even more inclined to accept Hevelius' practices after having been an eye-witness, and actual participant, in Hevelius' observations.

Hevelius also received the support of Bernhard Fullenius, a councilor and son of a former professor of

---

[341] Hooke was about twenty-four years younger than Hevelius, Flamsteed was thirty-five years younger, and Halley was forty-five years his junior.
[342] Béziat, "La vie," 539.
[343] Ibid., 537.

mathematics at the University of Frankfurt-on-Oder.[344] Hevelius printed several of Fullenius' supporting letters in his Annus climactericus -- letters which clearly indicated Fullenius' position in the matter. Wallis, in his review of the text, summarized the contents of each of the twenty-seven letters printed by Hevelius. The ninth letter, written by Fullenius to Hevelius, is one of two

> gratulatory Letters upon the Edition of [Hevelius'] Organographia[345] and his Instruments therein described; highly commending them, and the great accuracy of the Observations made thereby (of which both of them had been Eye-witnesses, and esteem it a great happyness so to have been;) and with so great exactness (within less than 6 seconds) as, without having seen it, they could hardly have believed.[346]

Fullenius openly defended Hevelius in a letter dated March 1, 1675:[347]

> As for your Organa,[348] there is no reason for anyone to doubt their truth, accuracy, and reliability, of which I have such considerable proof that had I not previously been an eye-witness of your observations, I should not have the least conception of the like. The most extensive possible range of observations to the fifth magnitude, made in my presence, constitute evidence for this, sufficiently proving and demonstrating the trustworthiness

---

[344] Forbes, Gresham, 35.

[345] Machina coelestis

[346] (Emphasis mine) The other letter being referred to is the eighth from Boulliaud to Hevelius. Wallis added that the latter (Fullenius) "declares his suspicions of the uncertainty of Telescopick Sights, preferring others before them" (Philosophical Transactions, Vol. 15, no. 175, 1171).

[347] This may have been the letter Wallis mentioned as letter #9, and cited above.

[348] Machina coelestis

> and precision which has never given rise to an error of as much as six seconds. I bear witness that this duly occurred whenever you had me as a companion while observing the fifth magnitude. I found the same thing while I had your very dear wife and Johann (my printer who observes for me) as companions. Thus, since you have innumerable observations of the greatest [degree of] correctness, you can deservedly boast of the great accuracy of your instruments.[349]

Like Boulliaud, Fullenius not only supported Hevelius' use of open sights, but advocated their use as well.

Finally, Hevelius received the support of Titus Livius Burattini, an optician who established himself in Warsaw in 1670, where "he made several of the large instruments used by Hevelius."[350] Undoubtedly, he favored the use of open sights since he constructed instruments for Hevelius. Burattini also indicated his support for Hevelius and praise for his instruments and observations in a letter which Hevelius later published as letter nineteen in his Annus climactericus.[351]

The support Hevelius received from both Flamsteed and Halley differed considerably from the kind just cited. Unlike Wallis, Boulliaud, Fullenius, and Burattini, Flamsteed

---

349 (Emphasis mine) Cited in Forbes, Gresham, 35-36. This testimony was excerpted from Hevelius' letter to Flamsteed, dated June 24, 1676. Notice how, like Boulliaud, Fullenius stressed the importance of having been an eye-witness to Hevelius' observations and instruments. He also noted both the exceptional accuracy and precision of Hevelius' instruments and measurements. This is an indication that the distinction between accuracy and precision was not clear during this period.

350 Daumas, Scientific, 66. He was also an "assistant at [Hevelius'] Observations" (Wallis, Philosophical Transactions, Vol. 15, no. 175, 1175). That naturally made him an eye-witness of Hevelius' work.

351 Ibid.

and Halley were staunch advocates of telescopic sights, and their support was not for Hevelius' continued advocacy of open sights, but rather a defense against those who had ridiculed Hevelius, especially Hooke. Theirs, therefore, was a rather circumscribed defense.

Although Flamsteed thought open sights yielded inaccurate measurements, he was willing to support Hevelius against Hooke when he heard of Hooke's acrimonious attacks on Hevelius' work. Flamsteed offered "to defend Hevelius against Hooke's criticisms" because Flamsteed did not think Hooke could carry out any significant observations on his own.[352] In return for this defense, "Hevelius supplied Flamsteed with observations of Mars's position with respect to the same stars in Aquarius on which the latter had based his initial investigation of the solar parallax."[353] When the friendship between the two astronomers was temporarily truncated, however, it took a series of letters between Flamsteed, Oldenburg, and Hevelius, to restore the rapport between Flamsteed and Hevelius.[354]

Halley's support for Hevelius came late in the controversy, after Halley visited Hevelius in Danzig in 1679.

---

[352] 20 July 1676, Hall and Hall, Correspondence, Vol. 13, 20 n. 1. The editors have paraphrased the letter from the Royal Society MS. F.1, no. 118.

[353] Forbes, Gresham, 36. Flamsteed did not want to counter Hevelius' views because Hevelius had already threatened not to publish his star catalog, something Flamsteed was eager to read.

[354] Flamsteed to Oldenburg, 5 April 1677; Oldenburg to Hevelius, 19 April 1677; Hevelius to Oldenburg, 28 November 1677 (all in Hall and Hall, Correspondence, Vol. 13).

Halley, who had no reason to support Hevelius' use of plain sights, was, nevertheless, amazed at the accuracy of Hevelius' measurements. The testimonial letter which Halley left behind at Hevelius' request, undeniably attested to Halley's satisfaction that Hevelius' instruments and observations were comparable to those fitted with telescopic sights. Furthermore, Halley wondered why he ever doubted the accuracy of observations by open sights, and readily "offer[ed] himself a voluntary witness" to the accuracy of Hevelius' instruments and observations.[355] Once more, a witness to Hevelius' work, claimed that Hevelius' measurements were, for the most part, accurate. It is even more significant that this affirmation came from a champion of telescopic sights. Naturally, Hevelius was delighted that a member of the "new school" confirmed the accuracy of instruments from the "old school", but Halley eventually regretted leaving that letter behind.[356] Consequently, Halley's support, like Flamsteed's, was half-hearted.

---

[355] (Emphasis mine) 8 July 1679. Letter translated by Wallis in Philosophical Transactions, Vol. 15, no. 175, 1169.

[356] In a letter to Flamsteed, Molyneux stated that, although Halley wrote an epistle against Hevelius' Annus climactericus, he had already compromised himself by leaving behind that testimonial letter (20 February 1686, Hoppen, Papers, 1082-1083). Furthermore, Molyneux claimed that Halley's letter "grossly flattered" Hevelius, and that Halley has "imposed on the world" (Hoppen, Papers, 1140; and Simms, Molyneux, 27).

The Expressed Concern and Involvement of Others

The concern to vindicate their views was not limited to Hevelius and Hooke alone. Hevelius' claim that the naked eye was as accurate as the telescope for purposes of measurement, struck a raw nerve with astronomers like Halley, Flamsteed, and Molyneux. Although they recognized Hevelius as one of Europe's foremost astronomers, they were unsettled by his claims and did their best to try to refute his arguments.

Molyneux fully appreciated the significance of Halley's testimonial letter. He realized that because it came from an advocate of telescopic sights, it carried more weight, and Molyneux was vocal in expressing such an opinion.[357] However, he also "pointed out that Halley's testimonial to the accuracy of Hevelius's observations could only cast doubt on his own telescopic observations," and that furthermore, "Hevelius' [Annus climactericus] would lead to a slighting of telescopic sights."[358]

Halley also recognized the significance of Hevelius' continued advocacy of open sights and naked-eye measurements in a letter to Molyneux:

> The Controversy between Mr. Hevelius and Mr. Hooke, as you very well observe does, as Hevelius manages the matter, affect all those observers that use Telescopick sights, and myself in particular, and it is our common

---

[357] See n. 358.

[358] (Emphasis mine) Simms, Molyneux, 25-26.

> concern to vindicate the truth from the aspersions of an old peevish gentleman, who would not have it believed that it is possible to do better than he has done. . . .[359]

Ultimately, Halley realized that accepting the accuracy of Hevelius' measurements had been a mistake. By indicating that Hevelius' measurements were as accurate as his own, he bestowed uncertainty, not only to his own measurements, but those performed by others using telescopic sights.

Wallis' favorable review of the Annus climactericus, also elicited concern from certain champions of the telescope, including Flamsteed.[360] Flamsteed advised Richard Towneley in 1686 to ignore Wallis' review because it was written from a personal perspective. Flamsteed also argued that Wallis did not really understand the nature of telescopic sights:

> You need not be concerned at Dr. Wallis his account of Hevelius his booke, hee is onely minding to gratifie his old friend & speakes the better of him both because hee is sensible with ye rest of the World of Mr. Hookes intolerable boastes, as also by reason hee was never used to observations with great instruments and therefore understands not the advantages of telescope sights above plaine ones.[361]

---

359 (Emphasis mine) 27 March 1686, MacPike, Correspondence, 60.

360 MacPike, Hevelius, 94; and Simms, Molyneux, 25.

361 15 March 1686, MacPike, Hevelius, 95. This passage also suggests that Wallis was more tolerant of open sights because he was more familiar with their use than with telescopic sights (see p. 102).

Despite Flamsteed's censure, he decided not to criticize Wallis because, he argued, Wallis was too old and had "suffered much of late."[362]

Hevelius' career as an astronomer reveals, in part, why others besides Hooke, felt it was necessary to vindicate the precision of telescopic sights. Hevelius had been carrying out observations since the late 1630's, when his teacher Peter Krüger, prompted him to study astronomy. By 1644, he expanded his small, one-room observatory, into what became known as "Stellaburgum" -- a grandiose observatory by the standards of that age.[363] His renown spread throughout Europe as "savants, ambassadors, and princes themselves were curious to visit his magnificent observatory, and show. . . their admiration for this private individual who, in his love for science, did not shrink back from any financial sacrifice."[364]

In 1647, Hevelius published his first major work, Selenographia, which received great praise for its "lunar maps [which] were incredible and unrivaled for more than 100 years."[365] Furthermore, intellectuals such as Pierre Gassendi (1592-1655), Marin Mersenne (1588-1648), and Boulliaud, were impressed by the work, especially the lunar engravings.[366] Hevelius, who successfully carried out his observations for

---

362 Ibid.
363 North, "Hevelius," 360.
364 Béziat, "La vie," 536.
365 Volkoff et al., Hevelius, 18.
366 Béziat, "La vie," 513.

over twenty-five years before anyone challenged his use of open sights, was recognized as one of Europe's foremost astronomers, and he was distinguished as a skillful observer.[367] Consequently, he could not be simply brushed aside and ignored by those who claimed superior knowledge.[368] Moreover, after Selenographia, Hevelius continued to publish works which circulated throughout Europe, including London, where he would most often send copies of his texts to the Royal Society. Because Hevelius was an important author and contributed consequential observations, astronomers were naturally concerned with his opinions and views. And if Hevelius did not have enough authority to discredit telescopic sights, he was able to successfully challenge their superiority over common sights.

The Nature of Hooke's Attacks and Their Legitimacy

Nowhere is Hooke's manner of criticism towards Hevelius more apparent than in his Animadversions, where he did not limit his censure to open sights alone, but attacked Hevelius' work, observations, and instruments in general. Hooke managed to turn the controversy into a personal

[367] MacPike, Hevelius, 15.

[368] McMullin states, "Challenge from someone perceived (rightly or wrongly) as a crank or as incompetent does not suffice to create controversy" ("Scientific Controversy," in Engelhardt and Caplan, Scientific Controversies, 52).

battleground in which he often ridiculed and mocked Hevelius, as when he compared the latter's instruments with those used by Tycho Brahe, thereby insinuating that Hevelius' instruments were no more advanced than Tycho's.[369] Hooke added:

> But yet if he had prosecuted that way of improving Astronomical instruments, which I long since communicated to him, I am of the opinion he would have done himself and the learned World a much greater piece of service, by saving himself more than 1/10 of the charge and trouble, and by publishing a Catalogue ten times more accurate.[370]

Hooke also claimed that the use of plain sights was not only useless, but detrimental to astronomical progress as well,[371] thereby implying that Hevelius' measurements were inaccurate, and that his stubborn use of these instruments impeded with the progress of science.

Hooke also incorporated excerpts from Hevelius' correspondence with Oldenburg, where Hevelius listed his grievances against telescopic sights. Hooke claimed, however, that Hevelius "neither hath, had, nor can have any experience, to shew Telescopical Sights not to be as good as the Common, or that they are less applicable to large Quadrants, Sextants, Octants, or Azimuth Quadrants."[372]

---

369 Animadversions, in Gunther, Early, Vol. 8, 38. He added that he was "the more sorry to find that [Hevelius] hath proceeded to finish his Machina Coelestis, by instruments not more accurate than those of Ticho. . ." (Ibid., 42).
370 Ibid., 38
371 Ibid., 45.
372 Ibid., 77.

Moreover, Hooke suggested that Hevelius has "some dread of making use of Glasses in any of his Sights" whether it is because glasses (lenses)

> have some hidden, un-intelligible, and mysterious way of representing the Object, or whether from their fragility, or from their uncertain refraction, or from a supposed impossibility of fixing them to the Sights, or whether from some other mysterious cause, which [Hooke is] not able to think of or imagine, [he] cannot tell.[373]

Needless to say, Hooke pointed out some of Hevelius' virtues, but even those comments sounded begrudging:

> But this, though it were a very great unhappiness to Hevelius, that he was not furnished with better contrivances, yet it no ways tends to his dispraise, for his most extraordinary and indefatigable care, pains and industry, is so much the more to be admired, esteem'd and honour'd, and will be so much the more, by such as have by experience found the difficu'ty, of making any one Observation certain in that way.[374]

The nature of Hooke's objections towards Hevelius are not necessarily invalid. However, he was unable to separate his irascible behavior towards Hevelius from the dispute, behavior that was not reflected in the attitudes of all the others involved.[375] Hooke's peers did not fail to notice his disdain towards Hevelius. Wallis had claimed that Hooke had unnecessarily attacked Hevelius,[376] and even Newton, who hated Hooke, mentioned that Hevelius and others had complained

---

[373] Ibid., 47.

[374] Ibid., 102.

[375] Volkoff et al., Hevelius, 40.

[376] See pgs. 99-100.

about Hooke's hostile disposition.[377] Molyneux also took "Hooke to task" for the way in which he criticized Hevelius,[378] and he affirmed that the best way Hooke could have changed Hevelius' views, was by laying down the "Dioptrical reasons" for the "performance and exactness" of telescopic sights, thereby answering Hevelius' objections.[379] In a paper he presented to the Dublin Philosophical Society, entitled, "Concerning telescopic sights as adapted to astronomical and other instruments," Molyneux maintained that "Hooke could have been more prudent" in his "little English pamphlet," against Hevelius.[380] Molyneux agreed with Hevelius when he professed that "it is absolutely intolerable [for Hooke] to promise so much and perform so little."[381]

Overall, Hooke was correct when he claimed that telescopic sights were capable of greater precision than open sights.[382] However, he "underestimated the accuracy of naked-

377 "Mr. Hooks letter in several respects abounded too much with that humour [temperament] wch Hevelius & others complain of. . ." [Newton to Halley, 20 June 1686, H.W. Turnbull, The Correspondence of Isaac Newton, 1661-1709, Vol. 2, (Cambridge, 1960); cited in Volkoff et al., Hevelius, 40].

378 Simms, Molyneux, 25. Molyneux expressed this sentiment in his Dioptrica nova (see n. 327).

379 Molyneux, Dioptrica nova, 230-231.

380 Read to the Society, February 11, 1684, Hoppen, Papers, 605. Molyneux's reference to Animadversions as a "pamphlet", infuriated Hooke. Later, in a letter to Flamsteed, Molyneux related Hooke's unfounded reasons for his anger, and he redescribed Animadversions as a "vain, scurrilous, bragging pamphlet" (22 December 1685, Hoppen, Papers, 1071; and Simms, Molyneux, 26). Molyneux was so incensed with the matter that he stated: "[I hear that Wallis] has taken up the cudgels and vindicated Hevelius against Hook. I should be very glad if the doctor and I have jumped together. . ." (Hoppen, Papers, 1071).

381 Molyneux to Flamsteed, 18 April 1682, Simms, Molyneux, 25.

382 Volkoff et al., Hevelius, 45.

eye observations," and especially Hevelius' observational skills.[383] Hevelius' powers as an observer were responsible for his success at accurate measurements, regardless of the methods he used. Hooke also "overestimated the accuracy that could be achieved with telescopic sights by the contemporary observers."[384] Hooke argued that he could make observations with telescopic sights which were at least ten times better than those made with open sights,[385] and claimed that he could

> do more with a Quadrant, Sextant or Octant, of 1 foot radius, furnished with Telescopical Sights and Screws, then can possibly be done with any other Instrument, furnished only with Common Sights, though 10, 20, 30, nay threescore foot radius. . . Observations made with Common Sights,. . . are no ways capable of certainty to a minute or two.[386]

Hooke, however, never proved his claim, while Hevelius' observations remained almost as precise as Halley and Flamsteed's. The proof that telescopic observations supplied more precise measurements than open sights, did not become apparent until years after Hevelius' death, when Flamsteed completed his new star catalog.[387]

---

383 Ibid.

384 Volkoff et al., Hevelius, 45-46.

385 Animadversions in Gunther, Early, Vol. 8, 46. At other times, he claimed he could make observations which were forty times more accurate.

386 Ibid., 43.

387 Hall and Hall, Correspondence, Vol. 11, xix-xx; and Chapman, Dividing, 32.

Which Sights Yielded More Precise Measurements and Why?

It is not the purpose of this study to determine which of the two sights yielded more precise measurements, but it would suffice to compare the accuracy of each. In contrast to Hooke, Flamsteed took up Hevelius' challenge to observe the eight measurements of stellar distances using telescopic sights. The values between the two astronomers "agree, on the average, to less than one-half minute of arc (the standard deviation is twenty-three seconds)."[388] By comparing the values for the errors in the eight measurements, as produced by Tycho (~1585), Hevelius (~1670), and Flamsteed (~1680), with modern day figures, one notices that "not a single one of the errors of the three observers is as large as a minute of arc (60″)" and that furthermore, "Flamsteed's early telescopic measurements are not a factor of sixty, but only slightly better than those of Hevelius" (Figure 14).[389] Therefore, although there was a slight increase in accuracy between Hevelius and Flamsteed's respective measurements, the standard deviation between the two makes the decision to pick the more accurate sights almost negligible.

---

388 Volkoff et al., Hevelius, 46.

389 Ibid., 47. Volkoff et al assume that Hooke claimed telescopic sights were 60 times more accurate, but as Hooke argued, this was not the case. Nevertheless, Hooke did argue that telescopic sights were 40 times more accurate than open sights, thereby making the difference between 60 and 40 almost negligible.

If telescopic sights were supposed to yield, in theory, more precise measurements, then why were Hevelius' measurements, on average, as accurate as Flamsteed's early observations? The maturity of Hevelius' instruments and his remarkable eyesight are only part of the answer.[390] Hevelius had other qualities which guaranteed his success, including "a skillful hand at drawing and engraving, [and] an unfailing patience."[391] Furthermore, Hevelius' strong will to stay up all night, and expose himself to bad weather, contributed to his success at "observing for half a century with admirable constancy."[392] Molyneux further contemplated the reasons for Hevelius' success:

> And tho I must confess ingeniously, that this renowned astronomer, by his extraordinary diligence, great care, and perpetual long-continued practice, but chiefly by his peculiar sharpness of sight, had arrived to a great exactness of observation by plain sights (as I find by comparing the observations made by the most curious astronomers of our age, Flamsteed, Halley, Cassini, &c. by telescopick sights, with those observations made by Hevelius;) yet this we are to attribute more to the peculiar acuteness of his eye, and to his extraordinary diligence, and care in observation, than to the exactness of plain sights.[393]

Molyneux best summarized the reasons for Hevelius' success at precise measurements. In addition to his excellent eyesight,

---

[390] Chapman limits Hevelius' success to these two factors (Dividing, 32-33).
[391] Béziat, "La vie," 504.
[392] Ibid., 592.
[393] Molyneux, Dioptrica nova, 229.

"mature" instruments, and unfailing patience, Hevelius understood the importance of meticulously checking, and rechecking, all his values so that he could obtain the most accurate measurements possible. Furthermore, his years of practice provided him with the necessary experience to calculate these measurements. Hevelius' measurements were exceptionally precise, not because he used open sights, but because he had at his disposal, the necessary qualifications for success. For this, and other reasons, no one succeeded him. Hevelius' successor would have to have essentially the same characteristics as Hevelius in order to yield measurements of the same caliber.

Conclusion

The Intellectual and Social Contexts of the Hevelius-Hooke Controversy

Like other controversies of the seventeenth century, the Hevelius-Hooke dispute reveals the values and beliefs of the individual participants involved, and the interactive relationships among them. It offers an opportunity to understand better the new precision astronomy which emerged in the course of the seventeenth century, and how instruments such as the telescope, micrometer, and measuring instruments, influenced the new practice. A careful study of the arguments used by Hevelius and Hooke, also provides insight into the seemingly "strange" debate over the relative merit of open versus telescopic sights, and the reasons why the participants held such divergent views. Finally, the complex relationships among the participants provides insight into why the controversy deviated from the proper behavior expected of all those involved, and how these relationships affected the course of the controversy.

Adherents of telescopic sights, especially Hooke, believed that Hevelius refused to accept what they considered the revolutionary advances provided by astronomical instrumentation, especially optical devices. Since Hevelius insisted that naked-eye sights yielded measurements as precise as telescopic sights after the latter had become common, Hevelius was charged with the retardation of scientific progress. To his critics, then, Hevelius was misguided in his astronomical inquiries because he refused to supplement his senses with optical instruments.

Witnessing the production of knowledge played an important role in this controversy as well. According to Shapin and Schaffer's argument, the testimony of eye-witnesses was effective only if the witnessing experience was accessible, and the witnesses, themselves, were reliable.[394] Although Hevelius lived and worked in Danzig, far from London circles, the testimony of his eye-witnesses was effective enough to concern his critics. Supporters such as Titus Livius Burattini, Bernhard Fullenius, and Ismael Boulliaud stressed the importance of having eye-witnessed Hevelius' observations and instruments. Even Edmond Halley, the only adherent of telescopic sights to travel to Danzig, testified to the incredible accuracy of Hevelius' measurements. More importantly, Hevelius, himself, was considered a reliable witness by Royal Society members. Even before the founding of the Royal Society, astronomers and scientists in Europe

---

[394] Shapin and Schaffer, Leviathan, 336.

depended on Hevelius' observations and measurements. Therefore, Shapin and Schaffer's criteria for witnessing the production of knowledge do not apply to this case study.

Unlike their arguments concerning "virtual witnessing", Shapin and Schaffer's points on the physical integrity of instruments in producing "matters of fact", are relevant to this controversy. Halley's visit to Danzig, and his testimonial letter to Hevelius, confirmed the integrity and precision of Hevelius' measuring instruments -- to the dismay of those using telescopic sights. Perhaps other advocates of telescopic sights would have followed Halley if they did not question the physical integrity of his measuring instruments. However, astronomers in the Royal Society, especially Hooke, believed that his instruments were inferior to those equipped with telescopic sights because Hevelius' instruments were easily susceptible to bending, warping, and wearing away with time. Consequently, Hevelius' measuring instruments lacked the capacity to produce "matters of fact," and members of the Royal Society questioned the integrity of the observations produced using these instruments. Attacking the physical integrity of instruments was not limited to one side, however, as Hevelius criticized the use of telescopic sights, arguing that they interfere with observation, and lenses, which could easily fog up or break.

Despite the fact that disputes were supposed to occur safely within the Royal Society, the proper behavior expected of participants often escalated into a bitter controversy, especially where Hooke was concerned. However, Hooke was practically ostracized for his behavior. He not only criticized Hevelius' inaccuracies, but also attacked Hevelius for stubbornly refusing to modify his instruments, and by doing so, questioned Hevelius' integrity and reputation as an astronomer. Accordingly, he turned Hevelius from a "mere dissenter" into a "foe" (according to Shapin and Schaffer), and made Hevelius even more skeptical of the new astronomy. It is necessary, however, to differentiate between Hooke and the Royal Society. Not all members of the Royal Society sided with Hooke -- Oldenburg, Flamsteed, and Wallis were three members in particular who sided with Hevelius, without necessarily advocating the use of open sights. Their support was colored by the fact that all three did not get along with Hooke who appeared to have "set himself up as the ultimate judge of knowledge"[395] in matters concerning optical instrumentation.

Even though Hevelius was a respected astronomer, his old-fashioned practices eventually lost favor with the new adherents of optical instrumentation over the course of several decades. His great star catalog, _Prodromus astronomiae_ (1690), was eventually superseded by Flamsteed's star catalog (also published posthumously), _Historiae_

[395] Shapin and Schaffer, _Leviathan_, 78. Eventually, Halley's relationship with Hooke deteriorated as well.

coelestis britannicae (1725), which "formed a sound basis for precision astronomy for almost a century."[396] Nevertheless, the precision of Flamsteed's telescopic-sight observations did not surpass Hevelius' precision until *after* Hevelius' death.

It is not surprising that this controversy began in the mid-1660's -- the common use of telescopic sights and micrometers placed an ever-increasing emphasis on precision, creating an unstable condition in which this controversy could occur. As Hevelius and Hooke stubbornly upheld their own opinions, the politeness between the two deteriorated, and all the participants who became involved took sides.

Though the controversy was precipitated by a divergence of opinion over the relative merits of telescopic versus open sights, it was guided and influenced, for the most part, by social factors -- primarily, the personal relationships, interactions, and personality traits of all the participants, although the epistemology of the collective also heavily influenced the course of the dispute. Hooke's behavior towards Hevelius was discussed at some length, but the

---

[396] King, *History*, 63; and Volkoff, et al., *Hevelius*, 79. As in his *Selenographia* (in which he names features of the moon), Hevelius continued to use the old system of nomenclature for the stars in his catalog. Instead of using Greek letters to identify stars in a constellation, he used descriptions of locations, such as "on the right," or "on the left" [Karolina Targosz, "*Firmamentum Sobiescianum* -- The Magnificent Baroque Atlas of the Sky," *Organon* 24 (1988): 157-158; and Joseph Ashbrook, "The Star Atlas of Hevelius," *Sky and Telescope* 36 (1968): 370-371].

personal interactions affecting the controversy were not limited to this relationship only.

Oldenburg, for example, was a close friend of Hevelius and he not only provided him with scientific information, but acted as his agent in London. On the other hand, Oldenburg's relationship with Hooke was antagonistic in which one of the major reasons was because of the previous controversy between Christian Huygens and Hooke over the balance-spring watch. Oldenburg inevitably sided with Hevelius in the dispute, and this personal aspect surfaced in his correspondence with Hevelius in which he reported the latest news of the Royal Society. He also had the opportunity to defend Hevelius against those members of the Royal Society who sided with Hooke. Likewise, Wallis' own relationship with Hooke colored his views of the controversy, and Wallis publicly displayed his support with his review in the Philosophical Transactions, which was an attempt to persuade others that Hooke had acted ungentlemanly and that Hevelius was justified in using open sights.

Nor did Flamsteed and Hooke get along, although Flamsteed did not support Hevelius' use of open sights. Nevertheless, there is a distinct difference between Flamsteed and Hooke's behavior towards Hevelius. Since Flamsteed ultimately adhered to the prescribed behavior of participants in controversies, and both he and Hevelius respected each other's opinion (regardless of their divergent views), their disagreement did not intensify into a bitter controversy. Therefore, Hevelius was not necessarily provoked by Hooke's views that telescopic sights were more precise, but because of the ad hominem manner in which Hooke expressed his opinions towards Hevelius.

The controversy was never resolved. Even in old age, Hevelius continued his assault against telescopic sights with his Annus climactericus. The event which ultimately terminated the controversy was Hevelius' death, but even after that, Hooke continued vindicating the precision and importance of telescopic sights. Nevertheless, it seemed almost unnecessary for Hooke to continue since Hevelius was the last great European astronomer to use open sights -- the only other astronomer who supported his use of open sights was Boulliaud, and he died in 1694.[397]

The individual interactions among the participants, and the collective community of scientists, undoubtedly affected the course of the controversy, but ultimately, intellectual factors affected the decision to permanently incorporate telescopic sights into the new astronomy. Before the 1660's, astronomers improved the precision of measurements by dividing their graduated scales more cautiously. Essential to Tycho and Hevelius' success was their acquisition of a large number of "data points" which were only appropriated

[397] Boulliaud, however, had neither the diligence nor observational skills which were attributed to Hevelius' success.

through decades of observation. Using the "law of averages", they would calculate their measurements. By the mid-1660's, however, astronomers realized that the possibilities afforded by naked-eye measurements had been exhausted,[398] as open sights were limited by the resolving power of the human eye. As Hooke and Flamsteed argued, telescopic sights had the potential to reveal more precise measurements of the heavens.[399] Hooke, in particular, argued that one did not have to spend decades collecting a large number of "data points" using a variety of instruments in order to obtain precise measurements. Astronomers could use the same instruments, supplemented with telescopic sights (which extended the human senses), to arrive at the same results. Therefore, it appears both Hooke and Hevelius wanted precise measurements, but Hevelius relied on the accuracy of a large number of measurements, whereas Hooke argued that one could obtain preciseness through a few random calculations. Although the superiority of telescopic sights was not universally accepted until the 1720's, there was a distinct increase in precision between Hevelius and Flamsteed's observations (Figure 15).

Even though open sights were abandoned for telescopic sights as early as the 1660's, Hevelius did not believe that they yielded more precise measurements. Nevertheless, his

---

398 Chapman, Dividing, 39.
399 Ibid., 33.

views, as well as Hooke's and all the other participants involved in this controversy, help us understand why telescopic sights eventually surpassed open sights, and how relationships among both individuals, and the collective group of scientists, guided the dispute. On a larger scale, the dispute offers us an opportunity to understand better how instruments such as the telescope, micrometer, and graduated measuring arcs emphasized the importance of precision and the new astronomy which emerged out of the seventeenth century.

References

Allen, Phyllis. "Problems Connected with the Development of the Telescope (1609-1687)." Isis 34 (1943): 302-311.

Ashbrook, Joseph. "The Star Atlas of Hevelius." Sky and Telescope 36 (1968): 370-371.

Bedini, Silvio. "The Aerial Telescope." Technology and Culture 8 (1967): 395-401.

________. "The Tube of Long Vision; The Physical Characteristics of the Early Seventeenth Century Telescope." Physis: Rivista di Storia delle Scienza 13 (1971): 147-204.

Beers, Yardley. Introduction to the Theory of Error. Reading, MA: Addison-Wesley Publishing Company, Inc., 1957.

Bennett, J.A. The Divided Circle: A History of Instruments for Astronomy, Navigation and Surveying. Oxford: Phaidon, 1987.

Béziat, L.C. "La vie et les travaux de Jean Hévélius." Bulletino di Bibliographia e di Storia delle Scieze Matematiche e Fisiche 8 (1875): 497-558, 589-669.

Birch, Thomas. The History of the Royal Society of London: For Improving of Natural Knowledge from its First Rise. Vols. 1-4. The Sources of Science Series, ed. Harry Woolf, no. 44. New York: Johnson Reprint Corporation, 1968.

Brahe, Tycho. Tycho Brahe's Description of his Instruments and Scientific Work as Given in *Astronomiae instauratae* *mechanica*. Translated and edited by Hans Ræder, Ellis Strömgren, and Bengt Strömgren. Copenhagen: I Kommission Hos Ejnar Munksgaard, 1946.

Brooks, Randall. "The Development of Micrometers in the Seventeenth, Eighteenth and Nineteenth Centuries." Journal for the History of Astronomy 22 (1991): 127-173.

Chapman, Allan. "The Design and Accuracy of Some Observatory Instruments of the Seventeenth Century." Annals of Science 40 (1983): 457-471.

________. Dividing the Circle: The Development of Critical Angular Measurement in Astronomy, 1500-1850. London: Ellis Horwood Limited, 1990.

Darius, Jon and P.K. Thomas, "Crosswires in a Guiding Eyepiece." Journal of Physics, E: Scientific Instruments 14 (1981): 761-765.

Daumas, Maurice. Scientific Instruments of the Seventeenth and Eighteenth Centuries. Translated and edited by Mary Holbrook. New York: Praeger Publishers, 1972.

Delambre, Jean Baptiste Joseph. Histoire de l'Astronomie Moderne. Vol. 2. The Sources of Science Series, ed. Harry Woolf, no. 25. New York: Johnson Reprint Corporation, 1969.

Engelhardt, H. Tristram, and Arthur L. Caplan, eds. Scientific Controversies: Case Studies in the Resolution and Closure of Disputes in Science and Technology. Cambridge: Cambridge University Press, 1987.

Flamsteed, John. The Gresham Lectures of John Flamsteed. Edited by Eric G. Forbes. London: Mansell Information/ Publishing Limited, 1975.

Forbes, Eric G. "Early Astronomical Researches of John Flamsteed." Journal for the History of Astronomy 7 (1976): 124-138.

________. Greenwich Observatory. Vol. 1, Origins and Early History (1675-1835). London: Taylor and Francis Limited, 1975.

Gunther, R.T., ed. Early Science in Oxford. Vol. 7, The Life and Work of Robert Hooke (Part II), by Robert Hooke. London: Dawsons of Pall Mall, 1930; reprint, Winchester: Warren and Son Limited, 1968.

________, ed. Early Science in Oxford. Vol. 8, The Cutler Lectures of Robert Hooke by Robert Hooke. London: Dawsons of Pall Mall, 1930; reprint, Winchester: Warren and Son Limited, 1968.

Hall, A. Rupert and Marie Boas Hall, eds. The Correspondence of Henry Oldenburg. Vols. 1-11, 1641-1677. Madison, WI: The University of Wisconsin Press, 1965.

________. The Correspondence of Henry Oldenburg. Vols. 12-13. London: Taylor and Francis, 1986.

Hall, Marie Boas. "Oldenburg and the Art of Scientific Communication." British Journal for the History of Science 2 (1965): 277-290.

________. "The Royal Society's Role in the Diffusion of Information in the Seventeenth Century." Notes and Records of the Royal Society of London 29 (1975): 173-192.

Halley, Edmond. Correspondence and Papers of Edmond Halley, Edited by Eugene Fairfield MacPike. New York: Arno Press, 1975.

Hashimoto, Takehiko. "Huygens, Dioptrics, and the Improvement of the Telescope." Historia Scientiarium. International

Journal of the History of Science Society in Japan 37 (1989): 51-90.

Hetherington, Noriss S. "The Hevelius-Auzout Controversy." Notes and Records of the Royal Society of London 27 (1972): 103-106.

Hevelius, Johannes. Machina coelestis pars prior. Gedani, 1672.

Hooke, Robert. Philosophical Experiments and Observations. Edited by William Derham. London: Frank Cass and Company Limited, 1967.

________. The Posthumous Works of Robert Hooke. Cass Library of Science Classics Series, ed. L.L. Laudan, no. 8. London: Frank Cass and Company Limited, 1971.

Hoppen, K. Theodore, ed. The Papers of the Dublin Philosophical Society 1683-1708. Dublin: Irish Manuscripts Commission, 1983. Text-fiche.

Hunter, Michael and Simon Schaffer, eds. Robert Hooke: New Studies. Woodbridge, Suffolk: The Boydell Press, 1989.

Iliffe, Rob. "'In the Warehouse': Privacy, Property, and Priority in the Early Royal Society." History of Science 30 (1992): 29-68.

King, H.C. The History of the Telescope. London: Charles Griffin and Company Limited, 1955.

MacPike, E.F. Hevelius, Flamsteed, and Halley: Three Contemporary Astronomers and Their Mutual Relations. London: Taylor and Francis, Ltd., 1937.

McKeon, Robert. "Les Debuts de l'Astronomie de Precision: I. Histoire de la Realisation du Micrometre Astronomique."

Physis: Rivista di Storia delle Scienza 13 (1971): 225-288.

________. "Les Debuts de l'Astronomie de Precision: II. Histoire de l'acquisition des instruments d'astronomie et de géodesie munis d'appareils de visée optique." Physis: Rivista di Storia delle Scienza 14 (1972): 221-242.

Merton, Robert K. Science, Technology and Society in Seventeenth Century England. New York: Howard Fertig, 1970.

________. The Sociology of Science: Theoretical and Empirical Investigations. Edited by Norman W. Storer. Chicago: The University of Chicago Press, 1973.

Molyneux, William. Dioptrica nova. London, 1692.

Murdin, Lesley. Under Newton's Shadow: Astronomical Practices in the Seventeenth Century. Bristol: Adam Hilger Limited, 1985.

North, J.D. "Johannes Hevelius." Dictionary of Scientific Biography, Vol. 6. Edited by Charles Coulston Gillispie. New York: Charles Scribner's Sons, 1970.

Olmsted, John W. "The 'Application' of Telescopes to Astronomical Instruments, 1667-1669." Isis 40 (1949): 213-225.

Picolet, Guy, ed. Jean Picard et les Débuts de l'Astronomie de Précision au XVI Siècle. Paris: Centre National de la Recherché Scientifique, 1987.

Prince, C. Leeson. The Illustrated Account given by Hevelius in his 'Machina Coelestis' of the Method of Mounting his Telescopes and Erecting an Observatory. Lewes, England: Sussex Advertiser, 1882.

Rigaud, Stephen Jordan, ed. Correspondence of Scientific Men of the Seventeenth Century. Vols. 1-2. Hildesheim, Germany: Georg Olms Verlagsbuchhandlung, 1965.

Righini Bonelli, Maria Luisa and Albert Van Helden. "Divini and Campani: A Forgotten Chapter in the History of the Academia del Cimento." Annali dell' Istituto e Museo di Storia della Scienza di Firenze 6 (1981): 3-176.

Scott, J.F. The Mathematical Works of John Wallis, D.D., F.R.S. (1616-1703). 2d ed. New York: Chelsea Publishing Company, 1981.

Shapin, Steven and Simon Schaffer, Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life. Princeton: Princeton University Press, 1985.

Simms, J.G. William Molyneux of Dublin: A Life of the Seventeenth-Century Political Writer & Scientist. Edited by P.H. Kelly. Dublin: Irish Academic Press, 1982.

Targosz, Karolina. "Firmamentum Sobiescianum -- The Magnificent Baroque Atlas of the Sky." Organon 24 (1988): 153-179.

Taton, René and Curtis Wilson, eds. Planetary Astronomy from the Renaissance to the Rise of Astrophysics. Part A: Tycho Brahe to Newton. The General History of Astronomy Series, ed. Michael Hoskin, Vol. 2. Cambridge: Cambridge University Press, 1989.

Thrower, Norman J.W., ed. Standing on the Shoulders of Giants: A Longer View of Newton and Halley. Berkeley, CA: University of California Press, 1990.

Turner, Anthony. Early Scientific Instruments: Europe 1400-1800. London: Sotheby's Publications, 1987.

Van Helden, Albert. "The 'Astronomical Telescope,' 1611-1650." Annali dell' Istituto e Museo di Storia della Scienza di Firenze 1 (1976): 13-36.

________. "The Invention of the Telescope." Transactions of the American Philosophical Society 67, no. 4 (1977): 1-67.

________. Measuring the Universe: Cosmic Dimensions from Aristarchus to Halley. Chicago: University of Chicago Press, 1985.

________. "The Telescope in the Seventeenth Century." Isis 65 (1974): 38-58.

Volkoff, Ivan, Ernest Franzgrote, and A. Dean Larsen. Johannes Hevelius and his Catalog of Stars. Provo, Utah: Brigham Young University Press, 1971.

Wesley, Walter G. "The Accuracy of Tycho Brahe's Instruments." Journal for the History of Astronomy 9 (1978): 42-53.

Westfall, Richard S. "Robert Hooke." Dictionary of Scientific Biography. Vol. 6. Edited by Charles Coulston Gillispie. New York: Charles Scribner's Sons, 1970.

Wolf, Abraham. A History of Science, Technology, and Philosophy in the 16th & 17th Centuries. New York: The Macmillan Company, 1935.

Figure 1a - Hevelius in his later years. There are no illustrations of Hooke (Hevelius, Machina coelestis pars posterior, Gedani, 1679).

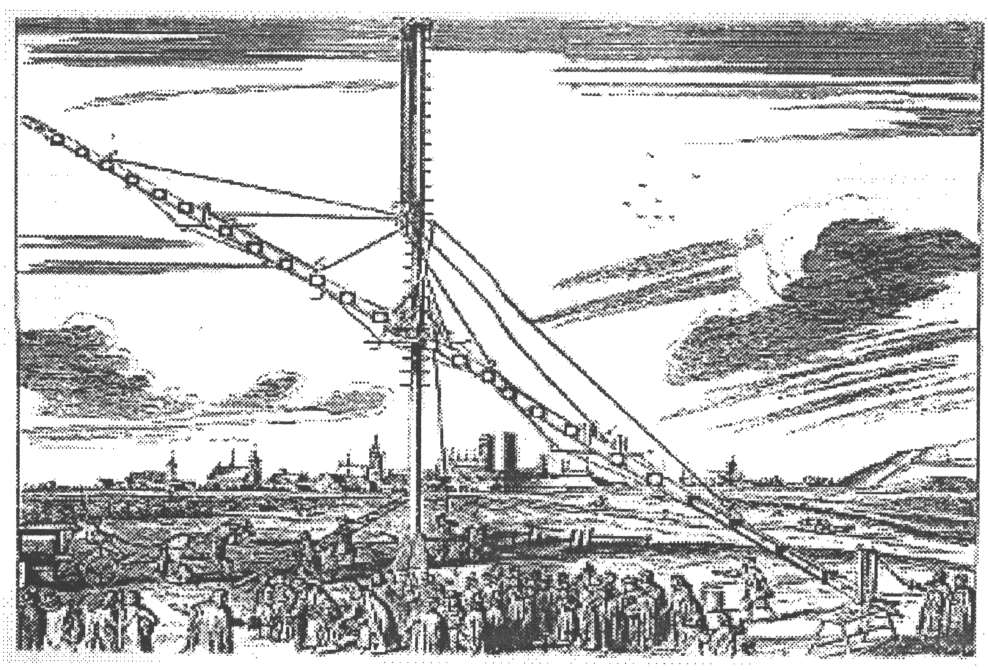

Figure 1b - Hevelius' 150-foot "aerial" telescope (Hevelius, Machina coelestis pars prior, Gedani, 1673).

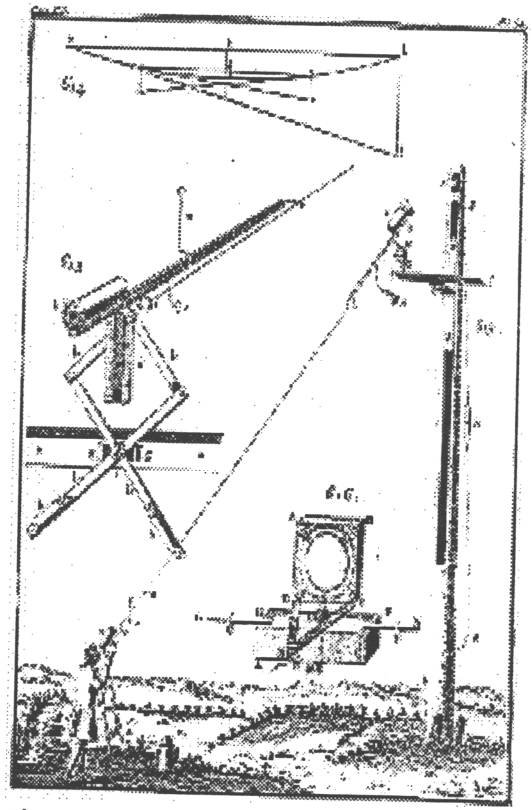

Figure 2 - Huygens' "air" telescope. A cord connected the two component lenses (Robert Smith, Compleat System of Opticks, Cambridge, 1738).

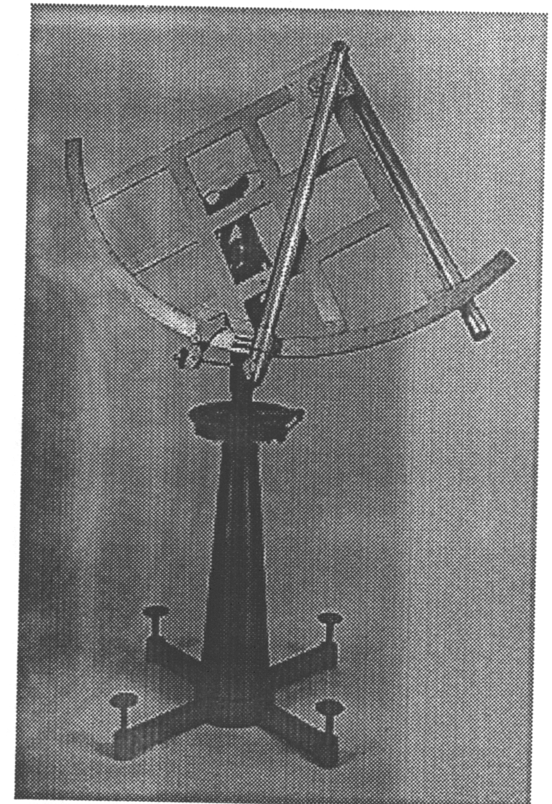

Figure 3 - Eighteenth-century quadrant designed by John Bird. The brass lattice frame became a common feature of eighteenth-century measuring instruments. Compare with Hevelius' instruments (Figs. 8, 12) (Turner, The Divided Circle, Phaidon, 1987).

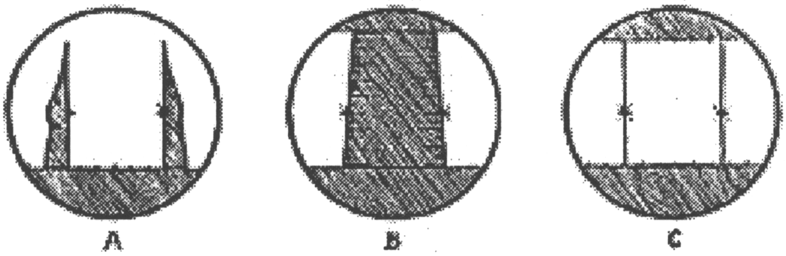

Figure 4 - Three different types of micrometers: (A) Gascoigne's metal-point micrometer, (B) Huygens' "virgula" (small metal plate), and (C) Hooke's hair micrometer (King, The History of the Telescope, Charles Griffin and Co., 1955).

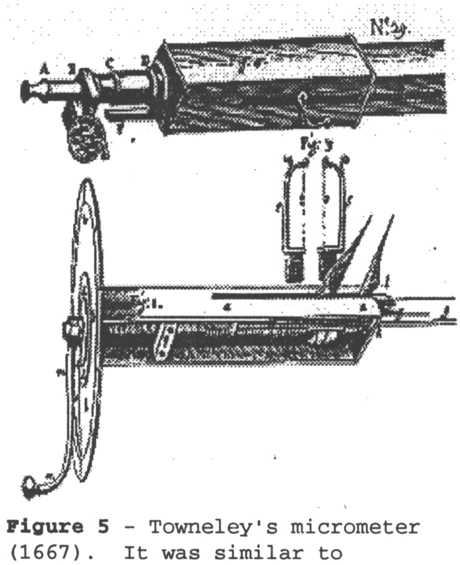

Figure 5 - Towneley's micrometer (1667). It was similar to Gascoigne's in design. This figure was drawn by Robert Hooke (Philosophical Transactions, 1677, Vol. II, no. 29).

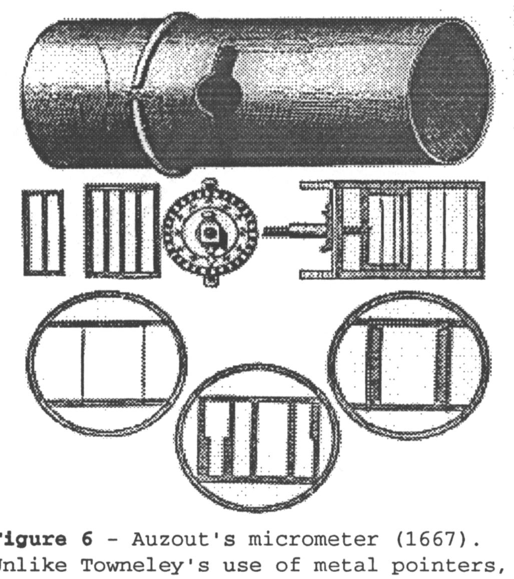

Figure 6 - Auzout's micrometer (1667). Unlike Towneley's use of metal pointers, Auzout used parallel wires, although these could be replaced with tapered metal bars (Auzout, Manière exacte pour prendre le Diamètre des Planètes in Histoire de l'Académie Royale des Sciences depuis 1666 jusqu'à 1699, Paris, 1733).

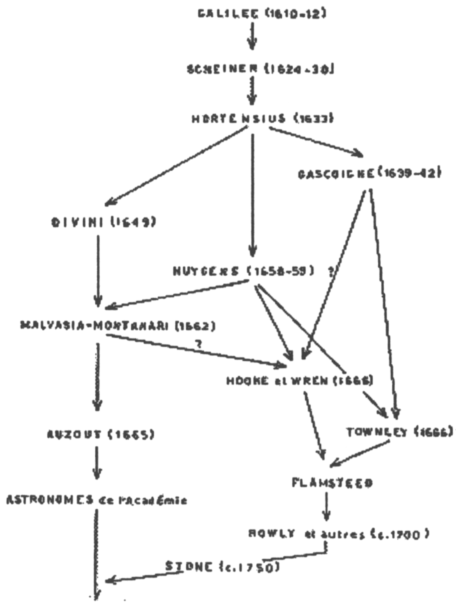

Figure 7 - Micrometer family tree. The micrometer was developed independently by the English and French (McKeon, "Les Debuts (Part I)," Physis, 13 (1971): 225-288).

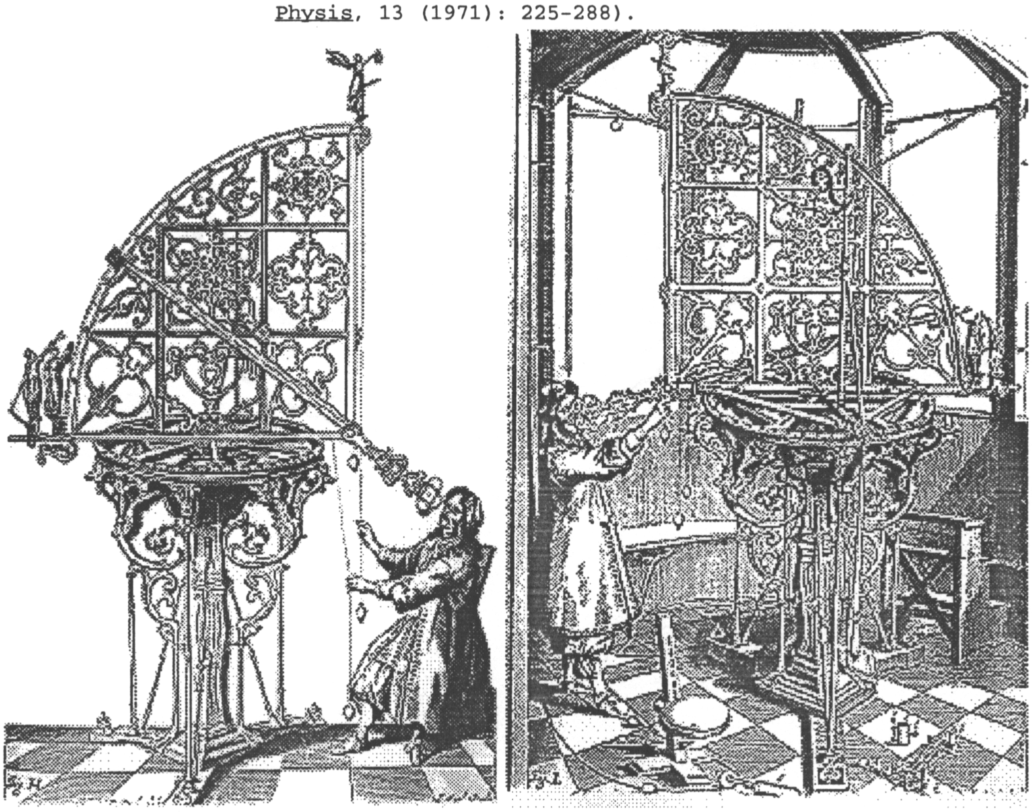

Figure 8 - Hevelius' great azimuthal quadrant from both the front and rear. Note the intricate artistic design including the figurine counterweights (compare with Fig. 3) (Hevelius, Machina coelestis pars prior, Gedani, 1673).

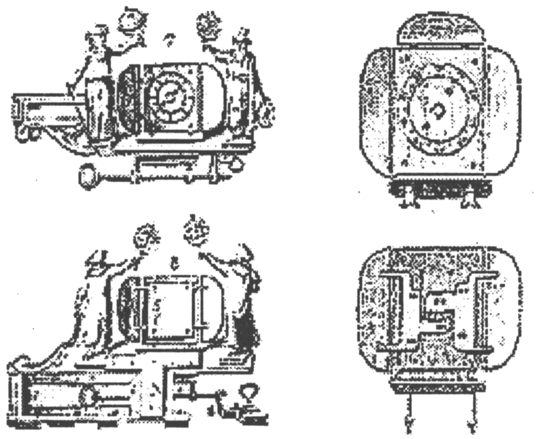

Figure 9 - Hevelius' pinnules (open sights) (Hevelius, Machina coelestis pars prior, Gedani, 1673).

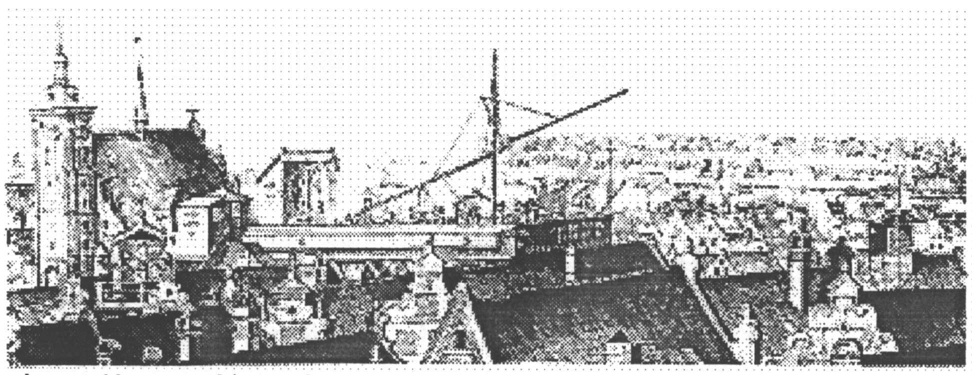

Figure 10 - Hevelius' observatory, "Stellaburgum", was built over the roofs of three adjoining houses (Hevelius, Machina coelestis pars prior, Gedani, 1673).

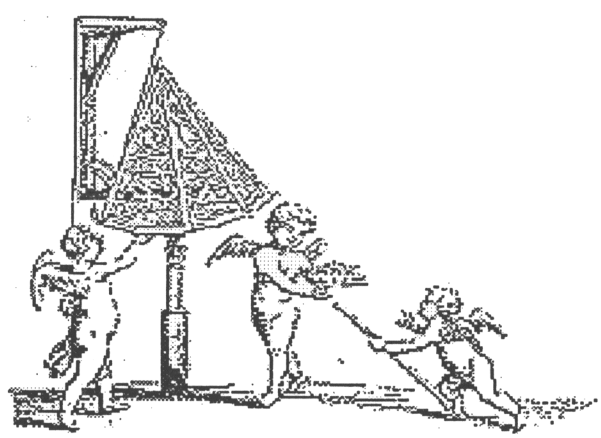

Figure 11 - The scroll reads "Praestat nudo oculo" ("I prefer naked-eye sights"). The other angel carries a telescope (Hevelius, Firmamentum sobiescianum, Gedani, 1690).

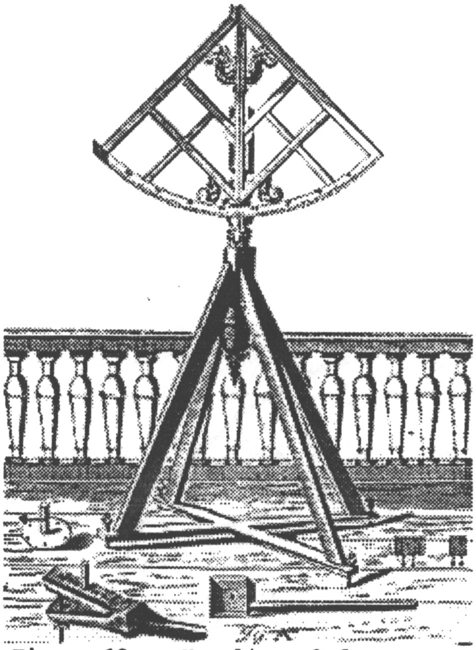

Figure 12a - Hevelius' 3 foot brass quadrant.

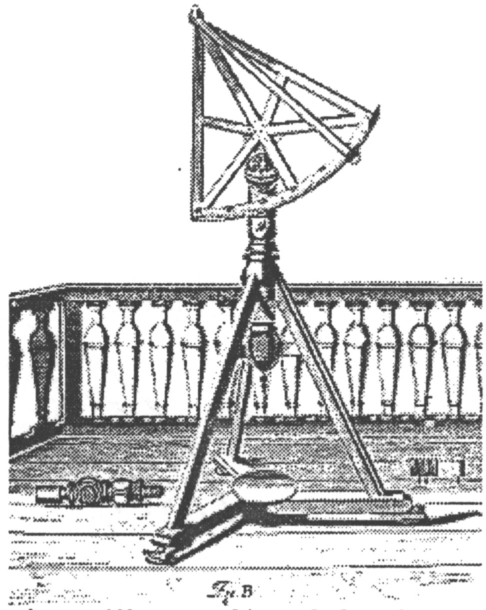

Figure 12b - Hevelius' 3 foot brass sextant.

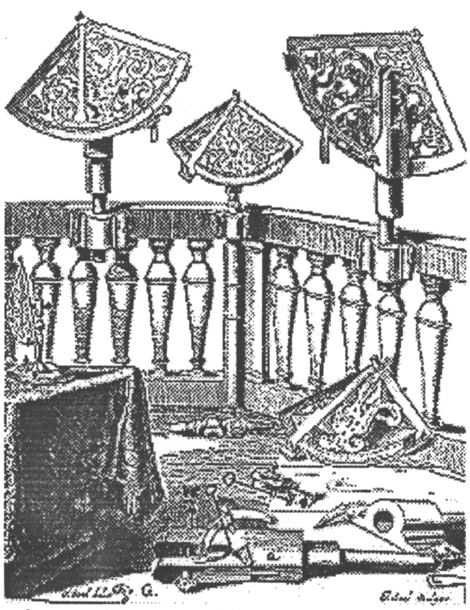

Figure 12c - Smaller quadrants of 1, 1 1/2, and 2 feet radii.

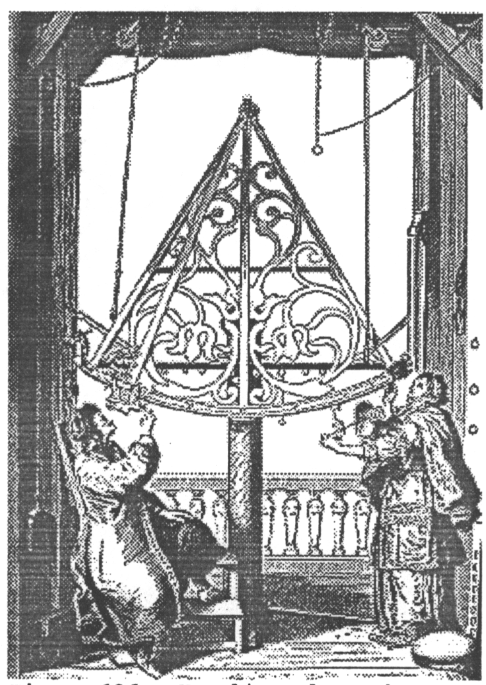

Figure 12d - Hevelius' large brass sextant (with his wife, Elizabetha).

(All from Hevelius, Machina coelestis pars prior, Gedani, 1673)

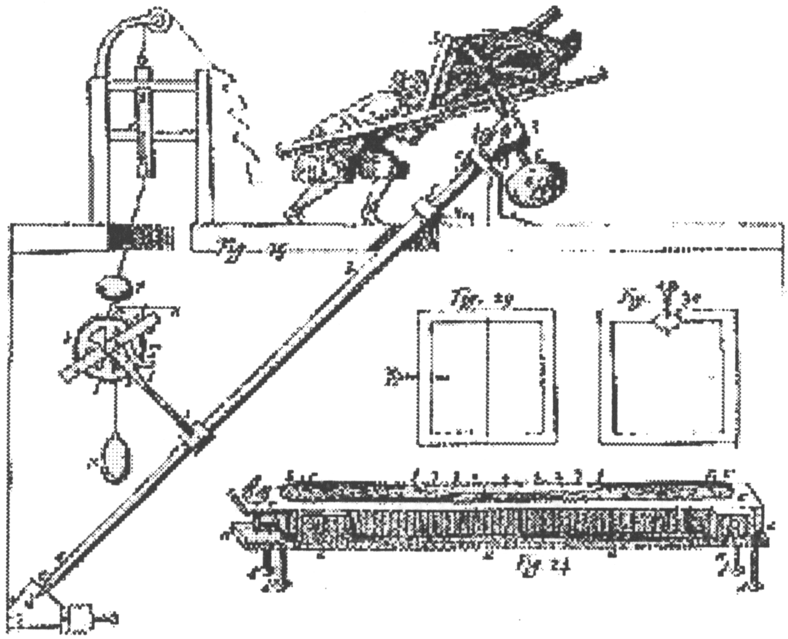

Figure 13 - Hooke's equatorial quadrant with clockwork drive. The instrument itself was never built (Hooke, Animadversions, London, 1674).

Pair of Stars	Tycho (~ 1585)	Hevelius (~ 1670)	Flamsteed (~1680)
1. α Arietis to Aldebaran α♈ α♉	−11″	+ 1″	− 9″
2. Aldebaran to Pollux α ♉β ♊	0	−28	−27
3. Pollux to Regulus β♊ α♌	−49	−50	+ 1
4. Regulus to Spica α♌ α♍	−14	−57	−24
5. Spica to δ Ophiuchi α♍ δ Oph.	+34	+ 7	− 3
6. δ Obiuchi to α Aquilae δ Oph. α Aqu.	−36	−14	−20
7. α Aquilae to Marcab α Aqu. α Peg.	+30	−14	−19
8. Marcab to α Arietis α Peg. α♈	+ 2	+ 1	+ 3
Standard Deviation	34″	27″	18″

Figure 14 - The table gives the values for the errors in the distances found by Tycho, Hevelius, and Flamsteed. The errors have been determined by comparing them with modern figures (Volkoff et al., Johannes Hevelius, Brigham Young UP, 1971).

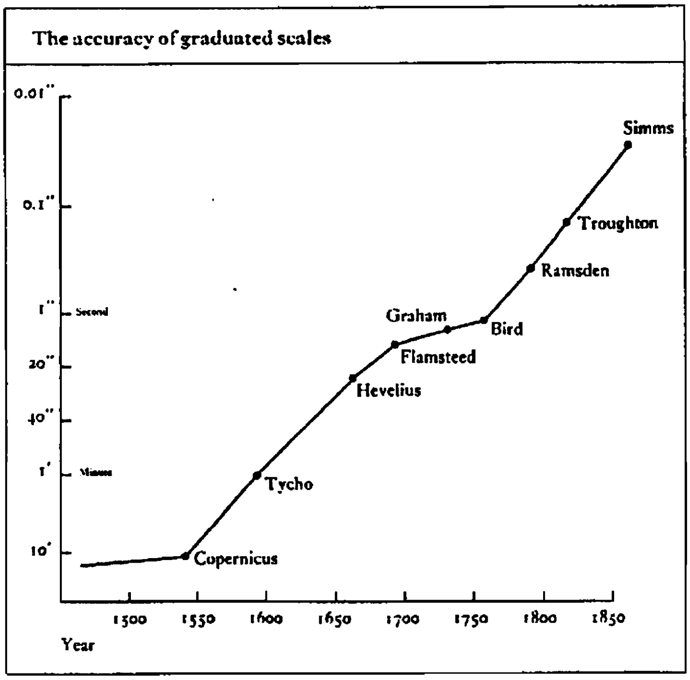

Figure 15 - Graph by Allan Chapman which shows the increase in the precision of astronomical measurement from 1500 to 1850. The graph levels off between Flamsteed and Bird because the potential of telescopically-mounted quadrants and sextants had been reached. After Bird, astronomers further developed their measuring instruments as they constructed full circle arcs (as opposed to partial arcs) which guaranteed more precision (Chapman, "The Design and Accuracy of Some Observatory Instruments," Annals of Science, 40 (1983): 457-471).

Curriculum Vita

VOULA SARIDAKIS

Current Address
208 Turner Street #317
Blacksburg, VA 24060
(703) 951-4790

Permanent Address
3288 Amsterdam Avenue
Cooper City, FL 33026
(305) 432-1438

EDUCATION

M.S., Science and Technology Studies, May 1993
Virginia Polytechnic Institute and State University
Blacksburg, VA
Thesis: Scientific Controversy and the New Astronomy: The Intellectual and Social Contexts of the Hevelius-Hooke Dispute
Advisor: Mordechai Feingold

B.A., History; Concentration: History and Philosophy of Science and Technology, May 1991
Cornell University, Ithaca, NY

HONORS AND AWARDS

Merit-based Assistantship, Virginia Tech, August 1991-May 1992; August 1992-present
Merit-based Tuition Scholarship, Virginia Tech, Fall 1991; August 1991-present
Dean's List, Cornell University

EXPERIENCE

Teaching

Teaching Assistant, Department of History, Virginia Tech, Blacksburg, VA, August 1991-May 1992; August 1992-present

- Graded quizzes and written assignments
- Guest-lectured in undergraduate history courses
- Assisted professor in Greek language proficiency

Research

Research Assistant, Department of History, Virginia Tech, Blacksburg, VA, Summer 1992

- Researched bibliographic resources in library
- Continued assisting professor in Greek language proficiency

PUBLICATIONS

Saridakis, Voula and Richard F. Hirsh, "X-ray Astronomy," History of Astronomy: An Encyclopedia, Garland Encyclopedias in the History of Science, 1993.

PRESENTATIONS AND LECTURES

"The Controversy between Hevelius and Hooke over the Relative Merits of Open versus Telescopic Sights." Thursday Lunch Discussion, Virginia Tech, Blacksburg, VA, April, 1993.

SERVICE

Assistant Rush Advisor, Delta Gamma Fraternity, Delta Rho chapter, Virginia Tech, Blacksburg, VA, August 1991-present

Voula Saridakis