Introduction

A new disciplinary awareness in astronomy arose in the late sixteenth century. In the centuries before the publication of Nicolaus Copernicus' (1473-1543) *De revolutionibus orbium coelestium* in 1543, natural philosophy and mathematics were considered separate university subjects with relatively no overlap. Natural philosophy was the search for either physical or natural causes, whereas mathematics "sought demonstrations based only on the formal properties of magnitudes. The two domains related to radically different kinds of questions about the world."[1] The disciplinary boundaries that existed between the two were so entrenched among scholars that challenges to these boundaries persisted well into the seventeenth century. Moreover, natural philosophy was clearly more prominent and important than mathematics, as the latter was rarely emphasized in the universities.

The publication of Copernicus' *De revolutionibus* helped create a disciplinary awareness in astronomy. Beginning in this period, the role of the astronomer became much less "fixed" and "static," and became more of a "dynamic, evolving, 'negotiated' process. . . ."[2] Astronomy steadily became a study to be pursued independently and with a renewed vigor that was unparalleled in previous centuries. The emergence of this disciplinary consciousness was furthered by printers who published astronomical treatises by individuals from different parts of Europe and even from different confessions.[3] In order to comprehend this new awareness in disciplinary astronomy in its fullest context, it is first necessary to briefly describe the relations between natural philosophy and astronomy and the views of three key figures in this period –

[1] Peter Dear, Discipline & Experience: The Mathematical Way in the Scientific Revolution (Chicago: The University of Chicago Press, 1995), 167. Dear provides a detailed explanation of the boundaries and relations between natural philosophy and mathematics in Chapter 6, "Art, Nature, Metaphor: The Growth of Physico-Mathematics" (Ibid., 151-179).
[2] Robert S. Westman, "The Astronomer's Role in the Sixteenth Century," History of Science 18 (1980): 106.
[3] Westman, "The Astronomer's Role," 105.

Tycho Brahe (1546-1601), Johannes Kepler (1571-1630), and Galileo Galilei (1564-1642) – towards that relationship.

The Emergence of Astronomy as a Discipline from the Late Sixteenth to Early Seventeenth Centuries

Copernicus was perhaps the first individual to make a contribution to disciplinary changes in astronomy. He managed to open new doors in the field with the publication of a single work. However, it took nearly a century for Copernicus' heliocentric system as proposed in his *De revolutionibus* to become widely accepted. In fact, by 1600, when Copernicanism was about sixty years old, only a handful of individuals accepted it.[4] Aristotelian cosmology coupled with Ptolemaic astronomy remained the dominant and accepted system of the heavens. In the late sixteenth and early seventeenth centuries, the most noteworthy system was the Tychonic system that excluded both the redundancy and awkwardness of Ptolemy's system while at the same time avoiding Copernicus' heliocentrism. There were also other acceptable systems among astronomical practitioners and mathematicians such as the system found in Thomas Lydiat's (1572-1646) *Praelectio Astronomica, De Natura Coeli* (1605) in which he proposed a system that was neither Tychonic nor Copernican. Lydiat's model even resembled Girolamo Fracastoro's (c. 1478-1553) system of homocentric spheres found in his *Homocentrica* (1538).[5]

Tycho's "compromise" between the two systems of Ptolemy and Copernicus rivaled Copernicus' own system until the middle of the seventeenth century when the Copernican system became dominant as indicated by the large number of heliocentric treatises produced at the time.[6] The victory of the Copernican system over all others was eventually "achieved by infiltration,"[7] largely facilitated by the handful of Copernicans in the fifty or so years after the publication of *De revolutionibus.* These included George Joachim Rheticus (1514-1574), Thomas Digges (c. 1543-1595), Michael Maestlin (1550-1631) and even Erasmus Reinhold (1511-1553), who although not a declared Copernican, published his own work using

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[4] See Westman, "The Astronomer's Role," 136 n. 6, for a list of known Copernicans in this period.
[5] Francis R. Johnson, Astronomical Thought in Renaissance England: A Study of the English Scientific Writings from 1500 to 1645 (New York: Octagon Books, Inc., 1968), 321.
[6] Johnson, Astronomical Thought in Renaissance England, 249.

Copernicus' mathematical methods.[8] Naturally, the impact of telescopic discoveries in the beginning of the seventeenth century should not be overlooked, as they too contributed to the eventual overthrow of the more traditional systems by confirming the veracity of heliocentrism.

Changes in the intellectual and social roles of the astronomer were encouraged by Copernicus' implicit assertion that an astronomer has the "right" to propose "new kinds of claims about the physical world." In disciplinary terms: an argument from geometry together with certain privileged observations is taken to be sufficient" for redefining reality, although this was not palatable to natural philosophers who questioned the validity of the senses that had been a part of natural philosophy for centuries.[9] Copernicus' views in his *De revolutionibus* did not agree with the preface written by Andreas Osiander (1498-1552) in which Osiander claimed that

> the duty of an astronomer [is] to compose the history of the celestial motions through careful and expert study. Then he must conceive and devise the causes of these motions or hypotheses about them. Since he cannot in any way attain to the true causes, he will adopt whatever suppositions enable the motions to be computed correctly from the principles of geometry for the future as well as for the past. . . . For these hypotheses need not be true nor even probable.[10]

The astronomer's role was to study celestial motions through the use of geometry, but because he could never know "true causes," he had to satisfy himself with suppositions and hypotheses, and make way for the domain of natural philosophers. Osiander's preface was not enough to stem the impact of Copernicus' work, however. Copernicus' innovation, which managed to "bridge disciplines," led to the negotiation of "the rules governing disciplinary behaviour."[11]

At the time that Copernicus' system was slowly "infiltrating" European astronomy, Jesuit colleges were on the rise throughout Catholic territories. Viewed as among the most important institutions of the early modern period, Jesuit colleges promoted the mathematical disciplines that, by the beginning of the seventeenth century, "had come to hold a comparatively prominent place in the courses of study offered by the Jesuits. . . ."[12] At the Collegio Romano, Christopher Clavius (1537-1612), the professor of mathematics from 1565 to 1612, was instrumental in

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[7] Thomas S. Kuhn, The Copernican Revolution (Cambridge, MA: Harvard University Press, 1957), 185.
[8] Kuhn, The Copernican Revolution, 187.
[9] Westman, "The Astronomer's Role," 111.
[10] Osiander's "Anonymous" preface in John G. Burke, ed., Science and Culture in the Western Tradition (Scottsdale, Arizona: Gorsuch Scarisbrick, Publishers, 1987), 95. In this translation, the word "hypothesis" appears eight times.
[11] Westman, "The Astronomer's Role," 134. These "rules" are discussed in later chapters.

promoting mathematics in the curriculum and elevating its status by arguing for its certainty in demonstrations.

Despite the efforts of individuals such as Clavius, one finds that nowhere in the mid-to-late sixteenth century were there opportunities to obtain an advanced degree in astronomy or mathematics: "no licensing, that is, which recognized that symbol of full disciplinary autonomy."[13] The graduate faculties of law, theology, and medicine dominated the universities of this period. Astronomy was still part of the quadrivium along with geometry, arithmetic, and musical theory. Mathematics, in general, existed as a "service role" for other fields, and any improvements in astronomy, which was practiced for certain consumer groups, usually meant the extent to which astronomy made practical contributions such as better astrological predictions and improved calendars. One does not see the academic as a researcher in problems concerning "cosmology and theoretical astronomy; it was largely a pedagogical position for an undergraduate subject."[14]

The lack of professionalization in astronomy may not appear to have been a serious problem, but the lack of a formalized discipline in general is significant. First, the subject is not perceived as important or necessary within the larger framework of university curricula, and is therefore subordinated to other subjects. This makes it challenging to attract students and promote the subject. Also, without formalized instruction, it is difficult to establish a common base with which to link people together into a community that makes it harder to collaborate. For those interested in the pursuit of astronomy, some of these points might have presented themselves as problems for astronomers in the late sixteenth and early seventeenth centuries. Nevertheless, certain individuals were able to rise above these obstacles and find ways of both acquiring an education in mathematical studies and find others who were willing to collaborate with them.

The English universities, specifically Oxford and Cambridge, may not have been as scientifically creative in the sixteenth and early seventeenth centuries as their continental counterparts. However, this "does not mean that scientific enterprise was lacking," and in fact,

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[12] Dear, Discipline & Experience, 33. Dear details the role played by the Jesuits in the mathematical sciences in Chapter 2, "Experience and Jesuit Mathematical Science: The Practical Importance of Methodology" (Ibid., 32-62).
[13] Westman, "The Astronomer's Role," 117.
[14] Westman, "The Astronomer's Role," 120.

"the universities played a vital role in this incubatory period of English science. . . ."[15] Contrary to conclusions made in recent scholarship, Feingold argues that there were no biases towards mathematical studies in the English universities. Moreover, students were exposed to mathematical studies on a regular basis and studied the material even further when pursuing an M.A.[16] The continuity and interest in mathematics set the stage for English science in the latter half of the seventeenth century.

After about 1550, the role of astronomers both within and outside the universities began to change. The impetus for this transformation initially came from Copernicus' own views of the astronomer's role combined with the impact of his system. In fact, those who seemed to be more receptive to Copernicanism were the ones who were more "actively engaged in reformulating" the astronomer's disciplinary role, especially outside the universities.[17] Even within the walls of the universities, however, a negotiation of new roles in astronomy was underway. In addition to the universities, the courts were also "sites" of astronomical activity.

In 1576, in an example of generous royal patronage, King Frederick II of Denmark granted the island of Hven to Tycho Brahe for the purpose of carrying out observations from what was to become the grandest of all observatories at that time. On his friends' advice, Tycho decided to take the island, and he was even offered the annual sum of "five hundred good old daler," a respectable amount of money for the times.[18] "Uraniborg," as it came to be called, was built between 1576 and 1580, and even more patronage meant that it would house the best instruments of the time as well. This "castle of the heavens" was considered the finest observatory in Europe until the very end of the sixteenth century not only because of its instruments, but also because of the instruction students received[19] and the treatises that were printed (as long as paper was available). With its reputation as the finest observatory in Europe,

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[15] Mordechai Feingold, The Mathematicians' Apprenticeship: Science, Universities and Society in England, 1560-1640 (Cambridge: Cambridge University Press, 1984), 214, 215.
[16] Feingold, The Mathematicians' Apprenticeship, 215.
[17] Westman, "The Astronomer's Role," 106-107.
[18] This was equivalent to about 114 £. J. L. E. Dreyer, Tycho Brahe: A Picture of Scientific Life and Work in the Sixteenth Century (New York: Dover Publications, Inc., 1963), 84-85. See also Victor E. Thoren, The Lord of Uraniborg. A Biography of Tycho Brahe (Cambridge: Cambridge University Press, 1990).
[19] In the 1580's, Tycho had anywhere from eight to twelve assistants/students who were mostly of Danish heritage, but some also came from Holland, Germany, and even England. Most of them were in their twenties and veterans of the universities [See John Robert Christianson, On Tycho's Island: Tycho Brahe and His Assistants, 1570-1601 (Cambridge: Cambridge University Press, 2000); and Victor E. Thoren, "Tycho Brahe as the Dean of a Renaissance Research Institute," in Margaret J. Osler and Paul Lawrence Farber, eds., Religion, Science, and Worldview: Essays in Honor of Richard S. Westfall (Cambridge: Cambridge University Press, 1985), 276-277].

it is not surprising that the island of Hven became a focal point for friends, students, and visiting scholars.

Tycho Brahe was aware of the stuffy and unsuitable atmosphere of the universities. He even perceived professors as "shadow chasers" and "those engaged in idle formalities and empty processions of words."[20] In effect, Tycho managed to free astronomical activities "from the old guild-like divisions of the academy, [and his] example became a role model for others, such as Kepler."[21] Freeing the academy from these "guild-like divisions" was made possible in part by Tycho's freedom to publish "what he liked, when he liked. . . and more importantly, *for whom* he liked" with his own printing press.[22] And he could send copies of his book to whomever he liked who could understand and appreciate his work.

Kepler replaced Tycho as Imperial Mathematician after Tycho's death, and it was his duty to complete Tycho's tables that were still used in making astrological predictions. Another two decades would pass before their completion only after Emperor Rudolph's death in 1612. His successor, Mathias, confirmed Kepler as the court mathematician, but because Kepler was a Protestant, his work was interrupted by the Thirty Years' War. Nevertheless, the influential *Rudolphine Tables* were finally published just three years before Kepler's death in 1627.

Kepler's vision of a new astronomy, according to Nicholas Jardine, was the "culmination of a progression of sixteenth-century attitudes" and was exemplified by his "plea for the astronomer to become a natural philosopher."[23] In Book I of the widely-read *Epitome of Copernican Astronomy*, Kepler clarified his views toward disciplinary astronomy:

> What is astronomy? It is a science setting out the causes of those things which appear to us on earth as we attend to the heavens and the stars, and which the vicissitudes of time bring forth: and when we have perceived these causes, we are able to predict the future face of the heavens, that is, the celestial appearances, and to assign particular times to things in the past.[24]

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[20] 8 December 1576, Tycho Brahe, *Opera Omnia*; cited in Westman, "The Astronomer's Role," 122-123.
[21] Westman, "The Astronomer's Role," 134. On a related note, Westman also argues that Tycho's rejection of the solid spheres "was a social act which involved the invasion of one discipline into the assumed domain of another. . . . Tycho marched his army of mathematicians into the fields of natural philosophy. . . . And the new cosmology which he proudly waved as his leading banner became a symbol of his rights to theoretical property" (Ibid., 125).
[22] Westman, "The Astronomer's Role," 125.
[23] Nicholas Jardine, The Birth of History and Philosophy of Science: Kepler's 'A Defence of Tycho Against Ursus' with Essays on its Provenance and Significance (Cambridge: Cambridge University Press, 1984), 257.
[24] Kepler, *Epitome astronomiae Copernicae*, 1618; cited in Jardine, The Birth of History and Philosophy of Science, 250.

Certainly, Kepler firmly believed that physical causes must be considered when deriving planetary orbits and the paths of other celestial objects at any given time. Moreover, he rejected hypothetical models and any kind of motion that did not have a causal explanation, including epicycles.

Kepler's attitude toward disciplinary astronomy as part of natural philosophy is best exemplified by a passage found in his *Apologia Tychonis*:

> For even though what [Ursus] mentions [that astronomers use hypotheses to calculate celestial motions and to predict future motions] is the primary duty of the astronomer, the astronomer ought not to be excluded from the community of philosophers who inquire into the nature of things. He who predicts as accurately as possible the movements and positions of the stars performs well the duty of the astronomer. But he who employs true opinions about the form of the universe does it better, and is held worthy of greater praise. The former, to be sure, draws conclusions that are true as far as what is observed is concerned; the latter not only does justice in his conclusions to what is seen, but also, as I explained above, in order to draw his conclusions embraces the innermost form of nature. . . . it follows that not more than one form of hypothesis can be true. And philosophers certainly have the capacity to judge which of them is true and which false.[25]

Predicting the movements and positions of the stars is certainly useful to the astronomer interested in astrological predictions, which was a large part of Kepler's work. More importantly, however, Kepler, like Tycho, recognized the necessity of removing constraints, academic and otherwise, from the discipline of astronomy, and forging a new direction for the discipline. The astronomer is also a philosopher, and like the philosopher who studies nature for answers to his questions, so too the astronomer studies the heavens for an understanding of what is observed. This new role would culminate in the work of Galileo, who unlike Tycho, and more than Kepler, used his telescope to determine the true construction of the heavens.

When Galileo turned his telescope towards the heavens, he had no pre-planned "agenda" to prove Copernicanism with it.[26] Galileo's own views toward the discipline of astronomy paralleled those of Tycho and Kepler, and he was cognizant of the disciplinary differences between philosophers and mathematicians:

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[25] Kepler, *Apologia Tychonis*, 242-243; translated by Jardine.

[26] Richard Westfall, "Science and Patronage. Galileo and the Telescope," Isis 76 (1985): 26. Also in Peter Dear, ed., The Scientific Enterprise in Early Modern Europe. Readings from *Isis* (Chicago and London: The University of

> I already seem to hear somebody shouting in my ears that it is one thing to treat things physically and another to treat them mathematically, and that the geometers should remain among their spinning tops without bothering with philosophical matters, whose truths are different from mathematical truths – as if truth can be more than one.[27]

Given his telescopic discoveries, Galileo placed great emphasis on indisputable evidence, and he did not accept novel theories without verifying them for himself through experiment and observation.[28] His passion for truth was expressed in a letter to Kepler dated August 4, 1597:

> I congratulate myself with all my heart to have such an ally in searching for the truth, such a friend too of this same truth. For it is a sad thing that the students of truth are so rare that there are only a few who do not follow the perverted way of philosophizing. However, this is not the place to deplore the miseries of our century; I should rather congratulate you on all the beautiful things you presented in support of the truth. . . . I should indeed venture to disclose my opinion [on Copernicus], if there were more men like you; since there are none, I shall desist from such a task.[29]

Accordingly, Copernicus' work helped Galileo bridge the disciplinary gap between the mathematician and philosopher. Already a mathematician, Galileo now also considered himself a "new philosopher" who believed that "mathematics was the only language in which astronomical matters *should* be discussed and judged."[30] Galileo even called this new type of philosopher an "astronomer-philosopher":

> [Scheiner] continues to adhere to eccentrics, deferents, equants, epicycles, and the like as if they were real, actual, and distinct things. These, however, are merely assumed by pure astronomers in order to facilitate their calculations. However, they should not be retained by astronomers-philosophers who, going beyond the demand that they somehow save the appearances, seek to investigate the true constitution of the universe – the most important and most admirable problem that there is. For such constitution exists; it is unique, true, real, and could not be otherwise.[31]

Chicago Press, 1997), 128. Westfall suggests that the telescope, as Galileo used it, was more an instrument of patronage than an instrument of astronomical discovery (Westfall, "Science and Patronage," 26).

[27] From Galileo's *Opere*, IV, 49; cited in Mario Biagioli, Galileo, Courtier. The Practice of Science in the Culture of Absolutism (Chicago and London: The University of Chicago Press, 1993), 222.

[28] Willy Hartner, "Galileo's Contribution to Astronomy," in Ernan McMullin, ed., Galileo Man of Science (New York: Basic Books, Inc., 1967), 189.

[29] Cited in Hartner, "Galileo's Contribution to Astronomy," in McMullin, ed., Galileo Man of Science, 181.

[30] Biagioli, Galileo, Courtier, 224.

[31] From Galileo's *Opere*, V, 102; cited in Biagioli, Galileo, Courtier, 224. For more discussion on "philosophical astronomers," see Biagioli, Galileo, Courtier, 223-224 n. 33.

Along with Tycho and Kepler, Galileo advocated going beyond the mathematical models that astronomers use to “save the appearances” of the heavens. The astronomer is also a philosopher who must delve into the true nature of the heavens.

Description, Historiography, and Methodology

Describing the Project

In this dissertation, I examine the nature of astronomical practice in the late seventeenth century by analyzing the nature of observation, instrumentation, and society as well as their interrelationships. My aim will be to demonstrate how the convergence of several overlapping factors including the implementation of new instruments, the mathematical and astronomical education of practitioners, the gradual assimilation of new ideas and systems, and the rise of societies and networks, laid the foundation for astronomical practice in the late seventeenth century. More specifically, I shall show how the controversy between Johannes Hevelius (1611-1687) and Robert Hooke (1635-1703) over the merits of naked-eye versus telescopic sights and the ensuing debate that involved a larger circle of individuals acted as a catalyst in the transformation of that foundation into an official discipline. In forcing individuals to take sides, the controversy pushed them to define both the appropriate characteristics of astronomical practice and the appropriate activities of the astronomer.

I begin my study in Chapter 1 with discussion of the nature of astronomical instruments used in the late sixteenth and seventeenth centuries. I also provide a history of instrumentation between the use of positional measuring instruments in the late sixteenth century and the more widespread use of micrometers and telescopically-mounted positional measuring instruments in the late seventeenth century. Crucial innovations in astronomical instrumentation and especially positional measuring instruments occurred in the sixteenth and seventeenth centuries. Until the time of Tycho Brahe’s work, mathematical models remained important components of astronomy, despite the decline of innovation in instrumentation. In the second half of the sixteenth century, interest in measurement increased and Tycho organized the construction of a collection of instruments that astronomers eventually used to make more precise measurements and observations.

The development of the telescope dominated much of the seventeenth century, yet it was the invention and ultimate implementation of micrometers and telescopic sights that turned astronomy from a field of discovery into one of measurement. Both of these latter inventions were first constructed by William Gascoigne (c. 1620-1644) in the early 1640’s, but were temporarily forgotten and not rediscovered until the 1660's. When they were rediscovered, however, they were immediately added to the instrumental arsenal of the astronomer who declared their use absolutely necessary in the serious pursuit of astronomical measurement. By the 1670's, the lack of use of micrometers or telescopic sights meant that an astronomer's observations would be seriously disputed.

Proceeding from the instruments to the people involved, I discuss the mathematical and astronomical community from the late sixteenth to the late seventeenth centuries in Chapters 2 and 3. The "community" included those individuals working within the universities as well as those outside the schools. However, there were strong connections and significant overlap among academic scholars and those working outside the universities – these communities were not mutually exclusive. Moreover, there were, in fact, many such "communities" during this period that defy the vision of the relatively isolated astronomer working within the courts of Europe until the mid-seventeenth century when the Académie des Sçiences and the Royal Society formed. Certainly, in the late sixteenth and early seventeenth centuries some individuals were physically isolated, but most were still a part of the scientific milieu of Europe. A brief survey of these early scientific circles and networks shows that not only did they exist, but they also helped promote astronomical studies by fostering an atmosphere of shared interests in which ideas and observations could be exchanged.

Of course, defining and describing a scientific community in any time period is seldom a straightforward task, especially because a "community is dynamic. . . it changes over time."[32] Mapping the boundaries of a community can be equally daunting, both in terms of who belongs to the community (membership), and the particular agenda and interests of its members. The process of defining a community becomes even more challenging at times when the community undergoes radical change. The community of astronomical practitioners of the late seventeenth century was faced with radical new changes that were brought about as a result of the nexus of the new instrumentation, the rise of institutional observatories, and the formation of scientific societies. Yet, it would be a mistake to assume that the boundaries of this community were clearly defined. Moreover, belonging to the community did not always ensure agreement among its members (as the Hevelius-Hooke controversy reveals).

The Hevelius-Hooke controversy over the relative merits of naked-eye versus telescopic sights discussed in Chapter 4 was the watershed in positional astronomy that defined the role of astronomers, shaped their methods of observation, and directed all future astronomical research. In effect, the dispute identified what was considered acceptable and unacceptable for astronomers to put into practice in the late seventeenth century. This case study, which may seem simple on the surface, is much more complex and involves the very transformation of astronomical practice in the seventeenth century. The issue involving the advantages of telescopic sights over naked-eye sights spilled out into larger issues concerning the practice of astronomy, and in the course of their dispute, Hevelius and Hooke involved most of the scientific community of the period. Moreover, they debated issues that went beyond the simple merits of one type of sight over another by invoking claims of authority and reputation. Moreover, each attempted in his own way to "recruit" allies, and in the course of the controversy, certain peculiar alliances were formed. Issues involving authority, reputation, and the recruitment of allies are part of the social context of controversy. Other issues explored include personality traits, witnessing the "production" of knowledge, and the complex and multifaceted relationships among the participants, both as individuals and as a collective.

In the final chapter of this study, Chapter 5, I shall discuss the work of Gian-Domenico Cassini (1625-1712) and John Flamsteed (1646-1719) at each of their respective observatories. The significance of the Hevelius-Hooke controversy is apparent in their work, and Flamsteed was especially influenced by Hevelius as well as by his own countrymen and immediate predecessors. Moreover, I shall look at the differences between each of the observatories and how these differences contributed to the astronomers' work and to the promotion of astronomy in the late seventeenth century. The rise of institutional observatories included the introduction of certain institutional pressures that had not been a part of the "private" observatory. Flamsteed, whose own personality served as his worst enemy, felt such institutional pressures most acutely.

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[32] John Lankford, American Astronomy: Community, Careers, and Power, 1859-1940 (Chicago and London: The University of Chicago Press, 1997), 4. Lankford provides a detailed description of the scientific community in his first chapter.

Historiography, Methodology, and the Internal vs. External Debate

In the last several decades, various scholars have written on instrumentation in the seventeenth century.[33] Such historical case studies have certainly enriched scholarly interest, but there remains a need to recast such histories through a fresh historiographical perspective that emphasizes the crucial role played by the institutionalization of science, primarily in the late seventeenth century. Such a study that accounts for the development of astronomical instrumentation in light of the changing nature of the practitioners and their workplaces provides a balance of historical perspectives for addressing a series of central and significant questions concerning the philosophical, social, and scientific aspects of this study.

The debate between the "internal" versus the "external" approach in the history of science has been ongoing for several decades with the most current view being that somehow we need both approaches. The problems between approaches stem, however, from the belief that science is just another social practice, and no more valid than other social practices. In light of recent historiography, the dynamic interplay between science and society serves as my guiding principle. There exists an interactive relationship between the two that makes it necessary to describe and develop the historical blending between the two. More specifically, the development of scientific societies as institutions in the late seventeenth century where community-based science became a means for legitimizing one's work necessitates the study of such a historic interplay and requires the adoption of such methods. I realize that there are no transcendental qualities about "science" or "society" as even during the course of the seventeenth century, the definitions of such terms evolved. Nevertheless, my attempt to bring a fresh historical perspective warrants that I provide a balance of methodologies without reducing or conflating the "rationalist" approach with the "sociological" approach.

The history of astronomy in the seventeenth century is full of historical complexity. It requires consideration of many different aspects of astronomical practice, and at the same time necessitates an understanding of the impact made by the institutionalization of science and the rise of scientific societies. Respect for observation and measurement must be balanced by issues of authority, reputation, and expertise. Disciplinary changes in the field are understood in light

[33] See for instance, J.A. Bennett, The Divided Circle: A History of Instruments for Astronomy, Navigation and Surveying (Oxford: Phaidon, 1987); Allan Chapman, Dividing the Circle: The Development of Critical Angular

of the combined context of changes in instrumentation, the scientific community, and astronomical education. By carefully integrating all these factors, we arrive at a more contextualized history of disciplinary astronomy in the early modern period.

Measurement in Astronomy, 1500-1850 (London: Ellis Horwood Limited, 1990); and Anthony J. Turner, Early Scientific Instruments: Europe 1400-1800 (London: Sotheby's Publications, 1987).

Chapter 1
The Changing Nature of Instruments, Techniques, and Measurement from the Late Sixteenth to Mid-Seventeenth Century

Astronomical Instrumentation to the Mid-Seventeenth Century

Centuries before the classical period of Greece, ancient civilizations used astronomical instruments and mathematical models to describe how the heavens worked. For example, some primitive cultures used stone arrangements to help them read the heavens for the establishment of agricultural calendars. In the fourth-century B.C., under the auspices of Plato, astronomy became part of the natural philosopher's domain – a noble study for those who wanted to account for the movements of the heavenly bodies. During the Hellenistic period, astronomers attempted to explain the physical construction of the heavens by using increasingly complicated models, and Hellenistic astronomers used simple instruments such as dioptras and armillary astrolabes in their observations. Despite the active pursuit of astronomical study, the ancients neither emphasized nor required the use of instruments.

Ptolemy (c. 87-c. 165), more than anyone else, emphasized that the main task of an astronomer was to "save the appearances" – explain the apparent and irregular movements of heavenly bodies by using epicycles, eccentrics, and equants – geometrical models that allowed the astronomer to compute the movements of the Sun, moon, and planets. Ultimately, Ptolemy preferred theory over observation, but he did not devalue the role of observational astronomy. Nonetheless, by the end of the Hellenistic period, "saving the appearances" was far nobler and more central to the pursuit of astronomical study than observation. Consequently, the development of instruments such as the astrolabe was not as important as the development of new theories of the heavens in the centuries that followed.

Nonetheless, Islamic astronomers continued modifying and using various astrolabes, and by the late medieval period, single-latitude astrolabes, universal astrolabes, and astrolabe-quadrants, became important calculating devices for both astronomy and geometry.[1] As interest in the use of astronomical instruments grew, practitioners introduced armillary spheres, cross-staffs, and other instruments into common astronomical practice. Mathematical models remained an important component of astronomy however, and by the second half of the sixteenth century, there was even a decline of innovation in instrumentation.[2] Tycho Brahe's dissatisfaction with the ability of existing instruments to conform to his rigorous expectations for precision drove him to organize the construction of a number of instruments that he eventually used to make more accurate measurements.

In the sixteenth century, the increasing emphasis on instrumentation complemented another aspect of scientific inquiry – natural philosophers believed that nature could only be understood in mathematical terms, but before the mathematical method could be applied, nature had to be carefully investigated. Because natural philosophers realized that their unaided senses alone could not always discern nature's complexities, they relied on instruments, and consequently, optical instrumentation developed rapidly in the first half of the seventeenth century. Before turning to the development of astronomical instrumentation in this period, I shall discuss several ideas concerning scientific instruments in the next few sections.

What is a Scientific Instrument?

Although it is not my purpose to examine the nature of scientific instruments in general, I shall provide a brief explanation of the term "scientific instruments," their types and uses, and the role of both random and systematic errors.[3] The term "scientific instrument" refers to instruments that are used for the purposes of collecting, observing, measuring, controlling, or recording specific data.[4] In most cases, scientific instruments are part of a larger system or

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[1] Turner, Early Scientific Instruments, 16.
[2] Turner, Early Scientific Instruments, 57.
[3] There are several articles and books available that delve into the nature of scientific instruments in greater detail including: Deborah Jean Warner, "What is a Scientific Instrument, When Did it Become One, and Why?" British Journal for the History of Science 23 (1990): 83-93; J.V. Field, "What is Scientific About a Scientific Instrument?" Nuncius 3 (1988): 3-26; Albert Van Helden and Thomas Hankins, eds., Osiris: Instruments, Vol. 9; and for a bibliographic survey of scientific instruments, see Gerard L'E Turner, "Scientific Instruments," Scientific Instruments and Experimental Philosophy: 1550-1850 (Aldershot, Hampshire, UK: Variorum, 1990), I, 243-258.
[4] The term "scientific" may be viewed as an anachronistic term since it is relatively modern, but the convenience and practicality of using it ensures its continued and consistent use [A.J. Turner, "Interpreting," in R.G.W.

network of instruments used by scientific practitioners working in a specific location such as an astronomical observatory.[5] The term "scientific instrument" was not in use during the sixteenth and seventeenth centuries. However, the seventeenth century is considered the period in which the scientific instrument was "born."[6]

There are many different types of scientific instruments including: instruments that produce natural wonders for our own pleasure, instruments that serve as models or analogies to nature, instruments that extend the senses, instruments of measurement, instruments that create extreme or varying conditions that do not exist naturally in nature, instruments used to control or analyze certain phenomena, and instruments used for visual or graphic display.[7] My emphasis is primarily on measuring instruments, instruments designed to aid the senses, or a combination of both. The use of certain instruments can secure a praiseworthy reputation for those that use them, and they can inform or instruct both public and private audiences.[8] The role of instruments becomes more complex when we take into account the scientific principles and theories of instrument-use.[9]

Anderson, J.A. Bennett, and W.F. Ryan, eds., Making Instruments Count. Essays on Historical Scientific Instruments Presented to Gerard L'Estrange Turner (Aldershot, Hampshire: Variorum, 1993), 20].

[5] Robert Bud and Deborah Jean Warner, eds., Instruments of Science: An Historical Encyclopedia (New York and London: Garland Publishing, Inc., 1998), x. See also Joseph C. Pitt, "The Discovery of Technology and the Technology of Discovery Telescopes, Discovery and Progress," in Joseph C. Pitt and Elena Lugo, eds., The Discovery of Technology and the Technology of Discovery (Blacksburg, VA: The Society for Philosophy and Technology, 1991), 457-470. Pitt argues that "progress in science is a direct function of increasing sophistication not merely in instrumentation, but in" what he calls "the technological infrastructure that underlies and makes mature science possible" (Ibid., 457).

[6] "Looking back at the period 1550-1700, which saw the birth of modern scientific instruments, we find, however, that these modern assumptions concerning instruments were not present and that these views became 'truths' only as the use of instruments became commonplace" [Albert van Helden, "The Birth of the Modern Scientific Instrument, 1550-1700," in The Uses of Science in the Age of Newton, ed. John G. Burke (Berkeley, CA: University of California Press, 1983), 49]. He concludes:

> It was only when the tacit assumptions we now have relative to scientific instruments were generally accepted – that is, when scientific instruments had obtained an unquestioned role in science, when their results and readings were no longer subject to questions beyond those of repeatability and standards of accuracy, when they had been firmly based on agreed principles, and when continuous improvement had become the quest of the community of scientists and instruments makers – that we can say the modern notion of a scientific instrument had been born (Ibid., 69).

[7] Van Helden and Hankins, "Introduction," in Van Helden and Hankins, eds., Osiris: Instruments, 4. Interestingly, many dictionaries tend to place a great deal of emphasis on measurement as a characteristic of scientific instruments (Bud and Warner, eds., Instruments of Science, ix).

[8] Van Helden and Hankins, "Introduction," in Van Helden and Hankins, eds., Osiris: Instruments, 5.

[9] Some discussion on theory versus practice within the scope of this study is found at the end of the chapter.

The terms error, precision, and accuracy also merit attention as I refer to them throughout this study.[10] There are several different meanings of the term "error". In mathematics, it usually refers to "the difference between a computed or measured value and a true theoretically correct value,"[11] but it may also refer to the estimated uncertainty in an experiment or measurement. These definitions differ from a "discrepancy" which is "the difference between two measured values of a quantity" such as the measured values by two different people or instruments.[12] There are two types of "error": random (or accidental) and systematic (or constant). Random errors occur when individual values are in error by various amounts, some closer to the "true" value than others. Random errors include errors of judgment (such as when an observer's measurement varies), errors created by constantly-changing conditions (i.e., temperature, weather), errors created by small disturbances (i.e., wind), and errors that are random by definition, such as when a certain quantity is not precisely defined because it varies from place to place or instrument to instrument. Unlike random errors, systematic errors occur when all of the individual values are in error by the same amount. These errors occur when instruments are not properly calibrated, when personal errors are introduced (i.e., the habits of individuals contribute to the accumulation of such errors), when inferior experimental or instrumental conditions exist (i.e., instruments are warped), or when imperfect techniques are consistently applied.[13]

The terms "accuracy" and "precision" are often confused in common usage. Accuracy is defined as the closeness of a measurement to its accepted value, and in the context of errors, an experiment or measurement is considered highly accurate when there are small systematic errors. For a measurement to be accurate it must deviate only within a certain acceptable limit from a standard value. Precision will be taken to denote the agreement between numerical values of two or more measurements that were made the same way, or the reproducibility of an experiment or

[10] The terms "accuracy" and "precision" are modern conventions, but historicizing these terms is beyond the scope of this study.
[11] The American Heritage Dictionary of the English Language, Third Edition, Houghton Mifflin Company, 1992. See also Yardley Beers, Introduction to the Theory of Error (Reading, MA: Addison-Wesley Publishing Company, Inc., 1957), 3; and W.J. Youden, Experimentation and Measurement (National Science Teachers Association, 1962; reprint, Mineola, NY: Dover Publications, Inc., 1998), 93-94.
[12] Beers, Introduction to the Theory of Error, 3.
[13] Beers, Introduction to the Theory of Error, 3, 5. There are those errors that may be called "blunders" that are a result of glaring mistakes in taking measurements, reading instruments, or making calculations that could have been eliminated had more care or concern gone into the experiment, or by repeating the experiment a number of times (Beers, Introduction to the Theory of Error, 6). In the case of positional measuring instruments, even if they are properly calibrated, they still do not yield true positions of heavenly bodies because of atmospheric refraction.

measurement. Experiments or measurements are highly precise when they have small random errors,[14] although there is always the possibility that experiments or measurements can contain both random and systematic errors.

The Evolution of Technology

In the evolution of technology,[15] several details pertaining to astronomical instrumentation in the early modern period merit attention. Because instruments are human-made artifacts and not biological systems, they do not evolve by themselves.[16] However, there are a number of possibilities as to what "evolves" – for instance, the instruments themselves (i.e., the entire instrument or apparatus), specific improvements of the instrument that do not change its overall structure or function (i.e., improvements on certain component parts, additions to the overall instrument), or even specific techniques designed to make using the existing instrument easier or more precise. The evolution of instrumentation can occur at a slow pace over a long period of time or rapidly. Tycho Brahe exemplifies such a period of rapid technological change in the late sixteenth century with the construction of instruments including a number of sextants, quadrants, and armillary spheres. Although Tycho did not invent these instruments, he was the first to use them systematically, and under his guidance and expertise, the overall accuracy of positional measuring instruments in astronomy improved significantly.

The transfer of various instruments from one culture to another and from one generation to the next is also relevant to the evolution of technology. There are several methods for technology transfer including transfer through exploration, travel, trade, war, and migration,[17] but there is also instruction between individuals of how to make and use instruments, something that was a closely guarded secret among those involved in the optical instrument trade in the

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Because incoming light is bent, adjustments need to be made to raw positional measurements to make up for the refraction and parallax error.

[14] Beers, Introduction to the Theory of Error, 4.

[15] See George Basalla, The Evolution of Technology (Cambridge: Cambridge University Press, 1988), and John M. Staudenmaier, Technology's Storytellers: Reweaving the Human Fabric (Cambridge, MA: The MIT Press, 1989) for a general study on the evolution of instruments; and Marice Daumas, Scientific Instruments of the Seventeenth and Eighteenth Centuries, trans. and ed. by Mary Holbrook (New York: Praeger Publishers, 1972) for scientific instruments of the seventeenth and eighteenth centuries.

[16] Basalla refers to it as a special type of "artificial selection" process.

[17] See Basalla, The Evolution of Technology, 78 for details on the various types of technology transfer.

seventeenth century. The instrument-making trade that was just getting started in the seventeenth century involved a relatively well-protected network of instrument-makers who passed down their techniques only to their own pupils.

Positional Measuring Instruments

By the end of the sixteenth century, there were a number of positional measuring instruments available for astronomical observation provided the astronomer had sufficient funds to build them himself or hire a skilled craftsman. In addition to astrolabes, cross-staffs, and armillary spheres, there were graduated arcs – octants, sextants, and quadrants – of various sizes, each type with its own specific function.[18] An octant, whose arc was an eighth of a circle, could still measure angles up to 90 degrees because it could be divided into ninety divisions, each division representing one degree.[19] The most common use for octants was to determine latitude by measuring either the altitude of the pole star or the meridian altitude of the sun that was done by pointing the direct sight toward the horizon as the instrument was held in a vertical position.[20]

Unlike the octants, sextants had a 60-degree arc but could measure up to 120 degrees. They were originally designed for navigational purposes (to measure lunar distances), which meant that most were portable and compact – although astronomers also eventually used them in temporary observatories or expeditions. Any altitude measurements on land required an artificial horizon that was created by using a flat reflecting surface consisting of either mirrors or mercury.[21] Earlier sextants were made of wood, but by the middle of the seventeenth century, most sextants had a metallic frame.

The invention of the quadrant goes back to Ptolemy's creation of a prototype that bore little resemblance to the grander quadrants built by Tycho and Hevelius. The quadrant was the

[18] For a more detailed account of all types of positional measuring instruments, see Bennett, The Divided Circle, Daumas, Scientific Instruments in the Seventeenth and Eighteenth Centuries, Gerard L'E Turner, Antique Scientific Instruments (Poole, Dorset, UK: Blandford Press, 1980), Harriet Wynter and Anthony Turner, Scientific Instruments (New York: Charles Scribner's Sons, 1975), and Nigel Hawkes, Early Scientific Instruments (New York: Abbeville Press Publishers, 1981).
[19] Bennett, "Octant," in Bud and Warner, eds., Instruments of Science, 419.
[20] Bennett, "Octant," in Bud and Warner, eds., Instruments of Science, 420.
[21] Bennett, "Sextant," in Bud and Warner, eds., Instruments of Science, 531-532. By the late seventeenth century, almost all sextants were affixed with telescopic sights.

principal measuring instrument in astronomy and produced the most precise measurements in the seventeenth century.[22] Of the various quadrants in existence by the early seventeenth century, some were very small and could be held in the palm of a hand, while others were large and heavy and required wall-mountings, hence the name "mural" quadrant. Among the smallest was the Gunter quadrant named after its inventor, Edmund Gunter (1581-1626), whose pocket-size invention used scales derived from the astrolabe.[23] Some quadrants were mounted into "ball and socket" joints making it possible to position them in either a horizontal or vertical plane, but they were usually positioned vertically to record measurements of declination or, the angular distance from the celestial equator.[24] Astronomers did this by aligning the sights on the sun or a star and reading the angle of elevation from the plumb-line that was kept vertical by a weight at the end of the cord.

Both instrument-makers and instrument-users realized the difficulty of dividing a curved scale into an equal number of degrees. Tycho Brahe was especially concerned about the accurate division of degrees because his main interest was measurement. The instrument-maker had to guarantee the physical integrity of his instrument as he marked the divisions onto the frame, and he had to ensure that the "built-in" integrity was preserved over time because instruments had a tendency to "age" over the years regardless of whether or not they were properly taken care of. The three most common problems of quadrants involved, constructing and assembling the quadrant in a perfect plane, producing an accurately graduated degree-scale, and preventing the warping of the frame due to thermal expansion (this was especially problematic when different metals were used to produce one quadrant).[25] Minimizing these problems meant fewer systematic errors. Smaller quadrants were originally constructed out of wood or iron, but

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[22] Bennett, "Quadrant," in Bud and Warner, eds., Instruments of Science, 501. This article contains a more detailed history of the development and use of the quadrant, including Islamic references.
[23] Wynter and Turner, Scientific Instruments, 23. See also Bennett, The Divided Circle, 79-80.
[24] Bennett, "Quadrant," in Bud and Warner, eds., Instruments of Science, 501; and Daumas, Scientific Instruments, 17.
[25] Daumas, Scientific Instruments, 17-18; and H. C. King, The History of the Telescope (London: Charles Griffin and Company Limited, 1955), 102. Daumas distinguishes between "geometric" and "practical" problems associated with dividing arcs. Geometric problems deal with accurately establishing the 0-90 degree points, and the practical problem deals with executing and engraving arcs with sufficient accuracy. One solution for the geometrical problem involved using "a chord equal to the radius of the arc [which] was constructed from this point with the aid of a beam compass." From there, the arcs would be bisected continuously on each side (Daumas, Scientific Instruments, 193).

increasingly in the seventeenth century, instrument-makers used brass. Larger and heavier quadrants needed to be strengthened with transverse supports.[26]

Because of problems associated with the physical integrity of graduated arcs, two particular features became necessary in their design. First, the instrument-maker's goal became "to support his limb on a construction of radial bars that resembled the spokes of a wheel to which the limb formed a segment."[27] Hevelius' instruments, for example, had more support than Tycho's, because these radial bars lessened instrument distortion. Second, builders concentrated mostly on the vertical rigidity of the arc, barely paying attention to the firmness of the lateral plane,[28] the limbs and radial bars were perpendicular, not parallel, to the ground. This was a legitimate concern for astronomers since any changes in an arc's verticality resulted in exaggerated errors of measurement.[29] Difficulties in the graduation of arcs were partly overcome by making larger instruments – instrument-makers could more easily divide on arcs of a larger radius.[30]

There were two primary methods of subdividing scales by the early seventeenth century. The first method developed used transversals, or diagonal scales. This method dates back to the early fourteenth century but the inventor is unknown.[31] Instrument-makers first engraved equidistant parallel lines on their instruments, and then divided the parallel lines with transverse lines, a technique that worked well on rules used by mathematicians. However, graduating arcs using this method was more difficult because, unlike straight lines, the division on arcs was approximate – instrument-makers had difficulty estimating the equidistance of circles and the transversals they made were not straight lines but rather, arcs of a circle.[32]

[26] Daumas, Scientific Instruments, 17.
[27] Allan Chapman, "The Design and Accuracy of Some Observatory Instruments of the Seventeenth Century," Annals of Science 40 (1983): 468.
[28] Chapman, "The Design and Accuracy of Some Observatory Instruments," 468.
[29] 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 Jeremiah 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" (Chapman, "The Design and Accuracy of Some Observatory Instruments," 468).
[30] King, The History of the Telescope, 102. Yet the idea of improving the accuracy of observation by building larger instruments originated at least as far back as Islam (See Willy Hartner, "The Rôle of Observations in Ancient and Medieval Astronomy," Journal for the History of Astronomy 8 (1977): 1-11 for details).
[31] Daumas, Scientific Instruments, 189.
[32] Daumas, Scientific Instruments, 189.

In 1542, the Portuguese mathematician, Pedro Nuñez (1502-1578), described another method of arc-division in a work entitled, *De Crepulis*.[33] His system, which eventually became known as the "nonius", involved tracing a number of concentric circle-arcs on an instrument. Despite the fact that his system was difficult to both engrave and divide correctly, Tycho used it on his instruments, thus making the method better known. In the early seventeenth century, Clavius who had been a student of Nuñez in Rome, wanted to avoid the rather difficult "nonius" method, and along with his student, Jacob Curtius, decided to modify the "nonius" method. Curtius became the first to use the "principle of fractional scales that was to form the basis of Vernier's method."[34]

The second method of dividing arcs that developed out of Clavius and Curtius' modifications of the "nonius," was significantly improved upon by Pierre Vernier (c. 1580-1637) who published the method for using this scale in 1631.[35] Vernier used two scales, the original scale on the limb of the instrument, and a second movable scale, or sector, attached to the end of the limb. Divisions were read off against the second movable scale rather than against a pointer, and this improved the accuracy of positional measuring instruments to within 1 minute of arc.[36] Moreover, the Vernier method increased the ability of instrument-makers to subdivide their instruments into increasingly smaller increments. By the end of the sixteenth century, instrument-makers could divide the scales of some observational instruments into quarters of degrees; by the end of the seventeenth century, subdivisions of 10 minutes had become common.[37]

Tycho Brahe never used the Vernier scale because it was developed thirty years after Tycho's death. Nonetheless, his use of transversals was a great success for several decades after his death because he consistently used them.[38] In fact, Tycho learned even more about instruments on his travels abroad, "making contacts with astronomers and collecting ideas for

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[33] Chapman, Dividing the Circle, 22; and Daumas, Scientific Instruments, 191.
[34] Daumas, Scientific Instruments, 191. See also Turner, Early Scientific Instruments, 134-135.
[35] Turner, Early Scientific Instruments, 134-135. This system did not become more popular until the end of the 18th century – more than a century after Vernier first described it (Daumas, Scientific Instruments, 191).
[36] King, The History of the Telescope, 94. King does not indicate whether this is in theory or practice. Dividing an instrument into arcminutes is not the same as actually producing a series of observations precise to within one minute of arc.
[37] Daumas, Scientific Instruments, 192.
[38] Turner, Early Scientific Instruments, 55; see also Victor Thoren, "New Light on Tycho's Instruments," Journal for the History of Astronomy 4 (1973): 29.

instruments wherever he went."[39] His instruments were neither randomly assembled nor haphazardly collected, but rather, were carefully gathered over a period of more than twenty years, although there were some instruments – or "intermediates" – that became superfluous and were succeeded by better versions.[40]

Tycho had an impressive group of instruments at the height of his career that included quadrants, sextants, and armillaries (sets of rings used to demarcate the zodiac and equator).[41] But perhaps the grandest of his instruments were his quadrants, and especially his great mural quadrant.[42] The largest of his quadrants measured approximately 10 feet in radius. Tycho also significantly improved upon one of the most common sighting problems of the sixteenth century. Before Tycho, the most common sights were two pinnules, one on each end of the alidade aligned for sighting the target object, such as a star. There were difficulties involving the size of the pinnules – if they were too small, fainter stars could not be seen, and if they were too large, an uncertainty referred to as "parallax" crept into the measurement. In order to eliminate this problem, Tycho introduced "parallel" sights, or slits, onto the alidade, that were composed of four adjustable slits parallel to each side of a brass plate in the shape of a square.[43] These parallel sightings allowed Tycho to line up an object between two parallel sightings and adjust them according to the object's size.

In addition to Tycho's precautions when designing his slits, he also used larger instruments. Unfortunately, larger instruments were more likely to bend and warp under their great weight, although Tycho found that he could minimize systematic errors by using

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[39] John D. North, The Norton History of Astronomy and Cosmology (New York: W.W. Norton & Co., 1995), 301.
[40] Thoren, "New Light on Tycho's Instruments," 25, 41.
[41] A detailed listing of instruments is available in Tycho Brahe, Tycho Brahe's Description of his Instruments and Scientific Work as Given in *Astronomiae instauratae mechanica*, trans. and ed. by Hans Ræder, Ellis Strömgren, and Bengt Strömgren (Copenhagen: I Kommission Hos Ejnar Munksgaard, 1946). For secondary source information on his various instruments, see Chapman, "The Tychonic School and Its Approach to Instrumentation," Chapter 2, Dividing the Circle; Chapman, "Tycho Brahe – Instrument," in Allan Chapman, Astronomical Instruments and Their Users: Tycho Brahe to William Lassell (Aldershot, Hampshire, Great Britain: Variorum, 1996), III, 71-2 to 72-3; and for a genealogical chart of his instruments from 1569 to 1591, see Thoren, "New Light on Tycho's Instruments," 36.
[42] This is the quadrant that one often sees in the well-known painting by Hans Kneiper, Hans van Steenwinckel, and Tobias Gemperle of 1587. Tycho, located in the foreground, points to the front sight of the quadrant, and the message conveyed by the painting "was that Tycho Brahe had created a research institute to probe the secrets of the heaven and earth, and that the institute itself comprised a microcosm in harmony with the cosmos" [Christianson, On Tycho's Island, 118].
[43] King, The History of the Telescope, 22. Another type Chapman refers to is the "peg and slit" sight (Chapman, Dividing the Circle, 18). Actually, Tycho first worked with a "slit-sight" before using "parallel" sightings, but the problem with "slit-sights" was that they only worked for one sighting, not two at the same time. Parallel sightings allowed for an object to be seen along both of the sightings (Thoren, "New Light on Tycho's Instruments," 27-29).

transversal scales because they provided a clearer reading of the divisions.[44] Furthermore, Tycho used wood and steel for his instruments and avoided adding unnecessary parts. This made his instruments lighter, and some of these instruments could even be disassembled through removable screws.[45]

Tycho also established a specific set of rules that were a part of his methodology of observation. He believed that higher levels of precision depended on combining measurements from a variety of instruments over a long period of time thereby minimizing small, random errors, and also identifying and eliminating any systematic errors. Tycho's use of the law of "averages" to eliminate as many errors as possible worked well although even Tycho's accuracy had its limits. He also believed, however, that the more functions any one instrument performed, the less precise were its results, and therefore Tycho depended on each instrument "to fulfill only one specialised task" at a time, "and that could be left in one critical adjustment."[46] Tycho's observation methodology also involved the use of three assistants – one to view the object through the sights, another to record the results in a ledger, and a third to check and record the time of the measurement or observation. Tycho and his assistants also crosschecked their measurements with other instruments.[47]

The end result of Tycho's efforts to minimize errors and maximize the number of observations was an unprecedented accuracy in astronomical measurement – Tycho managed to come within approximately 1 minute of arc in the accuracy of his measurements. This led one of his biographer's to claim that "nobody before Tycho had both cared enough about high accuracy and taken enough other steps to achieve it, to make such a minor matter worthy of concern."[48] After Tycho's work, precision was no longer a minor matter but a major concern as astronomers in the beginning of the seventeenth century attempted to increase the limits of precision even further. But naked-eye sights had one severe limitation – the resolving power of the human eye – and it would take the invention and use of another instrument, the telescope, to move positional astronomy beyond this constraint.

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[44] Turner, Early Scientific Instruments, 56; and King, The History of the Telescope, 16.
[45] Turner, Early Scientific Instruments, 56.
[46] Chapman, "Tycho Brahe – Instrument," in Chapman, Astronomical Instruments and Their Users, III, 70-1.
[47] North, The Norton History of Astronomy and Cosmology, 302. Clocks were extremely unreliable in Tycho's time as they only had hour-clocks. But even their rate of movement varied. Minute hands first appeared after the middle of the seventeenth century (King, The History of the Telescope, 20).

The Development of the Telescope

The greatest 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" is debated, but actual recorded history of the telescope began with Hans Lipperhey's (c. 1570-1619) petitioning for a patent in 1608. Unfortunately for him, the States-General in the Netherlands that was appointed to examine his instrument, rejected his claim on the grounds that the telescope "could not be kept a secret" because it was easy to duplicate[49] – all that was needed in order to construct a telescope were two lenses and a tube. Galileo first heard about the invention in the summer of 1609, and he immediately constructed a telescope with a magnifying 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 he turned it towards the heavens. Because of his curiosity and need for observation, Galileo constructed the telescope with a 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*."[50]

The invention of the microscope also "took place in a non-learned, everyday context."[51] Both the telescope and the microscope had extraordinary consequences in the seventeenth century, because they not only revealed

> 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.[52]

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48 Thoren, "New Light on Tycho's Instruments," 27.
49 Albert Van Helden, "The Invention of the Telescope," Transactions of the American Philosophical Society 67, no. 4 (1977): 21. See also King, The History of the Telescope.
50 Van Helden, "The Invention of the Telescope," 26.
51 Turner, Early Scientific Instruments, 122.
52 Turner, Early Scientific Instruments, 122.

Other 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.[53]

Quantification was of paramount importance in some of these instruments, because without measurement, discovery was limited. This makes them different than the telescope or microscope that were "*exploratory* instruments without a capacity to measure."[54] Astronomers, in particular, considered it a disadvantage that telescopes alone could not initially deduce the particular measurements that 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 taken using traditional instruments such as the quadrant, sextant, and cross-staff; but the telescope was not powerful enough to replace these traditional measuring instruments. Only in later decades of the seventeenth century, when the telescope was eventually combined with traditional measuring instruments (in addition to new measuring devices), were "the great advances in practical astronomy" achieved,[55] such as the precise determination of the sizes and distances of the planets.

Galileo's legacy was to turn the telescope into a scientific instrument, nevertheless, it took over two decades for it to become more widely used as such because its full potential was not realized immediately.[56] However, after the telescope became recognized as an astronomical instrument of discovery, it was not the Galilean version that astronomers used, but rather, the Keplerian version, aptly referred to as the "astronomical" telescope that followed the invention of the Galilean, or "Dutch" telescope.

Kepler wanted to incorporate physics into the heavens by using both philosophy (theory) and mathematics (of the heavens). With the publication of his *Supplement to Witelo* in 1604 and *Dioptrice* in 1611, Kepler established for the first time a theoretical foundation for the study and

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[53] By the middle of the century, Christiaan Huygens (1629-1695) adapted his new pendulum clocks so that they could indicate minutes "with greater accuracy than the old clocks could indicate hours" (King, The History of the Telescope, 104).
[54] King, The History of the Telescope, 104.
[55] King, The History of the Telescope, 104. 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].
[56] Albert Van Helden, "The Telescope in the Seventeenth Century," Isis 65 (1974): 42-43.

understanding of lenses, something even Galileo had not accomplished despite all his incredible discoveries. Yet the work of Kepler is perhaps the only example of theory preceding practice as "the theoretical researches of the scientist into optical systems at no point in the seventeenth century led directly to demonstrable improvements in the telescope."[57] Based on theory, Kepler decided to change the component lenses within the tube; instead of using a convex objective lens and a concave eyelens, he inserted two convex lenses. By doing this, the Keplerian 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.[58] For astronomers, however, the most substantial advantage afforded by the "astronomical" telescope over the Galilean model, was its wider field of view. However, any advantages of the Keplerian telescope over the "Dutch" telescope "could not have been predicted by Kepler from theory: they had to be discovered by actually looking at the heavens through such a telescope."[59]

Because astronomers could see a larger area of the sky with one glance through the Keplerian model, the "astronomical" telescope, "almost entirely displaced Galileo's instrument for serious astronomical work."[60] In the 1630's, astronomers used telescopes more frequently than the previous two decades. But by the 1640's, the two types of telescopes diverged in terms

[57] Van Helden, "The Telescope in the Seventeenth Century," 49. Unlike the Galilean model, the Keplerian "is a telescope whose theoretical description preceded its actual construction" (Ibid., 40-41). Five years later, in 1616, the Dutch astronomer-mathematician Willebrord Snellius (1580-1626) discovered the law of light refraction at the age of 25 (Snell's Law).

[58] See King, The History of the Telescope, 36, 45 for a more detailed explanation of the differences between the two.

[59] Albert Van Helden, "The 'Astronomical Telescope,' 1611-1650," Annali dell' Istituto e Museo di Storia della Scienza di Firenze 1 (1976): 19. Van Helden also argues that Christopher Scheiner (1575-1650) "had actually *looked through* such a telescope and discovered its advantages," and although Francesco Fontana had also claimed to have made a telescope of two convex lenses as early as 1608, he probably learned about the "astronomical" telescope from Scheiner (Van Helden, "The 'Astronomical Telescope,' 1611-1650," 20, 25).

of their uses and users,[61] although astronomers soon realized that telescopes and their lenses, if not properly constructed, could pose serious problems to their observations due to flaws such as bubbles, fissures, or any other irregularities. Since most flaws were created during production, the best optical instrument-makers purchased the best glass for their clients, and Italian – Venetian glass – was considered the highest quality, followed by glass from Holland, then England, and finally, France.[62] For most of the seventeenth century, lenses were made by hand using metal tools and molds, although by the end of the century, lens-grinding lathes became more common.[63]

In addition to concerns over the quality and construction of lenses, astronomers also had to contend with spherical and chromatic aberration. Spherical aberration, which was caused by lenses with perfectly spherical curvatures, resulted in an image that was distorted along the edges, and clear 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.[64] A third problem which usually interfered with observation, was "uninvited", or stray, light which tended to "creep" into telescopes. Of these 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.[65] 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.[66]

Unfortunately, longer telescopes were the more difficult to use. Therefore, astronomers eventually developed the long-focus telescope that consisted of a long, solid, wooden tube,

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[60] Turner, Early Scientific Instruments, 91. One of the last individuals to use a Dutch telescope was Hevelius for studying the moon, leading to the publication of his *Selenographia*. Thereafter, he used "astronomical" telescopes chiefly for projecting the Sun's image (Van Helden, "The 'Astronomical Telescope,' 1611-1650," 34-35 ff. 102).

[61] Van Helden, "The Telescope in the Seventeenth Century," 44. Also in 1645, Antonius Maria Schyrlaeus de Rheita (1604-1660) published his *Oculus Enoch et Eliae* in Antwerp in which he became the first to coin the terms "objective" and "ocular" and use the term "astronomical" to refer specifically to telescopes with two convex lenses (Van Helden, "The 'Astronomical Telescope,' 1611-1650," 32).

[62] Daumas, Scientific Instruments, 32.

[63] The first lens-cutting machines actually appeared as early as 1650, with the construction of a lathe by an instrument-maker named De Meru, although his "machine does not seem to have roused much interest among his contemporaries" (Daumas, Scientific Instruments, 37).

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

[65] Allen, "Problems Connected with the Development of the Telescope," 303-304. See also Bennett, The Divided Circle, 66; and King, The History of the Telescope, 50. Astronomers believed that the colors would diminish if light traveled a longer distance between the object glass and eyelens.

[66] 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.

usually constructed by sections, and held up by a series of wooden planks, cables, pulleys, and a mast.[67] Even this telescope, however, was marked by problems including 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, astronomers implemented further modifications and Huygens developed the "air" telescope in the 1650's. The "air" telescope was a tubeless telescope in which the object glass component and eyelens component were connected by a cord that aligned and adjusted the two lenses.[68] Unfortunately, even the "air" telescope had its own share of problems because it was difficult to manipulate the cords and properly align the lenses. Towards the end of the century, the problems associated with long refractors prompted astronomers to search for other possibilities and, consequently, Newton produced the reflecting telescope in 1668. But although the substitution of objective lenses by mirrors seemed to correct certain problems that affected lenses, astronomers and telescope-makers were still plagued by 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.[69]

Between Galileo's first use of the telescope and the "second wave" of discoveries in the latter half of the seventeenth century, astronomical practitioners carried out a number of important observations and published their results. By 1630, learned men throughout Europe were still adjusting to Galileo's discoveries, as Galileo was *the* premier telescope-user. This, however, did not interfere with Scheiner's work on the sun and the publication of his *Rosa Ursina* in 1630. Furthermore, three observers witnessed a transit of Mercury in 1631: Remus in Alsace, Johann Cysat (1586-1657) (his correspondent) in Ingolstadt, and Pierre Gassendi (1592-1655) in Paris.[70] In 1639, Jeremiah Horrocks (1619-1641) observed a transit of Venus that he had predicted and which lasted less than an hour before the sun set; this still gave him ample opportunity to observe and measure Venus' diameter.[71] In the same year, William Gascoigne invented the micrometer and telescopic sights. During the 1640's, sporadic observations were being carried out by a handful of astronomers throughout Europe including Gassendi, Hevelius

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[67] Hevelius' telescope was 150 feet in length.
[68] Turner, Early Scientific Instruments, 92.
[69] Turner, Early Scientific Instruments, 93.
[70] Albert Van Helden, Measuring the Universe: Cosmic Dimensions from Aristarchus to Halley (Chicago: University of Chicago Press, 1985), 97.
[71] Van Helden, Measuring the Universe, 107. Unlike Horrocks who had the opportunity to view the transit completely, his friend, William Crabtree (1610-c. 1644), only partially observed it (Bennett, The Divided Circle, 93-94).

in Danzig, and Giovanni Battista Riccioli (1598-1671). Two of these astronomers published major works based on their observations – Hevelius, his *Selenographia* in 1647 (the premier work on the moon throughout the seventeenth century) and Riccioli, his *Almagestum Novum* in 1651 that contained a complete survey of celestial sizes and distances since the time of Ptolemy.

In March 1655, a young man who originally was not interested in pursuing astronomy seriously, used one of his telescopes to make "the first major discovery in half a century – a satellite of Saturn."[72] Christiaan Huygens benefited by growing up in an environment that influenced him favorably toward astronomy and astronomical instruments. His father, Constantijn Huygens, was an aficionado of the arts and sciences and he even corresponded with Marin Mersenne (1588-1648) whose contacts included René Descartes (1596-1650).[73] Christiaan Huygens was determined to discover the nature of Saturn's anses, and by March 1656, he set down a hypothesis of Saturn's rings in an anagram. But it was not until July 1659 that he published his major work, *Systema Saturnium*, a "thoroughly polished book on telescopic astronomy which far transcended the narrow problem of Saturn's anses," and which was "the most important work on telescopic astronomy since Galileo."[74] By openly boasting that he had made these recent discoveries with the best telescopes in Europe, Huygens created tension between himself and Hevelius (whose own telescopes were called into question), and initiated a controversy between himself and Divini (one of Europe's best optical instrument-makers). These animosities are better understood within the context of the telescope making-trade of the seventeenth century.

As the popularity of telescopes increased, both astronomers and craftsmen became increasingly interested in the competitive telescope-making trade. Telescopes were precious and expensive commodities because of their usefulness and because of the difficulty in constructing good lenses.[75] Not surprisingly, the chief interest of optical instrument-makers in the first half of the seventeenth century was to improve lens-grinding techniques that ultimately resulted in better-quality telescopes.[76] The artistic design of telescopic tubes was important to telescope-

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[72] Van Helden, "Huygens," in H. J. M. Bos, M. J. S. Rudwick, H. A. M. Snelders, and R. P. W. Visser, eds., Studies on Christian Huygens. Invited Papers from the Symposium on the Life and Work of Christian Huygens, Amsterdam, 22-25 August 1979 (Lisse: Swets & Zeitlinger B.V., 1980), 148-149.
[73] Bos, "Christian," in Bos et al., Studies on Christian Huygens, 11.
[74] Van Helden, "Huygens," in Bos et al., Studies on Christian Huygens, 150.
[75] 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.
[76] Van Helden, "The Telescope in the Seventeenth Century," 45.

makers; they constructed telescopes out of cardboard with rings of boxwood, ebony, or some other type of hard wood inserted within the tubes to hold the lenses in place.[77]

The instrument-making trade was relatively successful in some countries, while it stagnated in others, but it was not a seventeenth-century phenomenon – in sixteenth-century England, for example, there were a number of fine instrument-makers including Humphrey Cole, Richard Chancellor, and Thomas and Leonard Digges (c. 1520-c. 1559). Therefore, the instrument-making trade was already established in England by the beginning of the seventeenth century.[78] Flemish instrument-makers seemed to be productive in the sixteenth century, but Flemish workshops declined in number and even disappeared by the beginning of the seventeenth century. The Netherlands experienced a similar fate, although the works of some instrument-makers survived including works by Cornelius Drebbel (1572-1633), who taught others and made a variety of instruments; and in Amsterdam, Pieter Fransz (~1629) and Willem Jans Blaeu (1571-1638), who made planetaria, quadrants, and sextants.[79] Unfortunately, very little is known about the instrument-making trade further east in Europe during the early modern period, although there is evidence of instrument-making in cities such as Prague, Augsburg, Munich, Nuremberg, and Dresden.[80]

Because of the increasing demand of the telescope in the seventeenth century, some instrument-makers focused exclusively on optical instruments. One of the earliest telescope-makers was Ippolito Francini who by 1619, had his own shop and produced telescopes for Galileo and others until his death in 1653.[81] Another early workshop for optical instruments was that of Daniel Chorez in France whose advertisements for telescopes and binocular microscopes appeared as early as 1625, and he even wrote a pamphlet on instrument-use entitled, "Les admirables lunettes d'approche reduites en petit volume avec leur vray usage." A contemporary

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[77] Daumas, Scientific Instruments, 31. The Italians more than anyone else had a rich tradition of ornately-decorated tubes. The first brass tubes did not appear until the end of the seventeenth century.
[78] Daumas, Scientific Instruments, 67.
[79] When Antwerp fell in 1585, many scholars and instrument-makers fled from the southern Netherlands to the North. As a result, there was a solid tradition of instrument-making in the Dutch Republic throughout the seventeenth century.
[80] Daumas, Scientific Instruments, 83, 88. South-German (Imperial) cities had been centers of metalworking craft since the late Middle Ages. The tradition continued into the seventeenth century but was greatly affected by the Thirty Years' War.
[81] Silvio Bedini, "Lens-Making for Scientific Instrumentation in the Seventeenth Century," Applied Optics 5 (1966): 690.

of Chorez's working in France, was Antoine Ferrier who made lenses for Descartes and was actually "received by scholars as one of themselves."[82]

The optical instrument-making trade was more successful in London than anywhere else (with the exception perhaps of Italy) because London's economy flourished in the seventeenth century resulting in the prosperity of instrument-makers. Moreover, unlike the French whose guild restrictions controlled technical development and the number of workshops, the English were not restricted by such regulations.[83] Unfortunately, the Great Fire of 1666 destroyed nearly all of the early records of London guilds, including the Spectaclemakers' Company that listed many optical-instrument makers of England.[84] In July 1628, Robert Alte, a "spectacle-maker by trade, submitted a petition to Charles I on behalf of a group of spectacle-makers, requesting a royal charter for the creation of a separate guild for members of their craft working in the City of London,"[85] and on May 16, 1629, a royal charter was granted which established the "Fellowshippe of Spectacle makers of London." Before 1640, there were at least two optical instrument-makers working in London, Bates (~1626) and Drebbel, who came from the Netherlands (1605-1610, 1612-1633); in the 1640's, Richard Reeve (senior) established a workshop (1641-1666); and by the 1650's, the number of optical instrument-makers in London rose to six.[86]

Italian craftsmen and scientists, however, "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).[87] Fontana and Divini, in particular, 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." They became

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[82] Daumas, Scientific Instruments, 30, 71-72.
[83] Daumas, Scientific Instruments, 90, 93.
[84] Gloria Clifton, "Spectaclemakers', " in Anderson, Bennett, and Ryan, eds., Making Instruments Count, 342. See also her book, Directory of British Scientific Instrument Makers 1550-1851 (London: The National Maritime Musuem) 1995.
[85] Clifton, "Spectaclemakers', " in Anderson, Bennett, and Ryan, eds., Making Instruments Count, 350.
[86] Clifton, "Spectaclemakers', " in Anderson, Bennett, and Ryan, eds., Making Instruments Count, 357-361 and Table 2.
[87] Bonelli and Van Helden, "Divini and Campani," 4-5. See also the articles by Albert Van Helden, "The Accademia del Cimento and Saturn's Ring," Physis: Rivista Internazionale di Storia della Scienza 15 (1973): 237-259; and "Eustachio Divini Versus Christian Huygens: A Reappraisal," Physis: Rivista Internazionale di Storia della Scienza 12 (1970): 36-50.

part of the "scientific circles, [because] they were well informed, if not learned, experimented with new methods and optical systems, made their own observations, and published books."[88]

Rivalries were common among instrument-makers, but the rivalry between Divini and Campani was a highly visible affair, especially since both worked in Rome. Divini's telescopes were more reputable in the beginning, but Campani's telescopes eventually surpassed Divini's by the early 1660's. Theirs was neither the first nor the only competition, however, between telescope-makers. For instance, Fontana and Evangelista Toricelli (1608-1647) also competed against one another during the 1640's in a contest of telescopes known as a "paragone" which was intended to evaluate the instrument-making abilities of one individual over the other. Participants took certain precautions during the "paragoni" so that they were as objective and fair as possible; participants used the same length instruments, conditions were arranged so that neither party was favored, and the Accademia del Cimento developed sheets with writing on them (similar to the eye charts used today) in the early 1660's.[89]

With the publication of Huygens' *Systema Saturnium* in 1659, the issue of telescope quality became a highly publicized affair. Huygens claimed that only by using superior telescopes was he able to discern satellites of Saturn. Since this implied that other telescopes were inferior, Huygens' proclamation managed to offend "several men who took great pride in the excellence of their telescopes, among them Johannes Hevelius and Divini."[90] In Hevelius' case, however, Henry Oldenburg (?1618-1677) came to his defense against those who claimed Huygens' telescopes superior.[91]

By 1665, it was clear that Campani, the youngest of the three, had surpassed Divini's capabilities, and he "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."[92] Oldenburg informed Robert Boyle (1627-1691) that the "paragone" performed in Rome suggested that Campani's telescopes were "more successful,"[93] and news of the competition between Divini and Campani had even spread to France as a letter from Adrien

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[88] Bonelli and Van Helden, "Divini and Campani," 8.
[89] See Bonelli and Van Helden, "Divini and Campani," 30-39.
[90] Van Helden, "The Accademia del Cimento and Saturn's Ring," 241.
[91] Oldenburg to Saporta, 11 August 1659 [A. Rupert Hall and Marie Boas Hall, eds. The Correspondence of Henry Oldenburg, Vols. 1-11, 1641-1677 (Madison, WI: The University of Wisconsin Press, 1965), I, 300].
[92] Bonelli and Van Helden, "Divini and Campani," 4, 39.
[93] 5 December 1665, OC, II, 629.

Auzout (1622-1691) to Oldenburg indicates.[94] In the same letter, Auzout informed Oldenburg that Cassini had also become involved in observations of Jupiter; in the 1670's, Cassini eventually turned his attention to the satellites of Saturn.

Cassini's discovery of the four satellites of Saturn and surface markings on both Jupiter and Mars (using Campani telescopes) marked a new phase for astronomy. The inventions of the 1640's of the micrometer and telescopic sights, which lay dormant in the minds of only a handful of individuals, were about to reappear once again, more than twenty years after their initial invention. When they were re-invented and eventually used in conjunction with the telescope and traditional measuring instruments, the apparent shape and size of the heavens were transformed even further. While the telescope managed to bring the planets closer and push the stars further away, telescopes mounted with micrometers and telescopic sights made it possible for astronomers to determine the sizes of those planets and the distances between stars.[95]

The Application of Telescopes to Precision Astronomy

Astronomers considered turning telescopes into quantitative instruments as soon as they turned them towards the heavens because they realized that they had reached the uppermost limits of accuracy that could be reached with the use of traditional measuring instruments, chiefly those that depended on dividing arcs. This placed more pressure on craftsmen to come up with new techniques and instruments of greater accuracy. However, traditional instruments using naked-eye sights were limited by the resolving power of the human eye of approximately 1 minute of arc.

Since novelty must exist for technology to evolve,[96] two significant discoveries were made that contributed to the evolution of telescopes and measuring instruments – the micrometer and telescopic sights. These two inventions are revolutionary because of their bearing on the study of astronomy in the late seventeenth century. They went through what is referred to as "intermittent duration" – when an "artifact that has been discarded is revived and reinstated at a

[94] 25 April 1666, OC, III, 103.
[95] Van Helden, "The Telescope in the Seventeenth Century," 56-58.
[96] Basalla, The Evolution of Technology, viii.

later date."[97] The micrometer and telescopic sights were initially invented and used with telescopes in 1639 for a brief period of time, but were not reinvented until the mid-1660's by both the English and French independently.

By 1640, William Gascoigne had discovered the two methods of applying telescopes to precision astronomy. First, a smaller sized telescope, or "telescopic sight," was attached to a graduated arc (i.e., a quadrant or sextant), while cross-hairs were placed in the focal plane of the objective glass. Cross-hairs, which consisted of two hairs intersecting each other at right angles in the center of the field of view, were simultaneously in focus with the object.[98] The use of telescopic sights on measuring instruments created its own set of problems because by using telescopes to distinguish angular measurement, defects were magnified as a whole. Nevertheless, telescopes increased the magnification and improved the definition of target objects.[99]

The second method of using telescopes to measure small angular separations, was to use them alone, without affixing them to graduated arcs, but 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 that enabled one to measure small angles. Astronomers used the micrometer for different purposes; it was a small measuring device, made up of several component parts that worked together to measure small angular distances and apparent diameters of planets.[100] The device usually consisted of two parallel hairs (or metal bars), and some type of internal scale that measured angular widths.

The Micrometer

Interest in finding a method for determining the diameters of celestial bodies extends back to Galileo, and although he did not invent the micrometer, at least one of his telescopes

[97] Basalla, The Evolution of Technology, 188. "Discarded" is not the most accurate term to use in the case of the micrometer and telescopic sights. The premature deaths of those who were first familiar with these inventions is the chief reason for their temporary absence.
[98] Abraham Wolf, A History of Science, Technology, and Philosophy in the 16th & 17th Centuries (New York: The Macmillan Company, 1935), 167.
[99] See King, The History of the Telescope, 102.
[100] Bennett, The Divided Circle, 64.

incorporated a scale.[101] By January 1612, he created a grid on a circular piece of cardboard about 10 centimeters in length, and he used his left eye to observe while his right eye measured the diameter of Jupiter on the scale (though he did not have a watch to keep time).[102] This early effort to measure suggests an immediate desire to quantify observations.

Gascoigne, credited with the invention of the micrometer in 1639, died in 1644 when he fell with other loyal subjects of Charles I at Marston Moor during the English Civil War.[103] 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, a discovery that led to his invention of the micrometer. Common to both cross-hairs and micrometers was the *reticule*, which was the starting point for both telescopic sights and micrometers. Since Gascoigne first thought of 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, and these hairs were both economical and readily available.[104] Gascoigne soon realized that 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."[105] The object viewed was at infinity, outside the telescope. Since the hairs were in the focal plane of the objective lens, they appeared superimposed on the image of the object. Gascoigne immediately began using them in his observations.[106]

The evidence for Gascoigne's accidental discovery came chiefly from two letters he sent to 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

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[101] 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): 229-230.
[102] See McKeon, "Les Debuts de l'Astronomie de Precision (I)," 226-228.
[103] S.J. Rigaud, ed., Correspondence of Scientific Men of the Seventeenth Century, I, (Hildesheim, Germany: Georg Olms Verlagsbuchandlung, 1965), 34.
[104] Jon Darius and P.K. Thomas, "Crosswires in a Guiding Eyepiece," Journal of Physics, E: Scientific Instruments 14 (1981): 761.
[105] Darius and Thomas, "Crosswires in a Guiding Eyepiece," 761.
[106] 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 Scientific Instruments, 133]. He only used "astronomical" telescopes, since they were the only type of telescopes that could clearly focus the image of the hairs within the tube (See Van Helden, Measuring the Universe, 119; Van Helden, "The 'Astronomical Telescope,' 1611-1650," 29; and Bennett, The Divided Circle, 64).

> 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?[107]

Gascoigne's cross-hairs led to his development of a micrometer, and in a subsequent letter, he described 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 and two glasses. . . we might in a few nights find the true meridian and pole.[108]

Gascoigne also suggested shining a candle through the side of the tube helped one see the hairs without the light interfering with observation. He told Oughtred that he had shown his "internal" scale "and its use in a glass," to others who 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).[109]

Initially, the micrometer consisted of two separate instruments: a pair of reticule wires or plates, and screws with turning parts used to measure small distances. Furthermore, micrometers were usually small (about six inches in length), but easily inserted into the tubes of "astronomical" telescopes near the eyelens. Gascoigne's micrometer consisted of two thin pieces of metal mounted parallel to each other on screws that opened and closed the two blades. The number of revolutions needed to attain a required opening was shown on a scale and the fractions of a revolution on a dial that was divided into a hundred parts:

> If a hair be placed at right angles to the scale and pass through *q*, and be always moved in a vertical plane, in which setting it to one star, and the one of the brass pointers to another, and finding by both the pointers the distance between these stars, we have two sides, one of which is opposite to the right angels, and so a ready way to find the inclination, if this will content us.[110]

[107] Rigaud, Correspondence, I, 33-34.
[108] February, 1641, Rigaud, Correspondence, I, 51.
[109] Rigaud, Correspondence, I, 54. Horrocks knew about Gascoingne's invention as early as 1639, but he had no "access to such an instrument," and was thus only able to estimate the diameters of the superior planets (Van Helden, Measuring the Universe, 111).
[110] Rigaud, Correspondence, I, 49.

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.[111]

Despite Gascoigne's premature death, a small group of his friends, including Oughtred and Christopher Towneley (1604-1674) kept records of his invention, but they did not immediately publicize his work. In fact, the invention of the micrometer did not appear in print until 1659, when Huygens published his *Systema Saturnia*, in which he described his own version of Gascoigne's micrometer. Instead of knife-edge metal pieces, Huygens used a "virgula" – a solid metal bar that covered the target object or aligned with the edges of two objects. And he also used a pendulum clock to determine the diameters of objects, particularly planets, by determining the angle his field of view subtended. He then related all his measurements to the width of his field of view.[112] Huygens also found a solution to the problem of glare from bright planets that affected the accuracy of readings from the micrometer – he used soot from a candle to coat the eyelens of his telescope.[113]

By publishing an account of his micrometer, Huygens brought it to the attention of other astronomers who then experimented with their own designs.[114] Cornelio Malvasia (1603-1664) suggested "a grid network of squares formed by fine silver wire;" Divini redesigned a micrometer of "cross wires in focus;" and Robert Hooke developed his hair micrometer in which he used hairs instead of thin metal bars.[115] Richard Towneley (1629-1707), who initially learned about Gascoigne's micrometer from his correspondence, constructed a micrometer that most closely resembled Gascoigne's; the only substantial difference between the two was that Towneley used one screw to move one reticule while the other reticule remained fixed.[116]

Before the introduction of the micrometer, telescopes could neither measure the apparent diameters of celestial bodies, nor could they measure the angle subtended by two objects.[117] In fact, "the measuring of apparent diameters, was much more an art than a science," before the

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[111] Chapman, Dividing the Circle, 41; and Brooks, "The Development of Micrometers," 131.
[112] Huygens, *Systema Saturnium* (1559); in Œuvres Complètes de Christiaan Huygens, Vol. 15 (La Haye: Martinus Nijhoff, 1925), 350.
[113] Huygens, *Systema Saturnium* (1559); in Œuvres Complètes, Vol. 15, 348-350.
[114] Van Helden, "Huygens," in Bos et al., eds., Studies on Christian Huygens, 154-155.
[115] Turner, Early Scientific Instruments, 133.
[116] Brooks, "The Development of Micrometers," 131.
[117] See Chapman, Dividing the Circle, 42.

micrometer because accuracy depended on the observer's abilities.[118] Consequently, the micrometer changed the methods of observation.

Telescopic sights

Telescopic sights made as much of an impact on precision astronomy as micrometers. The improvement afforded by these sights "probably [constituted] the single most important technical advance in precision instrumentation in the seventeenth century."[119] Astronomers like Tycho Brahe used open sights on traditional measuring instruments, but these instruments were limited by the resolving power of the unaided human eye. Telescopic lenses subsequently increased precision and provided a more precise line of collimation (the alignment of cross-hairs with the optical center of the target object). Aligning the line of collimation was more difficult 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 object, making human error more likely.[120]

Although Gascoigne was the first to combine telescopic sights with measuring arcs successfully, Jean-Baptiste Morin (1583-1656), suggested replacing open sights with telescopic sights as early as 1634,[121] and he even tried to secure a telescope onto the alidade of his quadrant. It did not work, however, because by using a Galilean rather than an "astronomical" telescope, he did not have a common focus between the object glass and eyelens.[122] Knowledge of Gascoigne's invention of telescopic sights was even more limited than the micrometer because he did not share the results with Oughtred. Consequently, Gascoigne's invention was forgotten until 1665 when Richard Towneley reintroduced Gascoigne's work,[123] though Hooke and

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[118] Van Helden, Measuring the Universe, 121. Flamsteed 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].
[119] Bennett, The Divided Circle, 63.
[120] See 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.
[121] Daumas, Scientific Instruments, 49; Turner, Early Scientific Instruments, 137; and 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): 224.
[122] Daumas, Scientific Instruments, 49; and Chapman, Dividing the Circle, 35. Since only "astronomical" telescopes could be used on measuring instruments, the Galilean telescope was eventually abandoned in precision astronomy.
[123] McKeon, "Les Debuts de l'Astronomie de Precision (Part II)," 228.

Christopher Wren (1632-1723) began experimenting with telescopic sights as early as 1665.[124] Divini also independently rediscovered telescopic sights in 1663 when a friend of his asked him to construct an instrument for surveying.[125]

In the late 1660's, priority disputes broke out between the English and French over which country's astronomers discovered micrometers and telescopic sights first. In 1717, William Derham (1657-1735) responded to the French 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."[126] He also claimed that Richard Towneley sufficiently proved that the invention of the micrometer was Gascoigne's and not Auzout or Jean Picard's (1620-1682),[127] and he added that "Gascoigne was the first that measured the Diameters of the Planets, &c. by a Micrometer," and "he was the first that applied Telescopick Sights to Astronomical Instruments."[128] Derham referred to two letters written by Gascoigne to William Crabtree wherein Gascoigne described his micrometer and the application of "measuring glasses" to both quadrants and sextants.[129] Derham 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, 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.[130]

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[124] McKeon, "Les Debuts de l'Astronomie de Precision (Part II)," 229; and Chapman, Dividing the Circle, 37. Hooke claimed as much in his *Animadversions on the First Part of the 'Machina Coelestis' of. . . Johannes Hevelius* (London, 1674).
[125] McKeon, "Les Debuts de l'Astronomie de Precision (Part II)," 229. Divini described his telescopically-mounted sights in a letter dated July 15, 1663. It is interesting to note the progression of discovery from micrometer to telescopic sights in both Gascoigne and Divini's cases. The application of telescopic lenses on astronomical measuring instruments immediately followed the micrometer's invention (Ibid.).
[126] PT, XXX, no. 352, 603.
[127] 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 first discovered them.
[128] PT, XXX, no. 352, 604.
[129] 25 January 1641 and 24 December 1641 (PT, XXX, no. 352, 604-605).
[130] Unfortunately, he died prematurely (PT, XXX, no. 352, 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*, W. Derham, ed. (London, 1726; reprint, London: Frank Cass and Company Limited, 1967), 391].

After the introduction of cross-hairs, micrometers, and telescopic sights, all of which are credited to Gascoigne, the precision of measuring arcs with telescopic sights increased to about ¼ minute of arc, and the micrometer allowed measurements of small distances to be precise to a few seconds of arc.[131] Astronomical practitioners determined that the precise measurements of apparent diameters of planets, orbital calculations of comets, and the angles subtended by two celestial objects, were reliable only if one used either a micrometer-mounted telescope or a measuring instrument fitted with telescopic lenses and cross-hairs.[132] Telescopically-mounted measuring instruments were even more precise when micrometers were added, a method that was eventually employed by Flamsteed, Cassini, and Picard.[133] In the case of the micrometer and telescopic sights, the instruments were independently invented by the French (Picard, Auzout) and the English (Gascoigne, Wren, Hooke). But since "the institutionalized stakes of reputation are high and the joy of discovery immense," controversies across national lines inevitably developed.[134]

Conclusions

By 1660, astronomy was entering a new phase of the re-invention of instruments, the accumulation of fresh observations, and a higher level of precision than ever before. In the seventeenth century, the empirical approach of astronomical practitioners that emphasized the use of instrumentation became one of the key ingredients of successful observations and measurements. By the middle of the century, the use of telescopes and open sights alone was found insufficient, hence the invention of instrumental enhancements that increased precision. Combining instruments for measurement and increased precision transformed the discipline of astronomy in the latter half of the seventeenth century.

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[131] Astronomers of the seventeenth century were careful not to place too much weight on the precision afforded by the new instrumentation because although it "gave greater precision, it was not perfect" [C.R. O'Dell and Albert Van Helden, "How Accurate were Seventeenth-Century Measurements of Solar Diameter?" Nature 330 (1987): 630].
[132] 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.
[133] Forbes, "The Comet of 1680-1681," in Thrower, ed., Standing on the Shoulders of Giants, 319.
[134] Robert K. Merton, "Priorities in Scientific Discovery," in The Sociology of Science: Theoretical and Empirical Investigations, ed. by Norman W. Storer (Chicago: The University of Chicago Press, 1973), 314.

Until the mid-seventeenth century, the use of telescopes for astronomical observations did not depend on the theoretical knowledge of how lenses worked. Astronomers discovered the advantages of the "astronomical" telescope only by *using* it and even Kepler who made the first theoretical study of lenses was not initially aware of these advantages. The correspondence of Gascoigne also shows that there was no *a priori* theoretical foundation for the invention of either the micrometer or telescopic sights, and in fact, telescopic sights were discovered serendipitously. Until the middle of the century, astronomical practitioners were not as concerned with the theory behind the use of instruments. This "practice before theory" approach did not last very long, however, and theory caught up with practice in the second half of the seventeenth century, especially in the work of Huygens.

By the 1660's, astronomical instrumentation evolved at a relatively steady pace, but the need to combine instruments for greater precision initiated rapid technological change in the development of astronomical instrumentation that lasted for several decades. Unlike the telescope that "broke into the arsenal of the professional astronomer only gradually,"[135] most astronomers immediately recognized the potential for telescopic sights and the micrometer in the 1660's, despite initial resistance by only a handful of individuals. The first series of concerns did not revolve around who *had* the better micrometers or telescopic sights, but rather, whether or not one *used* micrometers or telescopic sights.

The invention and subsequent use of the telescope alone was sufficient in revolutionizing the discipline of astronomy as astronomers continuously gathered observations and outlined conceptions of the heavens. But the need to measure also captured the attention of astronomical practitioners, and led to the combining of the instrument of discovery (the telescope) with the philosophy of quantifying and measuring nature. Concerns over instrumentation are only part of the larger picture of how astronomy evolved over the course of the sixteenth and seventeenth centuries. In the next chapter, I explore the relationship between physics and mathematics in the universities and the nature of the mathematical and astronomical education of practitioners.

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[135] Van Helden, "The Telescope in the Seventeenth Century," 53.

Chapter 2
The Changing Nature of Astronomical Education and Astronomical Practice from the Sixteenth to Mid-Seventeenth Centuries

The Status of Mathematics in the Universities: Clavius and the Quaestio Debate

Scholars within the universities considered mathematics and natural philosophy separate university subjects up until the seventeenth century, and despite mathematical advancements, the level of mathematical competence expected of university graduates was minimal. The relationship between these two subjects was common throughout Europe, but became a significant issue within the Jesuit educational system when certain scholars began to question the disciplinary boundaries. At the Collegio Romano, the conflict between philosophers and mathematicians over boundaries persisted into the late seventeenth century despite Clavius' pleas for the more serious pursuit of mathematics within the Jesuit colleges.[1] The Jesuits faced a unique dilemma unlike university scholars in the more "progressive" Protestant countries. The mathematical and scientific training of the Jesuits allowed them to appreciate different cosmological systems, but their theological training and general conservatism kept them from accepting what may have been scriptoriously dubious and from disobeying the Pope.[2]

Clavius played a major role in the promotion of mathematical subjects within the Jesuit schools. One of Clavius' biographers noted: "Though lacking the retrospectively seductive aura of a great revolutionary, innovator, or martyr, Clavius was nonetheless a giant in his own time" because his life "allows us to witness the initiation and fruition of the astronomical revolution of

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[1] William A. Wallace, Galileo and His Sources: The Heritage of the Collegio Romano in Galileo's Science (Princeton: Princeton University Press, 1984), 283. The Jesuit curriculum will be discussed in detail later in this chapter.

[2] John L. Russell, "Catholic Astronomers and the Copernican System after the Condemnation of Galileo," Annals of Science 46 (1989): 376. Specifically, many Jesuit astronomers throughout Europe were "deflected. . . from theoretical to observational astronomy" through the Inquisition's influence because of the opposition to Copernicanism (Ibid., 386).

the late sixteenth and early seventeenth centuries."[3] The impressive mathematical textbooks of this self-taught mathematician and founder of Jesuit astronomical studies[4] were eventually incorporated into the standard curriculum of the Jesuit schools' quadrivium.[5] His mathematical textbooks were preceded by his first printed work in 1570 entitled *Commentarius in Sphaeram Joannis de Sacro Bosco,* which "both his own contemporaries and modern historians have judged. . . to be the greatest of all *Sphere* commentaries."[6]

As an astronomer, Clavius was not a careful and systematic observer of the heavens, although he carried out several solar eclipse observations in the late sixteenth century.[7] More importantly, he used the observations of others for his own, more theoretically-oriented work.[8] Throughout his lifetime, Clavius remained a staunch advocate of Ptolemaic astronomy – of the geocentric and geostatic system – and the solidity of the spheres in the heavens.[9] Because he believed that the order of the heavens rested on tradition, he took exception to the new system proposed by Tycho Brahe.[10] In fact, he complained to the Bolognese astronomer Giovanni Magini (1555-1617) that the "Danish astronomer is going to upset the whole of astronomy by his new 'Tychonian' system of the world."[11] Clavius made use of Magini's lesser-known work on a geocentric system, but "in comparison, the nearly complete silence on Brahe is striking."[12] A work on planetary theory is not among Clavius' published texts despite his objections to the

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[3] James M. Lattis, Between Copernicus and Galileo: Christopher Clavius and the Collapse of Ptolemaic Astronomy (Chicago: The University of Chicago Press, 1994), 5.
[4] Lattis, Between Copernicus and Galileo, 4, 17.
[5] Ugo Baldini, "Christoph Clavius and the Scientific Scene in Rome," in Gregorian Reform of the Calendar: Proceedings of the Vatican Conference to Commemorate Its 400th Anniversary 1582-1982, eds. G.V. Coyne, M.A. Hoskin, and O. Pedersen (Specola Vaticana: Pontificia Academia Scientiarum, 1983), 144; Peter Dear, "Jesuit Mathematical Science and the Reconstitution of Experience in the Early Seventeenth Century," Studies in History and Philosophy of Science 18, no. 2 (1987): 136.
[6] Lattis, Between Copernicus and Galileo, 37.
[7] Baldini, "Christoph Clavius and the Scientific Scene in Rome," 144, 153.
[8] Lattis, Between Copernicus and Galileo, 217.
[9] Lattis, Between Copernicus and Galileo, 4, 218.
[10] Michel-Pierre Lerner, "L'Entrée de Tycho Brahe chez les Jésuites ou le chant du Cygne de Clavius," in Les Jésuites à la Renaissance. Système Éducatif et Production du Savoir, ed. Luce Giard (Paris: Presses Universitaires de France, 1995), 150.
[11] Baldini, "Christoph Clavius and the Scientific Scene in Rome," 145. See also Lerner, "L'Entrée de Tycho Brahe chez les Jésuites," in Giard, ed., Les Jésuites à la Renaissance, 151-152.
[12] Lerner adds, "Le nom de l'astronome danois n'apparaît pas dans la liste des auteurs qui se sont illustrés dans la science astronomique – alors que Copernic, Maurolico, et Nuñez y figurent – ni dans celle des inventeurs d'instruments – où l'on trouve Ptolémée, Gemma Frisius et Jean de Rojas" (Lerner, "L'Entrée de Tycho Brahe chez les Jésuites," in Giard, ed., Les Jésuites à la Renaissance, 153-154).

Tychonic system, however, although the main reason he did not publish on this subject was because he felt that such a work required a solid observational programme.[13]

As a mathematician, Clavius believed in the certainty of mathematics and promoted the subject within the Jesuit schools:

> the preeminence of mathematics also derives from the certitude of its demonstrations, which remove all doubt and give birth to true science in the minds of its practitioners, and this can hardly be said of any other discipline, in most of which there are continual disputes and arguments that leave the students uncertain and in doubt.[14]

Moreover, he ridiculed philosophers who "take their inspirations from Aristotle," a slight against the rote memorization of philosophy still used in schools throughout Europe. Moreover, by arguing that natural philosophy could not be understood without mathematics, Clavius knew he was intensifying the debate over the status of mathematics – a constant concern in the Jesuit program of study.[15] Ennobling the intellectual status of the mathematical disciplines was significant because "it provided a basis for the treatment of aspects in the natural world that would be on equal terms with Aristotelian natural philosophy" and "hence on an equal methodological footing with physics."[16]

In his *Promotion of Mathematics* (1586), Clavius claimed that students' indifference to the mathematical disciplines suggested that mathematics did not hold a preeminent position within the schools. But if students were to see the "professor of mathematics taking part in [ceremonial] proceedings," they would be "convinced that philosophy and the mathematical sciences are connected," thereby sparking interest in the subject.[17] Clavius' own view is clarified in the following passage:

> Natural philosophy without the mathematical disciplines is lame and incomplete. . . . these sciences and natural philosophy have so close an affinity with one another that unless they give each other mutual aid they

[13] Baldini, "Christoph Clavius and the Scientific Scene in Rome," 145. Not all Jesuits sided with Clavius' traditional system, even if they admired his promotion of mathematical studies. Individuals included Schiener, Biancani, and Riccioli (Lerner, "L'Entrée de Tycho Brahe chez les Jésuites," in Giard, ed., Les Jésuites à la Renaissance, 164-184).
[14] Wallace, Galileo and His Sources, 138-139.
[15] Antonella Romano, La Contre-Réforme Mathématique: Constitution et Diffusion d'une Culture Mathématique Jésuite à la Renaissance (1540-1640) (Rome: École Française de Rome, 1999), 178.
[16] Dear, "Jesuit Mathematical Science," 137.
[17] Christopher Clavius, *The Promotion of Mathematics* (1586), in Descartes' *Meditations*: Background Source Materials, ed. and trans. by Roger Ariew, John Cottingham, and Tom Sorell (Cambridge: Cambridge University Press, 1998), 25.

> can in no way preserve their own status. For this to happen, it will be necessary first that physics students should study mathematical disciplines at the same time – a habit that has always been retained in the society's schools thus far. For if these sciences were taught at a different time, philosophy students would think, and understandably so, that they were in no way necessary to physics.[18]

Therefore, Clavius believed that philosophers should also be educated in mathematics since both mathematics and physics must co-exist and be taught at the same time. Moreover, one way of encouraging mathematics among students, Clavius argued, is by holding "individual conversations" between teachers and students in which "teachers would encourage students to learn mathematics, impressing on them its usefulness, and not doing the opposite, as many have done in the past." Mathematics could be inculcated even further "if once a month all the philosophers are assembled in one place, where a student would read a short appreciation of the mathematical disciplines, and then with one or two others would explain a problem from geometry or astronomy. . . ." Ultimately, candidates for the master or doctor degree "should be examined in mathematical matters," where a "mathematics professor be present with the other professors of philosophy."[19]

The "School of Clavius," based on Clavius' promotion of the mathematical disciplines was not an immediate "success," because "his teaching bore very little fruit in the form of students dedicating themselves to mathematics," and "his influence seems to have become felt at a point of time when he himself gave up his teaching, i.e. on his return from Naples."[20] Nevertheless, in the generation following Clavius, the Jesuit mathematical program produced mathematicians of the highest caliber who succeeded in turning "singular experiences" and "discrete events, into properly accredited knowledge about the natural world."[21] These disciples may not have advocated Clavius' Ptolemaic and Aristotelian cosmology, but like Clavius, they too believed in ennobling mathematical studies, stressing "the indebtedness of physics to the new 'mathematical' discoveries and demonstrations."[22]

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[18] Clavius, *Promotion*, in Ariew, Cottingham, and Sorell, eds., Descartes' *Meditations*, 26.
[19] Clavius, *Promotion*, in Ariew, Cottingham, and Sorell, eds., Descartes' *Meditations*, 27-28.
[20] Baldini, "Christoph Clavius and the Scientific Scene in Rome," 143.
[21] Dear, "Jesuit Mathematical Science," 173. This made challenges and controversies possible because it gave practitioners something to respond to (Ibid., 174).
[22] Dear, "Jesuit Mathematical Science," 166. Scheiner pointed this out in his *Rosa ursina* (Ibid.).

Clavius' attempt to raise the status of Jesuit mathematical studies was a sign of the debate over the nature of the traditional division between mathematics and physics, or natural philosophy. Pure mathematics usually included arithmetic and geometry, while the "mixed" subjects included astronomy, optics, and music. But all mathematical subjects fell under a "branch of physics that dealt with the natural body in abstracto, that is, without reference to its form or position," and as such, these subjects were more descriptive than causal.[23] Mathematicians could describe nature or natural processes, but "to do so required a constant policing of the disciplinary boundary between mathematics and physics."[24] Identifying causes fell outside the purview of mathematicians, but the boundary between disciplines actually protected the solidity of demonstrations within the mixed mathematical disciplines.[25] Nonetheless, the deep-seated prejudice against mathematical subjects often led teachers in other faculties to treat both the discipline and the mathematics teachers with a lack of respect.[26]

Among the factors that led to tensions over disciplinary boundaries between mathematics and natural philosophy was the debate referred to as the *Quaestio de certitudine mathematicarum* that began with the publication of a treatise by Alessandro Piccolomini (1508-1578) in 1547.[27] The two fundamental issues raised in this debate revolved around two questions: "What is the relationship between Aristotelian logic and Euclidean mathematics?" and "If mathematics does not derive its certainty by the form of its demonstrations, how are we to justify its certainty and evidence?"[28] Eventually, the *Quaestio* debate spread into other parts of Europe, including England, and chronologically lasted into the late seventeenth century. Three Englishmen in particular who investigated the *Quaestio* debate were John Wallis (1616-1703), Thomas Hobbes (1588-1679), and Isaac Barrow (1630-1677). Wallis' position (summarized) was that "some mathematical proofs are causal;" whereas Hobbes believed that "all demonstrations in

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[23] L.W.B. Brockliss, French Higher Education in the Seventeenth and Eighteenth Centuries: A Cultural History (Oxford: Clarendon Press, 1987), 381. For a general review of categories of disciplines during the Reformation era, see Joseph S. Freedman, "Philosophy Instruction within the Institutional Framework of Central European Schools and Universities during the Reformation Era," History of Universities 5 (1985): 117-166.
[24] Dear, "Jesuit Mathematical Science," 161.
[25] Dear, "Jesuit Mathematical Science," 163.
[26] Lattis, Between Copernicus and Galileo, 33.
[27] Paolo Mancosu, "Aristotelian Logic and Euclidean Mathematics: Seventeenth-Century Developments of the *Quaestio de Certitudine Mathematicarum*," Studies in History and Philosophy of Science 23 (1992): 243.
[28] Mancosu, "Aristotelian Logic and Euclidean Mathematics," 242. In Mancosu's book, Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century (New York and Oxford: Oxford University Press, 1996), 10, the questions are: "Does mathematics fit the definition of Aristotelian science or does it fall short of it?" and "If the certainty of mathematics cannot be argued by appealing to its logical structure on what grounds can we justify it?"

mathematics are causal."[29] Barrow not only knew his sources better than Wallis, he also agreed with Hobbes that all mathematical demonstrations are causal.[30]

Preoccupation with the *Quaestio* debate into the late seventeenth century did not preclude philosophers from tackling more advanced questions in geometry and from recognizing that mathematics was undergoing significant transformations.[31] Certainly, the shift had already been made from Aristotelian logic as the language of science to mathematics as the new paradigm of science.[32] In the earlier part of the seventeenth century, Galileo and Kepler influenced the direction that the *Quaestio* would take during the course of the seventeenth century. They rejected and readjusted the disciplinary boundaries, and the "physico-mathematician" was born.[33]

Astronomy as a Discipline and the Curriculum in the Universities of France, "Middle" Europe, and England

Since astronomy was part of "mixed" mathematics, issues pertaining to the status of mathematics and its relationship to natural philosophy also applied to astronomy. Astronomy was "mixed" because it combined number with the subject matter – that is, "astronomy was geometrical extension as specifically applied to the motions of the heavens."[34] But because astronomical practitioners were not supposed to ask questions related to causes in the heavens, they could only describe appearances and design models that accounted for the apparent motions of the celestial bodies. Moreover, like the work of mathematicians in general, astronomical computations and treatises were not as highly regarded as the work of the "physicists."[35] This

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[29] Mancosu, "Aristotelian Logic and Euclidean Mathematics," 254, 257.
[30] Mancosu, "Aristotelian Logic and Euclidean Mathematics," 258, 262. Mancosu concludes that Barrow's intentions for continuing a debate that should have ended by the late seventeenth century were to refute Gassendi who believed that the sciences could not provide causal (Aristotelian) knowledge (Ibid., 263). Mancosu adds that the "Aristotelian epistemological framework was pervasive in the seventeenth century and very influential indeed in later centuries," and "the attachment to the Aristotelian conception of science provides these mathematicians with a further stimulus to develop new and more direct approaches to mathematics, and to argue at the foundational level for their superiority in comparison to other approaches" (Mancosu, Philosophy of Mathematics and Mathematical Practice, 92, 93).
[31] Mancosu, Philosophy of Mathematics and Mathematical Practice, 4-5.
[32] Mancosu, "Aristotelian Logic and Euclidean Mathematics," 255.
[33] See the works by Peter Dear, Discipline & Experience, 168-179; and Revolutionizing the Sciences: European Knowledge and Its Ambitions, 1500-1700 (Princeton: Princeton University Press, 2001), 72-77.
[34] Dear, Revolutionizing the Sciences, 18.
[35] Dear, Revolutionizing the Sciences, 21.

was because of the traditional hierarchy of the disciplines that had astronomy subordinated to natural philosophy that was, in turn, subordinated to theology.

Certain astronomical practitioners were not daunted by such divisions, however, and attempted to answer questions pertaining to the true workings of the heavens; this threat of encroachment of astronomy into the domain of natural philosophy began as early as 1500.[36] Copernicus' *De revolutionibus* was such an attempt to cross over from "*a posteriori*" astronomy into "*a priori*" astronomy.[37] The famous Osiander preface was supposed to remind the reader, however, that the work was "*a posteriori*," and by doing this, any objections relating to its "incompatibility with accepted physics and theology" were supposed to be quelled. The traditional division between natural philosophy and astronomy was also supposed to deny "Copernicus's real aspirations to provide causal explanations where none had appeared before."[38] However, the growing number of Copernicans – those who accepted Copernicus' system as one that could provide *a priori* proofs in astronomy[39] – made it increasingly difficult for others to read *De revolutionibus* as an *a posteriori* work alone. Eventually, the status of astronomy (along with other mathematical sciences) was raised not because astronomical practitioners were able to prove that astronomy was on the same footing as natural philosophy, but rather, because of the advances made by the works of Copernicus, Galileo, and Kepler that "allowed the whole arid argument to dry up and blow away."[40] Nevertheless, there were vestiges of it left behind such as the *Quaestio* debate of the late seventeenth century in England.[41]

The creation of university mathematical chairs began in the late sixteenth century in England, France, the Netherlands, and to some extent, Italy. In England, chairs in Astronomy and Geometry were established at Gresham College in 1597. Clavius' promotion of mathematics inspired the founding of several mathematical chairs in France. Among the twenty-six French Jesuit colleges that established mathematical professorships in the seventeenth century, most

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[36] Dear, Revolutionizing the Sciences, 24.
[37] See Peter Barker, "The Role of Religion in the Lutheran Response to Copernicus," in Rethinking the Scientific Revolution, ed. Margaret J. Osler (Cambridge: Cambridge University Press, 2000), 72-88.
[38] Barker, "The Role of Religion," in Osler, ed., Rethinking the Scientific Revolution, 79.
[39] Barker, "The Role of Religion," in Osler, ed., Rethinking the Scientific Revolution, 81.
[40] Lattis, Between Copernicus and Galileo, 35.
[41] See Mancosu's article, "Aristotelian Logic and Euclidean Mathematics: Seventeenth-Century Developments of the *Quaestio de Certitudine Mathematicarum*."

were secured at the end of the century.[42] In the Netherlands, Prince Maurice of Orange established a mathematical chair in 1600 for training engineers and military officers in mathematics. Finally, the Collegio Romano in Rome benefited from Clavius' presence, but the reception of mathematics at other Italian universities, such as Pisa and Padua, was "mixed."[43]

The Jesuits: Italy and France

The only two institutions in Rome where the mathematical sciences were "taught and transmitted" in the sixteenth century were the University of Rome (La Sapienza) and the university of the Jesuits (the Collegio Romano), both of which had mathematical chairs.[44] But it was the Collegio Romano and the Jesuits who played the larger role in the teaching and transmission of that knowledge, in large part because of Clavius. If the teaching of the sciences seemed minimal in the Jesuit schools, we must remember that it "was conditioned by social pressures and biases" towards the subject matter, and it "must be understood in the context of what society of the time conceived as proper for the formation of the gentleman."[45] More specifically, public pressures were the reason that educators promoted literary courses over science courses for the appropriate education of students.[46] Nonetheless, there was a concerted effort by authorities within the Jesuit order to integrate the scientific disciplines, particularly mathematics, into a common shared culture of education.[47]

The curriculum for Jesuit colleges, established by the *Ratio studiorum* (1599), stressed the importance of mathematics (which various colleges emphasized to various degrees), and the "only real constraint appears to have been an insufficient number of competent teachers to go around."[48] According to the *Ratio*, students were expected to sit through forty-five minutes of mathematics every day and even more in the case of private tutors who were assigned to

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[42] Brockliss, French Higher Education, 386. Pont-à-Mousson was the only French Jesuit school to create a chair in mathematics (1592) before 1600. For a list of French Jesuit colleges and their dates for establishing mathematical chairs (1592-1637) see Romano, La Contre-Réforme Mathématique, 366.
[43] Robert S. Westman, "Humanism and Scientific Roles in the Sixteenth Century," in Humanismus und Naturwissenschaften, eds. Rudolph Schmitz and Fritz Krafft (Boppard: Harald Boldt Verlag, 1980), 94.
[44] Baldini, "Christoph Clavius and the Scientific Scene in Rome," 141.
[45] Aldo Scaglione, The Liberal Arts and the Jesuit College System (Amsterdam/Philadelphia: John Benjamins Publishing Company, 1986), 87.
[46] Scaglione, The Liberal Arts and the Jesuit College System, 87-88.
[47] Romano, La Contre-Réforme Mathématique, 522.
[48] Dear, Discipline & Experience, 33.

exceptional students.[49] At La Sapienza, mathematics and astronomy was at the elementary level, and Sacrobosco's *Sphaera* was the textbook of choice in the poorly attended astronomy lectures because his version was more popular and appealing to the average student (due in large part to its disregard of more technical details).[50]

The Collegio Romano fared only slightly better until the beginning of the seventeenth century when the effect of Clavius' contributions to the field of mathematics were more fully realized. By then, the antiquated hierarchy of the trivium and quadrivium was undergoing momentous transformations as were the mathematical subjects that "had achieved something like the status Clavius had envisioned for them."[51] Because of these transformations, the mathematician assumed several roles: "The 'mathematician' is not only a professor whose teaching we will study later, but is often a scientist and sometimes a practitioner."[52] Sadly, the originality and creativity experienced by the Jesuits at the Collegio Romano abated after 1640 – it was not what it had been in its first few generations. This decline in originality and creativity was perhaps a European-wide trend.[53]

The main emphasis in the French universities was Aristotelian until the late sixteenth century. The natural sciences, philosophy, logic, and all other subjects were thoroughly Aristotelian and students and teachers frequently used Aristotle's works. From the ten or so collèges de plein exercise and the Collège Royal (founded by François I) at the University of Paris, there was not a large group of individuals interested in pursuing the study of natural philosophy to more advanced levels even in the early seventeenth century. In fact, France is an example of the limits of the Jesuit mathematical project, and as a result, was not on the cutting

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[49] Scaglione, The Liberal Arts and the Jesuit College System, 87.

[50] Baldini, "Christoph Clavius and the Scientific Scene in Rome," 141, 142. Versions of the *De Sphaera* included general descriptions of spheres and circles and were written by a number of individuals. Other thirteenth-century authors of the *Sphaera* included Robert Grosseteste (~1215) who emphasized the importance and role of the *De Sphaera* for knowing natural philosophy, and John Pecham (late 1270's) who was motivated to correct the errors of Sacrobosco. In his version of the *Sphaera*, Pecham addressed the difference between mathematical astronomy and Aristotelian natural philosophy and the issue concerning the veracity of astronomical hypotheses [See Stephen C. McCluskey, Astronomies and Cultures in Early Medieval Europe (Cambridge: Cambridge University Press, 1998), 193-198, on the differences between various versions of the *De sphaera*].

[51] Lattis, Between Copernicus and Galileo, 37.

[52] "Le <mathématicien> n'est pas seulement un professeur, dont nous aurons à étudier ultérieurement l'enseignement, il est souvent un savant et parfois un praticien" [François de Dainville, L'Éducation des Jésuites (XVIe-XVIIIe siècles), textes rèunis et présentés par Marie-Madeleine Compère (Paris: Les Éditions de Minuit, 1978), 333].

[53] Scaglione, The Liberal Arts and the Jesuit College System, 82. See also de Dainville, L'Éducation des Jésuites.

edge of the Scientific Revolution at the beginning of the seventeenth century.[54] Despite the deeply-embedded Aristotelianism, however, there were a handful of courses that covered alternative theories in French universities before 1640. In Paris, professors were required to lecture on Euclid and the *Sphere*, but even such requirements could not keep mathematical instruction from being minimal and at times, even non-existent.[55] Despite the basic mathematical instruction in French schools, there were a growing number of chairs in mathematics throughout France in the early seventeenth century, including a chair at the Collège Royal in Paris occupied by Gassendi.[56] After 1640, professors noted the various advances in astronomy much more frequently.[57]

Because a majority of faculty in the schools were not interested in the more practical applications of mathematics and astronomy, most interests in experimental philosophy in the seventeenth century had to be pursued largely outside the university walls. In fact, during the course of the seventeenth century, more experimental philosophers interested in the data and information-gathering aspects of the sciences belonged outside of academic walls and many did not even have the educational background or "professional" preparation for the work they pursued. Consequently, there were a large number of practitioners "floating" outside the universities who occasionally came together to form small (and usually unstable) scientifically-select groups.[58] In astronomy, some of these groups were "schools" of thought.

Between 1610 and 1667, three main schools of astronomical thought flourished throughout France that are characterized as the "Provincial" school, the "Parisian" school, and the "Jesuit" school.[59] The Provincial school, referred to as "un foyer de découvertes," had as its advocates Joseph Gaultier and Nicolas de Peiresc (1580-1637) at Aix in Provence. Their group was particularly interested in the application of the telescope and in the discovery of new

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[54] Romano, La Contre-Réforme Mathématique, 524.
[55] Brockliss, French Higher Education, 381.
[56] See Romano, La Contre-Réforme Mathématique, 366. Dates for the creation of mathematical chairs include: Toulouse (1604), Paris (1620), Avignon (1600), La Flèche (1608), and Aix (1637). Curiously, colleges founded in the sixteenth century took anywhere from thirty-two to fifty-seven years to establish mathematical chairs (based on Romano's list of eleven colleges). French colleges founded in the seventeenth century took only five (La Flèche) to fourteen years.
[57] Brockliss, French Higher Education, 340.
[58] Brockliss, "The Scientific Revolution in France," in The Scientific Revolution in National Context, eds. Roy Porter and Mikulás Teich (Cambridge: Cambridge University Press, 1992), 61. These circles will be discussed in the next chapter. Brockliss claims that Jesuits actually supported many of the growing number of experimental philosophers in the mid-seventeenth century (Ibid., 60).

phenomena and previously unknown celestial objects. Their influence was considerable in that they communicated the results of their observations and theories to Galileo and other European astronomers, and they "established observational astronomy as a major tradition in French science, leading to the building of the Paris Observatory in 1667 and the arrival of Cassini to be its director."[60] The group was subsequently dissolved after the departure of two of its most esteemed members – Peiresc died in 1637 and Gassendi left for Paris soon after. Unlike the more practical interests of the Provincial school, the Parisian school was more absorbed by the theoretical issues of optics, hence their description as "une réunion de théoriciens." Comprised mainly of mathematicians and mechanists, this circle was inspired more by Descartes' *La Dioptrique* (1637) than it was with actual observations. The group, which included Giles Persone de Roberval (1602-1675), Etienne Pascal (1588-1651), and Bernard Frénicle de Bessy (c. 1605-1675), usually held its meetings at Mersenne's house.

The third and final group was the Jesuits, described as "un foyer d'enseignement" and consigned and committed primarily to instruction of mathematical and astronomical thought in the schools. They also oversaw the publication of a large number of astronomical (and related) works and they were active both in Paris and the provinces. In Paris, members included Fathers Bertet, Deriennes and Jean-Baptiste Régis (1663/4-1738); working in the provinces were Pierre Anthelme (1598-1668), Gabriel Mouton (1618-1694) and Vernier. It should be noted, however, that the boundaries of these schools were more fluid than has been presented here, especially the practical astronomical interests of the Provincial school and the theoretical interests of the Parisian school. This categorization of astronomical schools of thought does not suggest that there was no discovery among the Parisians, nor theory among the provincial astronomers.[61]

Regardless of where Jesuit mathematicians and astronomers worked, the new systems and observations from other parts of Europe infiltrated their current knowledge of the heavens. Attitudes towards Copernicanism were not entirely hostile, yet from the late sixteenth century to

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[59] The names of the "schools" as well as their various attributes are borrowed from Eric G. Forbes, Greenwich Observatory, Vol. 1, Origins and Early History (1675-1835), (London: Taylor and Francis Limited, 1975), 4-5.
[60] Frederic J. Baumgartner, "The Origins of the Provençal School of Astronomy," Physis: Rivista Internazionale di Storia della Scienza 28 (1991): 303. Unfortunately, the creation of the Paris Observatory also moved the center of astronomy from Provence to Paris (Ibid.).
[61] Pierre Humbert, L'Astronomie en France au Dix-Septième Siècle (Paris: University of Paris, 1952), 7. According to Humbert, the end of these schools came about on June 21, 1667, the day of the summer solstice, when, astronomers at the Académie des Sciences (including Auzout, Frénicle, Picard, and Richter) traced out and determined the meridian of the future Paris Observatory using two large sextants. Astronomy was no longer an activity for amateurs (Ibid., 14).

the mid-seventeenth, Copernican cosmology was largely ignored. Mathematicians and astronomers did not necessarily have to choose between Copernicanism and the Ptolemaic-Aristotelian world-view – there were more attractive alternatives. Moreover, Galileo's observations did not necessarily lead astronomers to abandon the Ptolemaic-Aristotelian world-view. For instance, observations of the phases of Venus were not decisive evidence against Ptolemaic cosmology and were even compatible within Aristotelian cosmology.[62] Likewise, Tycho's work on comets "produced no decisive change in theories of the nature of comets."[63] If comets caused any controversy for Aristotelianism, it would have concerned their constitution rather than their location.[64] Ultimately, the Aristotelians were "able to absorb and assimilate all invaders," and "astronomical observations, even the novel ones brought forth during the seventeenth century, did not deal a decisive blow against the so-called Aristotelian-Ptolemaic world-view."[65]

Astronomers were not limited to two choices. There were ways of assimilating novel observations into the established Aristotelian cosmology, and there were more appealing alternatives to Copernicanism such as the Tychonic system with its palatable geocentrism. The stages in which the Jesuits accepted the Tychonic system between 1590 and 1620 are not well-known, however.[66] Nevertheless, there were certain Jesuits who rejected the Magini-Clavius system in favor of Tycho's, and between 1620 (the date of Biancani's *Sphaera*) and 1651 (when Riccioli's *Almagestum nova* appeared), Tycho's system was well-received: "Riccioli écrit que la majorité des jésuites ont choisi ce système dans leurs traités *De cælo*."[67] The situation in France slightly differed from Italy in that the French were more likely to "turn their backs" on the "new" Copernican astronomy and were less interested in grand cosmological systems.[68]

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[62] Roger Ariew, "The Phases of Venus before 1610," Studies in History and Philosophy of Science 18 (1987): 82, 86; and Peter Barker and Roger Ariew, eds., Revolution and Continuity: Essays in the History and Philosophy of Early Modern Science (Washington, D.C.: The Catholic University of America Press, 1991), 7.
[63] Barker and Ariew, eds., Revolution and Continuity, 8. See also Ariew, "Theory of Comets at Paris during the Seventeenth Century," Journal of the History of Ideas 53 (1992): 355-372.
[64] Barker and Ariew, eds., Revolution and Continuity, 9.
[65] Ariew, "Theory of Comets at Paris," 355, 369.
[66] Lerner, "L'Entrée de Tycho Brahe chez les Jésuites," in Giard, ed., Les Jésuites à la Renaissance, 156.
[67] Lerner, "L'Entrée de Tycho Brahe chez les Jésuites," in Giard, ed., Les Jésuites à la Renaissance, 164, 178. Lerner concludes that Riccioli and the other Jesuits who welcomed these changes demonstrate the transformations undergone by the Tychonic system some fifty years after its introduction to the Jesuit order (Ibid., 183-184).
[68] Romano, La Contre-Réforme Mathématique, 524.

In spite of the competition amid systems, between 1640 and 1690, the hostility toward Copernicanism lessened significantly among the Jesuits in French schools.[69] Moreover, French astronomers Gassendi and Ismael Boulliau (1605-1694) promoted Copernicanism. After 1690, seculars did not find it so problematic to interpret the Bible metaphorically, only adding to a more general toleration, if not acceptance, of Copernicanism's central doctrine of a moving earth and stable sun. Notwithstanding the increased secular toleration (and even acceptance) of Copernicanism throughout the seventeenth century, Jesuits generally remained Tychonians even after 1690.[70]

"Middle" Europe

The Jesuit order was established as part of the Counter-Reformation, but it was the Reformation, particularly in the German lands that saw to the creation of a large number of Lutheran schools and the rise in the number of Lutheran natural philosophers and astronomers.[71] However, the willingness to either accept or reject new cosmological systems needs to be understood in light of the fact that Lutheran astronomers believed themselves to be Lutherans first, humanists second, and astronomers last.[72] Before turning to the Lutheran universities, it is necessary to discuss briefly mathematics and astronomy at the University of Cracow.

In Poland, the University of Cracow was a dominant intellectual force early on, although schooling in Danzig was also on the rise in the early seventeenth century. In most cases, students who matriculated to the universities and schools were from the class of burghers who wanted to raise their social status.[73] At the University of Cracow, which was founded in 1364 by King Casimir the Great, a special chair in mathematics and astronomy was established much earlier than most other universities. In 1405, a private citizen in Cracow by the name of Nicholas Stobner, founded this special chair with his specific wish "that there be lectures on

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[69] Brockliss, "Copernicus," in New Perspectives on Renaissance Thought: Essays in the History of Science, Education and Philosophy in Memory of Charles B. Schmitt, eds. John Henry and Sarah Sutton (London: Duckworth, 1990), 192-193.
[70] Brockliss, "Copernicus," in Henry and Sutton, eds., New Perspectives on Renaissance Thought, 195-196. According to Brockliss, University of Paris teachers of natural philosophy were not aware of Galileo's astronomical discoveries as found in his *Siderius Nuncius* (1610) for at least two decades after its publication (Brockliss, "The Scientific Revolution in France," in Porter and Teich, eds., The Scientific Revolution, 59).
[71] In fact, as Barker points out, "the Reformation began in universities" (Barker, "The Role of Religion," in Osler, ed., Rethinking the Scientific Revolution, 65).
[72] Barker, "The Role of Religion," in Osler, ed., Rethinking the Scientific Revolution, 86.

mathematics, theoretical astronomy, and that the almanac be kept current."[74] A chair in astronomy and astrology was not founded at Cracow until 1659 by Marcin Król of Zurawica.

By the late fifteenth century, tensions over disciplinary boundaries surfaced at Cracow. Questionable issues that revolved around the organization of knowledge included the classification of natural philosophy as either a theoretical or a practical science and its role within the curriculum. Over time, however, natural philosophers moved away from the more Aristotelian theoretical view towards a more practical, non-abstract view. This resulted in an arts curriculum that, in the late fifteenth century, appeared Aristotelian, but whose "content was increasingly designed to oppose ancient authority. . . ."[75] Philosophical study at Cracow included Aristotle's *De caelo* and astronomical lectures were based on John Pecham's (c. 1230-1292) optical study, *Perspectiva communis*, and the *Theorica planetarum* (Gerard of Cremona [c. 1114-1187] and Johannes de Sacrobosco's [c. 1195-1256] versions) that was a simple geometrical description of epicyclic models of planetary motion. Besides geometrical models of the motions of the Sun, moon, and planets, the *Theorica* usually included the definitions of technical terms, geometrical explanations of Ptolemaic astronomical tables, and instructions for computing positions of the heavenly bodies using tables.[76]

In Danzig, educators also made efforts to upgrade the education of the schools. Danzig already had a cultural and scientific milieu in place by the 1630's, and the quality of schooling only improved with the establishment of the *Gymnasium Academicum* in 1643. Peter Krüger, who eventually became Hevelius' teacher, was one of the chief professors of mathematics in Danzig at the time, although his work was mainly in astronomy, a subject that was not a part of the official curriculum of the *Gymnasium*.[77]

Mathematics and astronomy education was more prominent elsewhere in Europe especially at Copenhagen, Wittenberg, and Tübingen mainly because of the efforts of Luther's

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[73] Jerzy Dobrzycki, "The Scientific Revolution in Poland," in Porter and Teich, eds., The Scientific Revolution, 152.
[74] Knoll, "Arts," in The Copernican Achievement, ed. Robert S. Westman (Berkeley, CA: University of California Press, 1975), 144-145.
[75] Knoll, "Arts," in Westman, ed., Copernican Achievement, 150-151.
[76] Perhaps the most innovative of all the *Theoricas* was that of the German astronomer Georg Peurbach (1423-1461) who replaced the geometrical lines of the diagrams with three-dimensional solid spheres, thus interpreting "his mathematical devices as having physical referents" (Dear, Revolutionizing the Sciences, 22). Astronomical tables, which stood separately from the *Theorica*, became a part of the curriculum of the universities when they were added to the astronomical corpus in the fourteenth century.

chief lieutenant, Philip Melanchthon (1497-1560). Melanchthon was instrumental in the formation of "Philippism," which "was based on the humanist ideal of a well-rounded education and consisted of a reciprocal 'world of learning' or *orbis litterarum*, in which comprehension of each part depended upon mastery of the whole."[78] Melanchthon may have been a humanist first and a Lutheran second, but in promoting the astronomical disciplines in the Lutheran universities, he paved the way for the spread of Copernicus' ideas.[79]

Melanchthon's own interest in mathematics as a student was instrumental in shaping his views on the proper role of mathematics and astronomy within the universities. Early in his career, he believed that one could learn more from mathematics than from Aristotelian natural philosophy; by 1521, "he proposed replacing lectures on Aristotle's *Physica* with a course on mathematics;" and by the early 1530's, he was "an enthusiastic promoter of the mathematical sciences."[80] Astronomy was the highest of the mathematical subjects, according to Melanchthon, because it showed the will of God – without it, philosophy was "maimed and mutilated."[81] However, some students equated the "theological justification of astronomy" with the "theological justification for the exact study of the heavens which could legitimate the overthrow of traditional Aristotelian and Ptolemaic cosmology."[82] Melanchthon argued this was going too far.

Melanchthon had a "circle" that gathered around him and included Reinhold, Peucer, and Rheticus. This "informal circle of scholars [was] drawn together under [his] leadership. . . [and] it evolved *within* the walls of the university."[83] The group, albeit informal, had a significant impact on the reception of Copernicanism in German universities. On this point, Westman argues:

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[77] Marian Pelczar, "La Position et le Rôle de Gdansk dans la Science du XVIe au XVIIIe Siècle," in XIIe Congrès International d'Histoire des Sciences, Paris 1968: Tome XI Sciences et Sociétés, Relations – Influences – Écoles (Paris: Librarie Scientifique et Technique Albert Blanchard, 1971), 107-108.
[78] Christianson, On Tycho's Island, 15.
[79] Barker, "The Role of Religion," in Osler, ed., Rethinking the Scientific Revolution, 65. See also Robert S. Westman, "The Melanchthon Circle, Rheticus, and the Wittenberg Interpretation of the Copernican Theory," Isis 66 (1975): 164-193.
[80] Charlotte Methuen, "The Role of the Heavens in the Thought of Philip Melanchthon," Journal of the History of Ideas 57 (1996): 387.
[81] Methuen, "The Role of the Heavens," 393, 394. Methuen adds, "Astronomy thus becomes the vital first step towards understanding the order which God intended for the world, and a cornerstone for Melanchthon's ethical authority" (Ibid., 402). See also Barker, "The Role of Religion," in Osler, ed., Rethinking the Scientific Revolution, 60-62.
[82] Methuen, "The Role of the Heavens," 403.
[83] Westman, "The Melanchthon Circle," 167.

> Melanchthon and his disciples yet exercised considerable influence on the discipline of astronomy by staffing many of the leading German universities with their pupils and by writing the textbooks that were used in those institutions. The effect of this informal scientific group on the early reception of the Copernican theory cannot be underestimated.[84]

This implies that first, informal circles could still exercise a considerable amount of influence – formal and official academies were not the only organizations with power. Second, since the Melanchthon circle existed within university walls, this supports the idea that the universities were not entirely backwards or conservative.

Interest in astronomy was pervasive at the University of Copenhagen since the sixteenth century. The subjects found within the Master of Arts program including arithmetic, geometry, and astronomy, were neither unpopular nor marginalized by students. Indeed, "students in Copenhagen and other Philippist universities learned to consider mathematics and astronomy as mainstream subjects that every educated person should master, rather than as tedious disciplines accessible only to specialists."[85] This knowledge could then be used for calendar reform and the description of astrological influences. Melanchthon's ideals were eventually integrated into other Lutheran universities, and by the end of the sixteenth century, a professorial chair (mostly in astronomical studies) was already in place at Copenhagen.[86]

The Universities of Tübingen and Wittenberg were relatively stronger in mathematical and astronomical studies than Copenhagen, and it was at these universities where Melanchthon spent a greater part of his life – he was a student at Tübingen and a teacher at Wittenberg. Between the two, Wittenberg served as the "flagship of the newly-Lutheran universities of mid-century, acting as something of a model for others."[87] However, Tübingen was especially advanced when compared to other Lutheran universities, and even more advanced when compared to non-German universities.[88] Some of the astronomical texts used between the two universities were practically identical, such as those by Maestlin and Peter Apian (Apianus)

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[84] Westman, "The Melanchthon Circle," 168.
[85] Christianson, <u>On Tycho's Island</u>, 16.
[86] Thoren, <u>Lord of Uraniborg</u>, 11.
[87] Dear, <u>Revolutionizing the Sciences</u>, 44.
[88] Richard A. Jarrell, "Astronomy at the University of Tübingen: The Work of Michael Mästlin," in <u>Wissenschaftsgeschichte um Wilhelm Schickard</u>, ed. by Friedrich Seck [Tübingen: J.C.B. Mohr (Paul Siebeck), 1981], 9.

(1495-1552) at Tübingen, and included Proclus' *De sphaera*, Peurbach's *Theoricae Novae Planetarum*, Sebastian Theodoric of Winsheim's *De sphaera*, and Apianus' *Cosmographia*.[89]

Unlike the Jesuit mathematicians, the Lutheran mathematicians (particularly those under Melanchthon) had fewer difficulties in assimilating the Copernican system into their current knowledge of the heavens, but in most cases, Copernicus' central thesis was rejected. The principal tenant of their interpretation of Copernicanism – referred to as the "Wittenberg Interpretation" – was that the "new theory could only be trusted within the domain where it made predictions about the angular position of a planet."[90] Some members of the group, however, accepted certain Copernican models, and a very small group accepted the thesis that the earth moved and with more than one motion.[91] In addition to Melanchthon, another "Wittenberg astronomer" was Rheticus (a Copernican), who taught arithmetic and astronomy at Wittenberg and published the *Narratio prima* in 1540, a non-mathematical version of Copernican astronomy. Reinhold, a member of the group who did not accept the Copernican system, used Copernican models nonetheless, and set out to produce new celestial tables from them.[92]

The success of the Melanchthon circle seems to have been the exception rather than the rule. In the beginning of the seventeenth century, the Thirty Years' War created both division and decline throughout the German countryside that made it impossible to create an official academy. More specifically, "territorial boundaries were hardened by a firmer drawing of the religious map, which immediately affected the universities."[93] German culture was significantly affected by the collapse of humanism that had once been the pride of German intellectualism, and the closing off of borders throughout Germany made communication between Germans (let alone communication between Germans and foreigners) extremely difficult. Therefore, according to Evans, "in place of the easy Respublica Litteraria which Humanism had created, the next generations sought painfully to re-establish a minimum of intellectual intercourse both within the Reich and – no less important – beyond it."[94] In some cases, individual scholars emigrated to other countries. For example, Samuel Hartlib (d. 1662) and Theodore Haak (1605-

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[89] Westman, "Three," in Westman, ed., Copernican Achievement, 330.
[90] Westman, "The Melanchthon Circle," 166.
[91] Westman, "The Melanchthon Circle," 166-167.
[92] Barker, "The Role of Religion," in Osler, ed., Rethinking the Scientific Revolution, 67-68.
[93] R.J.W. Evans, "Learned Societies in Germany in the Seventeenth Century," European Studies Review 7 (1977): 130.
[94] Evans, "Learned Societies in Germany," 133.

1690) went to England and managed to involve themselves in the affairs of English scientific reform.[95]

In comparison, the parts of continental Europe where mathematics and astronomy seemed to lag behind were Vienna, Bohemia, and Moravia. Astronomy at the University of Vienna, particularly during the Renaissance, did not occupy any significant position in the arts curriculum. Furthermore, the work done by notable individuals like Peurbach and Regiomontanus (1436-1476) did not have a profound impact on their contemporaries within the university.[96] The situation was even worse in Bohemia and Moravia – even as late as the mid-seventeenth century, astronomy remained in a state of stagnation, and for most of the seventeenth century, natural philosophy and astronomy were still being influenced by ancient tradition and standard Aristotelianism. There was barely any "new" science being introduced into this part of Europe.[97]

England

By the late sixteenth century, the only centers of learning in England were Oxford and Cambridge. Neither had a mathematical chair established at the time and mathematical instruction was minimal, yet "there was a small but slowly growing number of students and teachers of that subject in England."[98] Other educational institutions included Gresham College, founded in 1597, which became one of the leading schools of England, particularly in mathematics and astronomy outside of Cambridge and Oxford, and was "throughout the first half of the seventeenth century, a general clearinghouse for information concerning the latest scientific discoveries."[99]

The curriculum at Oxford and Cambridge, aimed towards the education of undergraduates, was determined by a series of statutes – at Cambridge, the Elizabethan statutes of 1570 prevailed, and at Oxford, the Laudian Code of 1636 was established. These were not the only statutes, however; individual colleges within both universities had their own sets of statutes.

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[95] Evans, "Learned Societies in Germany," 134.
[96] Claudia Kren, "Astronomical Teaching at the Late Medieval University of Vienna," History of Universities 3 (1983): 18, 21.
[97] Josef Smolka, "The Scientific Revolution in Bohemia," in Porter and Teich, eds., The Scientific Revolution, 233.
[98] David Waters, The Art of Navigation in England in Elizabethan and Early Stuart Times (London: Hollis and Carter Limited, 1958), 94.

By the mid-seventeenth century, a typical guide of study for the serious Oxford or Cambridge student included: in the first year, systems of logic, logical controversies, and systems of ethics; in the second year, physics, controversies in logic, ethics, and metaphysics; in the third year, Aristotle's *Organon*, *Ethics*, and eight books of the *Physics*; and in the fourth year, Seneca, Aristotle's *De Anima*, *De Caelo*, and *Meteorologica*, and Homer's *Iliad* and *Odyssey*.

As early as the fifteenth century, undergraduates at Cambridge and Oxford were exposed to principles of cosmology, but not calculations or computational astronomy. Mathematics and astronomy were required as part of the MA curriculum, usually in the form of one term of arithmetic, two terms of geometry, and two terms of astronomy.[100] But by the beginning of the seventeenth century, mathematics and astronomy were not sufficiently emphasized for those interested in pursuing them on a more advanced level. Complaints about the lack of detailed mathematical and astronomical education in the universities were not uncommon. Undergraduate courses in astronomy stressed its practical usefulness but did not adequately stress planetary theory because such courses were intended "to provide only a panoramic view of the field, not mastery of it."[101] However, if the level of mathematical instruction seemed basic, we must remember that "parents did not send their sons to the universities to become natural philosophers."[102]

Despite the lack of intensity in the mathematical sciences, however, the material was taught, nonetheless, and what was learned in the public lectures was frequently supplemented with private tutorials within and outside of the colleges. Although the services of tutors in mathematics may have been used more frequently in the second half of the seventeenth century, their significance cannot be underestimated because they had exerted perhaps even a greater influence on students pursuing advanced study in mathematics and astronomy than the

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[99] Johnson, Astronomical Thought in Renaissance England, 263.
[100] Feingold warns us, however, to refrain from evaluating the curriculum based on the testimony of contemporaries and from what was written in the statutes [Mordechai Feingold, "The Mathematical Sciences and New Philosophies," in The History of the University of Oxford: Vol. IV Seventeenth-Century Oxford, ed. Nicholas Tyacke (Oxford: Clarendon Press, 1997), 361]. More detailed information on astronomical courses in the sixteenth and seventeenth centuries can be found in John T. Kelly, Practical Astronomy during the Seventeenth Century: Almanac-Makers in America and England (New York & London: Garland Publishing, Inc., 1991), 163-171.
[101] Feingold, "The Mathematical Sciences and New Philosophies," in Tyacke, ed., The History of the University of Oxford, 377.
[102] E.G.R. Taylor, The Mathematical Practitioners of Tudor and Stuart England (Cambridge: Cambridge University Press, 1954), 97. This was the same reason parents did not send their sons to the Jesuit schools!

universities.[103] By the second decade of the seventeenth century, these tutors selected the texts that students were expected to study during the tutorials and the tutors chose the background reading to be pursued on the students' own time – although different tutors used different texts. By the mid-seventeenth century, tutors and their students frequently used texts such as Gassendi's *Insitutio astronomica* and Moxon's *A Tutor to Astronomie and Geographie* (which discussed the Ptolemaic, Copernican and Tychonic systems).[104]

Henry Savile (1549-1622) was another Englishman interested in promoting mathematics education by setting up professorships in his name, although in this case, only two – the Savilian Professorships in Astronomy and Geometry, created in 1619.[105] In John Aubrey's (1626-1697) *Brief Lives*, Savile is curiously described as "an extraordinary handsome and beautifull man; no lady had a finer complexion."[106] But beyond his physical characteristics, Savile was also a self-educated mathematician, although he began as a student at Oxford. His preparation in mathematics and astronomy was based on both standard introductory texts and on more technical treatises, and his knowledge "demonstrate[s] a degree of understanding of the concepts and methods of ancient and modern astronomy unequalled in England at that time."[107]

Savile's mathematical education was supplemented by his travels to Continental Europe, where he

> travelled very well, and had a generall acquaintance with the learned men abroad; by which means he obtained from beyond the sea, out of their libraries, severall rare Greeke MSS., which he had copied by an excellent Amanuensis for the Greeke character. He gave his Collection of Mathematicall Bookes to a peculiar little Library belonging to the Savilian Professors.[108]

By traveling and interacting with scholars, he was able to bring the scientific community of Europe closer together by sharing the knowledge he had accumulated abroad, including the work

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[103] Phyllis Allen, "Scientific Studies in the English Universities of the Seventeenth Century," Journal of the History of Ideas 10 (1949): 223.
[104] Feingold, "The Mathematical Sciences and New Philosophies," in Tyacke, ed., The History of the University of Oxford, 377, 378.
[105] Gresham College was challenged by the creation of the Savilian chairs (Waters, The Art of Navigation in England, 411).
[106] John Aubrey, Aubrey's Brief Lives, ed. by Oliver Lawson Dick (Ann Arbor, MI: The University of Michigan Press, 1957), 267.
[107] Robert Goulding, "Henry Savile and the Tychonic World-System," Journal of the Warburg and Courtauld Institutes 58 (1995): 178.
[108] Aubrey, Brief Lives, 268.

of Tycho Brahe that the Silesian astronomer, Paul Wittich (c. 1546-1586), showed Savile.[109] The Tychonic system may have been more appealing to Savile – he was not a Copernican, and he rejected the idea of a moving earth even though he taught Copernicanism to his students as early as the 1570's.[110] Moreover, even the more moderate "Wittich Interpretation" did not impress him, and the attempts by Continental astronomers to reconcile heliocentrism with geocentrism "was evidently not so familiar at Savile's Oxford."[111]

When Savile drafted the Savilian statutes he was aware of two distinct problems of public lectures. First, Savile realized that public lectures were usually attended by a mixed audience in which some students were more advanced than others in mathematics and astronomy. Second, the more advanced students had "special needs" which made it essential for professors to move beyond the elementary public lectures and to instruct students who wanted additional (and more advanced) tutoring.[112] The statutes made mathematics a requirement after the student's second year and were eventually absorbed by the Laudian statutes in 1634.

Savilian professors of astronomy "were expected by their classical contemporaries to teach astronomy out of books, and if they felt any urge to observe the heavenly bodies they were expected to provide instruments at their own expense."[113] With the sparked interest created by the professorships, Seth Ward (1617-1689) was inspired to build an observatory at the top of Wadham College tower in 1648. From this observatory, Oxford astronomers carried out their astronomical observations in the 1650's. Instrument-making, unfortunately, was paltry (at least at Oxford) when compared to London since the instrument-makers were not as good. However, in the late seventeenth century, the Savilian professors became increasingly proficient at designing and constructing their own instruments, because of their desire to keep up with the Royal Society, but Oxford failed to do so because the majority of the mathematical talent wound

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[109] Goulding argues how documents originally written by Savile point to the extent to which Wittich contributed to Tycho's system and Tycho's originality. His general conclusion is that Wittich played a major role in Tycho's work, but the Savile papers demonstrate that Tycho took the final steps himself in refining his system (Goulding, "Henry Savile and the Tychonic World-System," 178).
[110] Goulding, "Henry Savile and the Tychonic World-System," 152, 154.
[111] Goulding, "Henry Savile and the Tychonic World-System," 178.
[112] Feingold, "The Mathematical Sciences and New Philosophies," in Tyacke, ed., The History of the University of Oxford, 385-386.
[113] R.T. Gunther, "The First Observatory Instruments of the Savilian Professors at Oxford," The Observatory 60 (1937): 195.

up in London.[114] Nevertheless, the Savilian professors used the Wadham College observatory to carry out observations until 1870, and stored their instruments there.[115] Among the Savilian professors of astronomy, there was at least one case in which a Gresham professor of astronomy gave up his post to become Savilian professor of astronomy – Christopher Wren – but this happened at a time when Gresham was declining.

Formal instruction in mathematics and astronomy improved with the establishment of professorships at Gresham and Oxford. Likewise, in the second half of the seventeenth century, the Lucasian Professorship of Mathematics (1663) at Cambridge also boosted mathematical studies, although unlike the required lectures of the Savilian professor, lectures from the Lucasian chair were voluntary.[116] In addition to the Lucasian chair, the Lowndean Professorship of Astronomy and Geometry was founded as well as the Plumian Professorship of Astronomy and Experimental Philosophy (1704).

Though the more practical aspects of instrument-making lagged behind at Oxford and Cambridge, the ideas arriving from the Continent were well-received by the English universities. Copernicanism may have been rejected in large part by Catholic universities and even some Protestant universities on the continent, but at Oxford and Cambridge, "no official attempt to curb the rapid dissemination of such ideas was ever felt necessary. Not that everyone was receptive. . . ."[117] Regardless of the lack of any opposition to Copernicus in England, however, "the scientific community in the eighty years leading up to 1640 was receptive to ideas

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[114] Feingold, "The Mathematical Sciences and New Philosophies," in Tyacke, ed., The History of the University of Oxford, 376, 437.

[115] Gunther, "The First Observatory," 193. More specifically, the instruments "were kept in their study in the upper storey of the Tower on the east side of the Quadrangle of the Schools" (Gunther, "The First Observatory," 190). At the time this article was written in the late 1930's, the instruments were being preserved in the Museum of the History of Science in the Old Ashmolean Building, Oxford, and an inventory of the instruments from the late seventeenth century was in the Bodleian Library Catalogus Librorum Manuscriptorum.

[116] In 1583, the Town's College, later to become the University of Edinburgh, was founded. Astronomy, which was a part of the Arts faculty, was a component of mathematics and taught by "Regents of Philosophy". One can find MA theses and observations performed in the early seventeenth century. A chair of mathematics was established there in 1620 and by the 1670's, James Gregory (1638-1675) had become one of Edinburgh's premier astronomers [Hermann A. Brück, The Story of Astronomy in Edinburgh from Its Beginnings Until 1975 (Edinburgh: Edinburgh University Press, 1983), 2].

[117] Feingold, "The Mathematical Sciences and New Philosophies," in Tyacke, ed., The History of the University of Oxford, 397. Taylor concludes:

> Nearly all the leading mathematical practitioners of the next reign were Copernicans, just as they were anti-Aristotelians (as Aristotle was understood in the Schools) in the resounding controversy between advocates of observation and experiment on the one hand and of acceptance of authority on the other, which was dividing scholars in Cambridge as well as in Paris (Taylor, The Mathematical Practitioners, 25).

infiltrating from the Continent, and so laying the foundation for the rapid fermentation that characterized the latter half of the seventeenth century."[118] Yet some modern scholars have underestimated the role played by the universities, and the amount of emphasis placed on mathematics, such as Richard Foster Jones who claimed that

> the unceasing war which the Baconians waged against the ancients in the seventeenth century, as well as the obsequious submission to the authority of antiquity in the sixteenth century, make it clear that the classical obstruction had to be removed before science could find a place in the sun.[119]

Similarly, E.G.R. Taylor stated in her The Mathematical Practitioners of Tudor and Stuart England that

> the history of the mathematical arts and practices during the sixteenth and seventeenth centuries is largely a history of disappointment and failure. . . . The early chapters of mathematics to everyday needs are often a story of frustration, of results that were merely derisory, of meagre successes.[120]

These arguments suggest that the universities were indifferent and antipathetic to the "new" learning, that English science was dominated by narrow Aristotelianism and a dependent attitude on ancient authority that prevented or hindered any forward movement or appreciation of the sciences, and especially mathematics. However, even John Wilkins (1614-1672) and Seth Ward, practitioners from the period, "rallied to the defense of the universities," because as they argued, "Oxford and Cambridge are certainly not tied to Aristotle's logic. . . . For men could be found at the universities (they do not say in what proportion) who supported. . . the Copernican astronomy."[121]

The traditional view of the role played by the English universities in the sixteenth and seventeenth centuries has been misjudged, however, because scientific and philosophical studies were more "progressive" than has often been represented. Not only was there forward movement in the scientific and philosophical circles of the English universities, the universities also provided the intellectual atmosphere within which new scientific and philosophical modes

[118] Feingold, The Mathematicians' Apprenticeship, 214.
[119] Richard Foster Jones, Ancients and Moderns: A Study of the Rise of the Scientific Movement in Seventeenth-Century England, 2nd ed. (St. Louis, MO: Washington University Press, 1961), 268.
[120] Taylor, The Mathematical Practitioners, 3.
[121] Taylor, The Mathematical Practitioners, 96-97.

of thought could be expressed.[122] Because of the perceptible liberality at Oxford and Cambridge, the intellectual freedom of the English universities was never seriously threatened by the new ideas in natural philosophy, and it was generally acceptable to teach the "old" and "new" learning at the same time in order to get the full benefit of a university education.[123] Any claims that university lectures in mathematics were obsolete or poorly attended are also inaccurate. Attendance, which was compulsory and usually involved fines for absenteeism, was not taken lightly, and there was no "general relaxation of university and college discipline as regards attendance at lectures."[124]

Discussion of the work and ideas of Kepler and Galileo were also frequently encouraged at Oxford and Cambridge in the first half of the seventeenth century. Several Englishmen became familiar with Kepler's *Astronomia nova* soon after its publication in 1609, including Thomas Harriot (c. 1560-1621), William Lower (?1570-1615), Christopher Heydon (15??-1623), Henry Briggs (1560-1630), and Thomas Lydiat. In the 1620's, the first Savilian professor of astronomy at Oxford, John Bainbridge (1582-1643), inspired his students to pursue astronomy based on topics concerning Galileo's discoveries and Kepler's optics and physics.[125] In fact, Bainbridge was the "first English Keplerian astronomer."[126] By 1650, Kepler's works were respected, used, and even recommended to students within the mathematical and astronomical circles of the universities, while concurrently, the works of other foreigners such as Boulliau had also become popular and respected. Nonetheless, at colleges such as Queen's College, Oxford, undergraduates frequently debated over the astronomical discoveries of Galileo and Kepler because of the general acceptance of Aristotelianism.[127] Such debates indicate that there were both advocates and opponents to the new systems, but more importantly, the new systems were not ignored.

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[122] Feingold, The Mathematicians' Apprenticeship, 215; Johnson, Astronomical Thought in Renaissance England, 12-13.
[123] Feingold, "The Mathematical Sciences and New Philosophies," in Tyacke, ed., The History of the University of Oxford, 402; Feingold, The Mathematicians' Apprenticeship, 120.
[124] Feingold, The Mathematicians' Apprenticeship, 46.
[125] A more detailed discussion of Kepler's acceptance into the English universities of the seventeenth century can be found in Adam Jared Apt, The Reception of Kepler's Astronomy in England: 1596-1650, D.Phil., St. Catherine's College, Oxford, 1982.
[126] Apt, The Reception of Kepler's Astronomy in England, 188.
[127] Feingold, "The Mathematical Sciences and New Philosophies," in Tyacke, ed., The History of the University of Oxford, 380, 392.

Science did not have to "find a place in the sun,"[128] nor did the universities cease to play an important role in the scientific and philosophical learning of the seventeenth century. Furthermore, the history of mathematics in the sixteenth and seventeenth centuries is not a history of disappointment or "meagre successes." While it may be true that mathematics was both basic and broad and private tutors had to supplement what was being taught in public lectures to those who were interested in more advanced mathematical studies, by the middle of the seventeenth century, the average undergraduate was sufficiently proficient with both the old and new mathematical and philosophical modes of thought. As Feingold concludes:

> In this manner Oxford and Cambridge not only trained more students who made science their vocation, but provided a considerate proportion of the educated public with at least a modicum of scientific knowledge, thus contributing to a relatively sizeable community of "virtuosi" who made possible the flowering of English science.[129]

This community of "virtuosi" was marked by a certain sense of "camaraderie" because of both the difficulty of the subject and the relatively small size of the community. Furthermore, this comradeship was so powerful and ubiquitous, that it "transcended college and university boundaries, as well as (often) ideological divides."[130]

Such a close community points to the larger context of astronomical networks in the first half of the seventeenth century in England that extended beyond university walls. Among the various groups, these included the northern astronomers (Towneley, Gascoigne, Horrocks, and Crabtree), Hartlib's group in London, and the Oxford group of astronomers of the 1640's and 1650's.[131] Even those working within Gresham in London were part of a larger scientific community – further indication that there were a number of networks "radiating out" from the various colleges and universities in England that were not limited exclusively to members of the universities. Furthermore, no one worked in isolation from the larger community as Feingold states:

> The networks of association that evolved around some of the most active men of science in our period intersected at a number of points to form one,

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[128] See the Jones passage above marked by ff. 119.
[129] Feingold, "The Mathematical Sciences and New Philosophies," in Tyacke, ed., The History of the University of Oxford, 426.
[130] Feingold, "The Mathematical Sciences and New Philosophies," in Tyacke, ed., The History of the University of Oxford, 386.
[131] See A. Rupert Hall, The Revolution in Science 1500-1750 (London and New York: Longman Group Ltd., 1983), 215-218.

> elaborate, interlocking scientific community. . . . [There is an] absence of any attitude of disrespect or superiority on the part of London-based men of science towards their counterparts in the universities.[132]

And Johnson adds:

> The English scientists of the sixteenth and seventeenth centuries, on whose ideas and writings our conclusions have been based, were not unknown investigators working in isolation, far removed from the main stream of contemporary life and thought. On the contrary, they were. . . actively associated with a large circle of persons famous for the parts they played in the history and literature of the Elizabethan age.[133]

These networks became increasingly complex over the course of the seventeenth century. It would a gross oversight, however, not to argue for the larger extension of these networks into the continent. Certainly, the ideas of individuals such as Copernicus, Galileo, and Kepler permeated English science and astronomy in the first half of the seventeenth century.

Mathematics and astronomy flourished in seventeenth-century England despite the arguments of certain modern scholars. There was even far less resistance to the "new" astronomy in England than there had been in France, and to some degree, Reformation Germany, and the mathematical subjects prospered not only outside university walls, but also within the universities themselves. The practitioners were part of a larger community of scholars, both in England and on the continent, who actively sought each other out and who were interested in absorbing new knowledge even if they ultimately did not accept it.

Individual Cases of Mathematical and Astronomical Education

Evidently, mathematical and astronomical education widely varied between universities and countries. Because mathematics was rudimentary, for the most part, it appears as though there were no opportunities for more advanced studies in mathematics and astronomy, but this was not the case, especially in England. In this section, I shall describe the specific education of certain key individuals in the history of sixteenth and early seventeenth-century astronomy who illustrate well the availability of mathematical education. But more important than the degrees

[132] Feingold, The Mathematicians' Apprenticeship, 215.
[133] Johnson, Astronomical Thought in Renaissance England, 288.

they received, their education was coupled with some type of mentoring process (or network), and their knowledge of mathematics and astronomy was instilled beyond elementary levels.

Tycho Brahe's education in the mid-sixteenth century laid the foundation for what was to become a fruitful career in astronomy, although in Tycho's case, in observational rather than theoretical astronomy. Tycho's university studies began in 1559 at the University of Copenhagen where he came into contact with a curious mix of the influence of Melanchthon, Aristotle, and the Scholastics.[134] He began his studies with rhetoric and philosophy, but showed a particular interest in the quadrivium, and he soon became interested chiefly in astronomy when he witnessed an eclipse in 1560 – by the end of the year, he had copies of Sacrobosco's *De sphaera*, Apianus', *Cosmographia*, and Regiomontanus' *Tabulae directionum*.[135] After leaving Copenhagen in 1562, Tycho attended the University of Leipzig (until 1565) where he followed the standard curriculum. On his own time, he managed to pursue more advanced astronomical studies, although he had to do this in secret so that his foster parents would not find out – advanced astronomical pursuits were not appropriate for future statesmen. In 1574, he agreed to King Frederick II's request to instruct students in astronomy at the University of Copenhagen, and Tycho was one of the first lecturers to introduce Copernican theory to his students. By "[moving] from one cultural space to the other, [and] blurring the lines between court and academy," he "demonstrated that an aristocrat and courtier could also be an academic scholar."[136]

Tycho's education extended beyond the universities of Copenhagen and Leipzig with the establishment of his magnificent observatory on the island of Hven. Here, Tycho had the opportunity to educate others in the pursuit of more advanced observational astronomy. In fact, many were "veterans of the university" who wanted more specialized training that could not be acquired in the academic setting of the schools.[137] Between 1582 and 1584, he accepted at least fourteen assistants he had personally interviewed, although their tenures at Hven were of short duration and many had difficulty getting along with Tycho.[138] A few assistants, however, stood out above the rest. One of Tycho's best assistants was not a university-trained student, but an

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[134] C. Doris Hellman, "Tycho Brahe," Dictionary of Scientific Biography, ed. Charles Coulston Gillispie (New York: Charles Scribner's Sons, 1970), II, 401.
[135] Hellman, "Tycho," DSB, II, 401; Thoren, Lord of Uraniborg, 10; Dreyer, Tycho Brahe, 13.
[136] Christianson, On Tycho's Island, 19, 20.
[137] Thoren, Lord of Uraniborg, 192. For a relatively complete list of assistants with short biographies, see Christianson, "Biographical Directory of Tycho Brahe's Artisans, Assistants, Clients, Students, Coworkers and Other *Famuli* and Associates," On Tycho's Island, 251-381.
[138] Thoren, Lord of Uraniborg, 196.

artisan, an instrument-maker from Germany by the name of Hans Crol, an indication perhaps that Tycho did not necessarily favor university-trained students over those with a firm hands-on knowledge of instrumentation. Tycho's best assistant, however (and a product of the universities), was Christian Sørensen, otherwise known as Longomontanus (1562-1647). Longomontanus first arrived on the island of Hven in 1590, but eventually returned to the University of Copenhagen in 1605 where he stayed until 1647, spending the last four decades of his life establishing his reputation as an astronomer and instructing others – he became Tycho's only student to gain a professorship in astronomy. In 1622, he published his *Astronomia danica*, a tribute to his own one-time teacher and fellow astronomer, Tycho Brahe.[139]

Beyond the island of Hven, there was a larger circle of followers and admirers of Tycho's work and astronomical observations. This more distant community of individuals "consisted of a small, elite network of academic and court astronomers located in several major cities of the German Hapsburg realms with links to Tycho's island fiefdom of Hven and to humanistic circles elsewhere in Europe."[140] One of these individuals included Wittich, who spent four months at Uraniborg in 1580.[141]

Instruction in astronomical and mathematical studies was pursued vigorously elsewhere in Lutheran universities. In the late sixteenth century, the University of Tübingen produced one of the greatest astronomers of the period, Johannes Kepler. Kepler's own teacher, Michael Maestlin, who was the assistant to and replacement for Apianus, was also a product of Tübingen, both for his B.A. (1569) and his M.A. (1571). Maestlin openly taught the Copernican system alongside the Ptolemaic and educated Kepler in both these systems. Kepler passed his Baccalaureate exam at Tübingen in 1588 and received his M.A. in 1591. Hevelius was also a product of a Lutheran education, although he did not receive formal schooling at a Lutheran university. From 1618 to 1624, Hevelius was a student at the Danzig *Gymnasium Academicum*. After a brief time away from Danzig to learn Polish, he returned in 1627 and began formal studies in astronomy and mathematics under the tutelage of Peter Krüger, whose private lessons also included instrument-making and engraving. In 1630, Hevelius went to Leiden to study

[139] Thoren, Lord of Uraniborg, 199.
[140] Owen Gingerich and Robert S. Westman, "The Wittich Connection: Conflict and Priority in Late Sixteenth-Century Cosmology," Transactions of the American Philosophical Society 78, no. 7 (1988): 2.
[141] The Gingerich and Westman article is rich with information on Wittich's life and scientific contributions. See also Christianson, On Tycho's Island, 379-381.

jurisprudence but did not receive a degree, and he left soon after for a tour of London and Paris where he stayed two years before being called home in 1634 to work at his father's brewery.

Contemporaries of Hevelius who also never received a university degree included Boulliau and Cassini. Boulliau's schooling included philosophy at Paris and law at Poitiers, but there are no records to indicate that Boulliau ever received a degree. Even Cassini did not receive a university degree, but rather, studied with the Jesuits in Genoa, and was tutored later by the Jesuit astronomers, Francesco Grimaldi (1618-1663) and Riccioli. Private tutelage in mathematics and astronomy was also a part of Christian Huygens' education. Huygen's own father was one of his first tutors, followed by private instruction in mathematics at Leiden under Frans Van Shooten in the 1640's.

During the same period in England, the emphasis on the schooling of certain prominent astronomical practitioners was on self-tutelage, usually following a certain degree of university training. Thomas Harriot was one of the few to actually complete his university training by receiving his B.A. from Oxford in 1580. Fascinated by mathematics, astronomy, and navigation, Harriot carried out his own telescopic observations of Jupiter and sunspots, and in the 1590's, began investigating optics. The "triumvirate" of English northern astronomers in the early seventeenth century – Crabtree, Horrocks, and Gascoigne – were dissatisfied with mathematics and astronomy within the universities, not because there was a lack of such studies, but because of the lack of depth in the material.

Of the three, Crabtree was the only one with no university training. The extent of his education seems to have been Grammar School in Manchester; he was self-educated in astronomy. Like Crabtree, Horrocks was also self-educated in astronomy. Born in Toxteth, near Liverpool, he matriculated to Cambridge as a sizar, an indication of his rather impoverished beginnings. He left Emmanuel College without his degree in 1635, dissatisfied with the mathematics instruction at Cambridge, and decided to teach himself mathematics. With frankness and sincerity, he explained to his readers in his *Venus in sole visa*:

> There were many hindrances. The abstruse nature of the study, my inexperience, and want of means dispirited me. I was much pained not to have any one to whom I could look for guidance, or indeed for the sympathy of companionship in my endeavours. . . . What then was to be done? I could not make the pursuit an easy one, much less increase my fortune, and least of all, imbue others with a love for astronomy. . . . I am determined therefore that the tediousness of study should be overcome by

> industry. . . and that instead of a master I would use astronomical books. Armed with these weapons I would contend successfully; and having heard of others acquiring knowledge without greater help, I would blush that any one should be able to do more than I. . . .[142]

Horrocks' dissatisfaction with his Cambridge studies was echoed by Gascoigne's reflections of his Oxford education. Unlike Horrocks' more impoverished beginnings, Gascoigne's father had been a member of the gentry class. But like Horrocks at Cambridge, Gascoigne was disappointed and disillusioned by the mathematical education he had received as a letter to Oughtred indicates:

> . . . . I betook myself to the country, having never had so much aid as to be taught addition, nor the discourse of an artist (having left both Oxford and London before I knew what any proposition in geometry meant). . . having had two years' interim between the school and university, where I dare say that I learned more than how those lived, that increased their knowledge.[143]

The frustration experienced by both Horrocks and Gascoigne seem to reflect the arguments made by Jones and Taylor mentioned earlier in this chapter – that universities in England were Aristotelian at their very core and hostile towards the introduction of material contrary to traditional disciplinary divisions. Horrocks and Gascoigne's sentiments reflect their beliefs that they did not experience *their* ideal version of a mathematical education, that mathematics and astronomy did not have the depth that they desired. Their other option would have been to hire private tutors, but they did not, probably because of the potential financial strains – Horrocks was impoverished his entire life and even complained about it.[144] A more likely reason, however, was because of their mistrust of authority – their objectionable experiences at Cambridge and Oxford would have only reinforced their suspicions of authority figures. Hiring a tutor, himself a product of the universities, would not have been ideal for what they were trying to achieve – the rejection of authority in astronomy through the fresh accumulation of observations. Therefore, they had no other recourse but to teach themselves material of their own choice and at their own pace and depth.

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[142] Horrocks, *Venus in sole visa*, in Arundell Blount Whatton, Memoir of the Life and Labors of the Rev. Jeremiah Horrox, Curate of Hoole, near Preston; To Which is Appended a Translation of His Celebrated Discourse Upon the *Transit of Venus Across the Sun* (London: William Macintosh, 1859), 13-14.
[143] ?July 1641, Rigaud, Correspondence, I, 35.
[144] See the previous passage marked by ff. 142.

Practical Mathematical Interests Beyond the Universities

Mathematics was traditionally connected to certain fields including navigation, geography, surveying, cartography, astrology, and even medicine. In the sixteenth century, England, more than any other part of Europe, made the most of these traditional connections by promoting the publication of a large number of published works and treatises that combined mathematical techniques with the specific subject matter. More importantly, they were written in English and meant to be read by common men. However, it is not entirely clear how applied mathematics fit into the greater context of "intellectualism" – "some emphasized a Baconian approach to nature while others extolled the virtues of the universities and the vices of 'hands-on' experimentation."[145]

In the late sixteenth century, there were an increasing number of astronomical practitioners who embraced the ideals of productivity that were later espoused by the Baconian programme. Not content to simply read the works of others, they carried out their own observations with instruments in their possession. Some of these mathematical practitioners included William Cunningham (1531-1586), educated at Cambridge and the University of Heidelberg, whose interest in practical aspects of mathematics led him to perform astronomical observations, calculate ephemerides, and construct his own instruments. Edward Wright (1558-1615), a fellow of Caius College, Cambridge and mathematical practitioner, observed the sun's altitude using a 6-foot quadrant. Charles Turnbull (1556-1608), a physician resident of Oxford, was also an instrument-maker and astronomical observer.[146]

A distinct regard for astronomy manifested itself in several works by certain sixteenth-century mathematical practitioners who were especially interested in teaching and passing on their knowledge to others. For example, in his work *Castle of Knowledge* (1556), Robert Recorde (1510-1558) presents a dialogue between master and scholar in which the master articulates the importance of acquiring a general understanding of astronomical knowledge and

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[145] Taylor, The Mathematical Practitioners, 96. Francis Bacon (1561-1626) himself believed that "man is prone to see more order in nature than exists. . . and has merely imposed his own invented scheme of things upon nature. . . . In astronomy, as in other sciences, [Bacon's] injunction is to abjure theories and find out by observation and experiment what is actually in nature" (Jones, Ancients and Moderns, 52).
[146] All these individuals can be found in the chapter "Biographies," in Taylor, The Mathematical Practitioners.

stresses that a student must know both arithmetic and geometry before pursuing astronomy.[147] Recorde wrote another mathematical textbook in 1557, *The Whetstone of Witte.* Overall, Recorde's work may have been basic, but it managed to bring "mathematics out of the scholar's closet into the merchant's counting-house and into the sea-captain's cabin."[148]

The work of Thomas Digges is also indicative of a serious interest by a mathematician in astronomical knowledge. Thomas was the son of Leonard Digges, another notable mathematician who originally wrote the work entitled, *Prognostication Everlasting*. Thomas' aim was to enlighten the artisans and craftsmen with mathematics that had been up until then, "locked up in strange tongues."[149] In 1576, Thomas edited his father's work, adding his own part entitled *A Perfet Description of the Caelestial Orbes*, that described and advocated both a Copernican system and an infinite universe. This work includes the section "Against the reprovers of Astronomie and science Mathematicall," in which he criticizes anyone (but especially religious figures) who is against the study of reason and astronomy.[150] Thomas even spent three months at sea "in order to demonstrate to seamen the truth of his mathematical proofs."[151] After the publication of Thomas' work in 1576, English authors mentioned Copernicus more frequently in their works – most astronomers felt that it was "necessary to pay some attention to the heliocentric theory, if only to try to refute it by the conventional Aristotelian arguments."[152] This more than likely contributed to elevating Digges' edited work as one of the most popular books of its time, with subsequent editions appearing in 1578, 1583, 1585, 1592, 1596, and 1605.

One of the most capable mathematicians of the sixteenth century was John Dee (1527-1608), a product of St. John's College, Cambridge who took a special interest in mathematics at an early age. He also studied for some time at Louvain University, and when he returned in 1547, he brought back a number of mathematical instruments that he eventually gave to Trinity

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[147] Robert Recorde, *The Castle of Knowledge* (London, 1556), facsimile copy (Amsterdam: Theatrum Orbis Terrarum Ltd., 1975). This work is one of the most comprehensive and original treatises on astronomy of the sixteenth century.
[148] Waters, The Art of Navigation in England, 95.
[149] Taylor, The Mathematical Practitioners, 23.
[150] Leonard Digges, *A Prognostication Everlastinge Corrected and Augmented by Thomas Digges* (London, 1576), facsimile copy (Amsterdam: Theatrum Orbis Terrarum Ltd., 1975), Fol. 1.
[151] Waters, The Art of Navigation in England, 98. Waters adds that Thomas "was only the first of many mathematicians to go to sea. Thereafter it did not take long for the value of mathematics in navigation to be grasped by the English" (Ibid.).
[152] Johnson, Astronomical Thought in Renaissance England, 180-181.

College, Cambridge to be used by fellows and scholars.[153] In Dee's *The Mathematicall Praeface* (1570), part of a new translation of Euclid's *Elements*, he categorized the various "artes and sciences" in a chart.[154] His *Mathematicall Praeface* was not reprinted until 1651, but it still made a significant assertion that mathematics is a serious study to be pursued by students, and useful for those interested "in the common lyfe and trade of Men."[155]

In promoting astronomy, Dee redefined its role within the general matrix of knowledge – his branching tree of subjects. Of the various "propre-named artes" in which Dee included astronomy, he claimed:

> But there are other (very many) Methodicall Artes, which, declyning from the purity, simplicitie, and Immateriality, of our Principall Science of Magnitudes: do yet nevertheless use the great ayde, direction, and Method of the sayd principall Science [of geometry], and have propre names, and distinct: both from the Science of Geometrie, (from which they are derived) and one from the other. . . . I thinke it necessary, orderly, of these to give some peculiar descriptions: and withall, to touch some of their commodious uses, and to make this Preface, to be a little swete, pleasant Nosegaye for you: to comfort your Spirites. . . .[156]

The categorization of subjects continued into the seventeenth century, and as the role of astronomy in the universities became more clearly defined, a greater number of astronomical works appeared. In fact, the idea of "newness" was pervasive in seventeenth-century astronomical texts, and the word "new" (or a variation thereof) appears in almost all the titles.[157]

Texts that focused primarily on instrumentation were an important component of English astronomy. Most of these texts were initially about astrolabes, but eventually more texts appeared that discussed and described the more complex and expensive instruments including quadrants such as the work *Quadrans vetus*.[158] Beginning primarily in the sixteenth century,

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[153] Waters, The Art of Navigation in England, 95.
[154] Besides the original work by Dee, this chart can also be found at the end of both Taylor, The Mathematical Practitioners and Dee, *The Mathematicall Praeface to the Elements of Geometrie of Euclid of Megara*, introduction by Allen G. Debus (New York: Science History Publications, 1975).
[155] Cited in Dear, Revolutionizing the Sciences, 79.
[156] Dee, *The Mathematicall Praeface*, 61 (modern pagination).
[157] For a listing of scientific and "pseudo-scientific" works with the word "new" appearing in the title, see Lynn Thorndike, "Newness and Craving for Novelty in Seventeenth-Century Science and Medicine," Journal of the History of Ideas 12 (1951): 584-598.
[158] See McCluskey, Astronomies and Cultures, 193, 202, on the role of instrument texts and the names and works of various thirteenth-century instrument makers. The theoretical foundations of mathematics were perhaps more important than knowledge about instruments. Certainly, there was some hostility leveled at scholars who seemed infatuated with instruments, but not those simply interested in them (Feingold, "The Mathematical Sciences and New Philosophies," in Tyacke, ed., The History of the University of Oxford, 373).

texts on instrument-making appeared more frequently, most written by mathematicians interested in the practical aspects of mathematics, navigation, and astronomical observation. William Bourne's (fl. 1565-1588) *A Treatise on the Properties and Qualities of Glasses for Optical Purposes* (1572) addressed the work of Leonard Digges and John Dee, although Bourne himself was not familiar with how "glasses" were made.[159]

In 1585, two works appeared, John Blagrave's (1558?-1612) *The Mathematicall Jewell. Shewing the making and most excellent use of a singular Instrument so called* (the "jewel" was a new type of astrolabe), and Charles Turnbull's *A perfect and very easie Treatise of the Use of the Celestial Globe*. Robert Tanner wrote a similar work in 1592 entitled, *A brief Treatise for the Ready use of the Sphere*, which was a highly simplified, non-mathematical, and brief (110 pages) version of the spheres. Tanner dedicated the work to Queen Elizabeth, and wrote that his purpose in the work was to "unlock the closet of Astronomie unto your Highnes."[160] In the same year, Emery Molyneux published his *The Globes Celestiall and Terrestrial set forth in Plano*, and Thomas Hood wrote *The use of both the Globes, Celestiall and Terrestrial.* A more academic work on spheres was published later in 1599 by the pupils of Thomas Hill entitled, *The Schoole of Skil, containing two Bookes, the first, of the Sphere of Heaven, etc.*[161]

The practicality and sheer number of texts on instruments in the sixteenth century points to the heightened interest of mathematicians in improving navigation for sailing (in which astronomical observations helped) and surveying the land. The fact that the emphasis is English (English works by English authors) underscores the broad transformation that England was undergoing at the time. Surveying the land was significant in the late sixteenth century mainly because of the enclosure of common land but also because of the newly-acquired church lands following the English Reformation.[162] The English were also a seafaring and trading people, for whom naval and commercial domination of the high seas was a top priority. Even the great

[159] Taylor, The Mathematical Practitioners, 321.
[160] Robert Tanner, *The Ready Use of the Sphere* (London, 1592), facsimile copy (Amsterdam: Theatrum Orbis Terrarum Ltd., 1973), Preface.
[161] This posthumously-published work was very elementary in its discussion of the spheres and the book itself most likely came from Hill's own lecture notes (Taylor, The Mathematical Practitioners, 335). Other popular textbooks of the sixteenth century, although not formally a part of the *corpus astronomicum*, included William Caxton's translation of *Image du monde* (*The Mirrour of the World*) in 1481, that eventually "became the best and most thorough presentation in English of the science of astronomy," and Robert Wyer's *The Compost of Ptholomeus, Prynce of Astronomye* (1532) which "became the chief source of astronomical knowledge during the first half of the sixteenth century" (Johnson, Astronomical Thought in Renaissance England, 71, 75).
[162] Dear, Revolutionizing the Sciences, 79.

power of Spain could not restrain them, and when the Spanish Armada was trounced in 1588, the English became a dominant naval power. Therefore, the interests of the government and commerce were not very far from the minds of Elizabeth I's loyal subjects, including the mathematicians. Overall, textbooks, instruments, charts, and ships were all improved, and "in the space of seventy years, the English from being ignorant of the art of navigation had, almost entirely through their own efforts, largely transformed it into a science."[163]

[163] Waters, The Art of Navigation in England, 500.

Conclusions

Histories of sixteenth- and seventeenth-century astronomy have traditionally focused on the great advances made in the subject such as the rise of Copernicanism, Galileo's observations, Kepler's Three Laws of Planetary Motion, the discoveries of Cassini, and the mathematical work of Newton. The role played by the universities in promoting mathematics and astronomy has been largely ignored or downplayed. Some scholars have argued that universities were lax in the teaching of mathematics and even hostile to potentially damaging anti-Aristotelian ideas and philosophies, suppressing the new cosmologies, and upholding the traditional divisions of the trivium and quadrivium. However, the sixteenth and seventeenth centuries saw momentous transformations taking place in the mathematical subjects. Preoccupation with the *Quaestio* debate, *a posteriori* astronomy versus *a priori* astronomy, and the traditional division of the disciplines is evidence that natural philosophers and mathematicians attempted to define the kinds of questions that mathematics and astronomy could both ask and answer.

Unlike natural philosophy, instruction in the mathematical subjects was meant to be basic and minimal mainly because there was only a brief amount of time available to teach students the material – university training was meant to be temporary. Universities were supposed to provide grounding in the entire encyclopedia of learning and they did not want to encourage specialization too early in a student's career. Specialized courses did not even exist until much later, and tutors were responsible for advanced learning both within and outside of the universities. Some mathematicians and astronomers were even self-taught, relying on neither the universities nor tutors. Nevertheless, it was not the degree that ultimately mattered, but rather the connections one made at the university, with a professor, or with a group of individuals.

The infiltration of the "new" science into the universities happened primarily because of the mathematics professors who doubled up as astronomy professors – there were no astronomy professors in this period. Some professors were Copernicans, accepting the central thesis, and some were not, accepting geocentrism instead (whether it was Aristotelian or Tychonic). However, most professors by the late sixteenth century were familiar with the new systems and almost all of them taught them alongside the Ptolemaic-Aristotelian world-view. This suggests that the new systems were neither ignored nor rejected even if the professors did not personally accept them. They needed a basis of comparison at the very least which was why teaching a number of different systems in tandem was actually preferable than teaching Ptolemaic cosmology alone. In England more than on the Continent, the very little philosophical tradition and teaching meant that there was far less resistance to contentious cosmological systems. This explains why it was easier in England for theoretical and practical mathematics to develop in and outside of the universities – although guided by university men. Consequently, these mathematicians often had multiple roles – while not neglecting pure mathematics, they were also *practitioners*, interested in the "hands-on" applications of the mathematical subjects.

There were two consequences of teaching the old and the "new" systems together to students. First, because not all professors accepted Copernicanism, students got mixed messages from different teachers and it was up to the students to decide which system to accept, although usually, a Copernican teacher influenced his student to accept Copernicanism. The second consequence concerns the texts used by the students. The content of the *astronomical corpus* seemed to vary little over the centuries, but the content changed dramatically. More technical and practical mathematical works appeared all over Europe, but especially England, and the newer mathematical texts of the late sixteenth and early seventeenth centuries addressed the various cosmological systems.

Another general conclusion drawn from this chapter concerns the concept of networks. Mathematicians communicated with each other all over Europe on mathematical matters. In some cases, the practitioners acquired new knowledge on their travels while others gained it through correspondence. They were far from isolated, however, and the intellectual camaraderie they shared brought them together despite the great distances between them. Even Hevelius, who by working in Danzig was perhaps the furthest away geographically from other astronomical practitioners, was not isolated, and had even forged friendships with the English and French virtuosi on his travels. This active exchange of ideas and participation in each other's work is proof that the lines of communication were open among the virtuosi well before the advent of formal scientific societies in the latter half of the seventeenth century. I shall explore this topic further in the next chapter.

Chapter 3:
Academies, Scientific, and Astronomical Circles
from the Late Sixteenth to the Mid-Seventeenth Centuries

The universities of the sixteenth and seventeenth centuries were not narrow-minded towards new scientific ideas, nor were they so conservative as to reject the latest scientific theories, such as the Copernican world-view. In some cases, even, mathematics professors introduced more progressive scientific ideas to promising university students such as Maestlin's introduction of the Copernican system to Kepler. As Westman argues, "While one would not wish to describe the sixteenth-century universities as hotbeds of scientific revolutionary activity, the opposite view is equally misleading. Recognition of the great range of intermediate responses to innovation helps to make the historical landscape less rigidly compartmentalized."[1] Accordingly, this less-rigid "compartmentalization" process accounts for those instances where university learning was more conservative and those where learning was more progressive. Many mathematicians became interested in what they saw as "a direct result of university teaching."[2] In fact, almost all astronomers were university-trained and "even the astronomical iconoclasts and innovators of the [seventeenth] century were university educated."[3] Astronomers trained within the universities were not always satisfied with the education they received, as in the cases of Tycho Brahe and William Gascoigne, but any suggestions that the universities attempted to prevent exposure of their students to the new cosmological systems is inaccurate.

Just as there existed a certain sense of continuity of scientific education within the universities of the sixteenth and seventeenth centuries, there also existed a sustained effort by learned and scientific individuals to come together into small groups or informal circles to share

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[1] Westman, "The Melanchthon Circle," 191.

[2] Marie Boas Hall, "Renaissance Science and Professionalisation," Annali dell' Istituto e Museo della Scienza di Firenze 7, no. 2 (1982): 63. In regard to the system of patronage mentioned earlier, Hall adds, "By the mid-seventeenth century, personal patronage of scientists had become the exception rather than the rule" within the university environment (Ibid., 64). This implies that the university was more supportive of practitioners by the mid-seventeenth century than is typically assumed.

in scientific ideas. Typically, one thinks of the Royal Society and the Académie des Sçiences as the first two concrete examples of formal scientific circles created to pursue science and experimentation in Europe. However, there were many predecessors to these two later institutions. In fact, in most cases, most of the individuals who comprised these various informal circles or first academies were part of a larger network of scientific practitioners of the European scientific community and the republic of letters. By discussing several of these first academies and scientific circles (with emphasis on astronomical activity in some cases), I intend to show the extent of the European scientific community's interconnectedness on mathematical and astronomical matters.

The particular characteristics that defined individuals as scientists are beyond the scope of this study, although they have been discussed in recent scholarship.[4] However, a few salient points relevant to this study are discussed by Ben-David in his "The Scientific Role: The Conditions of its Establishment in Europe." According to Ben-David, there are stages in the formation of an autonomous "scientific" role. In seventeenth-century Europe, certain individuals began "to view themselves as scientists and see the scientific role as one with unique and special obligations and possibilities." Natural science, which formally had been subordinate to theology and philosophy, eventually was viewed as more than just a "peripheral subject." More specifically, it

> came to be regarded by groups, with class, religious and political interests opposed to the established order, as intellectually more meaningful to them than the existing theological, philosophical, and literary culture. For these groups, the sciences became a central part of their culture. Under these circumstances, men interested in science were impelled to redefine their roles as philosophers in such a way that science became increasingly central instead of peripheral to their conception of what they were doing.

As the number of individuals with these interests increased, there was more of a reason and opportunity for them to meet to exchange scientific ideas and information, and by doing so, they "developed their own culture, their own norms and traditions in which their scientific work was

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[3] M.B. Hall, "Renaissance Science and Professionalisation," 56.
[4] See for example, Joseph Ben-David, "The Scientific Role: The Conditions of its Establishment in Europe," in The Rise of Modern Science: External or Internal Factors? ed. George Basalla (Lexington, MA: D.C. Heath and Company, 1968), 47-54; Joseph Ben-David, The Scientist's Role in Society (Englewood Cliffs, NJ: Prentice-Hall, Inc., 1971); J.V. Field and Frank A.J.L. James, eds., Renaissance and Revolution: Humanists, Scholars, Craftsmen and Natural Philosophers in Early Modern Europe (Cambridge: Cambridge University Press, 1993); and Tore

embedded."[5] Ben-David also claims that another important ingredient in the formation of academies and scientific circles in the sixteenth and seventeenth centuries is the

> relative openness and decentralization of the social system of European intellectual life. The Continent, including England, constituted a cultural whole, as a result of the unity of the church and its adoption of Roman traditions; persons and writings traveled across political borders with relative ease. Ideas evolved in one place would be readily appreciated in another.[6]

The ease with which "persons and ideas" traveled throughout Europe led to the cross-currents of information that coalesced to form the first networks and circles of scientific practitioners. By "banding together," scientific practitioners could then "obtain means for a laboratory supplied with the necessary instruments and to obtain the means of making those instruments and experiments which [they] forecast in their speculations. . . ."[7] Indeed, laboratories were not just places where one went to work or study. They also served as "whole dwelling places within which certain areas are designated for the practice of science;" therefore, we need to understand the laboratory within a "broader social context."[8]

The "amateur" in science is another important component of the rise of scientific circles in the sixteenth and seventeenth centuries. Ornstein describes the "amateur" in her <u>The Rôle of Scientific Societies in the Seventeenth Century</u>:

> Amateurs in science – "amateurs" in the accepted sense of the word denoting those that "practice their art not as a livelihood, but for the love of it" – were to be found in many places and among many classes of people in the latter part of the seventeenth century, mainly of course in circles which were sufficiently wealthy not to feel the immediate urgency of gaining a livelihood, and had therefore sufficient leisure to follow their inclinations.[9]

Frängsmyr, ed., <u>Solomon's House Revisited: The Organization and Institutionalization of Science</u> (Canton, MA: Science History Publications, 1990).

5 All citations above in Ben-David, "The Scientific Role," in Basalla, ed., <u>The Rise of Modern Science</u>, 48.

6 Ben-David, "The Scientific Role," in Basalla, ed., <u>The Rise of Modern Science</u>, 49.

7 Martha Ornstein, <u>The Rôle of Scientific Societies in the Seventeenth Century</u>, 3rd ed. (Chicago: The University of Chicago Press, 1938), 67. Ornstein claims, "It cannot be sufficiently emphasized that it was the experimental feature of science which called forth the societies" (Ibid., 67). This experimental "ingredient" will be discussed in the next section on Italian scientific academies.

8 Owen Hannaway, "Laboratory Design and the Aim of Science. Andreas Libavius versus Tycho Brahe," <u>Isis</u> 77 (1986): 586.

9 Ornstein, <u>The Rôle of Scientific Societies</u>, 55.

These were the individuals who became the "devotees of the new knowledge." Ornstein continues by discussing (among others) the Medicis and Federigo Cesi (1585-1630) in Italy, the Duke of Orleans and Peiresc in France, and Charles II and Robert Boyle in England. These seventeenth-century amateurs were, however, preceded by several sixteenth-century amateurs, particularly in England.

In addition to the distinction between amateur and professional, the distinction between scholar and craftsman merits some attention, a distinction discussed in great length by A. Rupert Hall and only briefly summarized here.[10] Hall describes scholars as those who showed an interest in "the information acquired by craftsmen, and their special techniques for criticizing established ideas and exploring phenomena afresh." Craftsmen, however, did not have the ability "to criticize the theories and procedures of science," especially since their function was "passive," unlike the scholar's "active" function.[11] Despite these differences, he "cautions" us about the distinction between a scholar from the university and the craftsman involved in a trade. At the outset, he claims that "we must be cautious in detecting polar opposites where there is in reality a spectrum,"[12] and in fact, there was a wide spectrum of possibilities differentiating the scholar from the craftsman. Interestingly, Hall claims that the work of astronomers during the Scientific Revolution was hardly craft-influenced, despite the craft invention of the telescope since its "scientific potentialities were perceived by scholars."[13] However, astronomy was to a certain extent "craft-influenced," even in the late sixteenth century.

Looking at the demography of the scientific community from 1550 to 1650, we find a number of scientific "academies," informal circles, and groups throughout Europe, including Italy, France, England, and Germany.[14] Furthermore, the two leading sciences at this time were mathematics and astronomy. However, because of the steady rise of interest in the other sciences beginning in the late seventeenth century, there was a decline in the total number of mathematical and astronomical practitioners.[15] In addition to Robert Gascoigne's demographical

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[10] A. Rupert Hall, "The Scholar and the Craftsman in the Scientific Revolution," in Critical Problems in the History of Science, ed. Marshall Clagett (Madison, WI: The University of Wisconsin Press, 1962), 3-23.
[11] A.R. Hall, "The Scholar and the Craftsman," 39, 40.
[12] A.R. Hall, "The Scholar and the Craftsman," 23.
[13] A.R. Hall, "The Scholar and the Craftsman," 37.
[14] For complete demographics through 1900 (including graphs for each country and science), see Robert Gascoigne, "The Historical Demography of the Scientific Community, 1450-1900," Social Studies of Science 22 (1992): 545-573.
[15] Gascoigne, "The Historical Demography," 562, 563. The graphs on these pages point to the marked decline of total numbers in mathematics and astronomy even though the total number of natural philosophers was on the rise.

studies, Richard Westfall has also "charted" the scientific community of the sixteenth and seventeenth centuries. Using the Dictionary of Scientific Biography (and after "purging" some inconsequential names), he was left with a study of 630 individuals in the proposed time-frame. For one of his parameters, which he refers to as "membership in scientific societies," he makes the following remark:

> I have collected extensive information about informal groups and networks of correspondence, and they seem of immense importance for the formation of true scientific communities. Increasingly, I am amazed at how little use historians have made of the correspondence of scientists in the sixteenth and seventeenth centuries.[16]

The relatively large numbers of scientific practitioners in the sixteenth and early seventeenth centuries does not reflect the existence of opposition to such circles and especially the first academies. This opposition usually came in the form of "princes who saw subversive teaching and political intrigue in [the academies'] secret meetings, and from the church, which feared the propagation of heresies and the power of uncontrolled associations."[17] Despite any initial opposition, however, the first few academies and scientific circles endured, if only temporarily.

One factor that contributed to the success of these circles and societies was the transmission of ideas. This topic cannot be fully understood without taking into account the tremendous impact of the development of printing. Although the printing-press had been in existence since the fifteenth century, its impact on the spread of scientific ideas through the publication of scientific texts was especially profound in the sixteenth century. In fact, the changes brought on by the development of printing may have even "had a more immediate effect on cerebral activities and on the learned professions than. . . other kinds of 'external events.'"[18] In astronomy specifically, the printing press had an unprecedented effect on observation because identical images and labels of the latest astronomical texts "enabled astronomers [and others] to expand data pools far beyond all previous limits. . . ."[19] Copernicus and Tycho are excellent

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Clearly, however, England had the highest total numbers in Europe during the peak of the Royal Society (Ibid., 555).

[16] Richard S. Westfall, "Charting the Scientific Community," in Trends in the Historiography of Science, eds. Kostas Gavroglu, Jean Christianidis, and Efthymios Nicolaidis (Dordrecht: Kluwer Academic Publishers, 1994), 13-14.

[17] Harcourt Brown, Scientific Organizations in Seventeenth Century France (1620-1680) (New York: Russell & Russell, 1934), xii-xiii.

[18] Elizabeth L. Eisenstein, The Printing Press as an Agent of Change: Communications and Cultural Transformations in Early-Modern Europe, Vol. II (Cambridge: Cambridge University Press, 1979), 689.

[19] Eisenstein, The Printing Press as an Agent of Change, 687.

examples in that “they were freed from copying and memorizing and could make use of new paper tools and printed texts.”[20] The printing process allowed for the easier collection of various astronomical works and tables so that, regardless of when these astronomical materials were printed, they “were being seen in the *course of one lifetime by one pair of eyes*. For Copernicus as for Tycho, the result was heightened awareness of, and dissatisfaction with, discrepancies in the inherited data.”[21] This process of collecting printed works, either older texts from antiquity that had been printed for the first time during the Renaissance, or more current works and tables of the sixteenth century, facilitated the ease with which the “democratization” of knowledge took place in the seventeenth century.[22]

In the next sections, I shall briefly introduce some of the more prominent of these first academies and scientific circles. This list is not meant to be exhaustive, but rather, illustrative – to show that there were a fair number of scientific circles and networks before the Royal Society and the Académie des Sçiences. Beginning with Italy’s first prominent scientific academies, the Accademia dei Lincei and the Accademia della Cimento, I continue with a brief sketch of early French “academies” and scientific circles. This chapter ends with a discussion of early scientific circles in England from the late sixteenth to the mid-seventeenth centuries, with emphasis on mathematics and astronomy circles.

Italy

Some of the first academies and scientific circles formed in Italy where the Renaissance was born. Out of the Renaissance emerged several types of learned societies including literary societies (a product of the Renaissance), scientific groups (including the Accademia dei Lincei

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[20] Eisenstein, The Printing Press as an Agent of Change, 602.
[21] Eisenstein, The Printing Press as an Agent of Change, 602-603.
[22] This issue is discussed at great length in Ornstein, The Rôle of Scientific Societies in the Seventeenth Century, especially Chapter II, “Rôle of Individual Scientists,” 21-69. For an analysis of the limitations of her work and more recent historiographical scholarship on institutions of the sixteenth and seventeenth centuries, see David S. Lux, “Societies, Circles, Academies, and Organizations: A Historiographic Essay on Seventeenth-Century Science,” in Barker and Ariew, eds., Revolution and Continuity, 23-43.

and Accademia del Cimento), and certain other "narrowly professional bodies."[23] The topic of Italian academies in this period has been discussed at great length in other works.[24]

According to Ben-David, academies in the sixteenth century began as "an attempt of persons. . . to create for themselves an intellectually more congenial institution than the universities provided. . . . They sought to enrich their understanding by each other's company in which they discussed things of common interest."[25] Over time, the groups once regarded as informal became increasingly formal in their organization that introduced a more complex social structure. In her chapter on Italian academies, Yates describes the academy as neither a "school," "nor. . . a university. It is an institution devoted to research, or to the perfection of some art. Moreover, it is a thoroughly European institution, to be found in every country in Europe and of fundamental importance in the development of the European mind."[26] She later concludes that "the academy is a child of the Italian Renaissance."[27] The appeal of Italian academies must have been considerable in the sixteenth century as "practically every Italian town became the home of one or more academies of 'curiosi' and 'dillettanti' devoted to philosophical or scientific investigations. Sometimes they were sponsored by the authorities, but more often constituted on a purely private basis."[28]

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[23] Evans, "Learned Societies in Germany," 129. Although the emphasis of the article is on German learned societies, the categories mentioned above refer to Italian societies and academies.
[24] Such works include Joseph Ben-David, "The Scientific Role," in Basalla, ed., The Rise of Modern Science: External or Internal Factors?, 47-54; Marco Beretta and Tore Frängsmyr, eds., Siderius Nuncius & Stella Polaris: The Scientific Relations between Italy and Sweden in Early Modern History (Canton, MA: Science History Publications, 1997); Olaf Pedersen, "Tradition and Innovation," in A History of the University in Europe. Volume II: Universities in Early Modern Europe (1500-1800), ed. Hilde de Ridder-Symoens (Cambridge: Cambridge University Press, 1996); Stillman Drake, ed., Galileo Studies. Personality, Tradition, and Revolution (Ann Arbor, MI: The University of Michigan Press, 1970); William Eamon, "Court, Academy, and Printing House: Patronage and Scientific Careers in Late-Renaissance Italy," in Patronage and Institutions: Science, Technology, and Medicine at the European Court 1500-1750, ed. Bruce T. Moran (Rochester, NY: The Boydell Press, 1991); Max F. Fisch, "The Academy of Investigators," in Science, Medicine, and History: Essays On the Evolution of Scientific Thought and Medical Practice Written in Honour of Charles Singer, ed. E. Ashworth Underwood (London: Oxford University Press, 1953), 521-563; Paolo Galluzzi, "The Renaissance Academies: A Commentary on Sessions I and II," in Frängsmyr, ed., Solomon's House Revisited; M.B. Hall, "Renaissance Science and Professionalisation," 53-64; Gabriel Maugain, Étude sur l'Évolution Intellectuelle de l'Italie de 1657 à 1750 Environ (Paris: Librairie Hachette et Cie, 1909); W.E. Knowles Middleton, The Experimenters: A Study of the Accademia del Cimento (Baltimore, MD: The Johns Hopkins Press, 1971); Ornstein, The Rôle of Scientific Societies in the Seventeenth Century; and Frances A. Yates, "The Italian Academies," chap. in Renaissance and Reform: The Italian Contribution (London: Routledge & Kegan Paul, 1983).
[25] Ben-David, "The Scientific Role," in Basalla, ed., The Rise of Modern Science, 51.
[26] Yates, "The Italian Academies," 6.
[27] Yates, "The Italian Academies," 7.
[28] Pedersen, "Tradition and Innovation," in de Ridder-Symoens, ed., History of the University in Europe, 481.

There were at least two antecedents to both the Accademia dei Lincei and the Accademia del Cimento in the late sixteenth century. Of these two, the Accademia della Crusca was formed in Florence and the Academia Secretorum Naturae in Naples. The Accademia della Crusca, whose most famous member was Galileo, was originally formed in 1582 by the Italian poet Grazzini and the Italian philologist Salviati. This Accademia, although not significant in terms of scientific advancements and ideas, managed to produce a dictionary of the Tuscan dialect used by Galileo for his scientific works.[29] Sometime in the mid-sixteenth century, the Academia Secretorum Naturae was established by the Italian physician Giambattista della Porta (1535-1615).[30] Della Porta traveled throughout Europe, including France and Spain, meeting other scholars and visiting their libraries.[31] The group met at his home, but membership was not open to just anyone – the group was both private and secretive.[32] Della Porta insisted that only those who had "made a new discovery or observation of a phenomenon of nature" be admitted.[33] During its short existence, the various members exchanged information, books, and even favors, and eventually, "as members of a similar social milieu, they also developed kinship ties. One can see in their intimate fellowship the embryo of patronage networks, all of which rest on exchange activities."[34] As indicated earlier, however, these first few academies were on delicate religious and political footing and were often dissolved just as swiftly as they had been formed –

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[29] Pedersen, "Tradition and Innovation," in de Ridder-Symoens, ed., History of the University in Europe, 481.

[30] Stillman Drake claims that della Porta founded this group in Naples about 1589 (Drake, ed., Galileo Studies, 80). However, according to Eamon and Paheau, della Porta's group was founded earlier in the 1560's [William Eamon and Françoise Paheau, "The Academia Segreta of Girolamo Ruscelli: A Sixteenth-Century Italian Scientific Society," Isis 75 (1984): 328]. Their article argues that there was another Accademia Segreti interested in experimental research that preceded della Porta's group by about twenty years. Founded by the well-known humanist Ruscelli, it had 27 members at its peak between 1542 and 1548, but was ultimately "disbanded or perhaps closed down by the Spanish viceroy Pedro of Toledo, following the tumult of 1547. . . ." (Ibid., 329, 336). Ruscelli's group apparently carried out experiments in a house or "lab" which they called the *Filosofia*. A translation of the "Description of the House and Laboratory which We Called by its Proper Name *The Filosofia*" is included in the article (Ibid., 340-342).

[31] Pedersen, "Tradition and Innovation," in de Ridder-Symoens, ed., History of the University in Europe, 481. See also Fisch, "The Academy of Investigators," in Underwood, ed., Science, Medicine, and History, 522. Fisch refers to della Porta's group as the "Segreti".

[32] Drake, ed., Galileo Studies, 80. Drake compares this to the Lincei which "from the outset declared its intentions not merely of studying the phenomena of nature but of attempting new discoveries and publishing them to the world" (Ibid.).

[33] Pedersen, "Tradition and Innovation," in de Ridder-Symoens, ed., History of the University in Europe, 481; Drake, ed., Galileo Studies, 80.

[34] Hahn, "Age," in Frangsmyr, ed., Solomon's House Revisited, 4.

such was the case with della Porta's group that was dissolved by order of the papal court under Pope Paul V.[35]

Unfortunately for Italian science, these first academies (and, as we shall see later the Lincei and Cimento) did not endure. The survival of such academies depended on certain conditions that were simply not met. More specifically, two unrelated factors made the survival of these academies difficult in the long run. The first was the significant rise of curiosity among Italian scholars. Natural philosophy was attracting a much larger number of individuals that ultimately "fostered the multiplication of circles, and in turn, stimulated competition and jealousies on the one hand and the desire to communicate between groups on the other." The second factor had to do with the larger world beyond. Because these Italian academies grew both in number and popularity, "the assemblies had to concern themselves with the world beyond their own circle" – certain codes of conduct, protocols, procedures, and timetables had to be instituted.[36] These factors alone made the survival of early Italian academies difficult, but when one takes into account the political and religious events of sixteenth-century Italy, it would have been practically impossible for these academies to flourish. The events that also contributed in general to Italy's increasing isolation in European affairs, included that Rome became the center of the Counter-Reformation in the sixteenth century, the Catholic church controlled the publication of texts after the invention of printing (after 1515, permission from a bishop was necessary before books could be published), Pope Paul III founded the Roman Inquisition in 1542, Pope Paul IV created the Index of Forbidden Works in 1559, and a congregation was created for the Index in 1571.[37]

Accademia dei Lincei

The Accademia dei Lincei followed on the heels of the sixteenth century academies, although unlike its predecessors, it may be considered "the forerunner of modern scientific societies."[38] First established in 1603, it floundered in the beginning, but became much more

[35] Fisch, "The Academy of Investigators," in Underwood, ed., Science, Medicine, and History, 522; Drake, ed., Galileo Studies, 80.
[36] Hahn, "Age," in Frangsmyr, ed., Solomon's House Revisited, 5.
[37] Toshio Hakata, "The Diplomatic Policy of Accademia dei Lincei and Its Cultural Background," Historia Scientiarum 39 (1990): 12 f. 12. Hakata adds, "these systems caused the standardization and stagnation of culture, and as a result, Italy was culturally isolated from northern countries at the end of the sixteenth century" (Ibid.).
[38] Drake, ed., Galileo Studies, 81.

active after 1610, and especially after Galileo became a member in 1611. Undoubtedly, Galileo's membership drew considerable attention to the group, but also the founder, Federico Cesi made the Accademia more visible and "less exclusive" after 1610 in order to ensure its survival.[39] They referred to themselves as the "lynx-eyed ones," and the *impresa* of the Lincei depicts a lynx

> gazing with its eyes up to heaven and beating down with its claws the infernal dog Cerberus. By this *impresa*, they meant that in the study of nature one must penetrate below the surface of the appearances of things as the lynx with its eyes sees through everything. By the lynx's upward look, they meant that from God alone comes all knowledge. By the attack on Cerberus they meant their aim of beating down vices and bad habits in themselves.[40]

The tradition of adopting such a vivid name and device was subsequently adopted by each of the four original members of the Lincei.

From its establishment in 1603, the group's meetings were held at Cesi's family home in Rome. The two primary requirements of its original members that can be gleaned from their official record, the *Linceografia*, were that each member had to instruct the others in some kind of science, and that each member had to hold an office within the group.[41] The first requirement is a good indication that the Lincei members were interested in the diffusion of knowledge. More specifically, however, the academy was "defined to be the acquisition and diffusion of knowledge," through the establishment of various branches, called *liceo*, throughout Italy, including ones in Naples and Padua. Each *liceo*, in turn, would be equipped with a library, laboratory, museum, and observatory. There were even hopes that branches of the Lincei would be established in other countries.[42] However, the meetings of the members at the various *liceo* were not like the later Accademia della Cimento, the Royal Society, or the Académie des Sçiences, which were either "private corporations" or "institutions controlled by a prince."[43]

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[39] Mario Biagioli, "Scientific Revolution and Aristocratic Ethos: Federico Cesi and the Accademia dei Lincei," in Alexandre Koyré: L'Avventura Intellettuale, ed. Carlo Vinti (Naples: Università degli Studi di Perugia, 1994), 294. For a detailed account of the Accademia dei Lincei and Galileo's involvement, see Stillman Drake, ed., Chapter 4, "The Accademia dei Lincei," Galileo Studies, 79-94.
[40] Yates, "The Italian Academies," 21.
[41] Drake, ed., Galileo Studies, 83.
[42] Hakata, "The Diplomatic Policy of Accademia dei Lincei," 2.
[43] Biagioli, "Scientific Revolution and Aristocratic Ethos," in Vinti, ed., Alexandre Koyré," 284. Biagioli claims that the Lincei "resembled knightly orders more than religious ones" (Ibid., 292).

Furthermore, *liceo* meetings were not meant to produce or certify knowledge, but rather to "discuss administrative and strategic matters."[44]

The particular philosophical agenda and interests of the early Accademia dei Lincei is summarized in the following:

> The early academy showed a marked interest in paracelsian doctrines and in comparing and harmonizing Platonism, Aristotelianism, and atomism. Moreover, it expressed strong encyclopedic ambitions, whose leanings towards the new science were coupled with the aspiration to moral, political, and religious reform.[45]

In its short-lived life, the Accademia promoted Galileo's work by publishing his *Assayer* and his *Letters on Sunspots* along with Stelliola's *Telescope*.[46] Whether all members of the Lincei shared the same philosophical interests to the same extent is uncertain. However, members of the Lincei certainly seemed to be more interested in what they could achieve through cooperation and assistance rather than through individual accomplishments. In this sense, there appears to have been a certain sense of solidarity among the members. As the total number of members grew, three distinct categories emerged: the *emeritus* who was recognized for his achievement, the *benefactor* who was recognized for his contributions, and the *studiosus* who was usually a promising young student admitted to the Lincei by examination.[47]

Despite the cooperative ventures of the Accademia dei Lincei and the aspirations of its founder, Cesi, the Lincei did not flourish for long. The difficulties encountered by the Lincei began as early as in the first few years of its existence when Cesi's father intervened in the affairs of what he considered to be a suspect organization whose son was a member and that met in his own home.[48] Cesi's father continued to play a role even as late as 1618 when, for economic reasons concerning the Cesi family, he sent his son to Acquasparta, and away from the center of the Lincei circle in Rome. However, this was not the only problem faced by later Linceans. The Linceans, who followed the creed of solidarity, continued to support their

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[44] Biagioli, "Scientific Revolution and Aristocratic Ethos," in Vinti, ed., Alexandre Koyré, 282. In the same article, Biagioli argues that Cesi saw dogmatism as a threat – "a sign of pedantry, that is of lower-class minds who got stubbornly entangled in petty technicalities" (Ibid., 291).

[45] Galluzzi, "The Renaissance Academies," in Frangsmyr, ed., Solomon's House Revisited, 307. Galluzzi argues that these interests remained even after Galileo joined the group in 1611 (Ibid.).

[46] Richard S. Westfall, "Galileo and the Accademia dei Lincei," in P. Galluzzi, ed., Novità Celesti e Crisi del Sapere: Atti del Convegno Internazionale di Studi Galileiani (Firenze: Giunti Barbèra, 1984), 189.

[47] Hakata, "The Diplomatic Policy of Accademia dei Lincei," 2, 4.

[48] Hakata, "The Diplomatic Policy of Accademia dei Lincei," 3-4. Hakata describes Cesi's father's intervention in detail.

esteemed member, Galileo, even after the condemnation of the Copernican theory in 1616. This did not cast them in a favorable light among outsiders. These difficulties were somehow endured by Lincean members, however, and the group continued to exist, although not for much longer. The end came after Cesi's death in 1630. With the academy's failure to elect a new president, and with its illustrious founder gone, it "lost its organization and gradually disappeared."[49]

Accademia del Cimento

Ornstein begins the second chapter in her The Role of Scientific Societies in the Seventeenth Century with the following reference to the Accademia del Cimento:

> Italy was the home of the first organized scientific academy, the Accademia del Cimento of Florence (1657-67). It illustrates more perfectly than any other the functions of such societies as centers of the cultivation of experiment. Here nine scientists, supplied with the means of scientific research, gave ten years of united effort to the elaboration of instruments, the acquisition of experimental skill, and the determination of fundamental truths: so completely were their efforts welded together that their work was sent into the world like that of a single individual; so exhaustive were their labors that the book they published became the "Laboratory Manual," so to speak, of the eighteenth century, and their own work and methods the model and inspiration of other learned societies.[50]

This "first organized scientific academy" was undoubtedly influenced by its predecessor, the Accademia dei Lincei, although the two differed in many ways despite the common scientific interest.[51]

The founders of the Accademia del Cimento were two prominent Italian nobles, the Grand Duke Ferdinand II de Medici (1610-1670) and his brother, Leopold de Medici (who also happened to be a scientific enthusiast himself), and it was in the residence of Leopold that most of the meetings were held. Although it may have been originally established by the Medici

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[49] Hakata, "The Diplomatic Policy of Accademia dei Lincei," 8, 10.
[50] Ornstein, The Rôle of Scientific Societies, 73.
[51] The main work on the Accademia del Cimento is W.E. Knowles Middleton, The Experimenters: A Study of the Accademia del Cimento. An older work with some information on the Cimento is Maugain, Étude sur l'Évolution Intellectuelle de l'Italie de 1657 à 1750 Environ. Concerning the influence of previous academies on the Cimento, Middleton states that among all the various academies and societies in Europe at the time, "the only two that are in the least likely to have influenced the founding of the Accademia del Cimento are the Accademia dei Lincei and the Accademia degli Investiganti," although even these two had little influence (Middleton, The Experimenters, 9).

brothers, Galileo served as the Cimento's "spiritual father."[52] Other members included Borelli (1608-1679), Vincenzo Viviani (1622-1703), Candido and Paolo del Buono (1625-1659), Lorenzo Magalotti (1637-1712), Alesandro Marsili (1658-1730), Francesco Redi (1626-c. 1698), Antonio Olivia, and Carlo Renaldini;[53] but if we include the correspondents to the Cimento, the number of individuals involved with the Cimento is impressive. An only partial list that reads like a who's who of famed European scientific practitioners includes names such as Hevelius, Bouillau, and Huygens. From what we know of the Cimento's daily operations, it had only one officer, the secretary, Magalotti, but unfortunately, no evidence has been uncovered concerning the formal rules of governance. Despite the lack of information, however, the Cimento had "something much more valuable than rules – a well-equipped laboratory; not to mention an apparently inexhaustible source of apparatus and materials."[54] Between 1657 and 1667, the meetings of the Cimento were sporadic because "activity seems to have come in bursts, interrupted for various reasons."[55]

The word "cimento" means "experiment," and the Cimento's motto "Provando e Reprovando" fits the interests of its members. It is not clear who first suggested the motto, but its intention was to convey a sense of the idea "by trial and error" or "testing and re-testing." The agenda of the Cimento stemmed from the members' early desire to refute the physics and astronomy of the Aristotelians. In his work on the Accademia del Cimento, Middleton argues that the influence of the Cimento group was in essence "neither Baconian nor Boylean. . . [but rather] a desire to continue the work of Galileo in demolishing the authoritarian pseudo-science of the schools."[56] This "desire" to put the extent of Galileo's experimental methods to work has been challenged in more recent scholarship, however. According to Galluzzi, for example, the sources reveal that "the experimentalist option was not freely decided upon by the fellows of the Cimento. It was rather imposed by Leopoldo and Ferdinand II dei Medici who intended to make it clear that the academy was not going to deal with delicate philosophical issues."[57]

The Cimento's avoidance of such "delicate issues" was successful, and in fact, they excelled in their quest for experimentation as evidenced in their work, the *Saggi di naturali*

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[52] Ornstein, The Rôle of Scientific Societies, 77.
[53] For a complete list of the few members of the Cimento with a short description of the life and work of each, see Middleton, The Experimenters, 26-39.
[54] Middleton, The Experimenters, 53.
[55] Middleton, The Experimenters, 61.
[56] Middleton, The Experimenters, 333.

esperienza fatte nell Accademia del Cimento (1667).[58] One of the most famous of their "experiments" (not listed in the *Saggi*) was the "testing" of the ring-theory of Saturn. The Cimento's most significant astronomical activity was in fact, study of the planet Saturn. In trying to decide the nature of Saturn's "rings," the Accademia was asked to intervene in order to put the issue to rest. The experiments, which actually involved the building of a model of Saturn and placing it at a certain distance to be viewed through various telescopes, followed on the heels of Huygens' publication, *Systema Saturnium*. It was the Cimento's task to determine whether Huygens' ring-theory was correct, or whether his challenger's (Honoré Fabri [1607-1688], a Jesuit astronomer using Divini telescopes) theory was correct.[59] At the end of the "experiment" with the model, Fabri came out the loser, but unfortunately for Divini, the reputation of his telescopes suffered even more. Regardless of the outcome, however, the Cimento's attempt to prove or disprove Huygens' ring-theory is an example of the "sophistication to which the experimental method had risen in Florence by 1660."[60] Furthermore, Fabri's involvement in this and other issues serves as an example that not all those associated with the Accademia del Cimento were in agreement with its programme. Fabri represents what one may call the "responsible opposition, conservative savants and pedagogues sincerely interested in their subjects, but unable, for one reason or another, to accept the entire program of the scientific revolution."[61]

The Cimento's one official, the secretary Magalotti, may be reminiscent of Henry Oldenburg, who served as the Royal Society's secretary. However, neither Magalotti nor anyone else could compare to Oldenburg; nor did the Accademia del Cimento have anything that compared to the Royal Society's *Philosophical Transactions*. Nevertheless, members of the Accademia made certain attempts to exchange official correspondence with both Parisian and

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[57] Galluzzi, "The Renaissance Academies," in Frangsmyr, ed., Solomon's House Revisited, 310.
[58] A complete translation of this work can be found in Middleton, The Experimenters, 83-254.
[59] See Van Helden, "The Accademia del Cimento and Saturn's Ring," 237-259; and his "Eustachio Divini Versus Christian Huygens: A Reappraisal," 36-50.
[60] Van Helden, "The Accademia del Cimento and Saturn's Ring," 259.
[61] John L. Heilbron, "Honoré Fabri, S.J., and the Accademia del Cimento," in XIIe Congrès International d'Histoire des Sciences, Paris 1968: Tome III B Science et Philosophie XVIIe et XVIIIe Siècles (Paris: Librarie Scientifique et Technique Albert Blanchard, 1971), 47. Heilbron explains that this group "sought not to destroy the new philosophy, but to channel it, to adapt and promote it, and to reconcile it with what they deemed valuable in the old" (Ibid.).

London scientific circles.[62] In fact, correspondence between scientific circles began on the Continent before it did in England, especially in the case of Prince Leopold, whose interest in astronomy led him to a fervent correspondence with Boulliau from 1649 to 1672, and fruitful correspondence with individuals such as Huygens and Hevelius in the 1660's.[63] There is also evidence that the Accademia del Cimento communicated specifically with the French Montmor academy through the French correspondent, Melchisédech Thevenot (c. 1620-1692).[64] Despite the attempts at correspondence with other like-minded individuals in other countries, the Accademia del Cimento did not make a significant impression on French and English scientific societies. The Académie des Sçiences, for instance, does not even mention the Cimento's *Saggi*, and in England, "the Royal Society was polite but unenthusiastic."[65] This does not detract, however, from the fact that the Cimento group made certain efforts at exchanging correspondence with other scientific circles in an attempt to accumulate and disseminate scientific knowledge. They had the inclination to communicate with others outside of Italy, and even their noble patrons, the Medici brothers, carried on scientific correspondence, setting an example for the members.

Unfortunately for the Accademia del Cimento, its agenda contributed to its decline and the subsequent disbanding of the members. The activities of the Cimento focused on experimentation and observations, but the Cimento group had so "limited" itself in this way, that it seems that experimentation was over-emphasized and there was hardly anything among the group's activities that could count as speculation or theory.[66] This self-induced limitation, alone, would not necessarily have led to the Cimento's decline. However, of even more stress to the Cimento were the problems among the members of the group, which for a small circle like the Accademia del Cimento, were disastrous. Among typical squabbles between the members was the clash of personalities and opinions between two members in particular – Borelli and Viviani. This situation was worsened by the fact that Borelli was perhaps the most accomplished of the Cimento's members, and consequently, was indirectly tied to its success. The end result of the

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[62] Galluzzi, "The Renaissance Academies," in Frangsmyr, ed., Solomon's House Revisited, 314. Harcourt Brown claims that the "chief link between the academies of Italy and those of Paris was Nicolas-Claude Fabri de Peiresc" (Brown, Scientific Organizations, 3).
[63] Middleton, The Experimenters, 296.
[64] Middleton, The Experimenters, 299. Middleton writes that this correspondence "took place indirectly, through Rome" (Ibid.). The Montmor academy is discussed later in this chapter in the section on French scientific circles.
[65] Middleton, The Experimenters, 335.
[66] Middleton, The Experimenters, 345.

quarrel between these two "was to discourage Prince Leopold, whose continued interest was essential to hold the centrifugal talents together."[67] Furthermore, Middleton adds, "I think that we may safely conclude that the Accademia del Cimento came to an end simply because there were not enough men in the court with an urge to carry on its work."[68] In 1667, the Cimento group was dissolved, and as Ornstein concludes:

> It is true that with the extinction of the Accademia del Cimento, Italy's leadership in physics had ceased. Her one-time preeminence in matters scientific reduced itself to furnishing Europe with the best telescopes and microscopes, and for a century Italians were the most famous makers of thermometers, and mechanical and optical instruments. But it would be drawing an unwarranted conclusion to say that overemphasis on experimentation caused this. Indeed, the emphasis on experimentation, elaboration of instruments, and experimental methods was a *conditio sine qua non* of scientific progress, and if the Accademia del Cimento stood for this, it certainly merits an important place in the history of experimental science.[69]

Middleton and Ornstein may disagree with whether or not experimentation was overemphasized, but it is clear that after the dissolution of the Accademia del Cimento, Italy fell rapidly behind England and France in scientific matters. Nevertheless, the desire for scientific communication beyond the borders of Italy briefly flourished.

France

In this section, I focus on several French circles that illustrate the existence and efficient level of communication of such groups before the formation of the Académie des Sçiences in Paris in 1666. This communication frequently crossed international boundaries, and was encouraged by the frequent travels of scientific practitioners throughout Europe. The incentive and relative success of the epistolary exchange of scientific information indicates a strong sense of continuity from the end of one century to the end of the next. In the case of France, this

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[67] Middleton, The Experimenters, 316. For details on the quarrel between Borelli and Viviani, see Middleton, The Experimenters, 313-314. Interestingly, Ornstein adds that "Borelli was the only member who revolted against merging his identity with his fellow-workers in the Cimento, and published widely outside its publications" (Ornstein, The Rôle of Scientific Societies, 79-80).
[68] Middleton, The Experimenters, 327.
[69] Ornstein, The Rôle of Scientific Societies, 90.

continuity prevailed throughout the course of the seventeenth century, and the sophisticated exchange of information was not simply a product of the Académie des Sçiences and the *Journal des Sçavants*.[70] Following the section on Mersenne, his correspondence, and his "group," I then discuss the *Cabinet* of the Dupuy brothers. In the last sub-section on French scientific circles, I mention two smaller "academies" – the Académie des Castres (an example that not all scientific circles flourished in Paris) and the Montmor Academy (which may have been an influence on the Académie des Sçiences).

Mersenne, His Correspondence, and His "Group"

Marin Mersenne's overall impact on the dissemination of scientific knowledge throughout Europe can in no way be underestimated. Historical scholarship of his life and work is usually favorable and laudatory.[71] A partial list of specific activity involving Mersenne's collaboration with others includes the following: he helped see Descartes' work into publication between 1632 and 1637, he played a major role in the diffusion of Galileo's work from 1634 to 1639, he maintained important correspondence with Fermat (1601-1665) from 1638 to 1640 and helped promote Fermat's work in Italy in 1645, he introduced the barometric work of Toricelli to French scholars after his return from Italy, and he had an "enormous correspondence" which

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[70] Frances Yates casts an even wider net on the issue of continuity within the French academies by stating, "French academies have a right to be regarded as, in one of their aspects, early scientific academies – links between the long mediaeval labours on the *Timaeus*, the wide speculations of Renaissance Neo-Platonism, and the seventeenth-century development of the organised scientific academy under royal patronage" [Frances A. Yates, The French Academies of the Sixteenth Century (London: The Warburg Institute, 1947; reprint, Nendeln, Liechtenstein: Kraus Reprint, 1968) 104]. For the influence of Copernicanism on French thought in the sixteenth century in the same work, see Chapter V: "Natural Philosophy in the Academies," 95-104.

[71] Works on Marin Mersenne that were useful for this study include: Jean-Robert Armogathe, "Le Groupe de Mersenne et la Vie Académique Parisienne," Publications de la Société d'Étude du XVIIe Siècle 44 (1992): 131-139; Armand Beaulieu, "La Correspondance du P. Marin Mersenne," Revue de Synthèse: Les Correspondances 81-82 (1976): 71-76; also by Beaulieu, "Le Groupe de Mersenne: Ce que l'Italie lui a Donné – Ce qu'il a Donné à l'Italie," in Geometri e Atomismo nella Scuola Galileiana, eds. Massimo Bucciantini and Maurizio Torrini (Florence: Leo S. Olschki, 1992); and, "Importance du Dialogue: Mersenne et Son Groupe," History and Technology 4 (1987): 351-364; Pierre Costabel, "Le Père Marin Mersenne (1588-1648)," Cahiers d'Histoire et de Philosophie des Sciences: Quelques Savants et Amateurs de Science au XVIIe Siècle 14 (1986): 3-19; H.J.M. Nellen, "Editing 17th-Century Scholarly Correspondence: Grotius, Huygens and Mersenne," Lias 17, no. 1 (1990): 9-20; and Peter Dear, Mersenne and the Learning of the Schools (Ithaca, NY: Cornell University Press, 1984). For the collection of the Mersenne correspondence, see Cornelis de Waard and Paul Tannery, eds., Correspondance du P. Marin Mersenne (Paris: Presses Universitaires, 1945-).

"reveals an expanse of human relations. . . ."[72] It is not surprising that modern scholars praise his accomplishments and claim that he "is responsible perhaps more than any other single person for the establishment of the intellectual centre of Europe in Paris" in the early seventeenth century.[73] Others add that his "connections and correspondence cannot be underestimated as through him science, philosophy, and literature were advanced."[74]

Until his death in 1648, Mersenne was considered Europe's intellectual "secretary," which may seem like an impossible task until we realize that he was assisted by his fellow friars in the order who actually had better writing skills than he did.[75] This does not detract, however, from his willingness to correspond with an impressive number of individuals all across Europe, and in this way perhaps, his correspondence "symbolizes the *European* nature of science."[76] Furthermore, Mersenne's "letter-box" was unique and original in that "it was plugged into a much larger communications network created by print. This larger network had several nodal points – all located outside Paris in northern Protestant regions, beyond the borders of France."[77] One of these regions was England, where Mersenne corresponded with Theodore Haak and his circle of friends. Mersenne's letters to England included all the latest scientific news not only from France, but also from the rest of the European continent. Accordingly, the favor was reciprocated – "Haak served as a source of information and news for the friends of Mersenne in France."[78] Mersenne also had a prolific correspondence with certain Polish intellectuals, but

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[72] Costabel, "Le Père Marin Mersenne," 5. For more on Mersenne's travels in Italy (1644-1645), see Armand Beaulieu's two articles mentioned above, "La Correspondance du P. Marin Mersenne," and "Le Groupe de Mersenne: Ce que l'Italie lui a Donné – Ce qu'il a Donné à l'Italie."

[73] Brown, Scientific Organizations, 32.

[74] Beaulieu, "Importance du Dialogue," 360.

[75] Costabel, "Le Père Marin Mersenne," 4. In fact, Armogathe compares Mersenne with the Royal Society secretary Henry Oldenburg and states that "Oldenburg is not Mersenne" because the term "secretary" implies "memory" which apparently Mersenne lacked (Armogathe, "Le Groupe de Mersenne," 136).

[76] H.F. Kearney, "Puritanism and Science: Problems of Definition," Past and Present July 29 (1965): 108. This article is a response to Christopher Hill's, Intellectual Origins of the English Revolution (Oxford: Clarendon Press, 1965). Further criticism from Kearney towards Hill includes the following statement that will be relevant later in this work: "Mr. Hill's approach, in contrast, is based upon the categories of the nation state. But 'Kepler's Bohemia' and 'Copernicus' Poland' illustrate a type of nineteenth-century concept from which the seventeenth century historian must try to escape. The intellectual Europe of the age of Galileo took no account of later national boundaries" (Kearney, "Puritanism and Science," 108-109).

[77] Eisenstein, The Printing Press as an Agent of Change, 643. Eisenstein adds an interesting argument by claiming that "the mistaken impression that a network centered in Paris provided the foundations for seventeenth-century scientific advance derives from the common practice of detaching early scientific communications and organization from the output of the scientific press" (Ibid.).

[78] Brown, Scientific Organizations, 48, 58-59.

especially the astronomer, Hevelius.[79] In the eighteenth and most of the nineteenth centuries, Mersenne's correspondence unfortunately became scattered and seemed lost but for a few random publications of his letters. Not until the end of the nineteenth century were his letters rediscovered and recognized as an important source of information for the history of science.[80]

For the purpose of defining Mersenne's "group," it is necessary to distinguish between the network of correspondents (also known as the "Academy of Paper"), the group conferences (or lectures) held for the exchange of information, and the "interior sociability" of the holy order within the everyday life of study where there were more scholars.[81] Located chronologically between the Italian academies on the one hand, and the Royal Society and Académie des Sçiences on the other, Mersenne's group in some ways already exhibited certain traits of the later scientific organizations. This included the level of specialization and the work of the community, the focus on questions concerning mathematics and physics, the "mobilization" of many persons, and the frequent use of Mersenne's "we" in his letters of the 1640's for expressing the interests of the group.[82] Some scholars believe that defining Mersenne's group poses certain difficulties, however, when compared to other groups of the period and the later scientific organizations. For instance, the Mersenne scholar Armand Beaulieu argues that it is not easy to clarify what is meant by Mersenne's "group" and that furthermore, it is *not* like other groups in the same period and even less like the Académie des Sçiences. Whatever difficulties may exist in trying to define Mersenne' group, however, Beaulieu maintains that the group "formed a certain unity around the Minim,"[83] which reminds us of the unity and solidarity discussed in the last section on Italian academies. But just as those who are included in a group help to define it, so too, those who are *excluded* also shape a circle of scholars. In the case of Mersenne's group, the one individual who seemed to fit that description was Tommaso Campanella (1568-1639) who, despite his anti-Aristotelianism, was avoided and criticized by Mersenne and others, including Descartes. This is especially peculiar since Mersenne, described

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[79] See Karolina Targosz, "Les 'Polonica' dans la Correspondance de Marin Mersenne des Années 1644-1645," Kwartalnik Historii Nauki I Techniki 24 (1979): 611-622. The article gives a detailed account of Mersenne's correspondence together with some information on his Polish correspondents.
[80] Nellen, "Editing 17th-Century Scholarly Correspondence," 11. Nellen's article emphasizes how modern scholars edit the correspondence of scientific practitioners of the past.
[81] Armogathe, "Le Groupe de Mersenne," 138-139.
[82] Armogathe, "Le Groupe de Mersenne," 136. Armogathe adds, however, that although "this assured the circulation of information" it did so "with some indiscretion due to 'incorrigible gossip'" (Ibid.).
[83] Beaulieu, "Le Groupe de Mersenne," 17-18.

as good-natured and friendly by almost all his correspondents, usually sought *everyone*.[84] In this peculiar way, Campanella "had unwittingly helped a community of scholars define itself by excluding him. Not exactly sure about what they were, the 'new scientists,' by policing themselves in this fashion, were growing ever more certain of what they were not."[85]

Another characteristic of Mersenne' group included the frequent visits by certain members of that group to Mersenne's lodgings, usually to hold meetings or conferences. Mersenne himself was responsible for bringing together, with some regularity, scientific minds such as Gassendi, Girard Desargue (1591-1661), Roberval (1602-1675), Descartes, both the Pascals, "as well as a dozen or more others less well known."[86] Since strangers outside of the order did not have access to the cells of the monks, Mersenne met with them in separate salons that, unlike the cells, at least offered chimneys for warmth.[87] However, Mersenne always received his guests warmly even though they did not meet at regular hours and dropped by unexpectedly during the day instead. Moreover, it is clear that Mersenne cared neither for religious nor nationalist prejudices[88] – further credit to his accomplishments since this served as an invitation and incentive for even more individuals to join Mersenne's group.

Clearly, the influence and impact of Marin Mersenne have been impressive, and he certainly deserves the compliments he has received in recent historical works. Acting as a "bridge" between scientific minds of the early seventeenth-century, his contribution to the science of the times cannot be ignored, even if he never carried out an experiment of his own, or put pen to paper to jot down a scientific theory. His travels throughout Europe, supplemented by the travels made by others to visit him, made the scientific community of Europe a more cohesive group of individuals. Mersenne's group was not the only one of its time, however – there existed others, including the scientific circle run by the brothers Dupuy, known as the *Cabinet* Dupuy, and one of the common denominators between Mersenne's group and the Dupuy circle was the astronomer, Ismael Boulliau.

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[84] See David Allen Duncan, "Campanella in Paris: Or How to Succeed in Society and Fall in the Republic of Letters," Cahiers du Dix-septième Siècle 5 (1991): 95-110.
[85] Duncan, "Campanella in Paris," 107.
[86] Brown, Scientific Organizations, 32. Making the acquaintance of Mersenne during his trip to Paris in 1625 seemed to have been Gassendi's only highlight of the trip. In a letter to the Dutch mathematician Willebrord Snellius, he indicated that French scholars seemed more interested in astrology rather than astronomy, and that there were only a few in number who actually carried out observations [Howard Jones, Pierre Gassendi 1592-1655: An Intellectual Biography (Nieuwkoop: B. de Graaf, 1981), 23-24].
[87] Armogathe, "Le Groupe de Mersenne," 133.
[88] Yates, The French Academies of the Sixteenth Century, 285.

The Cabinet Dupuy and the Boulliau "Circle"

Harcourt Brown describes the creation of the *Cabinet* Dupuy as

> an assembly of men of letters, scholars, amateurs of learning, and professional men, [who] met with regularity in the Hôtel de Thou, on the rue des Poictevins. . . . This, the famous *Cabinet*, at first *du Président de Thou*, later *des frères Dupuy*, was the sturdiest of the private assemblies of Paris, and in some ways the most desirable circle into which a Parisian could obtain entry.[89]

In the more recent work of Robert Hatch, the original Hôtel de Thou is described as "one of the most exclusive centers of scholarly activity in all of Europe," and had the second-largest library after the Bibliothèque du Roi.[90] The Dupuy brothers, Jacques and Pierre, inherited the library after their uncle's death. The intellectual interests of the brothers

> had no limit. During their early years they tended to the Biblioteca Thuana and developed skills as scholars, antiquarians, and historians. Both Jacques and Pierre were regular correspondents of Peiresc and, like their provincial counterpart, enjoyed learned company. [Their] assemblage of savants, poets, philosophers, and amateurs of science would include as many as fifty carefully-selected members. The reputation of the Académie Putéane spread throughout Europe, and its importance as a center of learning and communication cannot be overestimated.[91]

There does not seem to have ever been a list of procedures or rules of governance, however, and as a result, "the membership at any moment could turn the meetings to any end that suited their purposes, with the sole necessity of satisfying the tastes and ideals of their hosts."[92]

In 1636, the Hôtel de Thou (before it became known as the *Cabinet* Dupuy) became the new home of one of Europe's foremost astronomers in the seventeenth century, Ismael Boulliau.[93] Since, as already indicated, the library was impressive, Boulliau enjoyed his stay there, and had access to approximately 8000 books and 1500 manuscripts. In exchange for his stay at the Hôtel de Thou, Boulliau was assigned certain tasks including the development of a

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[89] Brown, Scientific Organizations, 6.
[90] Robert A. Hatch, The Collection Boulliau: An Inventory (Philadelphia, PA: The American Philosophical Society, 1982), xxxii. Hatch's work is rich with information on both the *Cabinet* Dupuy and the intellectual connections of Bouilleau.
[91] Hatch, The Collection Boulliau, xxxiii.
[92] Brown, Scientific Organizations, 9.
[93] For more on Boulliau's correspondence, see Hatch's, The Collection Boulliau; and, Henk J. M. Nellen, Ismaël Boulliau (1605-1694) Astronome, Épistolier, Nouvelliste et Intermédiaire Scientifique. Ses Rapports avec les Milieux du <<Libertinage Érudit>> (Amsterdam & Maarssen: APA-Holland University Press, 1994).

catalog for the library, which, given its impressive size and collection, would not have been easy or exciting. In fact, Boulliau complained of his "distaste and boredom" over the task.[94] Fortunately for Boulliau, these tasks did not seem to interfere with his own research – specifically in astronomy and mathematics.[95] In 1639, he wrote *Philolai sive dissertationis de vero systemate mundi libri IV*, a work "which provided a further defence of the Copernican propositions concerning the cosmic system, a system which had been borne out of Galilei's astronomical observations."[96]

Boulliau's correspondents and acquaintances extend beyond what was to later become the *Cabinet* Dupuy.[97] In some ways, he was like Mersenne, in that he encouraged correspondence, albeit between scholars and himself. But even more than Mersenne, this correspondence was promoted by his extensive travels throughout Europe. This is not to say that Boulliau had his own "group" like Mersenne, but rather, he actively pursued and created his own "circle" of acquaintances and correspondents, and shared information between them. The following examples illustrate this particular point. He traveled around France and met with Parisian astronomers early in his career in the 1620's. Continuing to meet scholars throughout the next two decades (including Gassendi in 1632), he met many more after moving into the Hôtel de Thou in 1636. In the late 1640's, Boulliau made a number of journeys to places such as Italy, Germany, Holland, Poland (where he met Hevelius), and even Constantinople. He even managed to stay in touch with Gassendi, Mersenne and the Dupuy brothers while on his travels. In Italy, he met members of the Accademia del Cimento, and initiated a correspondence with them after his departure; and it was his "extensive correspondence with Prince Leopold [that] opened lines of communication between France, Italy, Holland, and Poland."[98] In 1651, Boulliau spent almost the entire year traveling through Germany, Holland, and even Danzig, making and renewing acquaintances wherever he went, especially in Holland. When he returned to Paris in December of that year, he discovered some rather sad news – his "longtime friend and

[94] See Hatch, The Collection Boulliau, xxxiii f. 54 for the source of the original citation.
[95] Hatch, The Collection Boulliau, xxxiii.
[96] F.F. Blok, "Isaac Vossius and the Blaeus," Quaerendo: Quarterly Journal from the Low Countries devoted to Manuscripts and Printed Books 26 (1996): 78. The translation of the title is *Philolaus or a treatise on the true system of the world in four books*.
[97] The work by Nellen, Ismaël Boulliau (1605-1694) Astronome, Épistolier, Nouvelliste et Intermédiaire Scientifique, provides detailed information on Boulliau and his various correspondents.
[98] Hatch, The Collection Boulliau, xxxix.

colleague," Pierre Dupuy had died.[99] Boulliau moved in with Jacques Dupuy after his brother's death and stayed there until Jacques' death in 1656. During his stay with Jacques Dupuy, Boulliau "maintained an extensive correspondence tying together the corners of learned Europe" by writing to acquaintances including Jacques de Valois (an astronomer and friend of Gassendi's), Hevelius, Prince Leopold, Gassendi, and de Thou, among others.[100] The pattern of traveling through Europe, making and renewing acquaintances, and maintaining those acquaintances wherever he went, was something that Boulliau managed to do for the rest of his life. His impressive correspondence with Hevelius, a correspondence that lasted from 1648 to the mid-1680's, is testimony to his meticulous patience and the desire to share with others the latest scientific information.[101]

Boulliau died in 1694 after a long and productive life of correspondence with other like-minded individuals. His "circle" may not have extended beyond his own interests and pursuits and it may not have been an "official" group. Nevertheless, Boulliau's extensive correspondence with others is impressive, and even if the aim was not to spread information among others but to collect it for himself, Boulliau's letters to and from his peers, like most other correspondence of the period, tells us much about the life and work of these individuals. As Hatch pointedly observes:

> Correspondence – like biography itself – is a personal affair, and each manuscript collection is as unique as the personality it represents. The Collection Boulliau is no exception; it stands as an autobiography of sorts, a record of ideas and activities addressed (by intention at least) to an audience of one. But in cutting across the boundaries of science, philosophy, and practical affairs of state, the [Boulliau correspondence] collects the thoughts of men long dead, individuals divided by generation, class, temperament, and ability. The modern reader might be wary of contradiction, of bold opinions and honeyed words; correspondence (like conversation itself) is tailored to a known audience. But inevitably correspondence reveals. Let the letters speak as they will.[102]

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[99] Hatch, The Collection Boulliau, xli.
[100] Hatch, The Collection Boulliau, xlii-xliii.
[101] See Nellen, "La Correspondance entre Boulliau et Hevelius," Ismaël Boulliau (1605-1694) Astronome, Épistolier, Nouvelliste et Intermédiaire Scientifique, 469-496.
[102] Hatch, The Collection Boulliau, lvii-lviii.

The Montmor Academy and the Académie des Castres

This section on French scientific circles ends with a sub-section on what may be considered France's first two scientific "academies" – the Montmor Academy and the Académie des Castres. The former may have been an influence on the Académie des Sçiences, and the latter shows that not all scientific activity in France necessarily revolved around Paris. The Montmor Academy, certainly the more renowned of the two, was named after its patron, Henri-Louis Habert de Montmor (~1600-1679), who came from an "important Parisian family."[103] Meetings were usually held at his house and included such members as Descartes, Peiresc, Mersenne, Hobbes, and Gassendi. Clearly, many of the scientific minds of seventeenth-century Europe belonged to more than just one group or circle.

With such renowned members, it is important to bear in mind that the Montmor Academy consisted largely of amateurs who consistently met at de Montmor's house in order to share their common interests. According to Brown, however, "for others the academy of Montmor was a stepping stone to a larger organization, the realization of the broadly based ideal of the new philosophers best expressed in Bacon's *New Atlantis* as Solomon's House. . . ."[104] Concerning the "programme" of the Montmor Academy, Brown argues:

> Such was the programme of the Montmor Academy: a dual purpose, the knowledge of the natural world and the advancements of the comforts of life, to be attained through directed curiosity, periodical discourses, and systematic correspondence with scientists out of Paris. . . . *Out of its misdeeds and mistakes grew the conception of the Académie des Sçiences.*[105]

Brown further asserts, "it would seem to be a mistake to regard Colbert's creation as merely a continuance of the traditions of the past."[106] Indeed, most of the members of the Montmor Academy *did not* become a part of the Académie des Sçiences. Huygens, Frenicle, and Roberval are three of only a few to be a part of both. The reasons for the dissolvement of the Montmor Academy resembled those of the Accademia del Cimento:

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[103] Brown, Scientific Organizations, 66.
[104] Brown, Scientific Organizations, 118-119.
[105] (Emphasis mine) Brown, Scientific Organizations, 76.
[106] Brown, Scientific Organizations, 117. On the issue of the connection between the Montmor Academy and the Royal Society, Brown introduces the notion that some scholars have argued in the past that "the Académie de chez Montmor was the original and source of the Royal Society," although he believes that such a statement is "indefinite enough" (Ibid., 91).

> The story of the academy was marred by petty squabbles about personal dignity and disagreement over points of doctrine which rapidly led to insults and offensive behavior. . . . Montmor himself had not lost interest in the sciences of nature, but he must have felt that a renewed academy would stand a better chance of success if it could make a break with the past and its somewhat motley traditions.[107]

Montmor's death in 1679 came thirteen years after the formation of the Académie des Sçiences. By then, he was already familiar with the success and accomplishments of this "renowned academy" even though he never became a member himself.

At the time of the Montmor Academy's existence, there was also the provincial scientific academy, the Académie de Castres that came into being in 1648 but ended in 1670.[108] Originally composed of twenty members, and having at its peak a total of forty-eight, the Académie des Castres was not particularly interested in experimentation, but rather, the discussion of scientific issues; in this way, it cannot be compared to the Accademia del Cimento or the Académie des Sçiences. Nevertheless, it managed to provide a forum for the exchange of scientific information during its brief life.[109] Among its various scientific interests was astronomy, and the members managed to carry out their own observations and even discussed the dimensions of the world according to Copernicus and Galileo. With respect to outside visitors, one of the most notable was Henry Oldenburg who spent the winter of 1658-59 in Castres with some of the members, including Borel, Saporta, and Balthazar, and he continued corresponding with them in the months following his visit. Clearly, even an out-of-the-way scientific circle such as the Académie des Castres had members who were familiar with the more recent scientific works, and was successful enough to attract the attention of foreigners.

In sum, the cases of Mersenne, the *Cabinet* Dupuy, Boulliau, the Montmor Academy, and the Académie des Castres are different in some ways, yet they share a few notable characteristics. Despite the differences, all five cases are evidence that not only were there scientific circles in France before the formation of the Académie des Sçiences, but also that these circles were relatively productive and successful at their peaks. It would not be fair to try to compare them to the creation of Louis XIV and Colbert in 1666, however. Furthermore, they all

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[107] Brown, Scientific Organizations, 133-134.
[108] Pierre Chabbert, "Problèmes Scientifiques Évoqués à l'Académie de Castres (1648-1670)," Actes du 100e Congrès National des Sociétés Savantes, Paris 1975: Les Sociétés Savantes Leur Histoire (Paris: Bibliothèque Nationale, 1976), 21. Chabbert calls it the "oldest [academy] of France" (Ibid.).
[109] Chabbert, "Problèmes Scientifiques Évoqués à l'Académie de Castres," 21, 29.

exhibit to varying degrees a level of sophistication in communication with both fellow countrymen and with foreigners. Their willingness to communicate with others, but also share that information, is impressive even when compared to the later Royal Society and Académie des Sçiences, particularly since the relevant population was notably smaller in number in the earlier half of the seventeenth century. Another characteristic shared among these various circles is the cross-border profile of their members; and in the cases of Mersenne and Boulliau, their tendency to travel throughout Europe. For instance, foreigners had access to the great library of the brothers Dupuy, and even some of the Montmor Academy members were from outside France. Mersenne's group is perhaps the most obvious in crossing national boundaries, as he himself traveled extensively and members of his "group" came from all over Europe. Likewise, Boulliau's travels also helped create his "circle" of correspondents.

England

The English virtuosi shared both similarities and differences with their continental counterparts.[110] Paolo Rossi argues that "the English *virtuosi* of the seventeenth century were the expression of a society that saw its well-being rapidly increase in consequence of the rapid improvement of technics," and he argues even further that "they had a keen passion for science and for the study of nature, but the 'advancement of learning' signified progress in technics and the accumulation of riches."[111] Rossi's argument suggests that the English placed at least as much emphasis on technology as they did on scientific ideas and theories. In fact, the English were very adept at instrumentation and technology, and this passion even surpassed the abilities of those on the continent. The emphasis, moreover, was mathematical, and one sees a proliferation of English mathematical practitioners beginning in the sixteenth century. It should

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[110] Perhaps the most prominent of differences is the issue of religion and Protestantism in England. The Protestant spur in science is addressed in other works: Robert Merton, The Sociology of Science: Theoretical and Empirical Investigations; T.K. Rabb, "Puritanism and the Rise of Experimental Science in England," in The Rise of Science in Relation to Society, ed. Leonard M. Marsek (New York: The Macmillan Company, 1964), 54-67; and Hill, Intellectual Origins of the English Revolution. The argument that Catholicism was just as receptive to science in France and Italy as Protestantism was in England is made in François Russo, "Catholicism, Protestantism, and Science," in Basalla, ed., The Rise of Modern Science, 62-68.

[111] Paolo Rossi, Philosophy, Technology, and the Arts in the Early Modern Era, trans. by Salvator Attanasio and ed. by Benjamin Nelson (New York: Harper and Row, Publishers, Inc., 1970), 122-123.

be noted, however, that English mathematical practitioners were a diverse group and "we should, therefore, not expect to isolate any ideal or archetypical practitioners: there is no single role which encompasses the full range of their backgrounds and livelihoods." However, similarities included their being brought together by the "idea of the mathematicalls," and to a "shared perception of mathematics as a vernacular, practical, accessible, and worldly activity," in which "instruments served as a concrete embodiment of [their] values."[112]

London was the best provider of instrument-makers and printshops in all of England, and it was considered the English epicenter for mathematical practitioners. Not all instrument-makers or mathematical practitioners were located in London or emigrated to London from elsewhere in England, however. Nonetheless, the more "metropolitan" London was a likelier locale for sharing the latest scientific advances and earning an income off the instrument-trade. Those that comprised the "London audience" included not only craftsmen, but also gentlemen, and this served as the equivalent of European court culture on the continent.[113] Furthermore, the English emphasis on instrumentation opened up certain "social spaces" for mathematical practitioners. As Stephen Johnston maintains:

> [Instruments] played a more subtle role in opening up a social space for the mathematical practitioner. [They] figured prominently in his efforts to sanction the intellectual and social elevation of the mathematical practitioner at the expense of the mechanician. The humble mechanician was to have a double dependence on the mathematical practitioner, for the practitioner was both to provide the expertise which the mechanicians lacked and was also to act as an intermediary between mechanicians and their social superiors.[114]

He adds that "the mathematical practitioners were able to persuasively argue that their form of expertise was essential to the state" – that they were indispensable to the government.[115] In such a way, these mathematical practitioners were seen as necessary not only by other scientific practitioners in general, but also by "outsiders" – politicians, members of the court, and those who benefited from their work and instruments, such as sailors, surveyors, and architects.

English virtuosi, like their Continental counterparts, also encouraged each other to correspond on scientific matters. The circles that we see forming even as early as the late

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[112] Stephen Johnston, "Mathematical Practitioners and Instruments in Elizabethan England," Annals of Science 48 (1991): 341.
[113] Johnston, "Mathematical Practitioners," 343.
[114] Johnston, "Mathematical Practitioners," 324-325.

sixteenth century are a testimony to this and taken even further, the English cooperative scientific character exhibited international characteristics, especially in the seventeenth century.[116] The encouragement of scientific correspondence by one scientific practitioner to another is illustrated in a letter from Lydall to Aubrey, dated September 18, 1649:

> I shall bee very happy & willing to preserve continued intelligence with so lively, sincere & ingenious a friend, and that we may exclude idle compliments. I desire you to write what new discovery or experiment you shall chance to make or meet within your studys, & the like (if you approve the motion) I promise to you.[117]

But such encouragement came even earlier for the English in the form of informal scientific circles beginning in the late sixteenth century.

It is apparent that the earlier scientific circles were mathematically-oriented. In later scientific circles of the early to mid-seventeenth century, however, the interests seemed to shift and create more of a balance between various scientific studies, although clearly, emphasis on experimentation as espoused by the Baconians seemed to be on the rise throughout the century.[118] I shall highlight some of the more prominent circles from the late sixteenth to mid-seventeenth century and tie together certain characteristics between English scientific circles with the groups in continental Europe. I begin with discussion of the Thomas Harriot "circle," followed by John Dee and his group, Gresham College and the Greshamites, and lastly, the Gascoigne-Crabtree-Horrox connection.

Thomas Harriot and His "Circle"

The first "circle" for discussion revolved around the prominent English mathematical practitioner, Thomas Harriot. After graduating from Oxford in 1579, he went into the service of the famed Sir Walter Raleigh as his mathematical adviser, and even accompanied Raleigh to

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[115] Johnston, "Mathematical Practitioners," 330.
[116] Dorothy Stimson, "Amateurs of Science in 17th Century England," Isis 31 (1939): 39.
[117] Reprinted in Robert G. Frank, Jr., "John Aubrey, F.R.S., John Lydall, and Science at Commonwealth Oxford," Notes and Records of the Royal Society of London 27 (1972-3): 214; from 304r of MS Aubrey 12, Bodleian Library, Oxford.
[118] This is consistent with Robert Gascoigne's study on the demographics of the scientific community mentioned in the beginning of this chapter.

Virginia in 1585.[119] The move into the Raleigh circle was more than just a wise career choice for Harriot – he also managed to make the acquaintance of some of England's top scientific minds of the time including John Dee, Thomas Digges, and perhaps even William Gilbert (1544-1603).[120] Beyond his strong interest in instruments such as compasses, clocks, and glasses, he was also concerned "with the solution of all outstanding navigational problems," and while at Durham House on the Strand, taught students how to use navigational instruments and tables. Fortunately for scholars, his surviving papers to various individuals, "afford evidence of the way information was compared, studied, and sifted by scholars who stood behind the men of action during the time of maritime expansion."[121]

Harriot's association with Raleigh also led to introductions between Harriot and Lord Henry Percy, Earl of Northumberland, who in addition to being Raleigh's "friend and card-playing associate," was also a patron of learned men.[122] Furthermore, as a noble patron interested in scientific studies himself, he "financed the activities of many learned men, and maintained a library that was considered quite extensive for the day. He owned, for example, works by Bruno, Gilbert, Napier, Kepler, Tycho Brahe, Paracelsus, della Porta, and Alhazen."[123] A more complete list of the "scientific" members of the Northumberland group included Harriot, North, Warner, Torporley, Hill, and Hues. Not only were these members familiar with the Copernican system, but they were also in agreement with it. However, in the case of atomism, some of the members were in disagreement (Torporley and perhaps Hues), proving that not everyone agreed on all issues within this small group.[124]

Harriot's acquaintances extended even further, however, to include Sir Charles Cavendish (fl. c. 1630) and even Kepler. Cavendish met Harriot when he was introduced into the circle, and it was after making this connection that Cavendish also met other prominent

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[119] See Taylor, The Mathematical Practitioners of Tudor and Stuart England, 182-183 for a short biography on Harriot. However, the most extensive of biographies is John W. Shirley, Thomas Harriot: A Biography (Oxford: Clarendon Press, 1983). On Harriot's astronomical work, see Allan Chapman, "The Astronomical Work of Thomas Harriot (1560-1621)," The Quarterly Journal of the Royal Astronomical Society 36 (1995): 97-107. Other articles include Robert Kargon, "Thomas Harriot, the Northumberland Circle and Early Atomism in England," Journal of the History of Ideas 27 (1966): 128-136; and Johs Lohne, "Thomas Harriot (1560-1621): The Tycho Brahe of Optics," Centaurus 6 (1959): 113-121.

[120] Chapman, "The Astronomical Work of Thomas Harriot," 97-98.

[121] Taylor, The Mathematical Practitioners, 183.

[122] Kargon, "Thomas Harriot," 129.

[123] Kargon, "Thomas Harriot," 133.

[124] Kargon, "Thomas Harriot," 135. Details on Torporley's opposition to Harriot (which caused tension within the group) are in Ibid., 135-136.

individuals such as William Oughtred, John Wallis, and John Pell (1611-1685).[125] The Harriot-Kepler correspondence, initiated by Kepler in 1606, could have been beneficial to both men, but unfortunately, it was both brief and unsuccessful. As Shirley states in his biography of Harriot, "The Harriot-Kepler correspondence, that might have brought Harriot into international prominence for his work on optics, appears to have tapered off into generalities without really exciting the interest of either man."[126] The year before Kepler initiated correspondence with Harriot (1605), the Earl of Northumberland was implicated in the Guy Fawkes conspiracy to blow up the Houses of Parliament, and soon thereafter, both the Earl and Harriot were arrested and imprisoned despite the fact that nothing incriminating Harriot had been found.[127] He was eventually released, and the events did not seem to affect his work, but it had been deteriorating since his correspondence with Kepler a year earlier. In the few weeks before his death, he asked his friends to do something "he had been unable to force himself to do, to order his notes, organize them, and see them through the press."[128] Unfortunately, Harriot also requested his executors destroy certain papers which he thought wasteful.[129] Regardless of what survived or was destroyed, Harriot was an influential teacher and scholar to many younger individuals during his lifetime.

John Dee's "Circle"

In the late sixteenth century, the two individuals who promoted astronomical studies more than anyone else in England were John Dee and Robert Recorde. Not only were they familiar with the Copernican system, but they also helped spread it among English virtuosi in the second half of the sixteenth century.[130] Dee's influence over others (like Harriot) stands out as his legacy. On this point, Johnson states, "Dee's great significance in English science is due to his work as teacher, adviser, and friend to most of the English mathematicians, astronomers, and geographers of his day. . . . Dee's [influence] was exerted through his personal advice and

[125] Taylor, The Mathematical Practitioners, 202.
[126] Shirley, Thomas Harriot, 386. Harriot was growing increasingly weaker due to cancer of the lip.
[127] Kargon, "Thomas Harriot," 132-133.
[128] Shirley, Thomas Harriot, 469.
[129] Shirley, Thomas Harriot, 470.
[130] Johnson, Astronomical Thought in Renaissance England, 120.

teaching, and passed on by the pupils he had trained."[131] In greater detail, Shumaker adds that Dee's contributions

> were promotional and pedagogical: he advertised the uses and beauties of mathematics, collected books and manuscripts, and assisted in saving and circulating ancient texts; he attempted to interest and instruct artisans, mechanics, and navigators, and strove to ease the beginner's entry into arithmetic and geometry. It is in this last role, as pedagogue, that Dee displayed his competence, and made his occasional small contributions (which he classed as great and original discoveries) to the study of mathematics.[132]

Dee worked mostly out of his own home at Mortlake near London, and it was here that he had his own library, laboratory, and astronomical instruments.[133] Moreover, his library, "was undoubtedly the greatest scientific library in England, and probably not surpassed in Europe, [and it] was always at the disposal of [his] fellow scientists among his friends and pupils. . . ." Dee and his circle of friends and acquaintances often gathered here where they formed what Johnson refers to as an English "scientific academy." [134] Moreover, Dee brought back the latest mathematical discoveries from scholars of other countries, and his fame is due to "his journeys abroad and his friendship and intimate correspondence with nearly all the leading European scientists of his day."[135]

As the "guiding spirit of the English school of mathematicians," Dee had as his closest English friends (and some of the leading mathematicians of the day), Leonard and Thomas Digges, Thomas Blundeville, and Thomas Harriot.[136] On his travels to Louvain, Brussels, and Paris from 1547 to 1550, he became acquainted with some of Europe's foremost mathematicians, and even managed to remain in correspondence with some of them after his return to England. One of the most notable of his foreign acquaintances was Peter Ramus (1515-1572), whom he

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[131] Johnson, Astronomical Thought in Renaissance England, 137.
[132] Wayne Shumaker, ed. and trans., John Dee on Astronomy. *Propaedeumata Aphoristica* (1558 and 1568), Latin and English (Berkeley, Los Angeles, London: University of California Press, 1978), 17.
[133] Interestingly, Johnson argues, "London, and not the universities, became the scene of scientific study and scholarship in this period" (Johnson, Astronomical Thought in Renaissance England, 137-138). He also argues, "Dee's circle might truly be termed the scientific university of England during the period from about 1560 to 1653" (Ibid., 139).
[134] Johnson, Astronomical Thought in Renaissance England, 138. Dee's circle was both a "university" and an "academy" according to Johnson.
[135] Johnson, Astronomical Thought in Renaissance England, 135.
[136] Johnson, Astronomical Thought in Renaissance England, 135, 136-137. A longer list of names within the Dee circle and those he corresponded with can be found in an article by Francis R. Johnson, "Gresham College: Precursor of the Royal Society," Journal of the History of Ideas 1 (1940): 424-425.

met in Paris and who became an admirer of Dee's.[137] But perhaps the most famed of Dee's acquaintances (although the two never met) was the astronomer Tycho Brahe who, in 1590, sent both Dee and Thomas Digges greetings from the island of Hven in which he referred to both Englishmen as the "most noble, excellent and learned" mathematicians.[138]

Dee, much like Mersenne and Boulliau mentioned in the previous section, was an intellectual who traveled throughout Europe who cultivated (and maintained) various acquaintances. Dee's circle of both Englishmen and foreigners may not have equaled those of Mersenne's in number, but certainly, the quality of those relationships was not superficial. Dee was one of the first *Europeans* to travel throughout Europe and create and maintain a "circle" of mathematical practitioners. In addition to the circles that gathered around these two individuals, there were also more formal attempts to bring England's best mathematicians together for educating others on a wide variety of subjects. Gresham College, which turned out to be a somewhat different type of educated community of scholars, was also a product of the late sixteenth century.

Gresham College and the Greshamites

Years before Oxford or Cambridge established chairs in mathematics, Sir Thomas Gresham – financial adviser to Queen Elizabeth and founder of the Royal Exchange[139] – created a "scientific centre" where a "free atmosphere that enabled like-minded individuals to draw

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[137] Taylor, The Mathematical Practitioners, 170-171. This is from the brief biography on John Dee in Taylor's work.

[138] Taylor, The Mathematical Practitioners, 171; Johnson, Astronomical Thought in Renaissance England, 137.

[139] Among the great number of works on Gresham and the College: Francis Ames-Lewis, ed., Sir Thomas Gresham and Gresham College: Studies in the Intellectual History of London in the Sixteenth and Seventeenth Centuries (Aldershot, Hampshire, UK: Ashgate Publishing Company, 1999); I.R. Adamson, "The Administration of Gresham College and its Fluctuating Fortunes as a Scientific Institution in the Seventeenth Century," History of Education 9 (1980): 13-25; by the same author, "The Royal Society and Gresham College 1660-1711," Notes and Records of the Royal Society of London 33 (1978): 1-21; John William Burgon, The Life and Times of Sir Thomas Gresham, Vols. I-II (London: Robert Jennings, 1839); Allan Chapman, "Gresham College: Scientific Instruments and the Advancement of Useful Knowledge in Seventeenth-Century England," Bulletin of the Scientific Instrument Society 56 (1998): 6-13; Sir Harold Hartley and Sir Cyril Hinshelwood, "Gresham College and the Royal Society," Notes and Records of the Royal Society of London 16 (1961): 124-135; F.R. Salter, Sir Thomas Gresham (1518-1579) (London: Leonard Parsons Ltd., 1925); and John Ward, The Lives of the Professors of Gresham College: To Which is Prefixed the Life of the Founder Sir Thomas Gresham (London: John Moore, 1740), facsimile copy (New York and London: Johnson Reprint Corporation, 1967). For details on the life of Sir Thomas Gresham, see the Ward work, especially 1-32. The Gresham genealogy (written out) can be found in the same work in the Appendix, "V. A Genealogy of the Gresham Family," 11-16. However, a family tree (chart) of the Gresham family is in Burgon, The Life and Times of Sir Thomas Gresham, I, 456-457.

together, receive instruction in the most recent scientific developments, and lay open their ideas and problems to the view and criticism of the Gresham professors as well as other practitioners," was encouraged.[140] According to Johnson:

> The opening of Gresham College was the culmination of a long effort in Elizabethan England to bring about the establishment of a permanent, endowed foundation which would offer instruction and further research in the mathematical sciences and provide a convenient rallying point for all who were concerned with promoting progress in the practical application of these sciences to useful works.[141]

The London and Mercers' Company were responsible for choosing Gresham professors who were usually graduates of Oxford or Cambridge. The professors were expected to lecture in exchange for 50£ per annum and lodgings at Gresham college which included use of the gardens. Of all the stipulations, the one that would have met with the most resistance was that the professors remain unmarried.[142] Gresham professors lectured to anyone who was interested in listening rather than restricting the transmission of knowledge to only a select few.

The group that had perhaps the most detrimental effect on Gresham College was the Royal Society. However, coffee-houses also posed a threat, not only to Gresham College, but also to the Royal Society because "they became the meeting places of choice for scientific instruction, public demonstrations, and even meetings of the members of the Royal Society."[143] In addition to the Royal Society and the coffee-houses, the third reason for the eventual decline of Gresham College was that "by the second half of the seventeenth century the educational task of the Gresham professors was largely complete," and the new mathematicians "were now in a position to sustain the community independent of either the Gresham professors or the universities."[144]

The interests of many of the professors tended toward practice rather than theory, but Feingold argues that they were more than just applied mathematicians – "not only were the Gresham professors akin to university mathematicians in their perception of the 'proper'

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[140] Mordechai Feingold, "Gresham College and London Practitioners: the Nature of the English Mathematical Community," in Ames-Lewis, ed., Sir Thomas Gresham and Gresham College, 174-175.
[141] Johnson, "Gresham College," 423.
[142] For a more detailed list of the requirements of Gresham professors, see Johnson, "Gresham College," 426.
[143] Feingold, "Gresham College and London Practitioners," in Ames-Lewis, ed., Sir Thomas Gresham and Gresham College, 185.
[144] Feingold, "Gresham College and London Practitioners," in Ames-Lewis, ed., Sir Thomas Gresham and Gresham College, 185-186.

relationship between theory and practice, but their writings, too, had an informed readership, not the 'vulgar', in mind even when they published practical treatises."[145] Members of the "Greshamites" included more than just those immediately involved with the College; indeed, the Gresham circle also included "prominent officials, captains, and shipbuilders of the English navy,"[146] as well as mathematicians from Oxford or Cambridge.

One of the most prominent of the College's "members" was Cambridge-educated William Oughtred whose *Clavis Mathematicus* (1631) became famous throughout Europe:

> From Henry Briggs in the 1610's to Christopher Wren and Robert Hooke in the late 1650's, he was admired by a succession of Gresham Professors, as he was by craftsmen. . . . Everybody who was anything in English mathematics between 1600 and 1660 had dealings with William Oughtred.[147]

Described by Aubrey as a "little man" with "black haire, and blacke eies (with a great deal of spirit). . . [whose] head was always working,"[148] Oughtred had his own students (including Seth Ward, Sir Jonas Moore (1627-1679), John Wallis, and Christopher Wren) who lived with him for half a year to learn mathematics, and "he taught all free."[149] His fame as a mathematician even extended beyond England, and "he was more famous abroad for his learning, and more esteemed, than at home. Severall great Mathematicians came over to England on purpose to converse with him."[150]

Because of the practical interests of the Greshamites, they promoted "hands-on" knowledge of instrumentation as illustrated by the work of a couple of the Gresham Professors of Astronomy. One of the most gifted in instrument-making was Edmund Gunter (1619-1626), a graduate of Christ Church, Oxford who had an interest in both mathematical and more practical pursuits. Ward's warm regard for him is evident: "When [Gunter] settled at Gresham College, his diligence in his profession, and the great improvements he made in mathematical science, soon discovered the right judgment of his electors, and how much they had benefited the public,

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145 Feingold, "Gresham College and London Practitioners," in Ames-Lewis, ed., Sir Thomas Gresham and Gresham College, 184. Feingold claims that Adamson exaggerates in the assumption that Gresham professors were not at the forefront of scientific advances and in the practicality of their work (Ibid., 179).
146 Johnson, "Gresham College," 438.
147 Chapman, "Gresham College," 7.
148 Aubrey, Brief Lives, 222.
149 Aubrey, Brief Lives, 223.
150 Aubrey, Brief Lives, 222-223.

in their choice of him. . . ."[151] Samuel Foster actually served twice as Gresham professor of astronomy (1636-1637 and 1641-1652). The first graduate from Cambridge (Emmanuel College) to serve in this position, Foster wrote the popular work, *The use of the quadrant* in 1624. He "quit" in 1637, but continued to pursue his studies afterwards. The issue of why he "quit" raised some speculation, primarily because there was evidence to support that he was released from his position instead because he would not kneel at the communion table when taking communion.[152] Despite the interruption, Foster became "particularly famous for inventing and improving many planetary instruments."[153]

As part of its general purpose, "Gresham College brought mathematics, the language of science, within the reach of common men and harnessed it to their service, and formed a nodal point for the collection, discussion, and systematic examination of scientific problems."[154] This nodal point contributed to the relatively fluid movement of scholars between London, Oxford, and Cambridge. In fact, the "Oxbridge" and Gresham "movement" was typical, especially in the 1650's.[155] In some cases, the astronomy (and geometry) professors would return to Oxford to become Savilian professors in astronomy (or geometry) as was the case for Briggs, Greaves, and Wren.[156] This process of "osmosis" between Oxford, Cambridge, and London suggests that the scientific community in England in the first half of the seventeenth century was not restricted to the active city life of London. Moreover, the universities were not narrow-minded, conservative places where scientific minds were repressed. In fact, Gresham professors were usually products of either Cambridge or Oxford, and in some cases, returned to the universities when a position became available.[157] The Greshamites were the successors of mathematicians such as Recorde,

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[151] Ward, The Lives of the Professors of Gresham College, 78. Biography in Ibid., 77-81.
[152] Joannes Huniades to William Oughtred, 15 December 1637, Rigaud, Correspondence, I, 30.
[153] Ward, The Lives of the Professors of Gresham College, 86. Biography in Ibid., 85-88 (first term), 90 (second term).
[154] Waters, The Art of Navigation in England, 244.
[155] Feingold, "Gresham College and London Practitioners," in Ames-Lewis, ed., Sir Thomas Gresham and Gresham College, 180; and Hartley and Hinshelwood, "Gresham College and the Royal Society," 130.
[156] Chapman, "Gresham College," 7.
[157] Feingold, "Gresham College and London Practitioners," in Ames-Lewis, ed., Thomas Gresham and Gresham College, 180. In the article by Johnson, "Gresham College: Precursor of the Royal Society," he argues that Gresham is the true precursor to the Royal Society, and that Gresham's roots go back further than 1645 and the Oxford group (Johnson also lists those "societies" that were *not* precursors to the Royal Society, 423-424). The view that Gresham was the precursor to the Royal Society, is shared by Brown, Scientific Organizations, 63, in which he states, "The Royal Society grew directly from the meetings of the amateurs of the sciences who met in Gresham college." The argument that there was a connection, although unclear, is made in Hartley and Hinshelwood, "Gresham College and the Royal Society."

the Digges, and John Dee, and they ultimately raised "the level of competency within the English mathematical community."[158]

The Horrocks-Crabtree-Gascoigne "Circle"

In the 1630's and early 1640's in northern England, there existed a relatively small yet important circle of astronomers whose work and instruments changed the course of astronomical practice.[159] Building on the astronomical and mathematical work of both their foreign and English predecessors, their inclination was to repudiate authority by ignoring the ancients and moving beyond even their immediate predecessors whom in some cases, they found inadequate. Their correspondence is also revealing. While Crabtree was at the center of the correspondence, it also extended to other astronomers of the period. Lines of correspondence developed that linked these earlier astronomers to the astronomers of the late seventeenth century, primarily Flamsteed. Consequently, this early circle of north-country astronomers contributed significantly to the astronomical tradition of late-seventeenth century English astronomy.

Current scholarship on the life and labors of Jeremiah Horrocks points out that Horrocks was one of England's greatest astronomers, but until the twentieth century, a relatively obscure figure.[160] Some of the praise for Horrocks even includes the argument that the astronomical "revolution" was already underway during his lifetime and because of his work,[161] and that he was *the* link between Kepler and Newton:

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[158] Feingold, "Gresham College and London Practitioners," in Ames-Lewis, ed., Thomas Gresham and Gresham College, 178.
[159] See Allan Chapman, Three North Country Astronomers (Manchester, UK: Neil Richardson, 1982).
[160] His last name is also spelled *Horrox*. The following is a list of sources on the life and work of Horrocks: Wilbur Applebaum, "Between Kepler and Newton: The Celestial Dynamics of Jeremiah Horrocks," Actes du XIIIe Congrés International d' Histoire des Sciences 5 (1971) 292-299; Wilbur Applebaum and Robert A. Hatch, "Boulliau, Mercator, and Horrocks' *Venus in Sole Visa*: Three Unpublished Letters," Journal for the History of Astronomy October 14 (1983): 166-179; John E. Bailey, "Jeremiah Horrox and William Crabtree, Observers of the Transit of Venus, 24 Nov., 1639," The Palatine Note-Book 2 (1882): 253-266; V. Barocas, "Jeremiah Horrocks (1619-1641)," Journal of the British Astronomical Association 79, no. 3 (1969): 223-226; Allan Chapman, "Jeremiah Horrocks, the Transit of Venus, and the 'New Astronomy' in Early Seventeenth-Century England," The Quarterly Journal of the Royal Astronomical Society 31 (1990): 333-357; Allan Chapman, Three North Country Astronomers; S.B. Gaythorpe, "Jeremiah Horrocks: Date of Birth, Parentage and Family Associations," Transactions of the Historic Society of Lancashire and Cheshire 106 (1954): 23-33; H.C. Plummer, "Jeremiah Horrocks and His *Opera Posthuma*," Notes and Records of the Royal Society of London 3 (1940): 39-52; Colin Ronan, "Jeremiah Horrocks and Astronomy in his Time," Journal of the British Astronomical Association 86 (1976): 370-378; and Whatton, Memoir of the Life and Labors of the Rev. Jeremiah Horrox.
[161] Barocas, "Jeremiah Horrocks," 226.

> At the time of Kepler's death the seeds which he had sown had to all appearances fallen on barren soil more favourable for the cultivation of a crop of weeds. Yet at the same, time, certainly before the *Principia* was written, these precious seeds had been rescued, their true value had been recognized, at least in England, and a wonderful harvest was on the point of being reaped.[162]

Horrocks, together with Boulliau and Nicolaus Mercator (1512-1594), formed a "rare conjunction of post-Keplerian astronomers," which "occurs in a brief exchange of letters in 1659 and early 1660" between Mercator, Boulliau, and Hartlib.[163] In the end, what made Horrocks' life and work even more impressive is that he managed to produce a large amount of astronomical work in a lifespan of only twenty-two years before his untimely death.

After leaving Cambridge, Horrocks performed astronomical observations using measuring instruments with naked-eye sights, and he found discrepancies in the tables of Philip Lansberg (1561-1632) by checking Lansberg's predictions against the actual positions of planets. However, he accepted Kepler's elliptical orbits, and he "accepted Kepler's challenge to astronomers to unify astronomy and physics."[164] Based on these preliminary observations, Horrocks predicted the transits of Venus in the year 1639, and he and Crabtree used Galilean telescopes (without micrometers) to estimate its diameter by comparing it with the Sun's diameter. In fact, Horrocks was the first ever to observe transits of Venus, and since then, only four more have been observed – in 1761, 1769, 1874, and 1882.[165] At the time of the observed transits, he was a curate at the chapel in Hoole, near Liverpool (although there are no records to indicate that he was ever ordained), and it was because of this that he was not able to observe the transit in its entirety. The transit occurred on a Sunday, and he was called away temporarily for religious duties after the transit began.[166]

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[162] Plummer, "Jeremiah Horrocks," 43. He later adds that both Horrocks *and* Hooke were links between Kepler and Newton (Ibid., 52).

[163] Applebaum and Hatch, "Boulliau, Mercator, and Horrocks' *Venus in Sole Visa*," 166. The three letters exchanged between Boulliau, Mercator, and Hartlib are printed in Latin with English translations. Concerning this relatively small correspondence, Applebaum and Hatch conclude (Ibid., 171):

> The manuscripts described above illustrate the inner workings of a little known network of correspondents on astronomical questions, a distinct sub-group within a larger fraternity of scientific correspondents. . . . In the early decades of telescopic astronomy, the foundations were thus laid for programs of systematic observation soon to be formalized in government-sponsored observatories.

[164] Applebaum, "Between Kepler and Newton," 297.

[165] The next one is due on June 7, 2004 (Barocas, "Jeremiah Horrocks," 225).

[166] Ronan, "Jeremiah Horrocks," 375.

The opinions Horrocks held of astronomers and the study of astronomy in general, most of which he discusses in his *Venus in sole visa* are enlightening. For instance, we know which books he owned because they are listed in his work;[167] and preserved in the library of Emmanuel College, Cambridge, is one of his own textbooks in which he listed his astronomical influences including Tycho Brahe and Kepler.[168] Horrocks described astronomers by stating, "we astronomers have as it were a womanly disposition, and are overjoyed with trifles and such small matters as scarcely make an impression upon others; a susceptibility which those who will may deride with impunity, even in my own presence, and, if it gratify them, I too will join in the merriment."[169]

Horrocks promoted and encouraged multiple observations of the same event. On the issue of the testimony of witnesses over the same observed phenomenon, he claimed:

> I not only determined diligently to watch the important spectacle myself, but exhorted others whom I knew to be fond of astronomy to follow my example; in order that the testimony of several persons, if it should so happen, might the more effectually promote the attainment of truth; and because by observing in different places, our purpose would be less likely to be defeated by the accidental interposition of the clouds or any fortuitous impediment.[170]

Indeed, Horrocks was aware of various types of impediments including the possibility of instrumental errors, and in order to minimize such errors, he "applied computed corrections to compensate for eye eccentricity and other quantities."[171] Moreover, he believed that the naked eye could easily deceive an astronomer in his observations because of the "moisture of the beholder's eye" which made bright objects appear magnified against black backgrounds. This could be resolved, however, when an astronomer used a telescope – evidence of Horrocks' "instrumental view of science."[172]

On January 3, 1641, just one day before a scheduled visit to his close friend and confidante, Crabtree in Broughton, Horrocks died unexpectedly. Crabtree lamented his death:

> Mr. Jeremiah Horrocks' letters to me for the years 1638, 1639, 1640 up to the day of his death, very suddenly, on the morning of the 3rd January; the

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[167] Horrocks, *Venus in Sole Visa*, in Whatton, Memoir of the Life and Labors of the Rev. Jeremiah Horrox, 11.
[168] Chapman, Three North Country Astronomers, 9.
[169] Horrocks, *Venus in Sole Visa*, in Whatton, Memoir of the Life and Labors of the Rev. Jeremiah Horrox, 129.
[170] Horrocks, *Venus in Sole Visa*, in Whatton, Memoir of the Life and Labors of the Rev. Jeremiah Horrox, 112.
[171] Chapman, "Jeremiah Horrocks," 342.
[172] (i.e., his support of instrument-use) Chapman, "Jeremiah Horrocks," 345-346.

> day he had arranged to come to me. Thus God puts an end to all worldly affairs. I have lost, alas, my dear Horrocks. Hinc illa lachrimae [thus the tears fall]. Irreparable loss.[173]

In a letter to Gascoigne dated December 6, 1641, Crabtree lamented for Horrocks, "whose immature Death [which was suddenly, and about the Age of 25.] there is yet scarce a Day which I pass without some pang of sorrow."[174] Even as late as 1673, his death at so young an age was observed with sorrow:

> I received these letters ye mention, as also that box, together with the things contained, and particularly Horrocii Posthuma, for which I must acknowledge myself exceedingly engaged to you. I have perused him, and am satisfied with him beyond measure. . . . It was a great loss that he died so young, many naughty fellows live till eighty.[175]

William Crabtree, the common correspondent between Horrocks and Gascoigne, was a man of some means and had at least enough funds to purchase works by Europe's foremost scientific minds including texts by Tycho, Kepler, Galileo, Descartes and Gassendi, and ancients such as Ptolemy and Hipparchus.[176] Born in Lancashire, he lived the longest of the three (he died at age thirty-four) but died in the same year as Gascoigne (1644). The one common denominator between Crabtree and Horrocks (and it probably contributed to bringing them closer together) was that Crabtree had no university education, and like Horrocks, was self-educated in astronomy.

William Gascoigne, the last of the three, seems to have shared the misfortune of Horrocks and Crabtree in that he also died at a young age (thirty-two) when he fell at the Battle of Marston Moor during the English Civil War.[177] After a brief and uninspiring stay at Oxford, Gascoigne left to pursue more advanced astronomical studies on his own. Gifted in the construction and use of astronomical instruments (again, he had invented telescopic sights and the micrometer), he

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173 Cited in Chapman, Three North Country Astronomers, 29.
174 PT, XXX, 609.
175 J. Gregory to Collins, 7 March 1673, Rigaud, Correspondence, II, 248.
176 Chapman, Three North Country Astronomers, 5.
177 The Civil War was very disruptive to the activities of the northern "group". In addition to Gascoigne, Charles Towneley, Richard Towneley's father, also died at Marston Moor, while Christopher Towneley, Richard's uncle, had been captured and imprisoned [C. Webster, "Richard Towneley (1629-1707), the Towneley Group and Seventeenth-Century Science," Transactions of the Historic Society of Lancashire and Cheshire 118 (1966): 61-62]. A Towneley family tree from the early sixteenth to early eighteenth centuries can be found in the Webster article (53). Webster argues that Gascoigne *did not* fall at the Battle of Marston Moor, but rather, at Melton Mowbray in 1645. The mistake, he argues, was originally made by the Towneleys; but Webster claims that he got his

procured his own telescope. Like Horrocks, he also disagreed with Lansberg's tables after carrying out his own observations.

Unfortunately, all three individuals died at very young ages. However, as already indicated, they shared similar sentiments towards the limited mathematical educations they had received, and they were dissatisfied with the more recent astronomical tables. Instead of depending on the work and calculations of others, they were determined to make new calculations and fresh observations The rejection of authority appeared, for instance, in a letter from Crabtree to Gascoigne, reprinted in part in the *Philosophical Transactions* in which Crabtree stated:

> I cannot but commend you, and especially for preferring Reason before any Mans Authority. . . . I do not value the Authority of *Galilaeus* (though reputed the greatest Speculative Mathematician in Europe) nor yet *Kepler* (though *Astronomorum facile princeps*) further than either Demonstrative, or the most probable Reasons confirm their Opinions. Nor will I stick to subscribe to the Man whosoever shall bring better Reasons for his Opinion. . . . so by diligent Inquisition, the desired truth may (may we have that happiness) be better found out by us.[178]

Beyond the repudiation of all authority, they also used instruments, including telescopes, consistently in their observations of the heavens.

In regard to the epistolary exchange between the three, correspondence centered around Crabtree most likely because "Manchester was [more] advantageously placed than the opposite end of the country for communicating with astronomers at a distance; and there was a constant interchange between Crabtree" and others.[179] Horrocks and Gascoigne never corresponded with each other directly, and the three rarely met. This did not prevent them, however, from maintaining a fruitful and prolific correspondence over a period of almost seven years.[180] Even more interesting are the logistics of the postal service in the 1630's and 1640's. The postal service was undoubtedly slow and expensive and in most cases, merchants or other frequent travelers of England carried the mail. Consequently, "how Horrocks, living well off the beaten

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information from "various accounts," and does not cite any sources. Therefore, his argument that Gascoigne fell anywhere other than Marston Moor is questionable.

[178] PT, XXVII, 280.

[179] Bailey, "Jeremiah Horrox and William Crabtree," 263. Bailey suggests that the briefer Horrocks-Crabtree correspondence indicates that it was more isolated than the others.

[180] Chapman, Three North Country Astronomers, 4.

track, managed is not clear, although Crabtree's commercial connections were probably invaluable as a way of keeping in the mainstream of communication."[181]

The Crabtree-Horrocks correspondence began in 1636 with a letter from Horrocks to Crabtree.[182] Crabtree, who was several years older than Horrocks, shared a common interest in astronomy and had already established a "lively scientific correspondence with others," before he began helping Horrocks.[183] In the first few letters, Crabtree and Horrocks corresponded over the errors of Lansburg in the *Tabulae Motuum*, "which gave rise to the substantial body of correspondence which passed between them over the next five years. . . . Their correspondence revolved around three general topics: planetary theory, the lunar orbit, and instrumentation."[184] After determining that Kepler's tables were the most accurate, their aim became to improve on those tables, and they were forever comparing positions predicted in the tables with observed positions.

They were also interested in lunar theory, and even carried out observations of the same phenomena (which is consistent with Horrocks' views on multiple witnesses), such as on March 28, 1637, when they observed the Pleiades together.[185] The last letter to Crabtree from Horrocks is dated December 19, 1640, and in it Horrocks mentioned his intended visit to his friend and colleague on January 4 of the following year, adding, "nisi quid praeter solitum impediat, me tunc expectes."[186] Sadly, however, he never made it, dying by all accounts, one day before the visit to Broughton.

The Crabtree-Gascoigne correspondence began a few years after Horrocks began writing to Crabtree, but Gascoigne had also corresponded with others before he began his epistolary exchange with Crabtree in 1639 or 1640, and continued to do so until his death (other than

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181 Chapman, Three North Country Astronomers, 27.
182 There seems to be some disagreement on the exact date. According to Whatton's introduction, correspondence began in 1636 (18). Likewise, Plummer, "Jeremiah Horrocks," 47, claims the acquaintance was also made in 1636, but suggests that they actually met since Crabtree lived only twenty-four miles away in Broughton, near Manchester. According to (and cited in) Bailey, "Jeremiah Horrox and William Crabtree," 257, 261, the first letter from Horrocks to Crabtree was in June 1635, with a probable visit in January 1641. Chapman, Three North Country Astronomers, 10, claims that how the acquaintance began remains uncertain, although it probably "originated through Dr. John Worthington of Manchester, who is thought to have known Horrocks in Cambridge, and later attempted to rescue Crabtree's papers after his death. At all events, Horrocks was corresponding with Crabtree by June 1636."
183 Ronan, "Jeremiah Horrocks," 372. For a detailed account of both the Crabtree and Horrocks ancestry, see Bailey, "Jeremiah Horrox and William Crabtree," 253-254 (for Horrocks), 262 (for Crabtree).
184 Chapman, "Jeremiah Horrocks," 339.
185 Chapman, Three North Country Astronomers, 7.
186 Cited in Bailey, "Jeremiah Horrox and William Crabtree," 261.

Crabtree, his main correspondent was William Oughtred).[187] Most of the letters that passed between Crabtree and Gascoigne were written between 1640 and 1642, after which the English Civil War interrupted most of the correspondence, especially letters from Gascoigne who served in the Royalist army.[188] We do not know the total number of letters written between the two or necessarily the order that some were written, but according to William Derham who published some of the correspondence in the *Philosophical Transactions*, the first letter from Crabtree to Gascoigne is dated August of 1640 (a reply to Gascoigne's first letter to Crabtree).[189]

By October 1640, Crabtree had already become familiar with Gascoigne's instruments (as a result of a visit to Gascoigne in Yorkshire), and in a very eloquent letter to Gascoigne commented:

> I cannot conceal how much I am transported beyond my self with the Remembrance. . . of those admirable Inventions which you shewed me when I was with you. I should not have believed the World could have afforded such exquisite Rarities, and I know not how to stint my longing Desires, without some further Taste of these selected Dainties. Happier had I been, had I never known there had been such secrets, than to know no more, but only that there are such. . . . I should prize any thing that you should be pleased to communicate to me in those Optick Practices. Could I purchase it with Travel, or procure it for Gold. . . .[190]

By December of 1640, Gascoigne had invented his famous micrometer and wrote to Oughtred that he had stumbled upon a little discovery that would be "able to bring sufficient aid to your aged eyes;" before Gascoigne sent it to anyone else, he wanted Oughtred to examine it.[191] Between December 1640 and December 1641, Gascoigne and Crabtree exchanged a series of letters in which they discussed mainly Gascoigne's invention of the micrometer and telescopic sights. The letters, either in whole or in part, were printed in the *Philosophical Transactions* by William Derham.

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[187] Chapman, Three North Country Astronomers, 15.
[188] The main letters are printed in Latin translation in the beginning of Flamsteed's *Historia coelestis Britannica* of 1725 (Chapman, Three North Country Astronomers, 21).
[189] Mostly on sunspots (PT, XXVII, 280-289).
[190] PT, XXX, 607. As far as instrument-makers are concerned, Gascoigne mentioned something about "workmen" in the North, but there certainly does not seem to have been many of them (Chapman, Three North Country Astronomers, 23).
[191] 2 December 1640, Rigaud, Correspondence, I, 33. Details on Gascoigne and the invention of the micrometer are in the first chapter on instrumentation. Another longer letter on the micrometer from Gascoigne to Oughtred follows in Rigaud, Correspondence, I, 35-59, no date given. On Derham's defense of Gascoigne as the inventor of the micrometer and priority over the French, see PT, XXX, 603-604.

The connection between Crabtree, Gascoigne, and Horrocks came in a letter from Crabtree to Gascoigne in which he told Gascoigne:

> My Friend Mr. Horrox professeth, that little Touch which I gave him of your Inventions, hath ravished his Mind quite from it self, and left him in an Extasie between Admiration and Amazement. I beseech you, Sir, stack not your Intentions for the perfecting of your begun Wonders. We travel with Desire till we hear of your full Delivery. *You have our Votes, our Hearts, and our Hands should not be wanting, if we could further you*.[192]

In another letter to Gascoigne, Crabtree again referred to the micrometer and also mentioned Horrocks' work on lunar theory:

> That which you give me a full Projection of was above my Hope: and if the Screws keep an exact Equality of Motion forward in each Revolve, it is a most admirable Invention; and with the Accommodations,[193] I had almost said without Compare. But that the Divisions of a Circle should be measured to Seconds, without the Limb of an Instrument, or that [measurements] should be taken all at one Moment. . . or that the Diameter of Jupiter should be projected in such prodigious Measures. . . were enough to amuse and amaze all the Mathematicians in Europe, and may indeed be rather a Subject of Admiration than Belief. . . .[194]

In the same letter, Crabtree (through the words of Derham) explained to Gascoigne that his observations were consistent with Horrocks' lunar theory.[195] The last known letter from Gascoigne to Crabtree is dated June 21, 1642 – after that, Gascoigne had little time to write because of his involvement in the Civil War.[196] When he fell at Marston Moor in 1644, he was in the company of the Towneleys – Charles, who died with him, and Christopher, who fortunately survived. Because of the efforts of Christopher Towneley and his nephew Richard, at least some of the papers of the three north-country astronomers survived.[197] Christopher Towneley was

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[192] 28 December 1640, PT, XXX, 608 (emphasis mine).
[193] i.e., telescopic sights?
[194] 6 December 1641, PT, XXX, 608-609.
[195] PT, XXX, 609.
[196] Chapman, Three North Country Astronomers, 29.
[197] Webster proposes an interesting argument involving the Towneley family's Catholicism. He mentions that the Towneleys were members of the Catholic gentry class, which is consistent with their involvement in the English Civil War as Royalists (Webster, "Richard Towneley," 74-75). In the case of Richard Towneley, Webster argues:

> It is only when he is seen as a representative of the Lancashire Catholic gentry that his role in the seventeenth century scientific movement can be assessed. His Catholicism was an important factor in limiting his contacts with the Royal Society; his friends were predominantly Catholic; and, most significantly, his education had given him familiarity with the current intellectual movements in France.

personally acquainted with Horrocks, Crabtree, and Gascoigne, and collected many of their papers after their deaths.[198]

Eventually, Jonas Moore, another northerner and best-known as Flamsteed's patron until Moore's death, became involved in this circle when he began corresponding with Christopher Towneley. Aubrey described him as one who was "inclined to Mathematiques when a boy, which some kind friends of his putt him upon, and instructed him in it, and afterwards Mr. Oughtred more fully informed him; and then he taught Gentlemen in London, which was his livelyhood."[199] Moore and Christopher Towneley established a connection before the English Civil War, when Moore gained access to Christopher Towneley's library in Lancashire where he came into possession of Crabtree's version of the Rudolphine Tables.[200]

It is likely that Moore also knew Horrocks and eventually inherited some of Horrocks' papers and tables. In fact, Wallis claimed that Wren had seen the Horrocks papers in Moore's possession.[201] This was eventually announced to the Royal Society who, desirous of Horrocks' astronomical tables, asked Aubrey to intercede on its behalf and ask Moore to turn the tables over to the group.[202] Apparently, however, Richard Wroe (1641-1718) was also able to trace some of Horrocks' other papers and tables to John Worthington (a northerner from Manchester), who was subsequently asked to turn over what he had of Horrocks' to the Royal Society.[203] The two main published works attributed to Horrocks are his *Venus in sole visa* and the *Opera posthuma*. Hevelius first published the *Venus in sole visa* in 1662, which surprised Flamsteed because he found Horrocks' work not "exact enough" and too outdated to be published.[204] Other papers were received by Sir Paul Neile (1637-1670) who indicated that he would show this work to the Royal Society at the meeting of February 17, 1664. The work was eventually turned over on March 16 instead, and subsequently, the Royal Society loaned the material to Wallis so that

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Moreover, Webster sets out to show that "while the role of Catholics in the English scientific movement was limited, it was not necessarily reactionary" (Ibid., 76).

[198] Webster, "Richard Towneley," 59.

[199] Aubrey, Brief Lives, 209.

[200] Bailey, "Jeremiah Horrox and William Crabtree," 265.

[201] Wallis to Oldenburg, 6 April 1664, OC, II, 164.

[202] OC, II, 165 n. 8.

[203] Willmoth, "Models for the Practice of Astronomy: Flamsteed, Horrocks, and Tycho," in Flamsteed's Stars: New Perspectives on the Life and Work of the First Astronomer Royal (1646-1719), ed. Frances Willmoth (St. Edmundsbury, Suffolk, UK: The Boydell Press, 1997), 53-54. Willmoth's article recounts in greater detail how Horrocks' work survived over the years. Worthington attended the same college as Horrocks (Emmanuel College, Cambridge) in roughly the same years (1632-1635) (as had Wallis and Foster, later Gresham Professor of Astronomy) (Ibid.).

he could inspect the contents. The material was published collectively in 1672 as Horrocks' *Opera posthuma*.[205]

The material that Christopher Towneley had in his possession eventually fell into the hands of his nephew Richard Towneley who was freely allowed to show the letters and papers to visitors. Unlike his uncle, Richard was involved in astronomical matters and he even improved Gascoigne's filar micrometer – his improvement became known as the Townelian micrometer.[206] About the same time Flamsteed cultivated his acquaintance with his future benefactor, Sir Jonas Moore, Flamsteed also performed observations with Richard Towneley, and in visiting Richard, accessed some of the papers and letters of Crabtree, Horrocks, and Gascoigne. The visit in the summer of 1671 was of particular importance to Flamsteed, and he mentioned to Oldenburg that he had been lately to Lancashire visiting Richard Towneley "who hath put some letters of Mr Gascoigne to Mr Crabtree with the answers into [Flamsteed's] hands."[207] Flamsteed believed that Moore had more papers in his possession, and he asked Collins to assist him in procuring the material. Flamsteed was also excited to see a copy of Cassini's *Ephemerides mediceorum syderum* (1668) at Towneley's.[208] Richard Towneley confirmed Flamsteed's visits to Oldenburg the following year and added that on several occasions, Flamsteed had studied the papers of Horrocks and Crabtree, and had copied some work on dioptrics by Gascoigne earlier that year (1672).[209] Unfortunately, many of these papers were subsequently destroyed in a fire shortly after Flamsteed had an opportunity to examine them.[210]

From the 1630's to the end of the century, a group of individuals extends to form a link of astronomical tradition. They communicated to each other by letter, yet "had close enough links with the wider world for their correspondence and other records of their work to be

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[204] Flamsteed to Oldenburg, 13 December 1670, OC, VII, 319.
[205] OC, II, 165 n. 5.
[206] Flamsteed occasionally called it that. It appears as Plate II in OC, III, between pages 290 and 291. Also in PT, no. 29, 11 November 1667.
[207] 1 August 1671, OC, VIII, 179-180. Flamsteed related the whole history many years later to William Molyneux (10 May 1690) [Eric G. Forbes, Lesley Murdin and Frances Willmoth, eds., The Correspondence of John Flamsteed, The First Astronomer Royal. Volume Two 1682-1703 (Bristol and Philadelphia: Institute of Physics Publishing, 1997), II, 420-421].
[208] All in the same letter. Flamsteed's own library before 1685 was impressive, and included works by Horrocks, Harriott, Hevelius, and Oughtred among others [see Eric G. Forbes, "The Library of the Rev. John Flamsteed, F.R.S., First Astronomer Royal," Notes and Records of the Royal Society of London 28 (1973): 119-143]. There is also mention in the *Philosophical Transactions* of another visit by Flamsteed in September of 1671 in which Flamsteed and Towneley observed Mars together (PT, VII, no. 89, 1672).
[209] 30 September 1672, OC, IX, 268, 269 n. 8.
[210] See OC, VIII, 180 n. 3.

preserved and publicized."[211] Ultimately, it was "the thoroughness with which their work was recorded and the enthusiasm with which others then collected, circulated and published the manuscripts [which] made this group of provincial astronomers so influential."[212] A sketch of correspondents and acquaintances in the earlier part of the century can briefly be summarized as follows: Crabtree corresponded with both Gascoigne and Horrocks, although the latter two never corresponded directly with each other; Crabtree also corresponded with Christopher Towneley who eventually acquired the surviving papers of Crabtree, Gascoigne, and Horrocks; Christopher Towneley, in turn, corresponded with Moore and passed down the surviving papers to his nephew Richard Towneley; Richard Towneley became acquainted with John Flamsteed, and Moore became Flamsteed's patron.

Given the views of the original three (Horrocks, Crabtree, Gascoigne), especially those of Horrocks, there was as early as the 1640's, confidence in the new instrumentation (although they still used naked-eye measuring instruments), an emphasis on multiple observers (transits of Venus are rare and bad weather could foil one observer), and the rejection of past tables and measurements that did not agree with their own fresh observations and calculations. When one looks at astronomy toward the end of the seventeenth century, the similarities between the work of Flamsteed and the northern astronomers is striking. There was an emerging astronomical tradition in England that these three individuals were responsible for in many ways.

Conclusions

One of the first conclusions we can draw from the discussion on circles, groups, and societies is that they all shared a quality of "internationalism." This "internationalism" was not imposed, but rather, it came about as a result of the desires of the practitioners who sought it out as a means of bringing the European scientific community closer together. In some cases, this meant certain individuals traveled to other parts of Europe who then brought back the scientific knowledge and theories from foreign countries and introduced it to their own fellow countrymen. Mersenne, Boulliau, Dee, and Savile are examples of this. Travels both within and outside one's

[211] Willmoth, "Models for the Practice of Astronomy," in Willmoth, ed., Flamsteed's Stars, 53.
[212] Willmoth, "Models for the Practice of Astronomy," in Willmoth, ed., Flamsteed's Stars, 55.

home country reinforce the point that there was a certain sense of “fluidity” and movement among individuals. One could be a “member” of multiple circles or groups, and it certainly was to the individual’s advantage to be a part of as many groups as possible in the promotion of his own work. The idea that groups of practitioners inside the universities and those outside of the universities were disconnected or mutually exclusive is a myth. There were, in fact, strong connections between them, and not only were circles outside of the universities usually staffed by university men, individuals within these circles also made a point of staying in constant touch with academic scholars.

Related to this “internationalism” was the “mobilization” of these individuals, which was the purpose of circles, informal groups, and especially correspondence that made it possible to communicate with others on the latest scientific theories and ideas and turned most scientific circles (such as Mersenne’s) into “virtual” circles – when members of any group could not be there in person, they were represented through their work and correspondence. Communication involved collaboration and discouraged individuals from carrying out their work in isolation or seclusion. In such a way, the general interests of practitioners were emphasized more than their private interests.

These networks of correspondence brought about a sense of interconnectedness to these circles, and we see the rise of networks – nodal points – throughout Europe in which most individuals participated. The overlap among various members of the European scientific community even as early as the sixteenth century is significant. What initially began as the cross-currents of information developed into relatively sophisticated scientific circles and groups before the formation of the Royal Society and the Académie des Sçiences. The sophistication of such groups was most obvious in the high frequency of correspondence in the scientific community. Furthermore, as a result of the exchange of information and diffusion of knowledge, groups had the opportunity to be more successful and productive.

The cohesiveness of the scientific community also brought a sense of solidarity to individuals who might otherwise have been threatened by those who did not understand their work or intentions. This solidarity and cohesion was fostered by the encouragement of bright students in the universities to pursue more advanced studies. Practitioners outside of the universities also supported others with similar interests in pursuing mathematics and astronomy. The benefit of this solidarity and cohesion was that it increased what could be done together as a group rather than individually, as illustrated by the example of the north country astronomers who felt like a "united front" against authority. The desire for solidarity extended beyond national boundaries, and there did not seem to be any obvious scientific competition among European scholars. Scientific interests had no national "walls" and made the attempts of like-minded individuals to "band together" on an international level less difficult. Consequently, the nature of the community was such that encouragement and support was always available for mathematical and astronomical practitioners.

Taken together, the "internationalism," mobilization of like-minded individuals, and interconnectedness of groups, support the conclusion that astronomical practitioners in the late sixteenth and early to mid-seventeenth centuries were not lonely figures working in isolation. Instead, they actively participated in the promotion of mathematical and astronomical knowledge through various channels including the universities and the growing number of circles and societies. Being a part of these "nodal points" throughout Europe contributed to defining the role of the astronomical practitioner in the seventeenth century. These networks brought practitioners together "socially," but knowledge about the heavens depended upon comparisons between the results of individuals. Presenting one's observations and measurements to the larger European community fashioned his identity as an astronomical practitioner.

The foundation for an astronomical research "programme" in Europe began as early as the sixteenth century. The seeds were sown throughout Europe for the promotion of mathematical and astronomical studies not only in the universities, but also in the small circles and groups *outside* the universities. Since astronomy was promoted in all "spaces," it was not a barren or marginalized field that began to take shape only in the 1660's, and only after the rise of the scientific societies. Throughout the seventeenth century, astronomical instrumentation continued to evolve and mathematical and astronomical studies were continuously pursued both inside the universities and outside in scientific circles and groups, with relative fluidity and movement between both. Toward the end of the century, however, a conflict of attitudes and "schools" – the Hevelius-Hooke controversy discussed in the next chapter – forced astronomers to redefine and affirm the nature of astronomical practice.

Chapter 4
Instrumentation, Measurement, and Observation After 1660: What the Hevelius-Hooke Controversy Reveals

After 1660, instrumentation, measurement, and observation evolved in fundamental ways that involved significant changes to certain instruments, the nature of measurement, and the methodology of observation. The controversy between Johannes Hevelius and Robert Hooke regarding the use of new astronomical instruments, which ultimately embroiled a larger circle of late seventeenth-century practitioners, has not been given the attention it deserves. Yet, it not only sheds light on the methodology of observation, but it also served as a catalyst for the new astronomical research programme that emerged by the end of the century.

Since the controversy also entailed such issues as witnessing, etiquette of communication, and the role of the Royal Society as a forum in which individuals could express dissent, it is necessary to briefly address these topics. The expressed concern and involvement of participants in the Hevelius-Hooke controversy, in which personality traits and modes of communication were called into question, point to how it extended beyond concerns over technical and scientific arguments. The exchange between Hevelius, Flamsteed, and Oldenburg in the mid-1670's provides a clearer understanding of what were considered the proper avenues and modes of expressing dissent over contested issues. Issues of authority, both personal and communal, witnessing the accumulation of knowledge, and the proper conduct in disputes, add an even richer texture to the controversy. Ultimately, we cannot fully understand the nuances of this case study without weaving together several strands of both an epistemological and sociological nature.

Witnesses, Rules of Etiquette, and The Role of the Royal Society in Controversy

The study of scientific controversies and priority disputes which were fairly common during the latter half of the seventeenth century, benefits historians and sociologists in two fundamental ways. First, controversies usually involve disagreements over issues or agendas that are not supposed to be problematic, let alone settled, and help us see even deeper into these issues.[1] Second, during a controversy, the participants "attempt to deconstruct the taken-for-granted quality of their antagonists' preferred beliefs and practices," although one must be careful, however, not to depend on the testimony of only one side of the controversy.[2] In addition to identifying the intellectual changes and developments in science, the study of scientific controversies also warrants an examination of the social interactions of the participants.[3] Scientific disputes "become an integral part of the social relations between scientists," often guiding their actions and behavior, before, during, or after, a controversy.[4] Social factors play an important part in scientific controversy, but the intellectual factors are more likely to determine the outcome.[5] 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.[6]

Since scientific controversies entail the social interactions of participants, they are characterized by the fact that they are both public (many individuals beyond the immediate participants involved are aware of the controversy) and continuous (they last for a specific length of time). Therefore, although they may begin with only two individuals, they ultimately become

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[1] Using H. M. Collins' metaphor to illustrate this point, "institutionalized beliefs about the natural world are like the ship in the bottle;" scientific controversies allow us to see that the ship was first "a pile of sticks and string, and that it was once outside the bottle (Collins, "The Seven Sexes;" cited in Steven Shapin and Simon Schaffer, Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life (Princeton: Princeton University Press, 1985), 7.
[2] Shapin and Schaffer, Leviathan and the Air-Pump, 7.
[3] 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.
[4] Robert K. Merton, "Priorities in Scientific Discovery," in Storer, ed., The Sociology of Science, 289. Merton refers specifically to priority disputes, but I include all types of controversies.
[5] Ernan McMullin, "Scientific Controversy and its Termination," in Engelhardt and Caplan, eds., Scientific Controversies, 88.

a "community activity."[7] Such "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."[8] According to Shapin and Schaffer, the collective worked together to filter out deviant errors and behavior.

Scientific norms (especially originality) pressure even the meek and unaggressive to assert their claims and press for their own priorities in science.[9] Merton claims that originality is responsible for 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, and these rewards provide further incentive for the pursuit of science.[10] Specifically in the seventeenth century, the "idea of progress became widely current," as a result of the developments in science and technology (instrumentation).[11] Yet the emphasis on originality and the desire to demonstrate new "truths," could lead to what was perceived to 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."[12] If the standards that govern "conflictful interaction" deteriorate, a controversy develops which is "reinforced by group loyalties," and "there develops an atmosphere of thoroughgoing hostility and mutual distrust."[13]

The Royal Society of London, a group that "presented their own community as an *ideal society* where dispute could occur safely and where subversive errors were quickly corrected," produced "matters of fact" (objects of perceptual experience) when "the community freely

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[6] McMullin, "Scientific Controversy and its Termination," in Engelhardt and Caplan, eds., Scientific Controversies, 60.
[7] McMullin, "Scientific Controversy and its Termination," in Engelhardt and Caplan, eds., Scientific Controversies, 51-52.
[8] Shapin and Schaffer, Leviathan and the Air-Pump, 78. Shapin and Schaffer argue that it was Boyle's social technology that made "the production of knowledge visible as a collective enterprise" (Ibid.).
[9] Shapin and Schafer claim:

> [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 (Shapin and Schaffer, Leviathan and the Air-Pump, 292-293).

[10] Merton, "Some Social and Cultural Factors in Scientific Advance," in Science, Technology and Society in Seventeenth Century England (New York: Howard Fertig, 1970), 236.
[11] Merton, "Some Social and Cultural Factors," in Science, Technology and Society, 232, 236.
[12] Merton, "Priorities," in Science, Technology and Society, 311-312.
[13] Merton, "Priorities," in Science, Technology and Society, 314.

displayed its joint assent." According to Shapin and Schaffer, 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 "no isolated powerful individual authority should impose belief;" knowledge came from nature, not from "privileged persons."[14] In fact, true knowledge was only considered objective when it was "produced by the collective, and agreed to voluntarily by those who comprised the collective."[15]

In Leviathan and the Air-Pump, the authors also discuss the concept of "virtual witnessing" – the collective act of witnessing the production of knowledge, an important practice for empirically-based knowledge. 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 met only if one was part of the collective.[16] During scientific controversies, witnesses became allies, usually siding with one of the participants.

Since members argued that disputes could occur safely in the collective, Robert Boyle suggested several rules of etiquette in scientific controversy that had to be observed.[17] 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 to attack [one's] character."[18] One had to be careful not to cross this personal boundary because doing so would risk "making foes out of mere dissenters," something that happened frequently in scientific disputes involving Hooke. Those who could contribute "matters of fact," regardless of their views, 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:[19]

> 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

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[14] Shapin and Schaffer, Leviathan and the Air-Pump, 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).
[15] Shapin and Schaffer, Leviathan and the Air-Pump, 78.
[16] Shapin and Schaffer, Leviathan and the Air-Pump, 336.
[17] Some of these "rules" are in his *Proëmial Essay* (1661).
[18] Boyle stated, "For I love to speak of persons with civility, though of things with freedom" [*Proëmial Essay* (1661)].
[19] Shapin and Schaffer, Leviathan and the Air-Pump, 73.

> 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.[20]

Boyle perceptively noted that this proper protocol was commonly violated, and unfortunately, the individual's character continued to be the subject of personal attacks in subsequent disputes.

In the early years of the Royal Society, 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, the controversy that developed between Hevelius and Hooke deviated from the course of what was considered proper behavior, and instead, precipitated an atmosphere of hostility and mistrust, although there was also a strange mixture of admiration and respect for Hevelius despite his use of naked-eye sights, especially on the part of Flamsteed and Halley. Certain social factors had direct bearing on this controversy including the personal interactions of *all* the participants involved, pressure from the Royal Society to eye-witness claims made by the participants, and hostility and xenophobia among the participants that crossed national boundaries.

Thomas Sprat listed the Royal Society's three goals: collecting data, deciding upon "matters of fact" by relying on the authority of numbers, and "establishing conclusions," a task in which the members were "not yet very daring."[21] 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.[22] What made communication with the wider scientific community novel in this period, was "that there should be a two-way traffic, that the advancement of natural philosophy demanded communication of knowledge as well as the reception of it, that societies

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[20] Boyle, *Proëmial Essay*, 312; cited in Shapin and Schaffer, Leviathan and the Air-Pump, 73-74.
[21] Thomas Sprat, History of the Royal Society, eds. J.I. Cope and H.W. Jones (St. Louis: Washington University Press, 1958), 95-100, 107.
[22] 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): 175. It was only through this time-consuming correspondence that scientists were aware of events elsewhere in scientific circles (Ibid., 176).

would, as much as individuals, thrive on imparting information instead of merely gathering it in. . . ."[23]

Henry Oldenburg thrived on a network of correspondents, and whenever the opportunity arose, he "cast the net of the Society's ideals and influence over all European nations," while retaining a sense of English patriotism without forgetting his own German origins.[24] He even wrote to a compatriot, Leibniz (1646-1716), "You will readily believe, I think, that as I am a foreigner in England I shall speak out without flattering or wheedling the English. There are many men among them excelling in their experienced judgment upon questions of mathematics and mechanics."[25] Ultimately, the novelty of Oldenburg's mode of communication was his systematic method of initiating and maintaining correspondence, using this correspondence to publicly announce discoveries and experiments, and encouraging discussion over disputes.[26]

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.[27] Oldenburg's letters were often read at Society meetings or published as articles in the *Philosophical Transactions*.[28] Astronomers, especially such Frenchmen as Auzout and Cassini, were interested in having their observations published in the *Philosophical Transactions*, and when material on instruments and observations from the *Journal des Sçavants* became available, it too was published in the English journal.[29] Therefore, scientists wrote to Oldenburg when they wanted "to be sure that the attention of the Society as a whole was drawn to their news."[30] 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

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[23] M.B. Hall, "The Royal Society's Role," 175.
[24] Marie Boas Hall, Promoting Experimental Learning. Experiment and the Royal Society 1660-1727 (Cambridge: Cambridge University Press, 1991), 57.
[25] 8 December 1670, OC, VII, 313.
[26] William Eamon, "From the Secrets of Nature to Public Knowledge: The Origins of the Concept of Openness in Science," Minerva: Review of Science, Learning, and Policy 23 (1985): 343.
[27] Rob Iliffe, "'In the Warehouse': Privacy, Property, and Priority in the Early Royal Society," History of Science 30 (1992): 39.
[28] OC, II, xxii.
[29] M.B. Hall, Promoting Experimental Learning, 152.
[30] Marie Boas Hall, "Oldenburg and the Art of Scientific Communication," British Journal for the History of Science 2 (1965): 285.

> the *Philosophical Transactions*, providing a medium for the publication of short scientific papers.[31]

Oldenburg also had at his disposal a network within a network – "a network of courtiers, statesmen, and civil servants across the Continent, the Near East, and the New World [that] provided Oldenburg with the machinery for collecting information and gaining new foreign agents."[32] Such assistance was not always helpful, however, especially against pirates or during wars when ships carrying letters were easily seized and correspondence lost, as a letter from Oldenburg to Hevelius illustrates:

> That letter of yours has also been delivered to me in which you inform me of the capture by our forces of a few ships conveying a great part of your fortune. We are truly tormented by utmost distress that the mad rage of war should violate even harmless lovers of philosophy, nor by our choice would anyone have to entrust his goods to ships and the sea.[33]

As the intermediary through which this correspondence passed, and as the editor and publisher of the *Philosophical Transactions*, Oldenburg was in a powerful, yet often "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 were extremely sensitive to issues of international property rights."[34]

Oldenburg believed that the best way to advance science was by having scientists compare and criticize each other's ideas, and he was even known for egging on his correspondents.[35] Correspondence afforded that opportunity because stating one's views publicly let others know what a particular individual was working on. 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

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[31] M.B. Hall, "Oldenburg and the Art of Scientific Communication," 277. The *Philosophical Transactions* first appeared in 1665, and was Oldenburg's own "private venture" because 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, "The Royal Society's Role," 184-186).

[32] Lewis Pyenson and Susan Sheets-Pyenson, Servants of Nature: A History of Scientific Institutions, Enterprises, and Sensibilities (New York: W.W. Norton & Company, Inc., 1999), 89.

[33] 16 August 1672, OC, IX, 215. This is a reference to the Anglo-Dutch War. Northern German ships were often confused for Dutch ships, and it is believed that the ships being referred to by Oldenburg were overtaken by privateers (see Ibid., n. 3).

[34] Iliffe, "'In the Warehouse,'" 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).

[35] OC, II, xxii-xxiii; and M.B. Hall, "Oldenburg and the Art of Scientific Communication," 287.

theory (the Royal Society would act as the adjudicator in the exchange of correspondence between the two).[36] 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."[37]

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.[38] Furthermore, the social status and manners of the participants mattered as much as any feature of discovery or invention. The Royal Society's function as adjudicator was a distinction recognized not only by its members in England, but also foreigners 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."[39] Ultimately, the Royal Society acted as adjudicator, not only for English, but also for international priority disputes and controversies. In reality, the number of international controversies equaled, if not surpassed, the number of English controversies. The diffusion of scientific information through the Royal Society would not have been complete without the input from foreign scientists. Foreign intellectuals welcomed such correspondence, as many of them felt unappreciated or isolated within their own countries. Such needs contributed to the rise of the Royal Society's popularity and prestige, and this even led some foreigners to introduce themselves to Oldenburg by mentioning how much they admired what they had read in the *Philosophical Transactions*.[40] Oldenburg mentioned this in a letter to Boyle in which he stated, "The fame of ye Society riseth very high abroad, and makes stranger[s] flock hither in troupes, insomuch that since this March I have had no lesse than two douzen travellors addressed to me. . . ."[41] The label of "Fellow" of the Royal Society even lended credibility as demonstrated in a letter from William Molyneux to Flamsteed:

> I have lately received a letter from my ingenious friend Mr. Ashe at Augsburg in Germany. He has made mighty acquaintance in these parts amongst the virtuosi; and he tells me, that the credit of the Royal Society is mighty high there, and that he gained much more respect by the

[36] OC, II, xxii-xxiii; and M.B. Hall, "Oldenburg and the Art of Scientific Communication," 287.
[37] Iliffe, "'In the Warehouse,'" 53.
[38] Iliffe, "'In the Warehouse,'" 36.
[39] M.B. Hall, "The Royal Society's Role," 184.
[40] M.B. Hall, Promoting Experimental Learning, 62.
[41] 30 March 1668, OC, IV, 282.

> character of being a Fellow thereof, than by any deserts he could pretend to, though I know these to be extraordinary.[42]

Because of the high visibility of foreigners, national priority claims became a central issue in any dispute that 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.[43]

Ideally, the Royal Society needed to demonstrate a non-partisan attitude in all international controversies, but by the mid-1660's, Oldenburg clearly sided with the English and was eager to defend English priorities.[44]

When Oldenburg died in 1677, so did the systematic correspondence. The decline in correspondence was not a domestic, but rather an international, problem. In view of the rapid turnover of the office of Secretary, foreigners found it difficult to know to whom they should address their letters.[45] Besides, it was more difficult to cultivate friendships with multiple secretaries. Hevelius, one of Oldenburg's most frequent correspondents, was not even aware that Oldenburg had died until almost a year after the event. Foreigners would have undoubtedly lamented the loss of such a noteworthy and diligent correspondent, not only on a personal level, but also on a professional level, because after Oldenburg's death, it became more difficult to learn of the most recent events and experiments of the Royal Society. In Dublin, William Molyneux felt compelled to ask Flamsteed to impart some news of the current interests of the Royal Society, complaining that "ever since the death of Mr. Oldenburgh, we strangers hear little or nothing of [the *Philosophical Transactions*]."[46]

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[42] 27 January 1689/90 [Eric G. Forbes, Lesley Murdin and Frances Willmoth, eds., The Correspondence of John Flamsteed, The First Astronomer Royal Volume Two 1682-1703 (Bristol and Philadelphia: Institute of Physics Publishing, 1997), 408].
[43] Merton, "Priorities," in Science, Technology and Society, 296. One is reminded of the two priority disputes over the micrometer and telescopic sights that transpired between the English and French in the 1660's.
[44] OC, III, xxv; and M.B. Hall, "Oldenburg and the Art of Scientific Communication," 287. This does not necessarily suggest that Oldenburg always sided with the English, regardless of the issues concerned.
[45] M.B. Hall, Promoting Experimental Learning, 99. Secretaries to the Royal Society included Nehemiah Grew (1677-79), Thomas Gale (1679-81 and 1685-93), Francis Aston, and, of course, Robert Hooke (1677-82).
[46] 25 March 1682 [Eric G. Forbes, Lesley Murdin and Frances Willmoth, eds. The Correspondence of John Flamsteed, The First Astronomer Royal. Volume One 1666-1682 (Bristol and Philadelphia: Institute of Physics Publishing, 1995), I, 871].

Developments in Positional Measuring Instruments and the Rise of Controversy

While telescopes underwent change throughout the course of the seventeenth century, traditional positional measuring instruments also evolved. By the middle of the seventeenth century, astronomers used their various measuring instruments to compute the precise positions of comets as well as the transits of both Mercury and Venus across the sun. By the 1660's, astronomers realized the great potential of replacing the more traditional open, or naked-eye, sights with telescopic sights on measuring instruments, such as quadrants and sextants, because, as Picard and Flamsteed believed, "telescopes mounted on astronomical measuring instruments, [improved] the accuracy of positional astronomy."[47] Although most European astronomers used sextants and quadrants mounted with telescopic sights by the late seventeenth century, the preference for telescopic sights over naked-eye sights became a controversial issue in the late 1660's when certain individuals challenged each other's position. The key participants in this "watershed in positional astronomy"[48] were Robert Hooke in London and Johannes Hevelius in Danzig, with the latter remaining a staunch supporter of naked-eye sights. A brief explanation of the implementation of micrometers and telescopic sights in the 1660's helps clarify the early history of the controversy between the two.

It was not until the late 1660's, after the French had developed Huygens' micrometer (his micrometer was originally a scale in the focus of a telescopic tube),[49] and as a result of a priority dispute, that the scientific community was at last informed about Gascoigne's micrometer.[50] Auzout first disclosed his micrometer invention in a letter to Oldenburg, dated December 18, 1666, in which he described 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 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 assured, not to deceive ourselves in 3. or 4. seconds.[51]

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[47] A. Pannekoek, A History of Astronomy (London: Allen and Unwin, 1961); cited in Van Helden, "The Telescope in the Seventeenth Century," 55.
[48] Van Helden, "The Telescope in the Seventeenth Century," 55.
[49] McKeon, "Les Debuts de l'Astronomie de Precision (I)," 236-241.
[50] Van Helden, Measuring the Universe, 119; and Turner, Early Scientific Instruments, 133.
[51] OC, III, 297.

When this letter was read before the Royal Society, Hooke and Wren declared that Picard and Auzout's method was not original since both Hooke and Wren were already aware of these methods. Since the two had not informed the French of their own methods for measuring the apparent diameters of planets, however, the French did not learn about the Englishmen's work until an extract of Auzout's letter was published in the *Philosophical Transactions*,[52] which "stimulated Towneley to send in an account of Gascoigne's device as improved by Towneley himself."[53] That year, Auzout developed a micrometer that was a variation of Pierre Petit's (1598-1677). It consisted of a series of parallel hairs (or tapered metal bars) on two grids (one fixed, the other moving).[54] After the publication of Auzout's letter, the use of micrometers for measuring small angular distances became widely known, and many astronomers attempted to design their own versions of the same device.[55]

Hevelius first heard about the use of telescopic sights in a letter from Oldenburg in February 1667,[56] although by then, Hevelius' reputation as one of Europe's foremost astronomers had already been established. This reputation arose from the expert use of his wide collection of instruments that were considered by many among the most spectacular astronomical apparatus in Europe. His quadrants and sextants "consisted of a grid of bars and 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."[57] For measuring purposes, Hevelius used the Vernier system, which was more precise than Tycho's method,[58] circular transversals for dividing his instruments into minutes, and an external micrometer screw for dividing them into seconds.[59] Another important characteristic of his instruments was that they were heavily ornamented with figurines and other decorations in the

[52] OC, III, 299 f. 6. The extract was published in the PT, I, no. 21, 373.
[53] Turner, Early Scientific Instruments, 133. This was an extract of Richard Towneley's letter to William Croune (1633-1684), written sometime before May 6, 1667. It was published in the PT, II, no. 25, 457-458.
[54] Auzout's micrometer became standard in France, but did not "catch so quick in England" [Robert McKeon, "Les Debuts de l'Astronomie de Precision (Part I)," 284]. This was not surprising considering the English favored Gascoigne's priority over the micrometer's invention.
[55] Brooks, "The Development of Micrometers," 131, 135.
[56] 27 February 1666/7, OC, III, 354.
[57] Daumas, Scientific Instruments, 48-49.
[58] Bennett, The Divided Circle, 66.
[59] L.C. Béziat, "La vie et les travaux de Jean Hévélius," Bulletino di Bibliographia e di Storia delle Scieze Matematiche e Fisiche 8 (1875): 534-535.

Baroque tradition, a style representative of "an industry that was not yet able to detach itself from the ancient tradition of the craftsman as an artist."[60]

Despite Hevelius' eventual opposition to the use of telescopic sights on measuring instruments, he nevertheless, used telescopes.[61] He used a Galilean telescope for his first major work, *Selenographia* (1647), but substituted the Galilean for the "astronomical" (or Keplerian) telescope, thereafter.[62] Hevelius used his telescopes to observe the moon (Galilean telescope) and the sun ("astronomical" telescope), but he also used them to observe Jupiter's satellites (in order to refute de Rheita's claims that he had found more satellites) and Saturn (that led to his 1656 work, *Dissertatio de nativa Saturni Facie*). These telescopes differed from the astronomical measuring instruments that lacked lenses. Instead of lenses, Hevelius used naked-eye sights, or pinnules, which helped him pin-point particular celestial objects without bringing their images closer. 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."[63]

Tycho and Hevelius shared both differences and similarities between their work and instruments that were significant to the subsequent controversy between Hevelius and Hooke.[64] 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 accuracy 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 and relied more on metals, especially brass, assuming that metals were less vulnerable to bending and warping than wood.[65] Finally, unlike Tycho, Hevelius used an "external" micrometer,[66] and other mechanical aids, to help track celestial objects with more

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[60] Daumas, Scientific Instruments, 48.
[61] 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.
[62] Van Helden, "The 'Astronomical Telescope,' 1611-1650," 35.
[63] Ivan Volkoff, Ernest Franzgrote, and A. Dean Larsen, Johannes Hevelius and his Catalog of Stars (Provo, Utah: Brigham Young University Press, 1971), 23.
[64] 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).
[65] Béziat, "La vie et les travaux de Jean Hévélius," 591.
[66] It was not, in reality, an external micrometer, but a threaded screw used for finely adjusting the alidade across the degree scale (Daumas, Scientific Instruments, 49).

accuracy.[67] 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."[68]

Besides the Tychonic slits, Hevelius and Tycho also shared certain views with respect to observation that were apparent in their work. In 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. 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."[69] By trying to eliminate as many of these "systematic" errors as possible, 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 that might accrue.[70] Despite the progress in instrumentation between the time of Hevelius and Tycho, Hevelius remained, for the most part, an adherent of the Tychonic tradition. What best characterizes him as the last astronomer of the Tychonic school was his continued advocacy of naked-eye sights even though he was aware of the opinions of others who depended on lenses for taking measurements. His deep-rooted conviction to defend naked-eye sights is demonstrated by the sentiment found in his work – "The naked eye is preferred."[71]

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[67] Chapman, Dividing the Circle, 32.
[68] Bennett, The Divided Circle, 66-67; see also J. A. Bennett, "Hooke's Instruments for Astronomy and Navigation," in Robert Hooke: New Studies, eds. Michael Hunter and Simon Schaffer (Woodbridge, Suffolk, UK: The Boydell Press, 1989), 22.
[69] Walter G. Wesley, "The Accuracy of Tycho Brahe's Instruments," Journal for the History of Astronomy 9 (1978): 51-52.
[70] Chapman, Dividing the Circle, 82. One other similarity that is obvious in Hevelius' *Machina coelestis pars prior* (1673), 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 Hven. See Brahe, Tycho Brahe's Description of his Instruments and Scientific Work for the comparison.
[71] "Praestat nudo oculo," from the celestial map of the northern hemisphere in *Firmamentum Sobiescianum*, Gedani, 1690 (Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 46). See also J.D. North, "Johannes Hevelius," DSB, Vol. 6, 364.

The First Stage: “War of Words” in the Correspondence

Despite the fact that correspondence between Hevelius and Hooke concerning telescopic sights began as early as the late 1660’s, their “disagreement [was] so politely handled on both sides that it cannot, at this point, be called a controversy.”[72] Hevelius and Hooke never met face to face, nor did they ever write directly to each other – all their correspondence passed through Henry Oldenburg. This is even more unusual considering that Hevelius directly corresponded with (or met) almost every other individual who was involved in the dispute, including Edmond Halley (1656-1742), Flamsteed, and Wallis.

In 1661, Hooke published a work entitled “Discourse of a new instrument to make more accurate observations in astronomy, than ever were yet made” (1661).[73] No copies have survived, but Aubrey described Hooke’s instrument as one that “performs more, and more exact, then all the chargeable apparatus of the noble Tycho Brahe or the present Hevelius of Dantzick.”[74] This indicates that Hooke was concerned with the precision of astronomical instruments as early 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, when 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.”[75] The only other evidence of correspondence during this year concerning the controversy, 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

[72] OC, V, xxiv.
[73] G. Keynes, A Bibliography of Robert Hooke (Oxford, 1966), 12; cited in Bennett, “Hooke’s Instruments,” in Hunter and Schaffer, eds., Robert Hooke, 21-22.
[74] Keynes, A Bibliography of Robert Hooke, 22; cited in Bennett, “Hooke’s Instruments,” in Hunter and Schaffer, eds., Robert Hooke, 21-22.
[75] 23 July 1665, OC, II, 443. Hooke is promoting here a Baconian point of view that better instruments can make good astronomers out of those with mediocre vision. Hooke later dropped the claim that one can train himself to distinguish less than one minute of arc.

largest instruments made the common way."[76] Hooke reprinted Hevelius' reply against telescopic sights, indicating that Hevelius had responded that same year.[77]

Hevelius and Hooke picked up their correspondence again in 1667 as each furnished his own arguments towards the use of telescopic sights. Hooke indicated to Oldenburg that he hoped Hevelius would consider using telescopic sights on his measuring instruments, and he was even willing to help Hevelius learn more about them, if necessary.[78] 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."[79] Hevelius' letter was read at the Royal Society meeting on November 4, 1667, and Oldenburg communicated Hevelius' request for a description of telescopic sights, along with the date that 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.[80]

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[81] 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.[82]

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[76] Robert Hooke, *Animadversions on the first part of the 'Machina Coelestis' of. . . Johannes Hevelius*, ed. R.T. Gunther, Early Science in Oxford, VIII, The Cutler Lectures of Robert Hooke (London: Dawsons of Pall Mall, 1931; reprint, Winchester: Warren and Son Limited, 1968), 41.
[77] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 41. 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.
[78] c. 20 February 1667, OC, III, 348.
[79] Hevelius to Oldenburg, 11 October 1667, OC, III, 519.
[80] Thomas Birch, 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), II, 209.
[81] Hooke is referring to a quadrant that he himself designed.
[82] 11 May 1668, OC, IV, 396-397.

For his part, Hevelius thanked Hooke by sending him a copy of his *Cometographia*, and requested that Oldenburg find some good, inexpensive lenses of long focal length for him. Responding to Hooke's arguments for telescopic sights, Hevelius argued that telescopic sights are not as reliable as open sights, even if properly mounted. Furthermore, telescopes may shift because they are not "firmly fixed. . . even if they are calibrated with all diligence."[83] Hevelius asked that someone provide him with proof that telescopic sights are more accurate by presenting and comparing with him eight observations of distances (observed using telescopic sights) between certain stars of Hevelius' choice.[84] 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."[85]

In this second stage of correspondence, the disagreement between Hevelius and Hooke became a bitter dispute.[86] 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.[87] 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."[88] Several days later, Oldenburg reported to Hevelius that, after having read the *Cometographia*, Hooke

> 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.[89]

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

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[83] Hevelius to Oldenburg, 3 June 1668, OC, IV, 447-448. This letter was read to the Royal Society on June 25, 1668 (Birch, The History of the Royal Society of London, II, 301).
[84] Hooke never took up Hevelius' challenge to find the eight distances listed. However, Flamsteed eventually made these eight observations and compared them to Hevelius' figures (Béziat, "La vie et les travaux de Jean Hévélius," 617).
[85] Oldenburg to Hevelius, 30 July 1668, OC, IV, 579.
[86] See OC, II, xxii-xxiii.
[87] North, "Hevelius," DSB, 362.
[88] Birch, The History of the Royal Society of London, II, 313.

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."[90] 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 film caused by the "vapor emitted from the mouth and eyes because of the very intense cold."[91] 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."[92]

This latest series of letters, which followed the publication of the *Cometographia*, is an indication of 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."[93] Although Hooke may have 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 because Hooke had still not sent him the "instrument for measuring small distances, promised long ago,"[94] and Hevelius expressed to Oldenburg his impatience with the man who was "all words and no deeds."[95]

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[89] 28 October 1668, OC, V, 115.
[90] 19 November 1668, OC, V, 186.
[91] 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 cited above.
[92] Hevelius to Oldenburg, 11 December 1668, OC, V, 244.
[93] 2 August 1669, OC, VI, 170.
[94] Hevelius first made this request in 1668! Oldenburg observed, "I see that you take it ill" that Hooke has delayed sending this instrument (9 November 1671, OC, VIII, 353).
[95] This phrase has a direct bearing on the controversy in later years. Hooke was frequently accused of this charge.

The Second Stage: From the *Machina coelestis pars prior* to the *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*.[96] The bulk of the book describes Hevelius' various astronomical instruments 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, 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 radius), with a wooden base; a brass sextant (3 feet radius), with a wooden base, requiring two observers; a brass sextant (4 feet radius), with a moving alidade; a wooden quadrant which rested on a vertical axis, and was controlled by cables, pulleys, and counterweights (these made moving the instrument easy); a large wooden sextant (6 feet radius), which had supports for both the observer's arms; a large wooden octant (8 feet 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, 1½, and 2 feet radii; a large azimuthal quadrant (4 feet radius) that Hevelius claimed was easy to use and dependable, adding that after thirty years of use, he had not been able to detect any changes; and finally, a brass sextant (6 feet radius) designed to be used by two observers to measure certain angular measurements between stars (each observer would find one of the two stars).[97] Most of his instruments appear in detailed engravings throughout the text and even his wife and assistant, Elisabetha, appears in two: the engraving with the large brass sextant and the engraving with the large brass octant.

Hevelius' telescopes were as grand as his measuring instruments. His objective when constructing his own telescopes was to simplify previous telescopes that he considered

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[96] Translated from the Latin by Oldenburg in a letter to Cassini, 11 September 1673, OC, X, 188. This work was reviewed in the PT, 22 December 1673, VIII, no. 99, 6171-6172.
[97] Jean Baptiste Delambre, Histoire de l'Astronomie Moderne, II, The Sources of Science Series, ed. Harry Woolf, no. 25 (New York: Johnson Reprint Corporation, 1969), 459-460. See also Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 24, 28.

cumbersome and unwieldy.[98] His largest telescope (described in the *Machina coelestis pars prior*) was 150 feet in length. It was suspended on a 90-foot mast that was deeply set into the ground and held down by four large cables that protected it during storms.[99] The telescope's tube was sectional and he controlled the entire instrument by a system of ropes and pulleys in much the same way as he controlled his measuring instruments. There were several problems with this instrument that made it highly impractical, however. First, one could only observe on the best nights since the slightest breeze caused the whole apparatus to shake. Second, since the planks that supported the sectional tubes were made of wood, they were subject to bending or warping. Finally, because the telescope was so long and consisted of segmented planks, Hevelius found it difficult to constantly align the lenses. Because of these impracticalities, he rarely used this particular telescope.

Of principle concern to the adherents of telescopic sights was Hevelius' chapter on open sights in which he explained his reasons for using such sights and stressed several specific reasons for his disregard of telescopic sights (lenses).[100] First, he argued that lenses cannot be 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." 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.[101] As a result, the lenses needed to be frequently removed and cleaned (which, undoubtedly, interrupted the observer's work).

Hevelius leveled objections, not only against lenses, but also cross-hairs which were usually affixed to telescopic sights used for measurement. He used the same arguments for cross-hairs that he had used for lenses, namely, that cross-hairs easily broke or became disarranged and it was tedious to constantly repair them, and he believed they were nothing more than contraptions that get in the way of an observer and his primary instruments.[102] Hevelius also

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[98] 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.
[99] Prince, The Illustrated Account given by Hevelius, 50
[100] 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 et les travaux de Jean Hévélius," 611-612; Chapman, Dividing the Circle, 38; and King, The History of the Telescope, 100.
[101] Hevelius had previously stressed these objections to Oldenburg.
[102] Hevelius could not tolerate anything coming between his eyes and the celestial objects he measured.

argued that cross-hairs (their intersection) tended to conceal small stars so that an observer had difficulty distinguishing them. One final 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 contributed to inaccurate measurements 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.[103] Although Hevelius took all his objections seriously, some were easily refuted.[104]

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,[105] nor did he send Hooke a copy of the book.[106] The nature of Hevelius' arguments, coupled with these personal slights, were enough to prompt Hooke to retaliate. 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,"[107] and he repeated the experiment a week later.[108] For his part, Hevelius marshaled further arguments against the use of telescopic sights, by contending that large instruments, such as quadrants and sextants, cannot be shifted or inverted so as to test telescopic sights, and besides, no one had yet to prove to him that telescopic sights were useful for observation. Hevelius also argued, for the first time, how one with good eyesight and experience could observe with the

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[103] See Chapman, Dividing the Circle, 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 Connected with the Development of the Telescope," 309).

[104] Hooke, Flamsteed, and Molyneux eventually refuted these objections.

[105] Béziat, "La vie et les travaux de Jean Hévélius," 613-614.

[106] Hevelius to Oldenburg, 13 August 1673, OC, X, 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 had sent him on November 20, 1673 (Birch, The History of the Royal Society of London, III, 110).

[107] Birch, The History of the Royal Society of London, III, 120. Also in R. T. Gunther, ed., Early Science in Oxford, VII, 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.

same accuracy as another using telescopic sights. Finally, he restated his argument for the repeatability of observations, a procedure that was impossible to carry out using telescopic sights.[109]

In addition to the arguments specifically against telescopic sights, Hevelius also argued how other astronomers (including Flamsteed) "have already pronounced their verdict on [his] observations before they have seen them, examined them or known anything at all of them."[110] Hevelius asked his critics to at least "suspend judgment" until after they had gained the necessary experience acquired through years of observation; only then could they sufficiently address these issues.[111] He pointed out, moreover, that it is only fair that one of the adherents of telescopic sights construct a complete star catalog using telescopic sights alone – then 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 such as 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.[112] Hooke retorted forcefully with his, *Animadversions on the first part of the 'Machina Coelestis' of the honourable, learned, and deservedly famous astronomer Johannes Hevelius* (1674),[113] which contains two distinct themes – Hooke's vindication of telescopic sights and his description of an equatorial quadrant and its graduation into degrees.[114]

Nowhere is Hooke's manner of criticism towards Hevelius more apparent than in his *Animadversions*, where he did not limit his censure to naked-eye sights alone, but attacked Hevelius' methods, observations, and instruments in general. Hooke managed to turn the controversy into a personal battleground in which he often ridiculed and mocked Hevelius, such as when he compared the latter's instruments with those used by Tycho Brahe, thereby

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[108] 22 January 1674, Birch, The History of the Royal Society of London, III, 121. Also in Gunther, ed., Early Science in Oxford, VII, 417.
[109] ?16 March 1674, OC, X, 520-522.
[110] OC, X, 521.
[111] Hevelius tried to draw attention to the fact that Hooke was not, by profession, an astronomer, nor had he carried out consistent and systematic observations over long periods of time.
[112] This letter was read during the Royal Society meeting on April 23, 1674 (Birch, The History of the Royal Society of London, III, 133). Also in Gunther, ed., Early Science in Oxford, VII, 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.).
[113] Hooke read this work to members of the Royal Society on December 11, 1673, although it was not published until 1674.
[114] Chapman, Dividing the Circle, 45.

insinuating that Hevelius' instruments were no more advanced than Tycho's.[115] Flamsteed certainly shared the same sentiment for many years as well, but had the tact not to directly insult Hevelius with such a remark. Hooke asserted:

> 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.[116]

Hooke even claimed that the use of plain sights was not only obsolete but also detrimental to astronomical progress,[117] thereby implying that Hevelius' measurements were inaccurate, and that his stubborn use of these instruments impeded the very 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."[118] Moreover, Hooke suggested that Hevelius had "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 I am not able to think of or imagine, I cannot tell.[119]

Needless to say, Hooke pointed out some of Hevelius' virtues, but even these 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.[120]

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[115] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 38. He added that he was "the more sorry to find that [Hevelius] hath proceeded to finish his *Machina Coelestis pars prior*, by instruments not more accurate than those of Ticho. . ." (Ibid., 42).
[116] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford y, VIII, 38.
[117] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 45.
[118] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 77.
[119] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 47.
[120] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 102.

Hooke repeated his claim of how he could 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 asserted (as he had many times before) that "observations made with common sights. . . are no ways capable of certainty to a minute or two."[121] 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 common sights.[122] Hooke also 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,"[123] and that Hevelius' instruments were not exempt from shrinking, swelling, bending, and warping.[124]

Hooke further argued that with his instruments, only one observer was necessary; Hevelius required two observers, and Tycho Brahe required four.[125] It became absolutely necessary, he stated, that there was a "unanimous concurrence" in their measurements, and if even one observer failed to give an accurate measurement, the entire measurement became uncertain and useless.[126] The argument could have been reversed by Hevelius – having more than one observer (which was a part of the Tychonic programme) eliminated the errors which might be made by one observer. 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 *less than half a minute, or thirty Seconds, and hardly one of a hundred can distinguish a minute*."[127] 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. Therefore, even if Hevelius was "one of a hundred," he could *never* accurately discern angles under 30" with open sights, according to Hooke.

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[121] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 43, 79.
[122] 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, The History of the Telescope, 100).
[123] King, The History of the Telescope, 43. Although Hooke's objection was as ostensible as Hevelius', Hooke felt compelled to address each of Hevelius' arguments.
[124] Hooke, *An Attempt to Prove the Motion of the Earth from Observations* (London, 1674). Published in his *Cutlerian Lectures* (see Gunther, ed., Early Science in Oxford, VIII, 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, Greenwich Observatory, 232).
[125] Not all of Hevelius' instruments required two observers.
[126] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 91.
[127] (Emphasis mine) Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 43. See also Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 36.

Like Hevelius, Hooke listed his objections, point by point, in his text. However, he denunciated Hevelius' work, methods, and observations even further by accusing 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.[128] The greatest insult to Hevelius was the comparison of his work and his instruments to Tycho Brahe's.[129] Hooke argued that because Tycho's instruments were as large as Hevelius', the measurements of the former were just as accurate.[130] Hooke even claimed that Tycho's large wooden quadrant and mural quadrant were generally better than Hevelius' instruments and Tycho's mural quadrant recorded more accurate measurements than Hevelius' instruments had,[131] 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 escalated.

In the second half of his *Animadversions*, Hooke described an equatorial quadrant of his own design that he believed greatly surpassed any instrument of Hevelius' making. 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 the need for adjustment by the observer.[132] Although Hooke's instrument was never built, these important innovations eventually influenced instrumentation in astronomy.[133] 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.[134] Wallis believed that Hooke had

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[128] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 38.
[129] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 38.
[130] In reality, Hevelius' measurements were more accurate than Tycho's (Béziat, "La vie et les travaux de Jean Hévélius," 591).
[131] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 70.
[132] See Bennett, The Divided Circle, 68-69.
[133] Bennett, "Hooke's Instruments," in Hunter and Schaffer, eds., Robert Hooke, 29.
[134] Wallis and Hevelius had been friends since the 1630's when Hevelius met Wallis in England.

been too "harsh," and he was incensed over Hooke's publishing a statement of his against Hevelius in the *Animadversions*.[135]

Overall, Hooke was correct *in theory* when he claimed that telescopic sights were capable of greater precision than open sights. However, he "underestimated the accuracy of naked-eye observations," and especially Hevelius' observational skills.[136] Hevelius' powers as an observer were responsible for his success at accurate measurements, regardless of his methods and instruments. Hooke also "overestimated the accuracy that could be achieved with telescopic sights by the contemporary observers."[137] Hooke argued that he could make observations with telescopic sights that were at least ten times better than those made with open sights,[138] but he never proved his claim, while Hevelius' observations remained almost as accurate as Halley and Flamsteed's (Flamsteed attained an accuracy of 10-15 arcseconds, while Hevelius attained an accuracy of about 30 arcseconds). Proof that telescopic observations supplied more accurate measurements than open sights did not become apparent until years after Hevelius' death, when Flamsteed completed his new star catalog.[139]

Hevelius' reaction to Hooke's mockery of his observations and instruments was to write an impassioned letter to Oldenburg informing him how he suffered at Hooke's insults. He began by declaring that Hooke should not have written in English as he wasted both Hevelius and his friends' time in trying to translate Hooke's work.[140] Hevelius 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.[141]

[135] 11 January 1675, OC, XI, 157.
[136] Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 45.
[137] Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 45-46.
[138] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 46. At other times, he claimed he could make observations that were forty times more accurate.
[139] OC, XI, xix-xx; and Chapman, Dividing the Circle, 32.
[140] Hevelius may have also been sensitive to the fact that by publishing in the vernacular, it could be read by a wider audience.
[141] (Emphasis mine) 21 August 1675, OC, XI, 467.

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. . . .[142]

Hevelius proceeded to describe Hooke as a "busybody," who "*labors in vain with words and deeds*," and was interested only in what others were doing, but never improved his own work.[143] Although Hooke continuously boasted about his precision, 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 *personal attacks or stinging remarks*, 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 *Animadversions* 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. . . .[144]

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

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[142] OC, XI, 467.
[143] (Emphasis mine) OC, XI, 468.
[144] (Emphasis mine) OC, XI, 469-471.

Hooke Defends His Actions

The nature of Hooke's objections towards Hevelius is 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 the others involved.[145] Hooke's peers did not fail to notice his disdain towards Hevelius. Wallis claimed that Hooke had unnecessarily attacked Hevelius, and even Newton noted that Hevelius and others had complained about Hooke's hostile disposition.[146] Molyneux also took "Hooke to task" for the way in which he criticized Hevelius,[147] 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.[148] 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.[149] Molyneux agreed with Hevelius when he professed that "it is absolutely intolerable [for Hooke] to promise so much and perform so little."[150]

From the beginning, Hooke felt it was necessary to defend 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

[145] Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 40.
[146] "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, II, (Cambridge, 1960); cited in Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 40].
[147] J.G. Simms, William Molyneux of Dublin: A Life of the Seventeenth-Century Political Writer & Scientist, ed. by P.H. Kelly (Dublin: Irish Academic Press, 1982), 25. Molyneux expressed this sentiment in his *Dioptrica nova*.
[148] Molyneux, *Dioptrica nova*, 230-231.
[149] Read to the Society, February 11, 1684 (K. Theodore Hoppen, ed., The Papers of the Dublin Philosophical Society 1683-1708 (Dublin: Irish Manuscripts Commission, 1983, text-fiche), 605. Molyneux's reference to the *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, ed., The Papers of the Dublin Philosophical Society, 1071; and Simms, William Molyneux of Dublin, 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, ed., The Papers of the Dublin Philosophical Society, 1071).
[150] Molyneux to Flamsteed, 18 April 1682, Simms, William Molyneux of Dublin, 25.

> observators might have been deterr'd from making any use of them, and so the further progress of astronomy might have been hindered.[151]

When the reactions of others (especially Molyneux's) mounted, Hooke found it necessary to fully defend his actions. He recounted the specific objections Molyneux had previously leveled against him in a letter read during a Royal Society meeting.[152] First, Royal Society members argued that what Hooke (and others) claimed was 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 because Hooke had described an instrument which he never constructed). In reply, Hooke accused Hevelius of being the "first Agressor in Print,"[153] and he added that discovering and communicating the truth out-weighed the consequences of diminishing the importance of another's work. Hevelius had done the same with Tycho's measurements and instruments, yet "Hevelius would be thought highly to value Ticho Brahe, and not to have made any Reflections upon him."[154]

Hooke also 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, except that 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

[151] Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 79. He admitted to writing the work as a response against Hevelius' objections.
[152] 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. This is unlikely, however, since there is no mention of Hooke at all in the letter (Birch, The History of the Royal Society of London, IV, 450). If Birch's volumes are accurate, then the letter had to have been read after Hevelius' death.
[153] (i.e., with his *Machina coelestis pars prior*) 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), xvi.
[154] Hooke, The Posthumous Works, xvii.

> others from making use of Telescope-sights, and to make better Observations with Instruments by that means more exact.[155]

Hooke also complained that he was misrepresented when others asserted that he had claimed telescopic sights yielded measurements 60 times more accurate than those using open sights.[156] Lastly, Royal Society members argued that Hooke never took up Hevelius' challenge to carry out measurements using telescopic sights.[157] 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" him from complying with Hevelius' wishes.[158]

The Relationship Between Hevelius and Flamsteed

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 really got along. Although Flamsteed believed Hevelius possessed good instruments and, for a time, argued Hevelius was one to be "emulated,"[159] he claimed telescopic sights provided him with more accurate observations than Tycho Brahe's use of plain sights.[160] 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 naked-eye 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

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[155] Hooke, The Posthumous Works, xvii. Hooke did not have to worry about others using open sights.
[156] I have come across both primary and 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. . ." (Hooke, *Animadversions*, in Gunther, ed., Early Science in Oxford, VIII, 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 was discussing the dimensions of the instruments, not the increase in accuracy between the two types of sights!
[157] Hooke, The Posthumous Works, xvi.
[158] Hooke, The Posthumous Works, xviii.
[159] 24 November 1669, Flamsteed to William Brounker, in Rigaud, Correspondence, II, 81.
[160] Murdin, Under Newton's Shadow, 124. Flamsteed eventually equated the accuracy of Hevelius' observations with Tycho's use of open sights.

> [to] perceive, that his friends noteing them in a civil, may prevent his enemies from commenting on them in a detracting way. . . .[161]

News of Hevelius' plans for a new star catalog had already reached the Royal Society by the early 1670's.[162] Interested in Hevelius' star catalog, Flamsteed eagerly awaited its publication because Tycho's catalog, which was the most current star catalog, 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."[163] Flamsteed repeated this concern in an epistle to Cassini wherein he claimed:

> I hear that the famous Johannes Hevelius has promised [to effect] this reformation,[164] 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.[165]

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

When Flamsteed was appointed Charles II's astronomical observer in 1675,[166] he became interested once more in the polemical exchange between Hevelius and Hooke.[167] Although Hevelius was aware of Flamsteed's preference for telescopic sights, he respected Flamsteed because he viewed him as a "proper" astronomer who regularly performed observations and did

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[161] Flamsteed to Oldenburg, 18 February 1671, OC, VII, 465. Flamsteed may be referring to Hooke in this passage, but he is also referring to himself as he 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, II, 120).
[162] Hevelius' star catalog, which was meant to supersede Tycho's, was not published until 1690, three years after Hevelius' death.
[163] Flamsteed to Oldenburg, 16 November 1672, OC, IX, 327.
[164] i.e., construct a more accurate star catalog.
[165] (Emphasis mine) 5 September 1673, OC, X, 194.
[166] Though Flamsteed was the first "Astronomer Royal," the term was not formally used. He occasionally called himself Astronomicus Regius, but preferred Mathematicus Regius in imitation of Tycho Brahe and Kepler (Frances Willmoth, "John Flamsteed," in Encyclopedia of the Scientific Revolution From Copernicus to Newton, ed. Wilbur Applebaum (New York and London: Garland Publishing, Inc., 2000).
[167] Béziat, "La vie et les travaux de Jean Hévélius," 614. Flamsteed, in his new, more powerful position, no doubt felt that it was necessary to vindicate the accuracy of telescopic sights.

not mock the work of others.[168] Even though Flamsteed respected the labors of Hevelius, however, he felt it was necessary to point out Hevelius' errors that were due to his use of naked-eye sights. Flamsteed, who was aware of Hooke's acrimonious attacks on Hevelius' work, offered "to defend Hevelius against Hooke's criticisms, expecting that the latter will accomplish little by way of solid observation."[169] 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.[170] By January 1677, Hevelius' bitterness towards Hooke's publication, together with Flamsteed's strictures, led to the Royal Society to assure Hevelius "that what Mr. Hooke had published against him, was done without any approbation or countenance from the Society."[171]

A series of letters in 1676 between Hevelius and Flamsteed ultimately forced the astronomers to define the boundaries of proper communication and conduct. Flamsteed, in his own outspoken way, informed Hevelius that open sights were not sufficiently accurate, and Hevelius found himself in a position of having to defend not only his use of common sights, but also what he considered the proper conduct of an astronomer. Until the summer of 1676, it was clear to each side that Flamsteed only used telescopic sights, and that Hevelius, while using the telescope for the purposes of observation, refused to use telescopic sights, even if he had expressed interest in earlier years to see them for himself. Moreover, Flamsteed was aware that Hooke had been quite vocal, and at times, acrimonious, towards Hevelius, which Flamsteed clearly disapproved of. In June of 1676, Hevelius wrote a letter to Flamsteed in which he told the English astronomer that he "would not urge others" who are unable to see small stars, or those who have not been trained to observe stars through naked-eye sights from a young age "to abandon telescopic sights" if those are the sights they are accustomed to.[172] He even reassured Flamsteed that Flamsteed was doing the right thing by "following the path on which [he] first set out." Essentially, Hevelius did not argue against the use of telescopic sights in all cases, because

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[168] Hevelius to Oldenburg, 11 March 1676, OC, XII, 216.
[169] 20 July 1676, OC, XIII, 20 n. 1. The editors have paraphrased the letter from the Royal Society MS.F.1, no. 118.
[170] 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, an object of disagreement between Hevelius and Flamsteed. When compared with Flamsteed's figures, Forbes found that Hevelius was sixty times more accurate than Flamsteed (Ibid., 134)!
[171] 25 January 1677, Birch, The History of the Royal Society of London, III, 332. This decision came three years after Hooke's publication.
[172] 14/24 June 1676, FC, I, 465-472.

as he had indicated to Flamsteed, there are some who are better trained to use either one type of sight or the other. It would only make sense, according to Hevelius, that astronomers should choose and use whatever works best for them on an individual basis.

In the same letter, Hevelius invoked the name of one of his "witnesses," Bernhard Fullenius (1640-1708), and even cited part of a letter from Fullenius to Hevelius in which Fullenius defended the use of open sights. Hevelius later added, "Yet, as they have up till now seen scarcely any of my observations, whilst they themselves have achieved nothing in that field, you can defend me until all my observations both of the fixed stars and of the planets are published, and at the same time my defense against Hooke." Hevelius' conviction that he had not been given a fair chance because a proper comparison between observations with both types of sights was lacking, eventually resurfaced as a major sore point for him in subsequent letters between the two astronomers.

Flamsteed replied to Hevelius within a month, and although he offered "to defend Hevelius against Hooke's criticisms," the letter caused a temporary rift in their relationship. Flamsteed first claimed that two of Hevelius' measurements were off by as much as two minutes and he could not account for such a large discrepancy. Flamsteed then added insult to injury by writing:

> You will perhaps say that these differences between your observations and mine are tiny. They are, I confess, such as I would readily have disregarded in Tycho's work. But, because you think that you yourself have achieved greater precision, I have thought it necessary to show you that you have not always aimed to the accuracy of one minute through plain sights. I believe it can scarcely be doubted that it is in fact possible to measure celestial distances more accurately with telescopic instruments.[173]

In an attempt to indicate that he anxiously awaited Hevelius' next great work, the *Machina coelestis pars posterior* (1679), he asked Hevelius to "hasten this eagerly-awaited work" and to not "delay because our Professor of Mechanics promises greater precision with lesser instruments. . . ."[174]

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[173] 20 July 1676, FC, I, 494.
[174] The reference to Hooke was unintentionally insulting.

Several months later, Hevelius responded with a letter of his own that left Flamsteed practically baffled.[175] With one exception, Hevelius deviated from more technical issues over the merits of his own sights and instead, touched on his opinions of quarrels, testimony and reputation, the need for astronomers to be certain before they make any knowledge-claims about the heavens, and even jealousy. Hevelius did not take long to get to the point, and early in the letter, he informed Flamsteed that it is "distasteful" in his opinion for friends to quarrel, "especially if [Hevelius] sees that things of [his], whatever their quality, communicated in good faith, have been seized upon only in their worse aspects, and indeed all and every one of them indiscriminately. . . ." He also argued that no one is perfect and that Flamsteed "made a mountain out of a molehill" because of two possible errors of measurement (Hevelius did not necessarily contest these specific measurements). Hevelius believed Flamsteed had judged all of Hevelius' measurements based on two potential errors, and he even suggested how Flamsteed could have phrased things more politely:

> And thus it can easily come about that even the most careful of all, especially while hurrying when we have arrived rather late for observations, can sometimes depart a little from the truth, at least in respect of a few seconds, although he may attach to his instruments the best available telescopes and the most accurate lenses. . . . *if every mistake were to be avoided, one must be an angel not a man*. . . . you ought not to have thus drawn a general conclusion. . . . you could have argued, if you wished to seize the worst in this way, that I had made a small error in two cases. Which can sometimes happen to you and to others, believe me, since you too are human. . . .[176]

In effect, Hevelius felt Flamsteed had no basis and no justification for generalizing by turning the potential errors into an issue of the reliability of Hevelius' methods, measurements, and instruments. To this end, Hevelius passionately wrote, "you need not have at once overthrown the whole observational method of another friend on account of one or two observations." Hevelius was personally affronted that Flamsteed was "straining every nerve (since [he] is prejudiced in advance) to slight all" of Hevelius' observations and to "demonstrate clearly to everyone and to posterity that" his measurements were not accurate to within two minutes. Hevelius was even convinced that Flamsteed and Hooke had joined forces against him:

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[175] 23 December 1676, FC, I, 516-525. Also in Forbes, "Early Astronomical Researches of John Flamsteed," 133-134; and John Flamsteed, The Gresham Lectures of John Flamsteed, ed. by Eric G. Forbes (London: Mansell Information/Publishing Limited, 1975), 37.
[176] FC, I, 518-519.

> But anyone who reads your letter will undoubtedly say that you are undertaking with all your strength and in fact with better instruments exactly the same thing that Hooke is trying to show with lesser ones: namely, that I cannot measure distances with my sextants, that is with the naked eye, except to three or at least to two whole minutes. . . . For now I know very well that I shall labour in vain with you; since I see clearly enough that you will never agree with me whatever else I cite and however many more observations I bring in to show the opposite.[177]

Hevelius also raised issues of personal testimony and reputation when he wrote, "since the testimony of the most honest and equally most learned men, well practised in this field (of whom I could also produce many), carries no weight with you, whatever other argument I bring forward is in vain." Unsurprisingly, Hevelius was frustrated because he felt that there was nothing he could do to convince Flamsteed to his point of view, regardless of the numerous observations and measurements he had at his disposal (and since Flamsteed had almost none). However, he made it very clear to Flamsteed that he himself would never change his mind: "Equally, you will never lead me to abandon my safer way and to embrace another which seems to me less safe, which is liable to so many obstacles, and which must always be entered upon with the greatest caution."

Flamsteed's singular observation cannot be the standard by which all others are based, according to Hevelius:

> . . . it is evident you attribute a great deal to your single observation made with the micrometer, so that immediately from this single observation of yours, before you have yet examined the matter a little more deeply and obtained the observations of others on the same point, you condemn and reject my observations, as if your observation alone was to be regarded as the standard by which the observations of all others were to be examined.[178]

Hevelius then instructed Flamsteed to be prudent about making knowledge-claims without double-checking his own work first ("for in no way at all can we fly before we receive wings"); in this way, if any faults are found, the astronomer cannot be blamed for incompetence. Moreover, he advised Flamsteed "not to boast so much before the victory, nor to heap so much reproach upon the labours of others. . . (since, as Scheiner says, it is quicker to speak than to act,

[177] FC, I, 520. In his translation, Forbes uses the word "weapons" rather than "instruments" in the first sentence (Forbes, "Early Astronomical Researches of John Flamsteed," 133-134; and Forbes, ed., The Gresham Lectures, 37).
[178] FC, I, 521.

and easier to criticise then to achieve more accurate results. . . ." Hevelius then took the opportunity to browbeat Flamsteed for his audacity in thinking that Hevelius was not concerned about "tiny" differences between his observations and Flamsteed's:

> As for the charge that I have not cared at all about two whole minutes in my observation, or still do not care, I am certainly not a little annoyed by it. You write (as though indeed you quite know my mind) as I would maintain, that an error of two minutes (as you say) is a tiny difference between your observation and mine. But, my friend, I am so far from despising two whole minutes that rather, so far as is ever possible in my measuring, *I would by no means willingly disregard even a few seconds*.[179]

Hevelius' reaction indicates that he believed Flamsteed thought he was not a serious astronomer because he was willing to overlook an error as large as two minutes.

Finally, the charge of jealousy against Flamsteed appeared at end of the letter when Hevelius remarked:

> For people have interpreted your letter as if most of it had proceeded from a neighbour's envious eye, as if you cannot tolerate colleagues who also attempt something in their own small way. . . . For, even if the crop is always more plentiful in someone else's fields and the neighbour's beast has a full udder, still I well know how much each man is pleased by his own.[180]

Hevelius probably showed Flamsteed's letter to others, although it is not certain whether charges of jealousy initially came from Hevelius or from those who saw it. Hevelius' letter, which was ultimately published in his *Annus climactericus* (1685), ended with a somewhat despondent and melancholy note: "It will be enough for me if I am merely numbered among those who were willing to attempt something with the greatest effort, although they did not always hit the mark in everything. For in difficult things it is enough to have wished to." He realized that it would never be possible to please everyone in every way and hoped only that he had accomplished his own personal best.[181]

Evidently, Flamsteed's previous support had not been sufficient in placating Hevelius who asked Flamsteed to allow him to his own opinion as he would allow Flamsteed to his. Flamsteed was dismayed that Hevelius had reacted in such a way to his letter, and he wrote to

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[179] (Emphasis mine) FC, I, 523.
[180] FC, I, 524.

Oldenburg, "I know not what passages of myne or formes of expression have deserved such censures and imputations as hee would fasten on mee. I think you know my temper better. . . ."[182] On April 5, 1677, Flamsteed asked Oldenburg to inform Hevelius that he had not, in fact, joined forces with Hooke, and that Hooke had no intention of performing any observations of his own, let alone with the accuracy he claimed he could attain.[183] Oldenburg ultimately was able to reassure Hevelius and smooth his ruffled feathers by explaining that Flamsteed never attempted to offend him in any way,[184] to which Hevelius responded that he was not offended and that he only wished Flamsteed not debase all his observations without having seen them first.[185] Though Flamsteed remained a firm advocate of telescopic sights, he continued to respect Hevelius' work and the merit of his observations. Several years later, Flamsteed wrote to Richard Towneley that he had reversed his opinion of Hevelius' measurements after examining his new work, the *Machina coelestis pars posterior* – a gift he had recently received from the author:

> [Hevelius'] observations of the distan[ces] of the fixed stars I find scarce ever differ a whole minute from mine, rarely halfe a minute. most commonly wee agree within 20" at which I cannot but wonder considering that he uses no glasses to assist his eyes and how difficult it is to see even the largest through so small a cleft as that in his sights.[186]

Flamsteed was just starting his career in the early to mid-1670's and was not yet aware of just how hard it was to make a sustained series of measurements of high quality. But by the end of the 1670's, he had come to appreciate Hevelius' achievements.

Hevelius' relationship with Flamsteed eventually mended, and although they never saw eye to eye with respect to the superiority of telescopic sights, they nonetheless continued corresponding and exchanging observations and measurements. Not only did Hevelius praise Flamsteed, but he also turned to him for complaints, such as when he mentioned to Flamsteed

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181 "I very much doubt whether I have satisfied everyone in everything; but can anyone please everyone to any great extent? Certainly nothing meets with approbation in every respect" (Hevelius to Flamsteed, 14/24 April 1679, FC, I, 687).
182 [Late] January 1677, FC, I, 539.
183 OC, XIII, 236.
184 19 April 1677, OC, XIII, 247-248.
185 28 November 1677, OC, XIII, 363. Oldenburg never had a chance to read this letter – he died before it reached England.
186 16 July 1679, FC, I, 697.

that his work had been misquoted in the *Philosophical Collections*, Hooke's creation intended to replace the *Philosophical Transactions*:[187]

> From your very recently published transactions, I see that a certain minor observation of mine, undoubtedly the occultation of Jupiter by the moon, has been inserted, but in part incorrectly. . . . But you, my friend, can easily discover by whose fault this was done, whether that of the compiler of the transactions (who elsewhere shows much too little favour towards the observer who made that observation) or whether [it can be blamed on] the compiler's negligence. I would like to believe that similar inaccuracies also crept into those pieces published in English (for I have much too poor an understanding of that language); if not, it would certainly seem to suggest a prejudiced approach to the authors.[188]

It does not appear that Flamsteed ever addressed Hevelius' complaint.

As the relationship between Flamsteed and Hevelius demonstrates, the controversy over sights went beyond mere technical arguments over which type of sight yielded more precise measurements. Moreover, the incident between the two intensified Hevelius' concern because Flamsteed's views had to be taken more seriously than those of Hooke. The temporary discord between the two astronomers that resulted with only a handful of letters was eventually repaired, fortunately with the assistance of Oldenburg just one year before his death. Had this happened after Oldenburg's death, the rift may never have been mended; Hevelius' despair at having no one to communicate with on astronomical matters would have intensified, as would his feelings of isolation. This is mainly because after Oldenburg's death, Flamsteed (and to some extent Halley) had become Hevelius' main correspondent. Even the French, according to Hevelius, were not interested in maintaining a correspondence (which he had complained about to Oldenburg).[189] Ultimately, the relationship between Flamsteed and Hevelius can be characterized as one that concerned claims about the nature of the efficacy of naked-eye sights. However, the personality traits of both astronomers along with issues over personal authority and reputation (as well as Oldenburg's intervention) played a role in guiding, and eventually determining, the

[187] The *Philosophical Collections* were meant to be a continuation of the *Philosophical Transactions*, but there were only seven published between 1679 and 1682 (one in 1679, two in 1681, and four in 1682).

[188] 21 June 1682, FC, II, 13. The "compiler," of course, was Hooke. Hevelius' article, "The Lord Hevelius his observation of the occultation of Jupiter by the Moon," appears in no. 1 (1679), 29-32 [FC, 14 n. 7].

[189] "For the Parisians hardly keep in touch with me at all, nor have Messrs. Cassini and Picard made any reply to my very long letter of 7 October last year, to which, however, they justly owed me a reply. . ." (31 May 1672, OC, IX, 85).

relationship between the two. After their brief clash, the relationship smoothed out even if their respective opinions on sights had not changed.

The Third Stage: The Controversy in Later Years

In 1676, young Edmond Halley embarked on an expedition to the island of St. Helena to observe the positions of southern stars not visible from Europe. He also planned observations of eclipses and a transit of Mercury across the sun.[190] His principal instrument was a sextant fitted with telescopic sights and about 5½ feet in radius with an iron frame and brass scales. During the year on St. Helena, he observed some 300 stars and compiled the first star catalog ever that used telescopic sights and micrometers – it was the first catalog of its kind and Halley "would be the first to use telescopic sights in any extensive programme."[191]

As certain members of the Royal Society became increasingly uncomfortable with recent events, Halley decided to visit Hevelius in Danzig in 1679.[192] 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.[193] The visit to Danzig was not a formal request of the Royal Society, however, and Halley already had plans to visit the famed astronomer's observatory.[194] A letter from Detlerus Cluver (1639-1708) dated March 4, 1679, as well as letters from Croone (a Royal Society Secretary at the time) and Wallis introducing Halley, may have given Hevelius the impression that Halley had been directly commissioned by the Royal Society to visit him.[195] Regardless, the letters of introduction signify that the Royal Society at least approved of the visit. "A tactful ambassador," Halley could not have been a better choice; he was the only astronomer at the time who had a compiled catalog of stars to his credit.[196] Flamsteed had just begun his work at the Greenwich Observatory, and Cassini, as yet, did not have the proper instruments he needed for his work in Paris.

When he arrived in mid-May, Halley brought with him a 2-foot quadrant with telescopic sights, which Hevelius later made the mistake of referring to as a telescopic sextant. Together,

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[190] See Sir Alan Cook, Edmond Halley: Charting the Heavens and the Seas (Oxford: Clarendon Press, 1998), 61-88.
[191] Cook, Edmond Halley, 71.
[192] Perhaps the most detailed and technical description of Halley's visit is in Cook, Edmond Halley, 89-105.
[193] 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.
[194] MacPike suggests that Flamsteed gave Halley the idea for the visit since there was no reason for Halley to be interested in the old-fashioned instruments [E.F. MacPike, Hevelius, Flamsteed, and Halley: Three Contemporary Astronomers and Their Mutual Relations (London: Taylor and Francis, Ltd., 1937), 85; see also Cook, Edmond Halley, 92]. Nevertheless, young Halley could not turn down the opportunity to meet the great European astronomer and become familiar with instruments that were slowly being phased out.
[195] Cook, Edmond Halley, 93.

they used three of Hevelius' instruments: the small brass quadrant (for measuring meridian altitudes), the large brass sextant (for determining angular measurements between objects), and a 12-foot telescope (for timing occultations and observing the moon). Hevelius and Halley were assisted by four other observers: Hevelius' wife (Elisabetha), J.E. Olhoff, Nathaniel Buthner, and the printer, referred to as Typographus. Together, they all performed several different types of observations in different combinations of observers (usually two at a time), using different instruments. For instance, measurements of angular distances between two stars, a star and a planet, and meridian altitudes were performed using Hevelius' large sextant and Halley's quadrant fitted with telescopic sights. Halley and Hevelius also derived the declinations of various stars from the meridian altitudes and measured the diameter of the moon.[197] An analysis of all the measurements reveals that Hevelius' large sextant was consistent to about 10 arc seconds, but "the differences between" Hevelius' sextant and Halley's quadrant "are substantial and evidently there were large systematic errors in one or both."[198]

Halley sent a letter to Flamsteed wherein he expressed his astonishment at the accuracy of Hevelius' measurements that had been repeated several times, and although he reserved judgment "as to the exactnesse of the Observation[s] of the Meridian Altitudes," he was "surpriz'd to see so near an agreement in" the distances measured with Hevelius' sextant and "dare[s] no more doubt of [Hevelius'] Veracitye."[199] At Hevelius' request, Halley left behind a testimonial letter that attested his high esteem for the accuracy of Hevelius' open-sight observations.[200] In the letter, Halley stated that he was "abundantly satisfied of the use and certainty of [Hevelius'] instruments and observations," and he wondered why he had ever doubted the accuracy of observations by open sights, readily "*offer[ing] himself a voluntary*

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[196] Cook, Edmond Halley, 93.

[197] In his biography of Halley, Cook has reprinted the tables from the observations performed during Halley's visit to Danzig. These include Table 4.1: "Angular distances between celestial objects as observed at Danzig, June-July 1679" (94-95), Table 4.2: "Declinations derived from meridian altitudes observed by Halley and Hevelius" (97), and Table 4.3: "The diameter of the Moon, Danzig, 23 June 1679" (99). Cook assesses all these measurements in detail.

[198] Cook, Edmond Halley, 98. Cook's conclusion is that "Hevelius did obtain remarkably reproducible results, probably the best possible with open sights, but his instruments and procedures were out of date and suffered from substantial systematic errors" (Ibid., 104).

[199] He later retracted this statement (7 June 1679, FC, I, 694; see also MacPike, Correspondence and Papers of Edmond Halley, 43; Birch, The History of the Royal Society of London, III, 488; and Folkes, "Memoir," in MacPike, Correspondence and Papers of Edmond Halley, 4). Although there were discrepancies between the two sets of observations, Halley wrote that the differences were negligible (Simms, William Molyneux of Dublin, 25).

[200] Dated 8/18 July 1679. Halley eventually regretted leaving written testimony behind, especially since Hevelius eventually printed it in his *Annus climactericus*. The letter is reprinted in Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 43-44.

witness, (of the almost-incredible certainty of those his instruments) against all who shall for the future call [Hevelius'] observations in question."[201] However, the contents of the letter are qualitative rather than quantitative – Halley did not include numerical values, nor did he mention the measurements made with the assistants. Halley acknowledged he had underestimated the accuracy of Hevelius' measurements, but at the same time, realized that Hevelius' instruments had their limitations.[202] Despite Halley's wariness, he was 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 the letter behind.[203]

On September 26, 1679, while Hevelius was away from his observatory, a servant of his, in a fit of vengeance, started a fire that eventually burned down the observatory, many of Hevelius' manuscripts, and all of his instruments. Capellus, a kinsman of Hevelius, informed Hevelius' brother of the losses that were incurred: almost all of his unbound books (including the *Machina coelestis pars prior*), all of his instruments, 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.[204] Despite Hevelius' devastation at the damage caused by the fire, he immediately began rebuilding his observatory and replacing it with new instruments.[205] In 1681, he informed Halley of the fire and he specifically asked him for his assistance in the procuring of texts and especially instruments that had been lost in the conflagration. In the same lengthy letter, he also included information on observations of the comet of 1680, an indication that Hevelius continued with his observations despite any interruptions that may have been caused by the fire.[206] Hevelius persisted in his request for new instruments from Halley and indicated that rebuilding the observatory had taken a toll on his

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201 (Emphasis mine) 8 July 1679. Translated from the Latin by Wallis, in PT, XV, no. 175, 1168-1169.
202 See Cook, Edmond Halley, 100. On the day Halley returned to London, Hooke wrote in his diary, "*Diary*, Halley returned this day from Dantzick (Hevelius Rods in pisse)" [14 August 1679, Henry W. Robinson and Walter Adams, eds., The Diary of Robert Hooke 1672-1680 (London: Taylor & Francis, 1935].
203 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 the testimonial letter (20 February 1686, Hoppen, ed., The Papers of the Dublin Philosophical Society, 1082-1083). Furthermore, Molyneux claimed that Halley's letter "grossly flattered" Hevelius, and that Halley had "imposed on the world" (Hoppen, ed., The Papers of the Dublin Philosophical Society, 1140; and Simms, William Molyneux of Dublin, 27).
204 Capellus, "A Letter on the Hevelian Conflagration. . . ." (1679), in MacPike, Hevelius, Flamsteed, and Halley, 109-110. Apparently, one of Hevelius' daughters, who was home at the time the fire started, saved the works by throwing them out the window before they could be damaged or destroyed.
205 Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 4. Unfortunately, the newer instruments did not compare with the previous ones.

ability to correspond with members of the Royal Society with the same frequency as in the past.[207]

Hevelius' correspondence with members of the Royal Society decreased in the years following the fire – in large part because of Oldenburg's death in 1677. Nevertheless, Hevelius kept busy by rebuilding his observatory and continuing with his observations, as demonstrated in a series of lengthy letters to the Royal Society in the years before his death. Some of these letters contained specific measurements and diagrams of lunar phases and eclipses;[208] others were on conjuctions of the superior planets;[209] still others were on occultations, especially of Jupiter by the moon.[210] In 1683, Hevelius even sent the Royal Society a "Historiola" of comets to 1683 that was read November 28, 1683, entered in the Letter Books and ultimately published in the *Philosophical Transactions*.[211]

The Annus Climactericus*, Reviews, and Supporters*

In 1685, Hevelius published the last work before his death, the *Annus climactericus*.[212] 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 1679, a year which began so auspiciously for Hevelius with Halley's visit, but ended tragically with the fire.[213] In great detail, Hevelius

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[206] Date uncertain, 1681, Dibner letter. At the top of the letter, the Royal Society Secretary wrote what the contents of the letter were, including "faccts of the Comet of 1680."

[207] 9 January 1682, Dibner letter.

[208] 9 January 1682, Hevelius to Gale, Dibner letter; 21 February 1682, Hevelius to Royal Society, Dibner letter; 10 December 1685, Hevelius to Royal Society, Dibner letter [at the end of this letter, he mentioned his *Prodromus Astronomiae* with the Catalog of the Fixed Stars (*Uranographia*)].

[209] Date uncertain, but read at the Royal Society meeting of 24 October 1683, Hevelius to Royal Society, Dibner letter. In this "Historiola," Hevelius specifically listed the distances between Saturn and Jupiter measured with his "micrometers."

[210] Date uncertain, but read at the Royal Society meeting of 24 October 1683, Hevelius to Royal Society, Dibner letter. Another letter came three years later and was written on April 10, 1686. There are two copies of the April letter, both of which contain observations of the occultation of Jupiter by the moon as well as measurements of the diameter of Jupiter. In the same letter, Hevelius includes a table comparing calculations obtained by himself, Kepler, Lansberg, Boulliau, Riccioli, and Wingio.

[211] (PT, no. 154) Date uncertain, Dibner letter.

[212] "Climactericals" refer to special years that are either any multiple of seven or an odd multiple of seven. Since 1679 was the forty-ninth year of Hevelius' observations of the heavens, both conditions were met (FC, II, 14 n. 6).

[213] Béziat, "La vie et les travaux de Jean Hévélius," 632. See also Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 56.

recounted Halley's visit, and reprinted twenty-seven letters from individuals who were personally familiar with Hevelius' observations and instruments.[214]

Hevelius' text was ready by April 1685 when he wrote to Francis Aston, then secretary of the Royal Society.[215] He specifically requested in the letter that his work and observations be defended if necessary against all who might be envious or malicious in the assessment of his work and he even referred to the controversy with Hooke. On July 17, 1685, Hevelius sent the Royal Society and several of its members, including, Wallis, Halley, and Flamsteed copies of his work "so that all England" could respond to it.[216] In reply to a letter 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."[217] He further wrote that his next great work, the *Prodromus Astronomiae* to which his Catalog of Fixed Stars would be attached, was almost ready.

William Molyneux presented the work to the Dublin Philosophical Society on November 9, 1685, and on November 16, he "read a large discourse containing his thoughts of Hevelius' *Annus climactericus*," in which he demonstrated that Halley and Hevelius' observations in Danzig differed significantly.[218] Molyneux also composed a review of his own that he sent to Halley on December 2, 1685, where he provided a more "objective" review than Wallis'.[219]

The handful of individuals who supported Hevelius during the controversy, were quoted at length in his *Annus climactericus*. They included Wallis, Boulliau, Fullenius, and Titus Livius Burattini, "who were in a position to judge for themselves the care and precision that [Hevelius] brought to all his observations."[220] Wallis had supported Hevelius from the very beginning of the dispute. In 1667, he wrote to Oldenburg that he "assents" to Hevelius' use of "Quadrants,

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[214] Béziat, "La vie et les travaux de Jean Hévélius," 617.
[215] Hevelius to Aston, April 4, 1685, Dibner letter. Also referred to in H.P. Kraus, Rare Books and Manuscripts. Hooke, of course, was not sent a copy.
[216] 19 May 1685, Hevelius to Aston, Dibner letter.
[217] 29 December 1685, Dibner letter. Also in H.P. Kraus, Rare Books and Manuscripts.
[218] British Library, Add. MS. 4811, ff. 5^v-7; cited in Simms, William Molyneux of Dublin, 25. The differences between measurements were as much as eight minutes!
[219] British Library, Add. MS. 4811, ff. 5^v-7; cited in Simms, William Molyneux of Dublin, 97. The letter was read during the Royal Society meeting on December 9, 1685 (Birch, The History of the Royal Society of London, IV, 450). Wallis' personal friendship with Hevelius contributed to his personal review of the work.
[220] Béziat, "La vie et les travaux de Jean Hévélius," 617.

sextants, & such other instruments (for places & distances) much before those of the Telescope; though this allso be in its kind an Excellent Instrument, where those can not be used."[221]

After the publication of the *Machina coelestis pars prior*, Wallis argued that there was "no reason to be displeased" with Hevelius' continued use of common sights on measuring instruments, and that it was better for Hevelius to continue using the instruments he was most familiar with.[222] However, Wallis added that Hevelius' instruments were not necessarily the best instruments, and that it would be improper to discourage others from using telescopic sights.[223] While Wallis did not 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.[224]

Wallis' favorable review of the *Annus climactericus* in the *Philosophical Transactions*[225] concerned many astronomers, including Flamsteed.[226] In a letter to Richard Towneley, Flamsteed suggested Towneley ignore the review because it was subjective and Wallis did not truly understand the particular advantages 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.[227]

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

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[221] 19 January 1667, OC, III, 313.
[222] Wallis to Oldenburg, 12 January 1674, OC, X, 432-433.
[223] "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" (Wallis to Oldenburg, 12 January 1674, OC, X, 432-433).
[224] Wallis to Oldenburg, 11 January 1675, OC, XI, 154-155.
[225] PT, XV, no. 175. See also Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 45; and Simms, William Molyneux of Dublin, 25.
[226] MacPike, Hevelius, Flamsteed, and Halley, 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, William Molyneux of Dublin, 25.
[227] 15 March 1686, MacPike, Hevelius, Flamsteed, and Halley, 95.

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

> 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.[228]

Wallis even told Flamsteed that if Flamsteed thought Halley "hath been too lavish in his commendations [of Hevelius],. . . Mr Hook hath been so in his reprehensions."[229] Although he castigated Hooke for his unprofessional attitude, Wallis recognized the perils involved when either party deviated from appropriate 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 to be preferred, cannot be otherwise determined than by experience.[230]

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."[231]

Ismael Boulliau supported Hevelius' commitment to using open sights, declaring that he was "of the same opinion" as Hevelius,[232] and he listed two specific reasons for his support:

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[228] PT, XV, no. 175, 1165. Wallis later added that Hooke had the audacity to censure others when he, himself, had 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).
[229] 12 February 1686, FC, II, 274.
[230] PT, XV, no. 175, 1165. Like Flamsteed, Wallis also stated that Hooke was never able to provide that experience and prove to Hevelius that telescopic sights were more accurate.
[231] Wallis to Oldenburg, 12 February 1686; cited in Scott, The Mathematical Works of John Wallis, 128-129.
[232] 6 February 1675, Birch, The History of the Royal Society of London, III, 181. The letter was read to the Royal Society on February 11, 1675. Others, like Leibniz, became aware of Boulliau'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. Boulliau seems to

> [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 useful to those who blink (myops and dim-sighted people).[233]

The nature of Wallis and Boulliau's support may have differed, but the personal interactions between the two with Hevelius may have biased their opinions. [234] Unlike most of the adherents of telescopic sights, Boulliau and Wallis had been personally acquainted with Hevelius for decades before the controversy erupted. Hevelius met both men on his travels around Europe in the early 1630's – Wallis in 1630 and Boulliau in 1631.[235] Furthermore, they were closer to Hevelius in age than other adherents of telescopic sights.[236] At the time of their meeting, Hevelius was nineteen, Wallis was fourteen, and Boulliau twenty-six. Consequently, Wallis and Boulliau were more familiar with naked-eye sights since telescopic sights were not commonly used before 1660. Though this does not suggest that they supported Hevelius solely because they had similar knowledge of astronomical instruments, it does indicate that Wallis and Boulliau were more tolerant of Hevelius because of their friendship with Hevelius and their familiarity with open sights.[237]

Hevelius and Wallis maintained a correspondence throughout most of the seventeenth century. Like the Oldenburg-Hevelius correspondence, the correspondence between Wallis and Hevelius formed 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. Boulliau's adherence to the use of open sights also may have been affected by his personal friendship with Hevelius. The two kept up an active correspondence after Hevelius' visit, and Boulliau never ceased to admire

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stand by Hevelius; Cassini and Picard think that telescopes are not to be neglected" (Leibniz to Oldenburg, 20 March 1675, OC, XI, 242).

[233] Boulliaud to Oldenburg, mid-April 1675, OC, XI, 277.

[234] Wallis, unlike Boulliau, did not necessarily believe that open sights yielded more precise measurements. He argued, instead, that the accuracy of Hevelius' measurements were due to Hevelius' powers as an observer.

[235] Béziat, "La vie et les travaux de Jean Hévélius," 500.

[236] G-D Cassini, Picard, and Auzout, adherents of telescopic sights who were born in the 1620's, were exceptions. Nevertheless, Wallis and Boulliau were still older (even if only by several years).

[237] Hooke was twenty-four years younger than Hevelius, Flamsteed was thirty-five years younger, and Halley was forty-five years his junior.

Hevelius' work.[238] Furthermore, Boulliau traveled to Danzig in 1661 in order to renew his friendship with Hevelius and to learn more about the nature of Hevelius' "instruments and methods of observation."[239] Boulliau even appears in one of the plates in Hevelius' *Machina coelestis pars prior* in which he is shown assisting Hevelius with a helioscope for observing eclipses and sunspots, and Hevelius mentioned Boulliau's trip to Danzig and his assistance in the text.[240] Boulliau was even more inclined to accept Hevelius' methods and instruments after having been an eye-witness and actual participant in Hevelius' observations.

Hevelius also received Bernhard Fullenius' support, a councilor and son of a former professor of mathematics at the University of Frankfurt-on-Oder.[241] Hevelius printed several of Fullenius' supportive letters in his *Annus climactericus* – letters which clearly established Fullenius' position. In his review of the text, Wallis 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*[242] 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.[243]

Fullenius openly defended Hevelius in a letter dated March 1, 1675:[244]

> As for your *Organa*,[245] 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 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

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[238] Béziat, "La vie et les travaux de Jean Hévélius," 539.
[239] Béziat, "La vie et les travaux de Jean Hévélius," 537.
[240] Hevelius, *Machina coelestis pars prior*, 374. The plate is on the opposite page.
[241] Forbes, ed., The Gresham Lectures, 35.
[242] *Machina coelestis pars prior*
[243] (Emphasis mine) The other letter referred to is the eighth from Boulliau to Hevelius. Wallis added that Fullenius "declares his suspicions of the uncertainty of Telescopick Sights, preferring others before them" (PT, XV, no. 175, 1171).
[244] This appears to be the ninth letter Wallis referred to.
[245] *Machina coelestis pars prior*

> 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*.[246]

Like Boulliau, Fullenius not only supported Hevelius' use of open sights, but advocated their use in general. 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."[247] Undoubtedly, he favored the use of open sights since he constructed instruments for Hevelius. In addition to his support, Burratini also praised Hevelius' instruments and observations in a letter that Hevelius later published as letter nineteen in his *Annus climactericus*.[248]

Molyneux and Halley's Opinions of Hevelius and His Work

Hevelius' claims that the naked eye was as accurate as the telescope for purposes of measurement continued to strike a raw nerve with other astronomers, especially Molyneux and Halley. 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. After the publication of the *Annus climactericus*, Halley and Molyneux exchanged a series of letters that dealt with Hevelius' seeming misrepresentation of Halley. Molyneux had become infuriated with Hevelius for three reasons: first, Hevelius claimed that Halley brought with him to Danzig a sextant, whereas Halley actually took a quadrant; second, Hevelius mentioned that Halley had been sent to St. Helena by the Royal Society, whereas Halley claimed that he went "on his own initiative and at his own expense;"[249] third, Hevelius claimed that Halley had been sent by the Royal Society specifically to examine Hevelius' instruments, but the Royal Society had little to do with

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[246] (Emphasis mine) Cited in Forbes, ed., <u>The Gresham Lectures</u>, 35-36. This testimony was excerpted from Hevelius' letter to Flamsteed, dated June 24, 1676. Fullenius 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.

[247] Daumas, <u>Scientific Instruments</u>, 66. He was also an "assistant at [Hevelius'] Observations" (Wallis, <u>PT</u>, XV, no. 175, 1175). That made him an eye-witness of Hevelius' work.

[248] Daumas, <u>Scientific Instruments</u>, 66. Except for Boulliau and Halley, Hevelius' witnesses were not practicing astronomers.

[249] Simms, <u>William Molyneux of Dublin</u>, 26-27. Halley wrote to Molyneux, "[Hevelius] sais I was sent by R.S. to St Helena at his request to observe the Southern Starrs (pag.14) whereas it is very well known to all our

Halley's decision to visit Danzig. Molyneux was also displeased with Hevelius because he believed Hevelius had misled him by insisting that Halley had used a sextant.[250]

Molyneux fully appreciated the significance of the testimonial letter Halley had left behind because it came from an advocate of telescopic sights, and carried more weight, and Molyneux was vocal in expressing such an opinion. 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*."[251] Halley, who recognized the significance of Hevelius' continued advocacy of naked-eye measurements, gave vent to his anger by informing Molyneux that:

> 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 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. . . .[252]

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.[253]

Molyneux replied that Halley should speak up before Hevelius' death so that he would have the opportunity to exonerate himself.[254] However, there is no indication that Halley ever publicly expressed his displeasure towards Hevelius, and he remained cordial towards Hevelius until his death. Ultimately, Halley realized that accepting the accuracy of Hevelius' measurements had been a mistake. But his 1679 testimonial letter was not mean to be about the future – that is, he knew that the future of positional astronomy lay in telescopic sights. By 1686, however, he felt

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Astronomers that at my own motion and charge I undertook that voiage above two years, before I had the honour of being a member of the R.S.. . ." (27 March 1686, MacPike, Correspondence and Papers of Edmond Halley, 60).

[250] Birch, The History of the Royal Society of London, IV, 477; and Simms, William Molyneux of Dublin, 26.

[251] (Emphasis mine) Simms, William Molyneux of Dublin, 25-26.

[252] (Emphasis mine) 27 March 1686, MacPike, Correspondence and Papers of Edmond Halley, 60.

[253] 27 May 1686, MacPike, Correspondence and Papers of Edmond Halley, 65. Halley did not have much of a wait – Hevelius died the following year.

[254] 19 June 1686, Simms, William Molyneux of Dublin, 27. Also in Hoppen, ed., The Papers of the Dublin Philosophical Society, 1140.

that his reputation as an astronomer had been called into question. By indicating that Hevelius' measurements were as accurate as his own, he bestowed uncertainty, not only on his own measurements, but those performed by others using telescopic sights. In this way, his own abilities as a reliable witness, his authority, and his reputation may have been questioned.

Molyneux published his *Dioptrica nova* in 1692, a work that mainly 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 pars prior.* Overall, he believed that Hevelius' instruments had one major flaw that rendered them imperfect – they lacked telescopic sights.[255] 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 that surrounds celestial objects. However, he also argued that Hevelius' exceptional powers as an observer allowed him to make more precise measurements.[256] 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.[257]

Molyneux leveled his most serious criticism against Hevelius' claim that the shortness of the line of collimation, which was between the eye and cross-hairs, 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."[258] Therefore, the cross-hairs were automatically superimposed onto the image of the object being observed. Despite initial problems with collimating telescopic sights, this method was eventually solved by the use of cross hairs, and astronomers such as Flamsteed agreed that the collimation afforded a greater degree of precision in measurement and

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[255] Unlike Hooke, Molyneux argued that, although Hevelius' instruments had their "open-sight" flaws, these instruments were not "useless, faulty, or no better than Ticho's. . ."[*Dioptrica nova* (London, 1692), 228].
[256] *Dioptrica nova*, 229.
[257] Molyneux claimed that it would be similar to watching the movement of a minute-hand through a microscope (*Dioptrica nova*, 244).
[258] *Dioptrica nova*, 231. Molyneux proposed this same argument on two previous occasions – in a letter to Halley, 8 April 1686 (Birch, The History of the Royal Society of London, IV, 477), and in a letter to Aston, 15 March 1684, read during the Royal Society meeting on 26 March 1684 (Birch, The History of the Royal Society of London, IV, 272).

simplicity in usage.[259] Molyneux responded to Hevelius' other objections against cross-hairs and lenses that he felt were also unjustified. For 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." 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 by holding the source of light off to one side. When Molyneux heard of Hevelius' assertion that lenses fog up on cold nights because of the observer's exhalations, he claimed:

> '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?[260]

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

Hooke, Oldenburg, and Hevelius

Throughout the controversy, Royal Society members appeared to be in a dilemma because 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 individuals. Oldenburg was especially incensed because he had developed a personal friendship with Hevelius since the beginning of their correspondence.[261] Oldenburg reassured Hevelius that he wanted to protect him and his work from "ill-wishers,"[262] and he added that Hooke's contentions had not "weakened the Royal Society's regard for Hevelius."[263] After Oldenburg's

[259] Murdin, Under Newton's Shadow, 124.
[260] Molyneux, *Dioptrica nova*, 245.
[261] 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 of Henry Oldenburg 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 few years of their correspondence.
[262] 15 May 1676, OC, XII, 295.
[263] 27 January 1677, OC, XIII, 197.

death, Hooke continued to argue that Oldenburg "had written to Mr. Hevelius more and different things, than he had been directed to do by the Royal Society."[264]

Hevelius heard about these charges and replied with a letter of his own in which he denied Hooke's claims by insisting that Oldenburg had always written to him in his capacity as secretary of the Royal Society.[265] In vindicating his friend, Hevelius claimed that regardless of Hooke's praise or censure, he wondered more and more that Hooke dared blame Oldenburg, former Secretary of the Royal Society, of some false charge. Oldenburg would have no doubt defended himself if he still lived, but now it is up to the Royal Society to avenge and free him of injuries committed against him by Hooke's aspersions and calumny. For his own part, Hevelius will continue with his plans to publish his *Prodromus astronomiae* together with his *Uranographia* that will, in their own way, discredit Hooke.[266] This was such a sensitive issue for the Royal Society that they wrote at the top of Hevelius' letter, "do not Print this - - - - - -."

Hevelius' Death and Thereafter

Hevelius' resentment towards Hooke lasted until his death that might have been 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.[267] Hevelius eventually died on his birthday, January 28, 1687. His "Catalog of the Fixed Stars," which had fortunately been preserved from the fire, had not been completely published by the time of his death. Two months after Hevelius' death, an assistant of his by the name of Christoph Colbe wrote:

> The book which we have in the press is the *Prodromus Astronomiae* and *Catalogi Fixarum*, as well as the copper etchings[268]. . . . The first and last of these are finished, the second, however, only half completed. Now, because of a shortage of paper, everything is still. Six weeks ago the king of Poland wrote to our Frau Hevelius and asked her about the manuscripts as well as instruments of the late Master, and also recommended that she should continue the publication of the works just mentioned. At the

[264] 17 April 1686, Birch, The History of the Royal Society of London, IV, 504.
[265] The letter from Hevelius is also mentioned in Birch (17 April 1686, Birch, The History of the Royal Society of London, IV, 504). The original letter was written April 17, 1686, but two more from Hevelius followed that referred back to the first, one dated July 9, 1686, and another dated September 17, 1686 (all Dibner letters). These were among the last letters to the Royal Society before Hevelius' death.
[266] 17 April 1686 (Dibner letter). The contents have been paraphrased from the Latin.
[267] Béziat, "La vie et les travaux de Jean Hévélius," 636. Though Béziat claims these are what hastened Hevelius' death, we really do not know what "hastened" it.
[268] The proper translation from the German is "engravings."

> moment, however, the king has not given us any further money to cover expenses. What will be the final outcome, time will tell. In the meantime Frau Hevelius has become very proud and the manuscripts, etc. have become very precious to her. . . .[269]

Indeed, the work of her husband was dear to Elisabetha, especially since she helped contribute to that work in many ways including helping him with the observations and measurements. Later in the same year as her husband's death, Elisabetha sent Thomas Gale, then Secretary of the Royal Society, an eloquent letter asking the Royal Society for its assistance in the publication and dissemination of the *Prodromus*.[270] After lamenting the loss of her husband, she indicated that fate has now pointed her towards the direction of the Royal Society. She touchingly mentioned in the letter why her husband chose to undertake the intense labor of compiling a catalog of the fixed stars, and she provided the Royal Society with a more detailed description of the work. Unfortunately, the work was interrupted by her husband's death. Since the Royal Society had already demonstrated interest in her husband's work and had been kind to him in the past, Elisabetha implored the Royal Society to "commit to safety" her husband's labors so that not only the work is "dignified" but also the "Domum Hevelianam," the "House of Hevelius." She added that by doing this, the Royal Society enhances its own reputation and does the learned world a great service. The letter ends with Elisabetha's final request for Gale to reply with the Royal Society's wishes as soon as it is convenient so that the work can be completed. However, the records of the Royal Society do not indicate that any action was taken on this letter, and it appears to have been ignored.

Elisabetha ultimately saw to the publication of her husband's works, which were all collected and published in one volume in 1690. *Prodromus*, which means "forerunner," contained Hevelius' methodology and instruments used in compiling his star catalog. The *Prodromus Astronomiae*, with a preface written by Elisabetha, was bound together with two other works, *Catalogus Stellarum Fixarum* and *Firmamentum Sobiescianum, sive*

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269 Cited in Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 56; from J. Bernoulli, "Fortgesetzte Nachrichten von Hevel's gelehrtem Nachlasse," Monatliche Correspondenz zur Beforderung der Erd – und Himmelskunde, VIII (November 1803), 403-404. Translated from the German.

270 1 November 1687, Dibner letter; also found in MacPike, Hevelius, Flamsteed, and Halley. The contents are paraphrased.

Uranographia.[271] Attached to the *Catalogus* were two of Halley's own works, *Catalogus Stellarum Australium ad annum 1700 completum reductus* (compiled in St. Helena) and *Tabula Motus Lunae libratorii.*

Hooke, in turn, faced a disastrous fate following Hevelius' death. His quarrels with others (especially Newton) and the death of his niece-mistress, also in 1687, turned him into "a recluse and a cynic."[272] 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' Machina Coelestis*."[273] Hooke may have replied to all complaints, but 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 remained bed-ridden and died on March 3, 1703.[274] In 1705, Richard Waller, then secretary of the Royal Society, published a collection of Hooke's papers and discourses which included a letter by Hooke vindicating himself against the charges by Wallis and Molyneux that he had been too harsh with Hevelius.[275]

Which Sights Yielded More Precise Measurements and Why?

It is not the purpose of this study to determine which of the two types of 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)."[276] 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

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[271] All of the constellations are displayed at the website, http://mahler.brera.mi.astro.it/HEAVENS/ATLAS/hevelius.html. Hevelius observed hundreds of new stars and created new constellations, some of which are still used to this day.
[272] Westfall, "Robert Hooke," DSB, VI, 487.
[273] "Mr. Waller's Observations upon Dr. Hook's Discourses, concerning Telescopes and Microscopes," in Hooke, Philosophical Experiments and Observations, 272.
[274] Westfall, "Hooke," DSB, 487.
[275] Found in "The Life of Dr. Robert Hooke," in Hooke, The Posthumous Works, 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).
[276] Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 46.

furthermore, "Flamsteed's early telescopic measurements are not a factor of sixty, but only slightly better than those of Hevelius."[277] 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.

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. Hevelius had other qualities that guaranteed his success, including "a skillful hand at drawing and engraving, [and] an unfailing patience."[278] 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."[279] 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.[280]

Molyneux best summarized the reasons for Hevelius' success at precise measurements. In addition to his excellent eyesight, "mature" instruments, and unfailing patience, Hevelius understood the importance of meticulously 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 and reduce 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 these reasons, no one succeeded him. Hevelius'

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[277] Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 47. Volkoff, Franzgrote, and Larsen 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.
[278] Béziat, "La vie et les travaux de Jean Hévélius," 504.
[279] Béziat, "La vie et les travaux de Jean Hévélius," 592.
[280] Molyneux, *Dioptrica nova*, 229.

successor would have to have essentially the same characteristics as Hevelius in order to yield measurements of the same caliber.

Despite the claims others made about the advantages afforded by telescopic sights, Hevelius remained a staunch supporter of naked-eye sights for several reasons. First, he did not want to add telescopic sights to instruments he had become accustomed to using.[281] He spent many years in constructing and perfecting instruments that he thought were especially useful for making astronomical measurements and adding telescopic sights would force him to relearn how to use instruments that he doubted were superior to his own in the first place. Wallis pointed out the futility of Hevelius' change of 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 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.[282]

Molyneux shared Wallis' main contention that Hevelius was better off using instruments he was most familiar with.[283] Furthermore, Hevelius argued that no one had proven to him the advantages of telescopic sights over plain sights. He believed that only those who carried out decades of observation using telescopic sights, could successfully evaluate the relative merits of each type of sight.[284] Nevertheless, he did not dispute the theoretical advantages of telescopes (namely, that they brought objects closer to the eye), although he argued that they were not

[281] Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 46.
[282] 12 January 1674, OC, X, 432-433.
[283] Molyneux, *Dioptrica nova*, 244-245.
[284] "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, OC, X, 521-522). The advantages in Hevelius' statement are clear – telescopic sights, although initially discovered in the 1640's, were not commonly used until the

superior to open sights in terms of practical measurement.[285] Hevelius also ran the risk of calling the accuracy of all his observations into question by acknowledging the superiority of telescopic sights.[286] 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.[287] Even worse than questioning the results of his past work, however, his future work and results would also be questioned. He had spent decades collecting and reducing the data that was ultimately published in his *Prodromus Astronomiae* and *Catalogus* of the fixed stars. He was not going to throw out his life's work. The controversy, therefore, *was not only about his past work, but also about his future publications*, and Hevelius was probably more aware of this than anyone else as he put the final touches on his catalog in the few years leading up to his death.

Finally, Hooke's reactions to Hevelius' continued use and support of open sights, contributed to Hevelius' continued advocacy of them. Hevelius believed that Hooke did not have enough years of experience using telescopic sights to draw any conclusions. 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 frequently argued that Hooke was all words and no action, and Hevelius believed in the Royal Society's motto:[288]

> 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 it his business to furnish not phrases only but also the facts he has promised, in the manner to which his duty binds him.[289]

Hooke's overall behavior towards Hevelius, his work, and his instruments, was enough to persuade Hevelius to retain his use of naked-eye sights.[290]

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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.

285 Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 46.

286 Hooke, The Posthumous Works, xv.

287 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 (Molyneux, *Dioptrica nova*, 244-245).

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

289 Hevelius to Oldenburg, 21 August 1675, OC, XI, 473.

290 "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, The History of the Telescope, 101-102.

Accordingly, Hevelius' career as an astronomer reveals, in part, why others besides Hooke, felt it was necessary to vindicate the accuracy 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 "Stellaburgum" – his grandiose observatory by the standards of that age.[291] 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."[292]

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."[293] Furthermore, intellectuals such as Gassendi, Mersenne, and Boulliau, had been impressed with the work and especially the lunar engravings.[294] Hevelius, who successfully carried out his observations for over twenty-five years before anyone challenged his use of open sights, was recognized as one of Europe's foremost astronomers and was distinguished as a skillful observer.[295] Consequently, he could not be simply brushed aside and ignored by those who claimed superior knowledge.[296] Moreover, after *Selenographia*, Hevelius continued to publish works that circulated throughout Europe, including London, where he often sent copies of his texts to the Royal Society. Because Hevelius was an important author and contributed trustworthy observations, astronomers were naturally concerned with his opinions and views. If Hevelius did not have enough authority to discredit telescopic sights, he certainly was able to successfully challenge their superiority over common sights. 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

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[291] North, "Hevelius," 360.
[292] Béziat, "La vie et les travaux de Jean Hévélius," 536.
[293] Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 18. See Mary G. Winkler and Albert van Helden, "Johannes Hevelius and the Visual Language of Astronomy," in Renaissance and Revolution. Humanists, Scholars, Craftsmen and Natural Philosophers in Early Modern Europe, eds. J.V. Field and Frank A.J.L. James (Cambridge: Cambridge University Press, 1993), 97-116.
[294] Béziat, "La vie et les travaux de Jean Hévélius," 513.
[295] MacPike, Hevelius, Flamsteed, and Halley, 15.
[296] 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, eds., Scientific Controversies, 52).

had changed to such a degree that to emulate Hevelius' methods was to go back on progress itself.

Conclusions

The controversy between Hevelius and Hooke neatly encapsulates the new methodology of observation that emerged in the late seventeenth century. It sheds light on the new instrumental programme of astronomical practitioners and at the same time, points to several procedures that were still considered important among astronomers regardless of the new faith in instrumentation. In a period of rapid technological change, the new instrumentation was still considered only as good as those who used it. Certainly, there was a fascination with the newness and novelty of certain new technologies in astronomy in the late seventeenth century, although in order to more fully understand the context in which astronomers used these instruments, factors like economic incentives and market demand must not be overlooked (although they are beyond the scope of this work).

Before the 1660's, astronomers improved the precision of measurements by dividing their graduated scales more carefully. Essential to Tycho and Hevelius' success was their acquisition of a large number of "data points" that were only appropriated through decades of observation. They would then calculate their measurements by averaging out their measurements. By the mid-1660's, however, astronomers realized that naked-eye 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. 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, both Hooke and Hevelius stressed the importance of 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 accuracy between Hevelius and Flamsteed's observations.

With the rediscovery of telescopic sights and micrometers in the 1660's, the initial issues were not based on how much precision was afforded through the use of such instruments, but rather, whether one was willing to *adopt* these new technologies, as the Hevelius-Hooke controversy reveals. Furthermore, astronomical practitioners graduated the limbs of arcs with increasing skill, but again this was a moot point unless one was willing to affix telescopic sights and micrometers to measuring arcs. All possibilities afforded by naked-eye sights had been exhausted in the view of almost every single astronomical practitioner in the late seventeenth century with the one glaring exception of Hevelius who would never admit that the possibilities of open sights had reached their uppermost limits, at least not without serious consequences to the reliability of all his observations. He refused to accept the premise that only by using the new instruments on his positional measuring instruments could an astronomer hope to reach new levels of precision. Herein lies the problem for Hevelius – by not using what were considered the best instruments by late seventeenth-century standards, his measurements, although not completely discredited, were seriously questioned. His reputation was not tarnished publicly, but seemed to slip at least in the minds of certain individuals.

The physical integrity of instruments in producing "matters of fact" is also 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 Hevelius' measuring instruments. But besides Hooke, astronomers in the Royal Society believed that his instruments were inferior to those equipped with telescopic sights because Hevelius' instruments were easily susceptible to bending, warping, and wearing down over 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 also criticized the use of telescopic sights (they interfere with observation) and lenses (they easily fog up or break).

Just as important to this controversy are the multifaceted relationships among the participants (as individuals and as a collective), and how these relationships guided the dispute. An understanding of the events and reactions of the participants in the controversy provides insight into why it deviated from the proper behavior expected of those involved, along with how personality traits played a role. Ignoring the social context of the Hevelius-Hooke controversy provides only a limited understanding of this episode in the history of science. The ideal is to intertwine the two strands of the intellectual (and technical) and the social, rather than treating them as separate categories. In doing this, the controversy is contextualized.

Witnessing the "production" of knowledge also played an important role in this controversy. According to Shapin and Schaffer's argument, the testimony of eye-witnesses was effective only if the witnessing experience was accessible and the witnesses were reliable.[297] Although Hevelius lived and worked in Danzig, far from London circles, the testimony of his eye-witnesses was sufficiently effective to concern his critics. Supporters such as Burattini, Fullenius, and Boulliau 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 was considered a reliable witness by Royal Society members because even before the founding of the Royal Society, astronomers and scientists in Europe depended on Hevelius' observations and measurements.

Despite the fact that disputes were supposed to occur safely within the Royal Society, the proper behavior expected of participants often escalated into bitter controversy, especially where Hooke was concerned, although he was practically ostracized for his behavior. Hooke not only criticized Hevelius' inaccuracies, but also attacked him personally 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," and made Hevelius even more skeptical of the new astronomy. However, not all members of the Royal Society accepted Hooke's more personal attacks such as Oldenburg, Flamsteed, and Wallis, whose opinions were colored by the fact that they disliked Hooke who appeared to have "set himself up as the ultimate judge of knowledge"[298] in matters concerning optical instrumentation. Though Hevelius was a respected astronomer, his old-fashioned practices eventually lost favor with the new adherents of optical instrumentation. His great star catalog, *Prodromus astronomiae* (1690), was eventually superseded by Flamsteed's star catalog (also published posthumously by his wife), *Historiae coelestis britannicae* (1725), which "formed a sound basis

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[297] Shapin and Schaffer, Leviathan and the Air-Pump, 336.

for precision astronomy for almost a century."[299] Nevertheless, the precision of Flamsteed's telescopic-sight observations did not surpass Hevelius' precision until *after* Hevelius' death.

The common use of telescopic sights and micrometers in the 1660's placed an ever-increasing emphasis on precision, creating an unstable environment 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 partly guided and influenced by social factors – primarily, the personal relationships, interactions, and reputations of all the participants. The personal interactions affecting the controversy were not limited to the relationship between Hevelius and Hooke, however, and Hooke did not even have "allies" in Flamsteed or Halley. Nevertheless, each of them independently argued for the superiority of telescopic sights and provided epistemological justification for the superiority of telescopic sights over naked-eye sights.

Oldenburg, though not involved with the more technical arguments of the controversy, provided Hevelius with scientific information and acted as his agent in London. Oldenburg's relationship with Hooke had also been antagonistic in part 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 their correspondence where Oldenburg reported the latest news of the Royal Society. 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* that was an attempt to persuade others that Hooke had misbehaved and that Hevelius was justified in using whichever instruments suited him best.

Hevelius had not been 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

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[298] Shapin and Schaffer, Leviathan and the Air-Pump, 78. Eventually, Halley's relationship with Hooke also deteriorated.

[299] Volkoff, Franzgrote, and Larsen, Johannes Hevelius and his Catalog of Stars, 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].

against telescopic sights with his *Annus climactericus*. The event that ultimately terminated the controversy was Hevelius' death, but even after that, Hooke continued vindicating the precision and importance of telescopic sights. It seemed 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 Boulliau, and he died in 1694.[300]

The controversy discussed in this chapter was not simply a debate between two individuals over the reliability of certain new instruments in astronomy – it went much deeper. It involved Europe's foremost astronomers who, though not as abrasive as Hooke, were nonetheless concerned with the message being sent by Hevelius' consternation towards telescopic sights. The new precision astronomy of the seventeenth century and the new instrumentation such as the micrometer and telescopic sights on measuring instruments influenced the new practice. The more "scientific" aspects of this controversy coalesced with social issues involving testimony, experience, and modes of communication that forced individuals to not only take sides, but also define those sides. In this way, the controversy acted as a catalyst in defining the astronomical research programme by the end of the seventeenth century, and the consequences of the controversy are most evident in the work of Cassini at Paris and Flamsteed at Greenwich in the years that followed.

[300] Boulliau, however, had neither the diligence nor observational skills that were attributed to Hevelius' success.

Chapter 5
The Rise of Institutional Observatories in the Late Seventeenth Century: The Paris and Greenwich Observatories

The Paris and Greenwich Observatories emerged as the dominant observatories in Europe by the end of the seventeenth century. Though not the only observatories at the time, nor the first to carry reputable observatories, their being national observatories and state-funded – though at England only later – ensured their permanency. In order to better appreciate the role of these observatories in the development of seventeenth-century astronomy, it is necessary to consider certain characteristics of contemporary observatories – for example Hevelius' in Danzig. The latter was instrumental in establishing new standards for not only the external layout of observatories but also for the internal allocation of space, thus maximizing the viewing capacity of the observers and their ability to use (and properly store) their instruments.

Following such discussion, I shall describe the establishment of the Paris Observatory and Cassini's early work there. I shall then turn to the founding of the Greenwich Observatory and narrate the activities, concerns, and dilemmas facing John Flamsteed, the first Astronomer Royal. I then address the relations between the two Royal Astronomers. Despite the competition between the two observatories and considerations of national pride, Cassini and Flamsteed were fully cognizant of the need to collaborate if astronomy were to progress, and I shall offer a few examples of their exchange. I shall conclude by addressing the issue of the astronomer's role during the late seventeenth century through an analysis of the manner in which the two Royal astronomers perceived themselves and their activities.

Observatories in Europe in the Seventeenth Century

Though quite a few observatories existed in the seventeenth century – Leiden, Utrecht, Copenhagen, and Lund, for example[1] – their histories are often overshadowed by those of the Paris and Greenwich Observatories – except, perhaps of the most prominent observatory prior to the establishment of the latter two, that of Hevelius in Danzig, established in 1641. Simpler ground-level observatories were less popular than rooftop observatories, as evidenced by the total number of each type that existed in this period.[2] Because long telescopes needed space (reflectors were not commonly used until the 1720's), rooftop designs were preferred. Moreover, as Donnelly notes, "storage was necessary for instruments to be used outdoors and for records, shelter was required for fixed instruments whose parts were so delicate in construction and adjustment as to be imperiled if exposed to the weather, and lodgings nearby were needed for those working at night."[3] Given these conditions, the space in which astronomers worked was a shared space where activities involving more than merely astronomical observations were carried out on a daily basis.

In the final chapter of his *Machina coelestis pars prior* (1673), Hevelius described his observatory, "Stellaburgum," as well as Tycho's "Uraniborg." The account was not intended to be merely descriptive for Hevelius wished to promote his vision of what the ideal observatory should look like, where it should be located, and the type of view one should strive for when constructing an observatory. His normative appeal to European astronomers was accompanied by detailed illustrations of his observatory that depicted most of his instruments on the rooftop – though some peeped out of side-windows. Hevelius argued that under ideal conditions, the observatory should be near home (if not part of it), because in adverse weather, distance can be "tedious," "molesting," and even "damaging," to the observer. Furthermore, it should not be too high up, and it should have a clear view of the horizon:

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[1] For a detailed account of European observatories, see Marian Card Donnelly, A Short History of Observatories (Eugene, OR: University of Oregon Books, 1973) and William Hunter McCrea, The Royal Greenwich Observatory (London: Her Majesty's Stationery Office, 1975). Donnelly's book contains seventeenth-century illustrations of the observatories at Leiden, Copenhagen, and Utrecht.
[2] Donnelly, A Short History of Observatories, 27. Locations of rooftop observatories of the late seventeenth and early eighteenth centuries included: Nuremburg (1678), Lyons (1690) (which actually had a revolving roof), Marseilles (1702), and Trinity College, Cambridge (1706) (which was eventually pulled down in 1797).
[3] Donnelly, A Short History of Observatories, 28.

> Hincque, si diu noctuque Speculam ab ædibus tam remotam toties adire, eamque per tot scalas ascendere oporteat, inprimis tempestate dubia & adversa, Observatoribus sanè admodùm est tœdiosum atque molestum, quin-etiam damnosum ipsis Observationibus.[4]

Towards the end of the chapter, Hevelius summarized again the necessary conditions for a successful observatory:

> The towers and temples of our city, although, in their own way they rise up high enough; nevertheless, they are not too close to this our Stellaburgum, so that they would impede the view of the stars in any way too much. So the hills situated to the west and southwest, although facing our observatory, since the highest of these hardly rises above the step, they do not in any way detract from the observations or are they able to fall between our prospect nor to obstruct [the view].[5]

In England, there were more observatories in the second half of the seventeenth century and, with the exception of Greenwich Observatory, centered around institutions such as Gresham College, Cambridge, and Oxford. In fact, most astronomers in England "worked for all or part of their careers in universities. . . or at Gresham College in London."[6] With regard to astronomical activity, however, the main "observatories" of England in the late seventeenth century were connected with institutions. Two tables in Murdin's text, Under Newton's Shadow, are particularly informative. The first is a table that lists the chief British astronomers active in the time of Newton.[7] He groups astronomers by the following categories: full-time professional astronomers, university men teaching astronomy or mathematics (including Gresham College), amateurs with income from land, amateurs with other jobs, paid workers in astronomy, and astrologers and almanac makers. According to this table, most astronomers fall into the category of university men. The second table lists undergraduate members of the universities with a major interest in astronomy, and he separates out the undergraduate members of Oxford (Boyle, Halley,

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4 Hevelius, *Machina coelestis pars prior*, 440.
5 A relatively literal translation of the following:

> Turres & Templa nostræ Urbis, ut ut suo modo satis quidem in altum assurgant, non sunt tamen huic nostro Stellæburgo nimis propinqua, ut adspectum Siderum quovis modo multùm impedire queant. Colles item ad Favonium & Africum siti, Observatorio nostro licet obversi, cùm vix altissimi eorum ultra gradum ascendant, haud quicquam Observationibus derogant, vel prospectum nostrum intercidere sive præpedire valent (Hevelius, *Machina coelestis pars prior*, 447-448).

6 Murdin, Under Newton's Shadow, 20.
7 Table 2.1, Murdin, Under Newton's Shadow, 8.

Hooke, and Wren) and Cambridge (Flamsteed, Newton, Oughtred, Wallis, Ward).[8] Cambridge seems to be only slightly ahead in the total number according to this table.

The conclusions drawn from Murdin's tables support the argument made in Chapter 2 of this study, that the English universities of the seventeenth century were not barren of mathematical and astronomical instruction. However, these subjects did not flourish because they were viewed as subjects better fit for sailors and merchants rather than academicians. Nevertheless, mathematics, astronomy, and physics all gained momentum in the curriculums of both Oxford and Cambridge during the course of the seventeenth century, and most of the noteworthy astronomers in the second half of the seventeenth century were university men.[9] The other trend that developed during the course of the seventeenth century involved instruction in the developments of contemporary science. As the century unfolded, the subjects of the lectures focused more on current science than on rehashing the work of the ancients.[10]

Astronomy and mathematics at both Cambridge and Oxford may have been pursued vigorously in the late seventeenth century, but the lack of endowed observatories was keenly felt by those interested in the pursuit of astronomy. Edward Bernard (1638-1696), Savilian Professor of Astronomy at Oxford, for example, informed Henry Oldenburg that "the want of an Observatory damps Astronomique studies here, For without the proofe of our owne eyes & Instruments, the whole businesse is too precatory,[11] & the lines that have been drawen upon trust to the certainty of former Observations too too many."[12] Moreover, royal patronage was granted in the establishment of both observatories and prominent architects from each country were involved in the design of each observatory. Furthermore, "it was recognized [at both observatories] that a single chamber for all instruments would not be adequate for an extensive observing program and that some differentiation of housing for the various kinds of instruments was desirable."[13] The main difference between the two observatories was that astronomers at the Paris Observatory "did not carry out the type of multi-year projects undertaken at Greenwich Observatory where there was a clearly defined observational program, a director, his assistant

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[8] Table 3.1, Murdin, Under Newton's Shadow, 38.
[9] Murdin, Under Newton's Shadow, 38.
[10] Murdin, Under Newton's Shadow, 39.
[11] The Halls indicate that Bernard most likely meant "precarious" (OC, IX, 243 n. 3).
[12] 14 September 1672, OC, IX, 242.
[13] Donnelly, A Short History of Observatories, 14, 22.

and calculators, all supervised by a committee of academicians."[14] Furthermore, Greenwich Observatory was founded for the express purpose of determining longitude, and committees were eventually created for this reason including the Greenwich Board of Visitors in 1710 and the Board of Longitude in 1714. The *Bureau des Longitudes*, designed to improve navigation and oversee the work at the Paris Observatory, was not organized until 1795.[15]

The Paris Observatory, Cassini, and Early Work at the Observatory

In 1666, Charles Perrault proposed the creation of a "General Academy" in which men with a wide breadth of interests in subjects such as history, philosophy, and mathematics could come together to share ideas.[16] The chief minister of France, Jean-Baptiste Colbert, saw this as an opportunity to glorify the monarch, Louis XIV, and create for himself the position of "Minister of Culture." However, this never materialized because "in practice, [Colbert] was limited, not so much by the king's personal whims as by already established cultural groups which developed an independent *esprit de corps* and resisted all attempts to have their prerogatives diminished."[17] Plans for a General Academy were therefore abandoned and Colbert had to contend with the creation of the Académie des Sçiences. Among its original fifteen members were Christian Huygens, Adrien Auzout, Jean-Baptiste du Hamel, Bernard Frénicle de Bessy, Gilles Personne de Roberval, and Charles Perrault's brother Claude (1613-1688), who later became instrumental in the construction of the Paris Observatory.

The reputation of exclusiveness "was not based upon a sense of snobbery," and in fact, "for Colbert and his advisors, the determining factor for eligibility was competence in scientific matters."[18] Nevertheless, artisans were excluded, as were the "rigidly-philosophic" Cartesians and Jesuits. Eventually, two sections of the Royal Academy emerged: "a mathematical one including all the 'exact sciences'; and a physical one concerned with the more 'experimental

[14] Kevin Krisciunas, Astronomical Centers of the World (Cambridge: Cambridge University Press, 1988), 69.
[15] Krisciunas, Astronomical Centers of the World, 69.
[16] Details of the creation of the Académie des Sçiences can be found in "Chapter 1: Initiating a Tradition," of Roger Hahn's, The Anatomy of a Scientific Institution. The Paris Academy of Sciences, 1666-1803 (Berkeley, CA: University of California Press, 1971).
[17] Hahn, The Anatomy of a Scientific Institution, 13-14.
[18] Hahn, The Anatomy of a Scientific Institution, 15.

sciences', such as physics, chemistry, anatomy, and botany."[19] Soon after, new members joined the original fifteen including Giovanni Domenico Cassini, Ole Roemer (1644-1710), and Nicolas-François Blondel. Among all the pursuits of the Académie in the first few years of its existence, the most notable involved the building of an observatory. Originally, the observatory was designed to serve as the meeting place for the Académie as a whole, and the architect selected for such a grandiose building was one of the original members, Claude Perrault, who was provided in 1667 with a piece of land south of Paris for that purpose.[20]

On June 21, 1667, the longest day of the year (summer solstice), the mathematicians of the Academy determined and traced on the ground a meridian line and other lines essential for determining the orientation of the building. The location was chosen carefully: to the north was the abbey of Port-Royal de Paris, to the east was the "noviciat des Capucins," to the west that of the Oratorians, and to the south was the countryside scattered with windmills.[21] Moreover, astronomers knew that an unrestricted southern horizon was important. Unfortunately, in Perrault's original design the horizon was not unrestricted in all directions which forced astronomers to build a smaller observatory on the roof of the main building.[22] Additional problems in the construction design involved arranging instruments on the rooftop – a drawback for any observatory whose instruments were exposed to bad weather and wind. Furthermore, since the instruments had to be removed in more severe weather, astronomers had difficulty accurately aligning and collimating them. In addition, observations from windows also had serious drawbacks since windows impeded "observing throughout the whole plane of the meridian, also necessary for proper collimation of the instrument. Similarly, that would exclude observing at the zenith."[23] However, despite these setbacks, the building itself was impressive:

> The plan of the Paris observatory is a rectangular block, 96 feet by 84 feet, to which are attached octagonal towers at the southeast and southwest

[19] Hahn, <u>The Anatomy of a Scientific Institution</u>, 16.
[20] See for instance, Wolf's, <u>Histoire de l'Observatoire de Paris de son Fondation à 1793</u>, (1902) which chronicles the history of the Paris Observatory from its origins to the French Revolution. Also, Suzanne Debarbat's, <u>L'Observatoire de Paris son Histoire (1667-1963)</u> (Paris: Observatoire de Paris), 1984, is a recent source of the history of the Observatory with many illustrations and photographs. Other secondary sources include, Monique Pelletier, <u>La Carte de Cassini: L'Extraordinaire Aventure de la Carte de France</u> (Paris: Presses de l'École Nationale des Ponts et Chaussées, 1990), 42-44; Krisciunas, <u>Astronomical Centers of the World</u>, 62-68; and Donnelly, <u>A Short History of Observatories</u>, 15-19. Pelletier includes a rare sketch by Claude Perrault of members scattered on the observatory's rooftop (Pelletier, <u>La Carte de Cassini</u>, 43).
[21] Debarbat, <u>L'Observatoire de Paris son Histoire (1667-1963)</u>, 5.
[22] Donnelly, <u>A Short History of Observatories</u>, 17.
[23] Krisciunas, <u>Astronomical Centers of the World</u>, 64.

> corners and a square tower on the north. Long, roundheaded windows in both uppers stories, rather closely spaced, are separated horizontally by a simple string course. The roof is surmounted by a balustrade, and except for a pediment sculpture now vanished from the north projection and a vertical band of sculpture on either side of the central south window of the second story, the building was almost without ornament. Presumably, neither iron nor wood were to have been used on the original construction, but tie rods were found during later repairs.[24]

Outside the building, astronomers erected telescopes slung on poles and, after 1685, the water-tower of Marly was added. This 130-foot wooden tower (originally from Versailles) was ideal for the objective lenses of long telescopes. A ladder and balcony were eventually added to ensure the safety of observers.[25]

News concerning the construction of the Paris Observatory quickly spread through correspondence. Henri Justel, Oldenburg's Parisian correspondent and secretary of the Académie, first mentioned the observatory in a letter dated April 15, 1668 – some four years before the completion of the building. Justel intimated that "Mr. Colbert has laid the first stone of the Paris Observatory, which will be magnificent" enclosing, later that month, a plan of the observatory that Auzout presented to Oldenburg.[26] Unfortunately, this plan has disappeared but the one sent by Francis Vernon (?1637-1677) on July 23, 1669 still survives.[27] It provided the dimensions of the observatory and was accompanied by a simple, marked floor plan. Apart from more technical measurements and information provided, Auzout's enclosure mentioned that the walk around the terrace would include "gardene beds wherein are to bee planted all such plants as have sympathy wth the heavens, & observe the motions of the sun and starrs as marigolds, heliotropes, Tragopogon, Roses &c."[28] The building was completed in 1672 – with further additions to the interior made later – but Oldenburg was informed that the telescopes were in use as early as 1671. As he wrote Hevelius,

> under the leadership of Cassini, the Parisian astronomers have at last fallen seriously to work upon celestial observations, the Observatory's telescopes having been erected. They lack nothing for the successful

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[24] Donnelly, A Short History of Observatories, 16. See also Wolf, Histoire de l'Observatoire de Paris de son Fondation à 1793, 13; and Debarbat, L'Observatoire de Paris son Histoire (1667-1963), 5-6.
[25] See Michael Hoskin, ed, The Cambridge Illustrated History of Astronomy (Cambridge: Cambridge University Press, 1997), 166-167. Hoskin includes a lavish color illustration of the immediate surroundings of the Paris Observatory.
[26] Justel to Oldenburg, c. 10 June 1668, OC, IV, 323, 462, 463n.
[27] Vernon to Oldenburg, OC, VI, 147-149.
[28] OC, VI, 147-148. "Tragopogon" is Goatsbeard whose flowers close at noon (OC, VI, 149 n. 2).

> pursuit of scientific goals, since the royal bounty has generously furnished all the finance needed for that purpose.[29]

The largesse of the "royal bounty" may have been an exaggerated assessment of royal interest in astronomy, however, because it was not until 1677 that the Dauphin visited the Observatory and only in 1682 did the King grace it with his presence.[30]

In 1669, Gian Domenico (Jean Domenique) Cassini arrived from Bologna to take over as the first director of the observatory, the first of four generations of the Cassini family that would hold the position until 1771.[31] Cassini I's reputation as an astronomer was recognized even before the formation of the Académie des Sçiences as indicated in Bernard Fontenelle's (1657-1757) éloge:

> M. Colbert qui par les ordres du Roy avoit formé l'academie des Sciences en 1666, desira que M. Cassini fût en correspondance avec elle, mais bientôt la passion qu'il avoit pour la gloire de l'etat, ne se contenta plus de l'avoir pour correspondant de son academie, il lui fit proposer par le comte grasiani ministre et secretaire d'etat du Duc de modène, de venir en france, où il recesroit une pension du Roy proportionnée aux employs qu'il avoit en italie. . . .[32]

His reputation as an astronomer became established with his work on the sun and its apparent motion, the obliquity of the ecliptic, and the positions of the solstices and equinoxes. This preliminary work led to his tables of the sun's motion published in 1662. Moreover, Cassini's telescopic observations of Venus, Mars, and Jupiter led to his calculation of their rotation periods. Observations of Jupiter's satellites led to Cassini's publication of two sets of tables for their motions, in 1668 and 1693.[33]

Cassini was paid a most generous salary at the Paris Observatory,[34] and began working immediately upon his arrival.[35] Unfortunately, his privileged background, authoritarian attitude,

[29] Oldenburg to Hevelius, 12 June 1671, OC, VIII, 98.
[30] See Wolf, Histoire de l'Observatoire de Paris de son Fondation à 1793, 116-119.
[31] For a more detailed account of the four generations of Cassinis, see Debarbat's, L'Observatoire de Paris: Son Histoire, 9-14.
[32] The manuscripts of the Paris Observatory contain a wealth of information on the work of Cassini. References to the Paris Observatory archives follow the notation I have used above. In most cases, the material is not paginated, but rather, collected together in a series of notebooks that are found listed in a general catalogue. The "PS" refers to the Paris Manuscripts. PS: $\mathbf{B4}_3$. 5*.16.
[33] Craig B. Waff, "Cassini, Gian Domenico (Jean-Domenique) (1625-1712)," in John Lankford, ed., History of Astronomy. An Encyclopedia (New York and London: Garland Publishing, Inc., 1997), 123.
[34] Specifically, Cassini was paid 1000 écus for the trip over, and was provided gracious accomodations and a pension of 9000 livres [Debarbat, L'Observatoire de Paris son Histoire (1667-1963), 10].

and weak command of the French language did not endear him on many academicians, and only through hard work did Cassini eventually win his colleagues over.[36] Over the course of forty-three years at the observatory, he continued his observations of solar system objects and in the process, discovered four Saturnian satellites,[37] observed the narrow band separating the planet's ring into two parts (Cassini's division), and determined the distance of the sun from the earth based on solar parallax and observations of Mars' opposition in 1672. Cassini was also interested in the shapes of planets, especially Jupiter and the earth. Unlike Huygens and Newton who believed in the flattening of the earth at the poles, Cassini argued for the sphericity of the earth, and in the last years of his life, proposed the Cartesian-favored view that the earth was elongated at the poles – a view defended fervently by his son, Jacques (1677-1756).[38]

One of Cassini's first tasks was to modify Perrault's design so that the observatory could better suit the housing of instruments and the procurement of observations. Like Hevelius, Cassini provided information regarding the construction of the Paris Observatory and recommended the ideal routine for an astronomer engaged in compiling a star catalog:

> On travaille depuis 24 ans a l'Observatoire a faire une description exacte des etoiles fixes et a determiner leur situation par rapport a l'Equinoctial et a l'Ecliptique. Cett' ouvrage sera utile a l'Astronomie, a la Geographie, et a la Physique, et il servira encore a faire connoistre s'il y a d'autres etoiles

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[35] The following account of Cassini's accomplishments is from "Des divers Instruments Astronomiques de M. Cassini:"

> M. Cassini a inventé divers instruments fort commondes qui facilitent beaucoup l'intelligence de l'astronomie. Il en a un qui represente les trois celebres sistemes de Ptolomée, de Copernic et de Tycho ou font décrits les Cercles des Planétes dans leurs justes proportions et dans leurs situations veritables; par le moyen de cet instrument on trouve entout temps le vray lieu des Planétes dans le Zodiaque et leurs distances a la Terre. . . . Il a trouvé la methods de decrire la ligne du movement composé des Planétes dans lequelle on voit a l'eil les degres de leurs vitesses les stations, les directions, les retrogradations et les lieux apparens des Planetes dans le zodiaque jour-par-jour. Cet ouvrage est d'une grande utilité dans l'astronomie parce qu'il fait conmoistre les temps les plus propres pour les Observations qui servent a établir les hypotheses (PS: $\mathbf{D1}_{13}$).

$\mathbf{D1}_{13}$ refers to the notebook entitled, "Cassini I – Ses premières observations à Paris. – Notes sur l'Observatoire. – Pièces relatives à Cassini I."

[36] Debarbat, <u>L'Observatoire de Paris son Histoire (1667-1963)</u>, 10. Debarbat is correct, although M. Du Hamel's account found in his "Extrait du I Volume de l'Histoire de l'Academie," differs:

> M. Cassini arriva en France vers ce temps la, ayant esté appellé d'Italie par le Roy invincible et souverain protecteur des sciences. Apres avoir salué le Roy a qui M. Colbert l'avoit introduit, il fust ?siège dans l'Academie *avec une joye universelle* (emphasis mine, PS: $\mathbf{B4}_{2}$. 4*.4).

[37] Iapetus (1671), Rhea (1672), Tethys (1684), and Dione (1684).

[38] Waff, "Cassini," in Lankford, ed., <u>History of Astronomy: An Encyclopedia</u>, 123. Cassini solved the problem of the obliquity of the Ecliptic through the expedition to Cayenne in 1673 by Jean Richer (1630-1696).

qui paroissent et qui disparoissent, outre celles que nous connoissons deja et qui ont cette proprieté.[39]

Cassini announced that such a star catalog was already underway, but in order to complete it, the purchase of another instrument (at the cost of 1000 francs) was necessary, as was its proper fitting in the observatory:

> La description des Etoiles est presque achevèe, et il ne reste a determiner que celles qui passent proche du Zenit. Pour la finir il faudroit avoir un instrument qui seroit propre pour ces observations et pour toutes les autres qui on fait ?foies les jours au meridien; cett instrument couteroit environ mille frans. Il faudroit encore faire batir dans la loin de l'observatoire un petit endroit avec un voit ouvert d'ou l'on puisse avec cett' instrument faire proche du Zenit les observationes qui ne se peutient pas faire dans lieu converts.[40]

Indeed, Cassini considered the Paris Observatory itself as an instrument that could be constructed and modified in order to yield observations of the highest caliber.[41] For instance, he wrote that observing stars at the zenith with "the most grand precision" requires the astronomer to cross the vault of the observatory, descend a round staircase, and move to the back of the "caves" from which he can observe the stars. At this location, the distance between the observer and a pinnule or objective lens was approximately 165 feet (the greatest distance between the bottom and very top of the observatory):

> Pour avoir la commodite d'observer les astres proches du Zénit avec la plus grande exactitude on a laissé des ouvertures rondes à travers les voûtes de l'Observatoire qui sont verticales les unes au dessus des autres, et répondent au fond des caves par lesquelles on descend par un escalier en rond dont le [?] forme une espèce de [?]. Du fond des caves on peut observer les astres à la distance de 165 pieds jusqu'au haut de l'observatoire où l'on place, quand on veut observer, une pinnule ou un verre objectif d'une longueur un peu moindre que cette hauteur.[42]

In addition to providing astronomers with instructions on using the observatory to their advantage, Cassini also described in detail how individual instruments should be used for

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[39] PS: **D1**$_{13}$.
[40] PS: **D1**$_{13}$.
[41] In his *Mémoires pour servir à l'histoire des sciences*, Cassini wrote, ". . . j'aurais voulu que le bâtiment même de l'Observatoire eut été un grand instrument" (293; cited in Donnelly, A Short History of Observatories, 16). Cassini also used the church of San Petronio in Bologna as a measuring instrument [See J.L. Heilbron, The Sun in the Church: Cathedrals as Solar Observatories (Cambridge, MA: Harvard University Press, 1999), 89-95].
[42] PS: **D1**$_{13}$.

specific types of observations. In his "instructions to those who work in the observatory," for example, he explained how a quadrant could be used to observe the sun at the horizon:

> Le matin au lever du Soleil il faut avoir preparé le quart de cercle de sorte que son filet horizontal dans la lunete qui sert de pinnule rase l'Horizon apparent a l'endroit ou le soleil se leve, et le filet du plumb rase le plan du quart de cercle et en l'instant que le bord anterieur du Soleil touche le filet Horizontal il faut marquer la seconde [?printhee] par la Pendule et escrire l'heure, minute, et secondes [?mintecs][43] par le filet du quart de cercle qui sont celles de la hauteur du bord anterieur du soleil.[44]

Cassini not only suggested, but required that astronomers mark the time according to a pendulum, and write down the exact hour, minutes, and seconds by the thread of the quadrant. Only such meticulousness could withstand the scrutiny of an astronomer's peers.

Cassini also had good telescopes at his disposal, one of which was a 17-foot Campani telescope that he had used to study the planets. In 1671, Cassini turned his telescope towards Saturn when the Earth passed through the (extended) plane of Saturn's ring, and witnessed the view of Saturn edge-on:

> At the same time that the New Spots of the Sun began to appear, Signor Cassini observed in Saturn also something remarkable, in regard of the unexpected Change of his Figure. Astronomers know, that this Planet is for the most part seen with Arms or Anses fastened to the two sides of his disque, when he is beheld with some great Telescope; and that he retaketh not his Round Figure, but every fifteenth year.[45]

According to Huygens' hypothesis and predictions of 1659, this "roundness" of Saturn should not have appeared until July and August of 1671. Huygens even came to his own defense in the matter by stating:

> Monsieur Huygens having examined these Appearances, and the Cause of the difference from what he predicted of them, finds not that they are contrary to his Hypothesis of the Flat Ring about Saturn, by the means of which he explicateth all the Changes of his figure. . . . Monsieur Huygens believeth also, that Saturn will appear the next summer, after his Conjunction with the Sun, with Arms like those he hath now; in which he amends his Prediction of A. 1659. . . .[46]

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[43] Probably "secondes minutes" – a common way to refer to what we call seconds.
[44] PS: **D1**$_{13}$.
[45] PT, VI, no. 78, 3024.
[46] PT, VI, no. 78, 3025. On the following page (3026) of the same volume, there is a translation of an extract of Huygens' response (dated 7 November 1671).

In a later volume of the *Philosophical Transactions*, a series of articles appeared regarding the discovery of two new satellites of Saturn entitled, “A Discovery of two New Planets about Saturn, made in the Royal Parisian Observatory by Signor Cassini, Fellow of both the Royal Societys, of England and France; English’t out of French.”[47] The observations described were performed between 1671 and 1673, and Cassini carefully described not only the observations in detail, but also the principal instruments he and others had used. For instance, in November 1672, Cassini had brought back with him a Campani-made telescope of 35-foot length that was set up in the Paris Observatory and used to make subsequent observations of Saturn’s satellites.[48] Interestingly, however, not all telescopes used during these observations were Campani-made. Observations of January 1673 were made by Cassini and others “not only with the Telescope of Campani, but also with one of Divini of 36 foot; and one of the same length of Mons. Borelli, who is a Member of the R. Academy of the Sciences.”[49] Clearly, if Divini’s telescopes had been entirely discredited through the Italian *paragone* of the 1660’s, a Divini telescope would not have been used to observe a phenomenon that was not even clearly understood by the best astronomers of Europe at the time. However, Divini and Campani telescopes were not the only telescopes used. In addition to the Borelli telescope mentioned above, a telescope made by a Monsieur Le-Bas of 20-foot length was used by Picard in February of the same year, “and sometimes [even] in the Company of Mr. Huygens and Mr. Mariotte.”[50]

In 1686, an extract from the *Journal des Sçavants* was published in the *Philosophical Transactions* in which it was announced that Cassini had discovered two more satellites in 1684. The introduction to Cassini’s discovery is an eloquent oratory of his contributions to the study of the heavens:

> The diligence of those that have gon [sic] before, having left only the most difficult and obscure Objects to discover, these Satellites of Saturn which are eminently so, by reason of their Smallness and great Distance from the Sun and Earth, have fallen to the share of Sign. Cassini, who being furnished with Telescopes of an extraordinary length and goodness, has been able to see deeper into the Heavens, than those that have hitherto attempted.[51]

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[47] PT, VIII, no. 92, 5178-5185. Titan had already been discovered by Huygens in 1655. The two satellites observed by Cassini and described in this article were Iapetus (1671) and Rhea (1672).
[48] PT, VIII, no. 92, 5180.
[49] PT, VIII, no. 92, 5183.
[50] PT, VIII, no. 92, 5183-5184.
[51] PT, XVI, no. 181, 79-85. These were Tethys and Dione. The next two Saturnian satellites were not discovered until 1789 by William Herschel.

In addition to using several Campani-made telescopes of 136, 100, 90 and 70 feet each, Cassini also observed the satellites "with Glasses of Mr. Borelli of 40 and 70 feet, and by those which Mr. Artouquel hath lately made, of 80, 155 and 220 feet." Cassini wrote about these latest discoveries and connected them to his methods of observation by which he was able to predict the movements of the satellites:

> Nous avions dêja decouverts deux satellites de Saturne par les observations du mois de mars et d'Avril de l'an 1684 faites sans tuyau quand M$^{r.}$ Huguens publia, et m'envia son Astroscopie, dans la quelle il enseigne a se servir des verres sans tuyau mettant l'objectif en equilibre sur un genou, quise hausse et se faisse sur un coulisse, et le dirigeant a l'Astre par un fil qui va de l'objectif a l'oculaire placè sur un pied, et dans la rêponse que je luy fis je luy envoiay ces observations faites par ma methode, qui sont les mesmes que j'ay emploiay pour trouver les movements de ces satellites et que je represente par ma Theorie aussi bien que les autres.[52]

Campani-made telescopes may have therefore been the most reputable and noteworthy telescopes of the late 1660's and 1670's, but certainly they were not the only telescopes used.

Regardless of the impressive collection of instruments, Cassini could not escape certain challenges in their construction and use. Among the more common problems that all astronomers faced – especially with rooftop observatories – was the constant difficulty of instrument-calibration that was usually compounded by ordinary wear and tear. Cassini was aware of the benefit of fair weather during observations, a sentiment he shared with Picard:

> Dans cette dernier observation le temps estoit fort beau [?], le satellite ma paru languir plus longtemps que d'ordinaire, estant reduit comme a un point un quart de minute avant qu'il disparut entrerement. Nous avons la plus belle saison qui ait jamais esté [?]. Je suppose que vous l'aures de mesme et que vous aurez bien du plaisir a observer. Je salue toute la compagnie.[53]

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[52] Cassini, "L'usage des verres sans Tuyau pratique dans les derniers decouvertes," PS: **D1**$_{11}$. In this case, Cassini did not use tubes, for as he also stated in a translation of an extract of the *Journal des Sçavants* published in the *Philosophical Transactions*, "We made use of them [glasses] without Tubes, by a more simple contrivance than those proposed either before or since" (PT, XVI, no. 181, May 25, 1686, 83).

[53] Cassini to Picard, 25 October 1680, PS: **D1**$_{11}$. In his article "Jean Picard's 'Membership' in the Académie," John W. Olmsted concludes that Jean Picard was not an original member of the Académie, that his admission was promoted by Auzout, and that Picard himself may have persuaded Colbert that his abilities as an astronomer were more important than his social status [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), 101].

When the weather was uncooperative, Cassini frequently complained, as he did in a letter to de la Hire in which Cassini griped that his attempts to observe Jupiter's satellites had been interrupted because "le Ciel estoit trouble, et le vent agitoit les Lunettes."[54] The academicians were aware that both random and systematic errors could easily creep into measurements and observations if astronomers did not take proper precautions to at least minimize such errors, and even Cassini admitted that observations were procured only with extreme difficulty – though he insisted that these were feasible if the observer was experienced, a point he defended in a letter to an unknown correspondent who had criticized his observations:

> Je vois par votre lettre que vous n'estes pas entierement content des observations que j'ay vous ay communiquees, pour satisfaire a vos demandes. Et je ne voit pas que dans la supposition ou vous estes que l'on puisse determiner les hauteurs des astres avec une entiere evidence jusques aux derniers secondes je pense jamais vous satisfaire entierement. Le grand nombre d'experiences que j'ay faites mon fait voir que c'est une chose d'une extreme difficulté.[55]

At the end of his life, Cassini wrote a poem in which he described the feebleness of the senses and the attainment of truth in astronomy. The fragment, preserved in the Paris Observatory archives reads:

> Ainsi, l'illusion, fatale à la science,
> Doit toujours sur nos sens nous mettre en défiance:
> La vérité sans cesse à nos yeux se soustrait,
> Si nous ne démêlons l'apparence du vrai.[56]

Concern over instruments and measurements were negligible compared to the pressing administrative problems faced by the Paris Observatory in the 1680's and 1690's. In this period, the Danish astronomer Ole Roemer returned to Denmark in 1681, Jean Picard died in 1682, and Huygens left Paris in 1681 due to illness and decided not to return after the revocation of the Edict of Nantes in 1685 that would have made life difficult for even a nominal Protestant. The "consequent neglect in observational and laboratory work" was exacerbated by the disinterest in "purely theoretical researches shown by Colbert's successor Francois Louvois" whose

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[54] Cassini to de la Hire, 27 October 1681, PS: **D1**$_{11}$.
[55] Cassini to ?, dated ?, PS: **D1**$_{11}$.
[56] "Fragment d'un poème sur l'Astronomie par Cassini," translated from Italian into French, PS: **D1**$_{11}$. By 1710, Cassini was blind. He died in 1712 and was buried in his own parish of Saint-Jacques-du-Haut-Pas [Debarbat, L'Observatoire de Paris son Histoire (1667-1963), 11].

appointment in 1683 as "Protector" of the Académie was followed by a period of relative inactivity until 1699 when the Académie was "radically re-organized and enlarged."[57]

The Greenwich Observatory and Flamsteed's Tenure

By the time the Paris Observatory was completed, plans to build a national observatory in England were only getting underway. The motives connected to the building of the Paris Observatory – an instrument of the Académie des Sçiences – were different from the more practical interests of the English.[58] In fact, compared to the relationship between the Académie des Sçiences and the Paris Observatory, the relationship between the Royal Society of London and the Greenwich Observatory was more limited. Whereas the Paris Observatory was originally intended as the locus of Académie activities, the Royal Society of London "had no formal responsibility for the [Greenwich] Observatory or its work during the first 35 years of its existence."[59] This does not mean, however, that Flamsteed had no close connections with the Royal Society. In fact, he interacted with members early in his career and the Royal Society was the "preferred medium" for his work.[60]

The decision to build a national observatory in England was informed by the desire to solve the problem of longitude that was necessary for the perfection of navigation – more specifically, for the accurate determination of the distance sailed in an east-west direction.[61] Determining latitude was easy but determining longitude at sea was virtually impossible and presented the "greatest source of error" in navigation during the seventeenth century. There

[57] Forbes, Greenwich Observatory, 7. Cassini's son, Jacques, sent his first letter to Flamsteed in 1699.
[58] Works on the RGO include: E. Walter Maunder, The Royal Observatory Greenwich (London: The Religious Tract Society, 1900); McCrea, The Royal Greenwich Observatory; Sir Harold Spencer Jones, The Royal Observatory, Greenwich (London: Longmans, Green, and Co., 1943); Forbes, Greenwich Observatory, Vol. 1, Origins and Early History (1675-1835); and the works of Derek Howse, Francis Place and the Early History of the Greenwich Observatory (New York: Science History Publications, 1975); Greenwich Observatory, Vol. 3, The Buildings and Instruments (London: Taylor and Francis Limited, 1975); and, Greenwich Time and the Discovery of the Longitude (Oxford: Oxford University Press, 1980).
[59] Sir Bernard Lovell, "The Royal Society, the Royal Greenwich Observatory and the Astronomer Royal," Notes and Records of the Royal Society of London 48 (1994): 283.
[60] Mordechai Feingold, "Astronomy and Strife: John Flamsteed and the Royal Society," in Willmoth, ed., Flamsteed's Stars, 32-33, 34. More specifically, "the early stage of Flamsteed's association with the Royal Society verged on the idyllic" (Ibid., 32).
[61] The role of maritime interests in the founding of the Greenwich Observatory is carefully detailed in Forbes, Greenwich Observatory, 8-17.

existed no precise method of determining the longitude of a ship based on meridian measurements of either a land-based observatory or the port from which a ship sailed; "but if both this coordinate and the corresponding value of the latitude were known," then the position of a ship at sea could be determined.[62] Longitudes are measured east or west of a prime meridian, which was arbitrarily selected in the late seventeenth century – for the French, the prime meridian cut across Paris, for the English it was London – at least until the construction of the Greenwich Observatory's.[63] A major problem for determining longitude involved the use of pendulum clocks that, though the best clocks available at the time, were practically useless on ships whose motion upset their proper working.

Although the building of the Observatory was already in the works, the proposal of the Frenchman le Sieur de St. Pierre to the court of Charles II that longitude can be determined by using the moon, was the incident "that hastened, if it did not occasion, the building of Greenwich Observatory."[64] St. Pierre's motivation stemmed primarily from his wish to impress Louise de Kerouall e, Duchess of Portsmouth and one of Charles II's most influential mistresses.[65] Flamsteed, however, was completely unimpressed with St. Pierre's method, arguing that the latter's proposal for longitude was ludicrous, impractical, and unoriginal:

> I am not easily persuaded to believe that this was really the Sieur de St. Pierre's invention since he has so cleanly betrayed his own ignorance; rather that he derived it from Morinus [Morin who first proposed finding longitude by a method based on lunar distance in 1634] or some other little-regarded author; but that more learned men were silent about it. . . because it was obvious to them that *no worse method could be devised*, not only because it is difficult to measure altitudes to individual minutes, but also because instruments are rarely made big enough to take them and they can be managed only in a steady position and with great difficulty. . . . I marvel therefore at the stupid effrontery of the man, that he thinks all our

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[62] Forbes, Greenwich Observatory, 11.

[63] Jones, The Royal Observatory, Greenwich, 29-30. In Germany, the prime meridian was in Berlin. Greenwich was chosen as the prime meridian of the world at a conference convened by the State Department in the United States in 1884. A system of standard (or zone) times based on Greenwich Mean Time, or GMT, was introduced at the same conference (Ibid.).

[64] Maunder, The Royal Observatory Greenwich, 32-33. Howse states that St. Pierre "failed to get his hoped-for reward – and though his identity is still a mystery – he has nevertheless secured for himself a place in the history of astronomy by acting unwittingly as a catalyst in the foundation of the Royal Observatory at Greenwich" (Howse, Greenwich Time, 30).

[65] Howse, Greenwich Time, 24. Howse discusses the Duchess' background in detail and includes an oil painting of her by Henri Gascar (Howse, Greenwich Time, 19-24).

countrymen are ignorant of these things, and are truly to be so easily deceived.[66]

Nonetheless, St. Pierre's proposal served its purpose, and Charles II issued a Royal Warrant in 1674 that established a commission to investigate St. Pierre's claims. The members of the commission included Lord Brouncker (c. 1620-1684), President of the Royal Society; Seth Ward, formerly Savilian Professor of Astronomy; Sir Samuel Morland, mathematician and inventor; Sir Christopher Wren, Surveyor-General and Savilian Professor of Astronomy (1661-73); Colonel Silius Titus, Gentleman of the Bedchamber; the mathematician John Pell; and Robert Hooke, curator of experiments at the Royal Society and Gresham Professor of Geometry.[67] During their deliberations, Flamsteed arrived in London (February 2, 1675) and stayed in the Tower with his patron, Sir Jonas Moore, who served as Surveyor-General of the Ordnance since 1669. In March of 1675, Flamsteed wrote his report on St. Pierre's proposal that was presented to the king who determined that the finding of longitude "must be pursued in a kingly manner, so that his ship-owners and sailors should lack no help which could be supplied from the heavens, [and] which would make navigation safer."[68]

On March 4, 1675, the king signed a warrant appointing Flamsteed the first Astronomer Royal, "for rectifying the tables of the motions of the heavens, and the places of the fixed stars, so as to find the so-much-desired longitude of places for the perfecting the art of navigation."[69] In becoming the first Astronomer Royal, Flamsteed "was inaugurating a new and unfamiliar breed: there had never been such a creature before in England."[70] He was acutely aware of his uncertain status, something he alluded to in his dioptrical discussions which he "elevated. . . into tools for determining the worth of his own identity."[71] Flamsteed was allowed the paltry salary of 100 pounds per annum and, to make matters worse, he was required to pay 10% of it in taxes. A Royal warrant was issued in June authorizing the construction of the Royal Observatory, "in order to the finding out of the longitude [of] places for perfecting Navigation and Astronomy, we have resolved to build a small observatorie within our Parke at Greenwich upon the highest

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[66] Flamsteed to Pell, 26 April 1675, FC, I, 337.
[67] Howse, Greenwich Time, 24-25.
[68] Allan Chapman, ed., The Preface to John Flamsteed's *Historia Coelestis Britannica* or *British Catalogue of the Heavens* (1725), trans. by Alison Dione Johnson (Greenwich, London: National Maritime Museum, 1982), 111.
[69] FC, I, 904.
[70] Adrian Johns, "Flamsteed's Optics," in Willmoth, ed., Flamsteed's Stars, 79.
[71] Johns, "Flamsteed's Optics," in Willmoth, ed., Flamsteed's Stars, 80.

ground."[72] Christopher Wren was asked to design the buildings and in August the foundations were laid; by Christmas, the exterior of the building had been completed.[73] The new observatory was built upon the foundations of an old tower that, unfortunately, did not have a true north-south axis. Consequently, the prime meridian line that is marked on the wall surrounding the building comes off the wall at an angle. The astronomer's lodgings were on the first floor, and an octagonal chamber above formed the observing room. This became known as the Octagonal Room, or Great Star Room,[74] but Flamsteed did not use it for serious astronomical work. Instead, he performed most of his observations out of a "small building at the bottom of the garden – the Sextant House and Quadrant (later Arc) House," where he constructed two meridian walls and installed his best instruments.[75] A mast was placed in the garden on May 1, 1676, and the first observations in the Great Room were made on May 31. Flamsteed, along with two of his "servants," Thomas Smith and Cuthbert Denton, moved into the Observatory on July 10, and the first sextant observation was made on September 16. At last, "the Royal Observatory was complete."[76]

Among the most prominent instruments at the Observatory were a 10-foot meridian mural quadrant and a 7-foot radius sextant – Flamsteed's main instrument until 1689. The latter was also the first among his large instruments to be fitted with a telescope.[77] In fact, Flamsteed was the first astronomer to systematically affix telescopic sights onto all his arc instruments.[78] By 1700, Flamsteed had completed collecting the instruments he needed. These included: mural instruments (Hooke's 10-foot quadrant [1676], a "Slight" mural Arc [1681], and a 140° mural

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[72] FC, I, 906.
[73] Forbes, Greenwich Observatory, 23. Derek Howse's Greenwich Observatory, Vol. 3, The Buildings and Instruments, contains a wealth of information on the buildings of the observatory including information on the specific buildings and work spaces of the Observatory in Flamsteed's time (1-6), drawings of Greenwich (plates between 32-33), an appendix with actual floor plans of the Observatory and how they changed over time (161-162), and an appendix listing the chronological changes to the Observatory (150-154). There is also a color illustration of Greenwich Observatory and environs in Hoskin, ed., The Cambridge Illustrated History of Astronomy, 178-179. The best collection of plates were drawn by Francis Place, and have been collected and published together in Howse's, Francis Place and the Early History of the Greenwich Observatory.
[74] Current visitors to this room will notice few changes with the Francis Place engraving, except that the instruments – a quadrant, telescope, and all but one of the Tompion clocks – are not the originals, nor do they work. Portraits of Charles II and his brother, James II, hang on the wall.
[75] Howse, Greenwich Observatory, 4.
[76] Howse, Greenwich Observatory, 5.
[77] FC, I, 1.
[78] McRea, The Royal Greenwich Observatory, 6. Chapman adds that "Flamsteed worked at the beginning of a new era of instrument technology, for it was at Greenwich that the telescopic sight, micrometer, pendulum and other devices were first put to rigorous use" (Chapman, ed., The Preface to John Flamsteed's *Historia Coelestis Britannica*, 6).

arc [1689]); a zenith instrument (Flamsteed's well telescope [1676]); a 7-foot equatorial sextant [1676]; various astronomical clocks; and minor astronomical instruments.[79] Some of the instruments were drawn by Flamsteed himself and are included in his "Originall draughts of my instruments," in the RGO archives. These included a micrometer, which was meant to be included in a letter to Hevelius,[80] his sextant with screw-control and notes,[81] and a close-up of a micrometer inserted into a telescopic tube.[82]

The physical integrity of an instrument, whether a telescope or a positional measuring instrument, was paramount in making precise measurements and observations. Spherical and chromatic aberration of lenses in telescopes continued to be a problem, as did stray light. But Flamsteed's main difficulties involved his measuring instruments. Measurements were inaccurate whenever there were errors of graduation in the limb, defects due to the wearing down of parts, or the warping of the frame over time. He rigorously double-checked all his instruments and knew that "when the screw method of graduation was used alone, without reference to the engraved divisions, it could mount up errors as big as one whole minute of arc."[83] Likewise, Flamsteed believed that it was disadvantageous to construct instruments that were too large, as Tycho and Hevelius had, because that would "render them Weighty & Consequently Cumbersome and Intractable."[84] If they bent even one quarter of an inch, the result would involve ten minutes of error at the zenith and five minutes of error at the ecliptic. The walls unto which his instruments were attached also created problems because they were subject to warping, while the ground underneath might sink as well.[85] Flamsteed complained about this problem by observing that, "The wall on which the Murall Arch is place was built. . . in the year 1675 but the instrument was not fixed to it till the yeare 1689 after which I found that the South end of the wall sunke every year and the errors of the instrument encreased [sic] some little annually."[86]

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[79] These are all described in detail in Howse's, Greenwich Observatory, Vol. 2, The Buildings and Instruments.
[80] RGO 1/69B, ff. 71^r.
[81] RGO 1/69A, ff. 26^r-27^r.
[82] RGO 1/69A, ff. 26^v.
[83] Chapman, ed., The Preface to John Flamsteed's *Historia Coelestis Britannica*, 5.
[84] 4 May 1682, Lecture 13 of the Gresham Lectures, in Forbes, ed., The Gresham Lectures, 232.
[85] In the nineteenth century, the electric railway, whose vibrations interfered with observations, had to be removed (Donnelly, A Short History of Observatories, 22, 25).
[86] Dated September 1716-August 1718, RGO 1/32C, ff. 66^r. He was referring to earlier events.

When instruments were not up to Flamsteed's standards, he was extremely critical as was the case with Robert Hooke. In fact, Flamsteed was quite censorious in judging Hooke's competence in designing instruments:

> I have long observed it in his nature to make contradictions at randome, and with little judgment, to defend them with unproved assertions. . . in the ende to magnifie or assert some stupendious invention of his owne which yet wee must expect an age or too, for hee has only the conceite not the experiment, which hee dares talke of, not put to triall.[87]

Flamsteed not only belittled Hooke's instruments but he also disparaged the person:

> hee pretends to know better than others, he makes questions to those hee knows are skilfull in them, and theire answers force him for assertions on the next occasion. this unworthy dealeing of his has a little exasporated mee I must confesse but I hope my Patrons and Reader will pardon mee and if they know but the impudence of this solely Mechanick Artist will thinke I take onely a moderate revenge in my justification whilest I endeavor to informe him better thinges then those on which hee grounds such monstrous boasts, and magnifies for such deepe knowledge as none besides himselfe is master of.[88]

The chief reasons for Flamsteed's dislike of Hooke went beyond personality conflicts. Hooke had boasted on more than one occasion of having constructed certain instruments, but these proved unreliable or unmanageable as Flamsteed eventually discovered. He reported in 1678 that "Hooke fills all with discourse and mighty projects of inventions and discoverys but they are seldome seene and when they are partiuriunt Montes."[89] In particular, Flamsteed complained about the 10-foot mural quadrant that Hooke constructed, an instrument that was ultimately abandoned when it proved too cumbersome and unreliable. As Flamsteed wrote Moore in 1678:

> I have often tried to make Mr. Hookes wall Quadrant give me altitudes and it has often deceived mee and lost its rectification. I tore my hands by it and had like to have deprived Cuthbert of his fingers. . . . But I am not much dissatisfied, since this has shown the difference betwixt boasts and performance.[90]

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[87] From "An Opticall Essay proveing the truth and accuracy of coelestiall observations made with longe telescopes occasioned by some discourse with Mr. Robert Hook," (RGO 1/50K, ff. 254^v-255^r).
[88] RGO 1/50K, ff. 254^v-255^r.
[89] Flamsteed to Bernard, 8 February 1677/8, FC, I, 600. The full proverb from Horace is "Partiuriunt montes, nascetur ridiculus mus" which translated means "the mountains give birth, an absurd mouse is born."
[90] 16 July 1678, RGO 1/36, ff. 61^r-62^r; also in FC, I, 645.

Flamsteed was forced to use a sextant instead in taking altitudes, which, he declared, was "never designed for that purpose."[91] Flamsteed did not know what to make of Hooke's "ill-contrived devices," but he wanted to convey to him how unreliable his instruments were and how detrimental to serious astronomical work:

> I am much troubled with Mr. Hooke who not being to be troubled with the use of any instruments will needs force his ill contrivd devices on us. . . . that an instrument of no more than 18 inches Radius should measure an Angle to less than 6 seconds, that hee has an instrument of Quad[rant] of 36 foot radius that weighs not a pound and which hee can put in his pocket. . . . hee is not to be persuaded that his unreasonable discourses are in the least erroneous: I know not how to deale with him but if nothing else will make him tractable have resolved to excerpe a Catalogue of his errors from his workes and represent them fairely to him to bring him to a freindlyer behavior.[92]

Hooke's own perceptions of Flamsteed are expressed in the terse entries of his diary. On May 28, 1674, for example, he wrote: "I shewd Flamsteed my quadrant. He is a conceited cocks comb."[93] At least on one occasion, however, Hooke stated that "Flamsteed and Lord Brouncker saw quadrant and approved."[94] Three years later, though, Hooke recorded seeing Flamsteed, that "Ignorant Impudent Asse" at Tompions, and the relations did not improve with time.[95]

Storage space for instruments and records was as crucial for the Greenwich observatory as it had been at the Paris Observatory. Other needs included lodgings for the astronomer and his assistants, especially since they worked primarily at night.[96] Unlike Cassini's displeasure and concern over Perrault's original design, however, the initial frustrations encountered by John Flamsteed were not directed towards the inadequacy of the original building, but rather at the lack of money for instruments as the Government did not provide funds. Even before the observatory was built, he was forced to borrow money from his father in order to purchase instruments. Unfortunately, his father was reluctant to accommodate his wishes:

> My father will not allow mee Monyes for a Pendulum clock nor can I, knowing how indulgent hee is otherwise, presse him this which hee had once denied. but must rest contented for a while till hee shall dismisse mee which hee has promised ere twelve moneths be over And allowd mee

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[91] Flamsteed to Boucher, 24 January 1677/8, RGO 1/43, ff. 36^r-38^r; also in FC, I, 595.
[92] Flamsteed to Richard Towneley, 3 July 1675, FC, I, 356.
[93] Robinson and Adams, eds., The Diary of Robert Hooke, 105.
[94] 16 July 1674, Robinson and Adams, eds., The Diary of Robert Hooke, 112.
[95] 27 November 1677, Robinson and Adams, eds., The Diary of Robert Hooke, 330.
[96] Donnelly, A Short History of Observatories, 28.

> that liberty which is necessary for a course of life I more affect. I meane these and my other studies.[97]

In 1678, Sir Jonas Moore procured for the Observatory a 3-foot quadrant made by Tompion according to Hooke's designs. When Moore died in 1679, Hooke demanded the instrument's return, and it was seized from Flamsteed's possession in October of the same year. Flamsteed claimed it "was done without my knowledge and so speedily and malitiously executed that I had not time to remedy it."[98] Fortunately, Flamsteed's construction of a 50-inch "voluble" (movable) quadrant was already underway and eventually, it enabled him to measure the altitudes of stars to within half a minute of arc.[99]

When Flamsteed's father died in 1688, Flamsteed was distraught. However, he inherited a small sum of money that he used to construct a 7-foot mural arc. Abraham Sharp (1653-1742), one of Flamsteed's most trusted assistants, constructed and "divided" the instrument under the watchful eyes of Flamsteed between August 1688 and September 1689. It covered an angle of 140 degrees (thus any star visible from the Observatory could be measured with it) and was fixed onto the meridional west wall of the Quadrant House. It became Flamsteed's favorite instrument for the next thirty years.[100]

Because of his financial constraints, it took Flamsteed fourteen years to finally assemble all the required instruments.[101] In a treatise on navigation, Flamsteed summarized his troubles (in the third person): "The Observer's allowance was very short of what the work required: No provision made for Assistants, nor for defraying the charge of Instruments wanting; which yet he has built at his own Expence, as he could afford."[102] Flamsteed often complained about the lack of proper instruments and the meagerness of his allowance that made the construction of new instruments difficult. In 1699, for example, he told Newton that his expenses had increased significantly:

> Sir My observations lie the King and Nation in at least 5000^{li}, I have spent above 1000^{li} out of my own pocket. in building Instruments and hireing a servant to assist me now neare 24 yeares: tis time for me (and I am now

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[97] Flamsteed to Oldenburg, 24 August 1673, FC, I, 245.
[98] Flamsteed to Richard Towneley, 25 October 1679, FC, I, 704. The entry in Hooke's diary for September 26, 1679 reads, "Flamsteed mad" (FC, I, 705 n. 5).
[99] See for instance, Flamsteed to Richard Towneley, 30 March 1678, FC, I, 616.
[100] John L. Birks, John Flamsteed The First Astronomer Royal at Greenwich (London: Avon Books, 1999), 57.
[101] Chapman, ed., The Preface to John Flamsteed's *Historia Coelestis Britannica*, 8.
[102] 19 April 1697, RGO 1/32D, ff. 126^{v}.

> ready for it) to let the World see I have done something that may answer this expence.[103]

Flamsteed's aggravation stemmed in no small part from the fact that without proper instruments his observations and calculations were no more precise than those of his predecessors. He compared his plight to Tycho's more fortunate situation:

> [Tycho Brahe] labored about 25 yeares on his Catalogue had a large estate of his owne with about 3000 dollars per Annum allowance from the King of Denmar[k] which in that cheap country and in those times was more then 3000 sterling here. he had 8 or 10 assistants. yet with his large apparatus of Instruments and helpe he left us not more then 780 places of fixed stars rectified by himselfe and them; and about 40 places of each planet deduced from his observations. . . . When I sat down here I had onely 100^{li} per Annum allowance from the Office of the Ordnance, with a surly silly laborer paid by the Office till about 6 yeares agone. . . . All the Telescope glasses, Tubes, and Quadrants I provided at my owne charge.[104]

Flamsteed even boasted that if he had all the necessary instruments for the rectification of tables and qualified assistants, Tycho's mistakes would have been corrected within a few nights.[105] Flamsteed was indefatigable in pleading his case to the proper authorities, hoping to generate support for his work, even claiming that national pride was at stake:

> Now instruments are to be made. more help allowed: to perfect the work that is greater than ever was achieved [sic] before it will be for the honor of the Nation to have [instruments] made and the observations continued to future Ages. Good Allowances to be made by the Admiralty and Navy boards for this purpose. . . . some of the principall officers [will] be good mathematicians and [will] understand the use of [the observatory] and the honor and advantage it will bring to the Nation. . . .[106]

Even if Flamsteed believed that all his instruments were in perfect working order, he still had to contend with the weather – the source of numerous complaints. Sometimes it was frost ("a verye Serene and Calme Morning, – But frosty, which often caused trouble to wipe the glasses. Yet the notes taken – I esteem very certain"[107]) or interfering clouds ("the clouds frustrated my designes of observing. . . it proving a very bad snowy morning, very troublesome

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[103] Flamsteed to Newton, 2 January 1698/9, FC, II, 739.
[104] Flamsteed to T. Smith, 26 October 1700, FC, II, 866-867.
[105] PT, VIII, n. 94, May 19, 1673, 6033.
[106] ?1694, RGO 1/26A, ff. 1^{v}.
[107] In Thomas Smith's hand, 16 November 1676, RGO 1/12, ff. 40^{r}.

to mee who was necessitated to take a journey that day sever^{ll} miles in bad weather and way"[108]) that were to blame. But the greatest adversary was wind. Under severe windy conditions, observations were not possible (the wind "shaked the tube," and this "prevented further observations"[109]). But even the slightest breeze could significantly affect telescopic observations and undermine the accuracy of Flamsteed's positional measurements – hence his relief "this evening was calm and serene, and therefore the following distances I judge very accurate."[110]

Flamsteed soon discovered, however, that even on clear and calm nights, his own servants could undermine his efforts for accurate observations. Assistants were indispensable for Flamsteed, especially when using the sextant that required at least three people to work – two observers "who ought to be skilfull in the businesse," and "any indifferent person of a Stronge able Body."[111] When his assistants did not live up to his expectations, Flamsteed vented his frustrations, as he did in a letter to Newton:

> And to assist me in all of this I have only a blockheadly fellow allowed to tell the clerk and move my Instruments. Another I keepe to copy my notes and write downe what I observed at my owne charge who can not be so usefull as he ought. . . . he is some distressed youth from a country schole with little inclination to those studies more than what necessity or want has given. Had I a skillful and diligent servant or assistant the worke might now proceed pleasantly.[112]

One such problematic assistant was Cuthbert Denton (fl. 1676-1680), of whom Flamsteed complained to Moore:

> On Monday last I gave Cuthbert leave to go to London to receave some Moneyes due to him, with charge to returne before night which he promised. but came not home till wednesday Morneing. so I lost two of the best and clearest nights wee have had this good while. . . . he is so carelesse of my injunctions still makeing good what hee told me formerly that hee will not be tied up to his houres by mee: tho hee had no cause to say I haveing never denied him any liberty of rambling but in the night time. And seldome then except it were cleare.[113]

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[108] Sharp to Flamsteed, 5 January 1702/3, RGO 1/34, ff. 17^{r}.
[109] RGO 1/12, ff. 5^{v}.
[110] In Thomas Smith's hand, RGO 1/12, ff. 38^{r}. At another instance, Flamsteed's amanuensis wrote: "The wind caused the instrument to shake sometimes, but the observations I held not to be the less accurate, for that I waited the calm seasons and repeated the observations often" (RGO 1/12, ff. 41^{r}).
[111] From a copy of the letter from Flamsteed to Edward Sherburne, 12 July 1682, RGO 1/42, ff. 173^{v}. Also in FC, II, 23.
[112] Draft of a letter from Flamsteed to Newton, ?date, RGO 1/35, ff. 8^{v}.
[113] 17 January 1677/78, FC, I, 591.

Among other paid assistants we find James Hodgson (1672-1755), Thomas Weston, Thomas Smith, Luke Leigh, John Stafford, as well as Flamsteed's closest and best assistants, Joseph Crosthwait and Abraham Sharp.[114] Flamsteed, of course, was not the only one to complain about assistants. "I also suffered another misfortune," Richard Towneley wrote him in 1676,

> in that when I came to review and reckon up the measurements I had taken of the phases,. . . the servant to whom I gave the task of taking the notes had sometimes written them down wrongly. I noticed this, however, and, I believe, corrected it. How good the observations may be, I leave to your judgment.[115]

The relations with some of his assistants transcended that of master and servant in view of the responsibility they were given and the diligent work they performed. One such person was James Hodgson who was Flamsteed's "servant" in 1696, but elected Fellow of the Royal Society in 1703 and, soon thereafter, appointed mathematics teacher at Christ's Hospital.[116] Flamsteed mentioned Hodgson in positive terms:

> My Amanuensis James Hodgson. knowes very well what Corrections are to be made in any of [the observations] and how to find them out when required He is a Sober Young man about 22 Years of Age. A very good Geometrician and Algebraist Understands the Series and fluxions tho I have not Suffered him to Spend much time in them. . . . he understands the Latin Tongue indifferently, having got since he became my Servant he knows my method and is acquainted with all my Labors and will easily finish and print them if God should call me hence before I shall have perfected them my Selfe.[117]

Crosthwait also moved from being Flamsteed's "man" and "servant" to "my amanuensis". But it was Abraham Sharp who was the closest and most trusted of Flamsteed's assistants. Flamsteed praised him as his "most faithful assistant, endowed with all the gifts and abilities which made him suitable to undertake such a delicate and difficult task,"[118] and claimed that Sharp was experienced in both mechanics and mathematics: "[Sharp] contrived each part of [the instrument]

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[114] A detailed listing of Flamsteed's assistants, the dates they worked at the Observatory, and the nature of their relationships to Flamsteed, can be found in H.P. Hollis, "The Greenwich Assistants during 250 Years," The Observatory 48 (1925): 388-398. There are none listed for Halley.
[115] Towneley to Flamsteed, 1 June 1676, FC, I, 454-455.
[116] Hollis, "The Greenwich Assistants," 390. Hodgson was also Flamsteed's nephew-in-law.
[117] Flamsteed to Wallis, 24 June 1701, FC, I, 898. Flamsteed indicated that Thomas Weston was, "My Youngest Servant Tho [he] has been Educated with Learning has a good Talent at drawing. . ." (Ibid.).
[118] Chapman, ed., The Preface to John Flamsteed's *Historia Coelestis Britannica*, 120. Sharp asked Flamsteed to "procure" for him a high-quality objective lens for a micrometer he was in the process of completing (Sharp to Flamsteed, 2 September 1702 [postcript], RGO 1/34, ff. 14^r).

so ingeniously, that his work was admired by all the expert craftsmen who beheld it."[119] After Flamsteed's death, both Crosthwait and Sharp collaborated with Flamsteed's widow, Margaret, to complete the *Historia coelestis Britannica*, which appeared in 1725, six years after his death.[120]

In addition to his assistants, Flamsteed frequently interacted with "private pupils" in both astronomy and mathematics that he was forced to take on because he was not amply paid. Between 1676 and 1709, he had at least 140 such students, many of whom came from the wealthiest families in England – a clear indication of Flamsteed's reputation. Student perceptions were that Flamsteed was a "hard taskmaster" who sometimes "had to teach by correspondence."[121] Nonetheless, Flamsteed believed that mathematics was extremely useful, particularly for sailors, and maintained:

> Learning (especially Mathematical) opens and inlarges the Mind, suggests profitable and laudable Designs, teaches to manage [men] with Discretion, keeps a Man from running into Vice for want of Employment, mollifies Men's Manners, [and] preserves them from Brutishness (the common fault of Sailors,). . . .[122]

Summarizing Flamsteed's career as a teacher of mathematics and astronomy demonstrates that he was relatively successful with students:

> Judging by his finished work, the training provided by Flamsteed in practical astronomy was effective. He produced a number of observers and calculators who were highly skilled in practice, although not inclined to produce original or wide-ranging work of their own. . . . Flamsteed and his assistants are the only example from the period of collaboration on a large enough scale to produce a comprehensive, quantitative work. The existence of some external forms of training in mathematics enabled Flamsteed to teach his youths the particular skills that they needed in a reasonably short time. The universities produced other great astronomers or contributed to their production, but played little part in the work of Flamsteed and his group.[123]

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[119] Chapman, ed., The Preface to John Flamsteed's *Historia Coelestis Britannica*, 120.
[120] For more on Margaret Flamsteed, see Rob Illife and Frances Willmoth, "Astronomy and the Domestic Sphere: Margaret Flamsteed and Caroline Herschel as Assistant-Astronomers," in Women, Science and Medicine 1500-1700: Mothers and Sisters of the Royal Society, eds. Lynette Hunter and Sarah Sutton (Stroud, Gloucestershire, UK: Sutton Publishing, 1997), 235-265.
[121] Murdin, Under Newton's Shadow, 51.
[122] From the same treatise on navigation cited above, April 19, 1697, RGO 1/32D, ff. 136[v].
[123] Murdin, Under Newton's Shadow, 52.

Astronomical Work at the Observatory and Later Years

Flamsteed wanted to elevate the status of astronomy by removing it from the domain of philosophers. In his later work, he made it clear that he favored observation over theories concerning the heavens.[124] In fact, he became increasingly bitter towards what he considered the prejudiced views of theorists who had no understanding of the difficult calculations involved in reducing astronomical observations collected over a long time. Although he did not discard theory entirely, Flamsteed believed that theorizing alone placed too many constraints on what was being observed, and could even divert the astronomer's attention away from what was truly important in astronomy. As early as 1678, he wrote to Moore that Hooke "professes himselfe to be a Philosopher not an Astronomer and therefore I have no more to say to him but that Astronomy is ill handled when it must be ordered by the Whimsyes of Philosophy."[125] Yet Flamsteed was "far too uncritical of his own methods and calculations," and he even had an "exaggerated confidence in the perfection of his own data and methods."[126] When discrepancies between his results and those of others appeared, he usually concluded that the mistakes were not his. This was one of the character traits that made Flamsteed a complex, often ill-tempered (although frequently with good reason), astronomer.[127] Flamsteed seemed to acknowledge his shortcomings:

> I love peace above all thinges. I delight to oblige ingenuous and knowing Men I hate to traduce any one: My nature inclines me to Civilyty and candor: I am something passionate yet no man is seldomer seene in Anger or sooner forgives an Injury or regrets it lesse. . . . God has given me so strong an aversion to any thing that relishes of Ingratitude or disingenuity that my spirits rise and I am not able to resist the passion of my resentments on the first apprehension sometimes I am carried beyond what I thinke is decent tho not unwarrantable. but when the first heat is over I chastise my selfe for it and rarely relapse, but now my reason persuades me to continue or at least to feigne a continuation of my resentments till

[124] See Jim Bennett, "Flamsteed's Career in Astronomy: Nobility, Morality and Public Utility," in Willmoth, ed., Flamsteed's Stars, 29-30.
[125] 2 April 1678, FC, I, 618.
[126] Forbes, "Early Astronomical Researches," 133, 134.
[127] His relative poverty certainly did not help the situation, but this was not uncommon for astronomers in the period. According to Murdin, "Looking at astronomers [in the late seventeenth century] as a group shows that they were not generally prosperous men. A few suffered from serious poverty. . . . The great majority earned a living which just barely allowed a surplus for astronomy" (Murdin, Under Newton's Shadow, 89).

> for a publick injury I have receaved publick satisfaction lest hee take occasion hereafter to repeate the like. . . .[128]

Abraham Sharp also "had ample experience of [Flamsteed's] great candour and power to forgive and forgett an offence, even when there are no apparent evidences of repentance."[129] Flamsteed was also frequently ill, especially in the last twenty years of his life, and he often complained of his various pains and distempers to anyone that would listen:

> My distemper has been very sharp. my heareing on one side is very much prejudiced by it I have used few remidies. finding those I employed ineffectual. I hope that time through Gods blessing will recover it. tho I have still paines of my head but not so great as to hinder me from pursueing my business if God send us fair weather.[130]

As one scholar put it,

> Flamsteed was an excessively serious conscientious man, who was in chronically poor health, and who spent a lifetime of overwork in a single job with very little assistance and not much encouragement from scientific colleagues. It is small wonder that he became tetchy and quick to note largely imagined defects in others.[131]

Such personality traits inevitably steered Flamsteed towards a strong dislike of Hooke and bitter disputes with both Halley and Newton. Ironically, Flamsteed told Oldenburg early in their correspondence that he was reluctant to engage in controversies: "as a devotee of Truth I have always regarded disputes as futile and undertaken only for the sake of capturing a little glory, to the extent that most obscure truth rather than illuminate it, yet I have never refused to engage in friendly debate, especially with ingenious men."[132] Unfortunately, he became embroiled in a number of controversies and whether or not he "feigned" his resentments towards Newton and Halley, he never received that "public satisfaction" that he demanded for his highly visible "public injuries."

Despite Flamsteed's eccentric personality traits, he was able to accomplish much through observation, though concerns over the publication of his star catalog exhausted most of his time and efforts in the last twenty years of his life. Flamsteed's observational programme largely entailed finding the dimensions of the heavens, and in particular, the distances of the sun and

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[128] Flamsteed to Molyneux, 12 February 1686/7, FC, II, 335.
[129] Sharp to Flamsteed, 8 December 1702, FC, II, 985.
[130] Flamsteed to Newton, 10 December 1694, FC, II, 541-542.
[131] McCrea, The Royal Greenwich Observatory, 7. See also, Murdin, Under Newton's Shadow, 53-57.

moon from the earth. In 1672, Flamsteed Observed Mars' distance from two stars over the course of several hours and calculated its parallax from these measurements. From these calculations, he was then able to determine Mars' horizontal parallax.[133] With this method, Flamsteed obtained 10 seconds of arc as the angle of solar parallax. Using Jean Picard's measurement of the earth's radius from his *Mesure de la Terre* (1671) of 3953 miles, this suggested that the sun was approximately 82 million miles away.[134] This was further away than was previously accepted, and seemed so astonishing that when Flamsteed communicated his results to Cassini, he indicated that 10 seconds of arc was the uppermost limit of the angle of diurnal parallax.[135] In addition to obtaining solar parallax, Flamsteed used his micrometers to determine the variation of the sun's apparent diameter and therefore, the variation of distance between the sun and the earth.

Flamsteed also wanted to recalculated the distance between the moon and the earth which he did using the moon's "horizontal" parallax.[136] By timing the moon's motion against the reference of background stars, Flamsteed determined that the distance between the center of the earth and the moon was approximately 60 times the earth's radius, or semidiameter.[137] Again, by using Picard's measurement, Flamsteed determined that the distance was approximately 237 thousand miles.[138] Like his observations with the sun, Flamsteed measured the moon's apparent diameter and its variations. Between 1694 and 1695, he obtained approximately 150 positions of the moon that he eventually turned over to Newton. These observations were intended to predict future positions of the moon, but it was not until 1755 that Tobias Mayer (1723-1762) published accurate lunar tables.

Flamsteed also observed and measured the planets, determined the latitude of the observatory, the "obliquity of the ecliptic,"[139] and the "precession of the equinoxes," proposed a

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[132] 25 January 1675/6, FC, I, 415.
[133] See Albert van Helden, Measuring the Universe: Cosmic Dimensions from Aristarchus to Halley (Chicago & London: The University of Chicago Press, 1985,) 135-137.
[134] Birks, John Flamsteed, 72-73.
[135] 7 July 1673, FC, I, 219. Cassini's response is discussed in the next section on Cassini and Flamsteed's mutual relations.
[136] See Birks, John Flamsteed, 75-77 for a detailed description and diagram. Flamsteed discusses this method in a letter to Richard Towneley, 11 April 1674, FC, I, 283-290.
[137] Flamsteed to Richard Towneley, 11 April 1674, FC, I, 285-286.
[138] Birks, John Flamsteed, 76. The current mean distance of the moon is 239 thousand miles with the eccentricity of its orbit producing variations between 226 and 252 thousand miles (Ibid.).
[139] For a history of observations of the ecliptic, including Greek and Arabic, see Bernard to Flamsteed, 14 August 1681, FC, I, 796-807.

cause for atmospheric refraction, and suggested a motion for comets. He shared his measurements of the observatory's latitude and the obliquity of the ecliptic with Newton in 1699 by including a long list of tables and measurements in a letter.[140] On atmospheric refraction, Flamsteed noticed that refractions were "bigger in the morneing then the evening,"[141] and proposed that the reason for this was because of increased moisture in the morning air: "That the morneing refractions generally exceed the evening by reason of the greater quantity of vapors breathd from the earth. in the night and digested by the heat of the day."[142] Finally, by observing the comets of 1680 and 1681, he determined that they were actually one and the same. This idea was originally rejected by Newton who wrote, "to make the Comets of November and December [of 1680] but one is to make that one paradoxical. Did it go in such a bent line other comets would do the like and yet no such thing was ever observed in them but rather the contrary."[143] Later, however, Newton accepted Flamsteed's explanation in his *Principia* of 1687 (Flamsteed was not given credit for the idea).

Perhaps Flamsteed's greatest labor of all was the rectification of the places of the fixed stars that were intended for publication in a star catalog. According to Flamsteed's own "Account of the Observatory," (1699) he had measured more than 20,000 objects using the sextant – an instrument accurate to about 10 seconds of arc.[144] Furthermore, with his mural arc of 1689,

> He has determined the Right Ascentions and distances from the pole, of fifteen hundred fixed stars, and calculated their Longituds and latitudes from them, to which he has added Variations whereby their Right ascentions and declinations may easily be found for any time past or to come. . . . There remaines about fifteen hundred fixed stars to be rectified (from the Observations allready taken. . . .[145]

By coming up with an ingenious method for determining absolute right ascensions, Flamsteed eliminated previous errors caused by using an "intermediary planet to measure the angle between sun and star," and he also "eliminated all uncertainties caused by parallax, refraction, and

[140] 16 January 1698/9, FC, II, 762-773.
[141] Flamsteed to Richard Towneley, 4 July 1678, FC, I, 639.
[142] Flamsteed to Newton, 31 December 1694, FC, II, 547.
[143] Newton to Flamsteed, 16 April 1681, FC, I, 786.
[144] Late 1699, FC, II, 798. See also Thoren, "Flamsteed," DSB, 24.
[145] Late 1699, FC, II, 798.

latitude."[146] After determining the positions of forty reference stars, Flamsteed computed the positions of 3,000 more stars for his catalog.

Flamsteed's preparation of his great star catalog directly led to the most acrimonious controversy of Flamsteed's career when he found himself pitted against Newton and Halley. Newton needed Flamsteed's observations to support his theoretical work and he felt that the Astronomer Royal was not even willing to communicate these privately to him. Halley's relations with Flamsteed also had been initially cordial based on professional respect. According to Forbes, their subsequent animosity was the result of opposing personalities:

> Temperamentally, the two men were totally different. Halley was healthy, an extrovert, adventurer, jocular in conversation, and atheistically inclined. Flamsteed was none of these things, and disapproved of Halley's close association with Hooke, for whom he had a strong personal dislike.[147]

But Flamsteed's animosity towards Halley was fueled by comments such as "why [Flamsteed] did not resign his office in order to let Halley take over."[148] Flamsteed believed that the envy for his position came from the very jibes of Halley himself, and he did not hesitate to insult Halley behind his back by claiming that Halley "now talks, swears, and drinks brandy like a sea-captain."[149]

In the late 1690's, tempers simmered between Newton and Flamsteed. Their initial animosity concerned Newton's theory on gravitation. Newton wanted immediate access to Flamsteed's observations of the moon's positions in the sky for his own lunar theory that would help clarify his universal law of gravitation. In fact, he originally wanted to publish Flamsteed's observations together with a subsequent edition of his *Principia* as confirmation of his theory of universal gravitation, and he was even willing to give Flamsteed credit for the observations:

> *As for your Observations you know I cannot communicate them to any body and much less publish them without your consent*. For all the world knows that I make no observations myself, and therefore I must of necessity acknowledge the author; and if I do not make a handsome acknowledgment, they will reckon me an ungrateful clown.[150]

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[146] Thoren, "Flamsteed," DSB, 25.
[147] Forbes, Greenwich Observatory, 74.
[148] Boucher to Flamsteed; cited in Forbes, Greenwich Observatory, 75.
[149] Flamsteed to Sharp, ? March 1704 [Francis Bailey, An Account of the Revd. John Flamsteed, the First Astronomer-Royal; Compiled from His Own Manuscripts, and Other Authentic Documents, Never Before Published. & Supplement to the Account of the Revd. John Flamsteed, with an Author Index (London: Lords Commisioners of the Admiralty, 1835; reprint, London: Dawsons of Pall Mall, 1966), 215].
[150] Newton to Flamsteed, 16 February 1694/5, FC, II, 569.

Flamsteed's delays in sending Newton his lunar observations led the impatient Newton to remark: "I want not your calculations but your observations only. . . . I will therefore, once more propose it to you to send me your naked Observations. . . and leave it to me to get [the moon's] places calculated from them."[151]

The relationship deteriorated even further in the closing years of the seventeenth century when a private letter written by Flamsteed to Newton was published in Wallis' *Opera mathematica*.[152] In the essay, which is often referred to as *On the parallax of earth's annual orb* (1699), Flamsteed described and listed his observations of the parallax of the pole star.[153] After quoting from Riccioli who claimed that the earth did not move because there was no detectable stellar parallax, Flamsteed asserted that, in fact, he had observed the Pole Star over the course of seven years and determined that it had a parallax of at least 40 seconds.[154] Wallis showed the letter to David Gregory (1659-1708), nephew of James Gregory (who proposed what is now called the Gregorian form of the reflector in his *Opticae promota* of 1663) and Savilian Professor of Astronomy at Oxford.[155] Gregory, in turn, advised Wallis not to publish the paragraph "which speakes of [Flamsteed] giving to Mr Newton Observations of the Moon."[156]

Flamsteed decided to correspond directly with Newton on the matter, and sent him two letters only a few days apart.[157] Because Flamsteed did not get an immediate response, he assumed Newton did not mind the reference, and Flamsteed advised Wallis "*that neither you nor I ought to take any more notice of it then Mr Newton does and therefore you may please to let that Paragraph and the next stand as it is* without alteration."[158] Newton's response, however,

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[151] Newton to Flamsteed, 29 June 1695, FC, II, 599.
[152] Wallis' *Opera* appeared in three volumes from 1693 to 1699. The Flamsteed letter was included in Volume 3 (1699), 701-708.
[153] The original letter in English from Flamsteed to Wallis (November/Early December 1698, FC, II, 706) has not survived. Wallis' Latin translation, with the controversial paragraph on Newton removed, has survived instead. This was translated into English by the editors of the Flamsteed correspondence (20 December 1698, FC, II, 720-734).
[154] 20 December 1698, FC, II, 721. Unfortunately, Flamsteed had measured the "aberration of light" rather than the true parallax of the pole star. The closest star (and therefore greatest measurable parallax) is Proxima Centauri with a parallax of 0.76 seconds of arc. The "aberration of light" was first explained by the third Astronomer Royal (1742-1762), James Bradley (Birks, John Flamsteed, 93).
[155] Wallis to Flamsteed, 10 December 1698, FC, II, 707.
[156] Wallis to Flamsteed, 28 December 1698, FC, II, 735.
[157] 2 January 1698/9 and 5 January 1698/9, FC, II, 738-741.
[158] 7 January 1698/9, FC, II, 744.

had been written the day before and arrived only after Flamsteed's letter to Wallis. Newton forcefully told Flamsteed that he did not want

> to be brought upon the stage about what perhaps will never be fitted for the publick and thereby the world put into an expectation of what perhaps they are never like to have. I do not love to be printed upon every occasion much less to be dunned and teezed by foreigners about Mathematical things or to be thought by our own people to be trifling[159] away my time about them when I should be about the Kings business. . . . But there may be cases[160] wherein your friends should not be published without their leave.[161]

Flamsteed immediately sent out two letters: one to Wallis in which he asked Wallis to remove the "Offensive Innocent Paragraph," [162] and one to Newton in which he apologized for the mistake but claimed he did not realize that he had committed any offense by telling the world that he had given Newton the lunar positions.[163] In fact, Flamsteed added:

> I thought not it cou'd be any deminution to you, since you pretend not to be an observer your selfe. I thought it might give some people a better notion of what was doeing here then had bin impressed upon them by others whom God forgive. You will pardon me this freedom and excuse me when I tell you if foreigners donn and troubl you tis not my fault but those who think to recommend themselves to you by advanceing the fame of your works, as much as they possibly can.

Flamsteed then directly addressed what was troubling him:

> I have sometimes told some ingenious men, that more time and observations are required to perfect the Theory but I found it was represented as a little piece of detraction *which I hate* and therefore was forced to be silent. I wonder that *hints* shoud drop from your pen, as if you Lookt on my business as *trifling* You thought it not soe surely when you resided at Cambridge it's property is not altered.[164]

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[159] Here, Flamsteed added the note, "Was Mr Newton a trifler when he read Mathematicks for a sallery at Cambridge. Surely the Astronomy is of some good use tho his place be more beneficiall" (FC, II, 743 n. 6).
[160] Here, Flamsteed added the note, "when persons thinke too well of themselves to acknowledge they are beholden to those who have furnisht them with the feathers they pride themselves in when they have great fr[iends] etc." The great friend was Charles Montague, Chancellor of the Exchequer, who saw to Newton's appointment as Warden of the Mint (FC, II, 743 n. 9).
[161] 6 January 1698/9, FC, II, 742.
[162] 10 January 1698/9, FC, II, 748.
[163] 10 January 1698/9, FC, II, 746.
[164] 10 January 1698/9, FC, II, 747. The text is in the hand of an amanuensis, but it was Flamsteed who underlined the passages indicated by the italics (FC, II, 747).

Flamsteed's defensiveness was an indication of his feeling that most of his peers neither understood nor appreciated the sheer effort that went into the collection of observations and the computing of tables. The function of the observatory and the integrity of accurate measurements and observations were of paramount importance to Flamsteed and he was quick to lash out when this was lost on others.

Flamsteed became more possessive of his lunar tables after this episode with Newton, even suspecting that Newton was sharing his results with Gregory and Halley based on tables already given to him by Flamsteed. This was a "Greater breach of Promise then if [Newton] had imparted the Observations themselves" – something the latter promised Flamsteed he would not do.[165] Because Newton had not properly credited Flamsteed's contributions, the Astronomer Royal indicated that he would "calculate the Moons place in [his] Correct Theory," adding,

> (I call it mine because it consits of my Solar Tables and lunar numbers corrected by my selfe. And *shall own nothing of Mr Newtons labors till he fairly owns what he has had from the Observatory*) and I believe that none but my selfe would have been at the paines to make so many tables as I have. for this purpose. . . .[166]

The relationship between Newton and Flamsteed further deteriorated in the following decade with the events surrounding the premature publication of Flamsteed's star catalogue in 1712 under the title *Historia coelestis*. Halley was selected as editor for what Flamsteed considered a garbled and mutilated edition of his observations. Certainly, the personalities of Flamsteed, Halley, and Newton had much to do with the events surrounding the publication of the *Historia coelestis*. But ultimately, Flamsteed's main concern was that "Halley's edition" was both incomplete and full of errors – both accidental and intentional. Furthermore, Flamsteed was concerned that any faults and inconsistencies would be a reflection on him, not on the Halley or Newton (and others) who pushed for the work's publication. Flamsteed was neither impressed nor intimidated by status and rank, and he could never accept the opportunistic approach of a powerful client when it involved the integrity of his life's work. In the end, accuracy and thoroughness were his most crucial concerns, and he felt that these had been compromised by Halley directly, and Newton indirectly, with the premature edition of his star catalog.

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[165] Flamsteed to Lowthorp, 10 May 1700, FC, II, 817.
[166] Flamsteed to Sharp, 18 January 1702/3, FC, II, 994.

When Flamsteed died in 1719, he had completed his survey of the progress of astronomy together with a description of his instruments and methods of observation. After more difficulties and delays involving the engravings, Flamsteed's *Historia coelestis Britannica* was finally published in 1725 under the auspices of his wife, Margaret. This was "fifty years after Flamsteed's first appointment [as Astronomer Royal], twenty-one years after his plans to publish, and six years after [his] death."[167] In 1720, when Halley arrived at the Observatory to become the second Astronomer Royal, he gave Margaret Flamsteed three days to leave and take all her belongings with her, which she did only after taking every major instrument and piece of furniture with her. When Halley proceeded to try to win the instruments back, he lost because legally, she had the right to take the instruments since they were either presents from Sir Jonas Moore, or had been made and repaired at Flamsteed's own expense.[168]

Cassini and Flamsteed: Anatomy of a Relationship

The relationship between the Paris and Greenwich Observatories may be best described as observatories where there was a healthy competition between their respective directors. It was based on mutual respect and understanding that emanated from knowing what proper astronomical activity entailed: conducting observations over a long period of time, using the best possible instruments and assistants, ensuring that the instruments were well-calibrated, and taking repeated measurements and observations in order to rule out errors. They corresponded frequently despite the competitive views espoused by others.

Undoubtedly, "national pride" was sometimes at stake between the two national observatories. In an early letter to Lord Brouncker, for example, Flamsteed called on English astronomers to move into action so that their labors could compete with foreign countries:

> up! generous English spirits! run and strive to obtain those prizes, which the excluded world endeavours surreptitiously to deprive you of. Shall we, who have supplied ourselves in all other knowledge, yet in Dioptrics and the Selenography borrow from them their faulty systems and delineations? As if our clime afforded not as food wits, curious

[167] Chapman, ed., The Preface to John Flamsteed's *Historia Coelestis Britannica*, 13.
[168] For similarities and differences between Flamsteed and Halley as Astronomers Royal, see Alan Cook, "Edmond Halley and John Flamsteed at the Royal Observatory," in Willmoth, ed., Flamsteed's Stars, 187.

> instruments, fair encouragements, and generous rewards for invention, arts, and knowledge, as foreign countries. no, those (I may speak it to our shame) we have we exceed them in. What then hath cast us behind them? not our want of wits, but loathe of pains. What hath made them so far outstrip us? not their acuteness, but industry.[169]

Regardless of Flamsteed's respect for Cassini's reputation, Flamsteed was not always impressed with the work produced at the Paris Observatory in the closing decades of the seventeenth century. Because of personnel changes during the 1680's and 1690's, the Paris Observatory lagged behind the Greenwich Observatory by the end of the century. This in no way prevented the English king, James II, from visiting the observatory for himself on August 20, 1690, along with some of his entourage.[170] Flamsteed even indicated in a letter to Worden that "the reall grounds of true Philosophy have been fetch'd from her Majesties Observatory," and that despite the "mighty Boasts" that the French have made of their Observatory, they have "done nothing Considerable. Forraign Nations as well as our own will derive the helps to their Ingenous studyes from her Majesties Observatory. . . ."[171] The instruments at the Paris Observatory also seemed to be inferior of their English counterparts. Based on Molyneux and Halley's descriptions from their recent visits, the instruments at Paris were smaller and more difficult to manage, leading Flamsteed to comment, "I have no very great opinion [of] them."[172]

Despite Flamsteed's claims, he could not afford to buy expensive telescopes and consequently, astronomers at Paris surpassed the English in planetary discovery. Flamsteed was not content to leave all discoveries in the hands of the French, however, and there is evidence in his correspondence that suggests that there was a certain amount of competition between rival astronomers in each country, as confirmed in a friendly letter by Auzout to Oldenburg wherein Auzout claimed that he was

> delighted to learn that such fine glass is now being made in England since, wherever it is perfected, it will be possible to obtain some. We [French] here have reason to hope that in the future we shall not need to envy you this happiness, because we have recently acquired at Paris a glassworks where they make the most beautiful glass ever seen, which appears to be

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[169] 24 November 1669, FC, I, 17.
[170] "Recit de ce qui c'est passé a l'observatoire le 23 Aout sous que le Roy angleterre y est venue," PS: $\mathbf{B4}_3$.5*.41.
[171] 22 May 1702, FC, II, 938-939. In a c. 1715 draft of the "Account of the Observatory," Flamsteed specified that the "French have done little towards the great Worke of the fixed stars [from] their Stately and Chargeable Observatory. . . " (RGO 1/35, ff. 146^r).
[172] Flamsteed to Newton, 25 September 1685, FC, II, 248.

> wonderfully good for telescopes since the workman makes it without veins and almost without tiny bubbles.[173]

Flamsteed, although perhaps biased because of his nationality, claimed that English telescopes and lenses were the best in all of Europe.[174] However, after Cassini's incredible discoveries made with Campani telescopes, the English were not as competitive thereafter.

For their part, Flamsteed's colleagues had jaded (and often suspicious) views of the French astronomers, especially since they did not correspond with them as frequently as Oldenburg and Flamsteed did. John Caswell (c. 1655-1712) even warned Flamsteed at one point to protect his work because his observations could be "sold to anyone that would give most for them: and in that case they would fall into the French kings hands, for that no body would bid so high: and then the French Astronomers would maim your observations, they would suppress some, and print others of yours in their own names."[175] Correspondence between astronomers was encouraged, however, not only by Oldenburg, but also by John Beale who argued as early as 1671 that,

> the parisian *observatory* cannot do ye greate things in Astronomy wthout correspondents amongst yu, or at greater distances. The Satyrist chides us againe. Ubi est Astronomia? Ubi consultissima Sapeintiae via. Quis apud nos venit in templum, et vocum fecit, si philosophiae fontem invenisset?[176]

Flamsteed initiated correspondence with Cassini before the building of the Greenwich Observatory in 1673, writing a letter to him without any previous introductions. He was formal yet polite, indicating interest in sharing observations:

> As I am about to write to you without being known to you, most distinguished Cassini, I would beg your pardon in many words for my boldness, did not the reason for my addressing you being the studies we have in common, your well-known noble conduct among astronomers, and the usefulness of the observations which I am going to share with you, persuade me that this would be altogether superfluous.[177]

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[173] 22 June 1665, OC, II, 420.
[174] Flamsteed to Brouncker, 24 November 1669, FC, I, 16.
[175] 8 May 1705, RGO 1/35, ff. 41^{r}. The irony is that this was already happening to Flamsteed, but by his own countrymen, not the French!
[176] 6 August 1671, OC, VIII, 186-187. The Hall's translation of the Latin portion is as follows: "Where is astronomy? Where is the most appropriate path to knowledge? Who among us has come to the temple and made an offering, that he might discover the fount of philosophy" (OC, VIII, 189 n. 6; from Petronius Arbiter, Satyricon, 172-175).
[177] 7 July 1673, FC, I, 215.

They established a cordial, though brief, correspondence that focused on the promotion of astronomy, a sentiment captured by Cassini in an early letter to Flamsteed: "Farewell, famous Sir, and since God Almighty has endowed you with an extraordinary gift for promoting astronomy, make use of it, and as you have begun to do, make us sharers in your observations and researches."[178]

Most of the correspondence between the two astronomers was exchanged in the 1670's. They discussed various astronomical topics and exchanged observations and measurements, although there were instances when their methods and figures did not agree.[179] One of the first topics they discussed concerned the maximum and minimum digressions of Jupiter's satellites from its center. Flamsteed provided Cassini with his own measurements of the digressions in his first letter, carefully describing his instruments and methods of observation. He even took the time to compare his measurements with those published in Cassini's *Ephemerides Bononienses mediceorum syderum* (Bologna, 1668), and noted the discrepancy between some of the measurements. Specifically, Flamsteed "found the motion of [Jupiter's third satellite] a degree-and-a-half less that [Cassini's] figures make it" although, as Flamsteed pointed out, this may have been caused by differences in "latitude, or by the eccentricity of the satellites orbit, things which are not yet fully explored."[180] Cassini responded that he had expressed the maximum distances of Jupiter's satellites "in round numbers only, restricted. . . to the year 1665," and that "if there is any discrepancy it is uncertain whether it is to be attributed to the difficulty of [making] the observations, or to a variation."[181]

Flamsteed shared his measurements of solar parallax with Cassini that was "at most 10" and the [Earth-Sun] distance 21,000 terrestrial radii" – an astonishingly high number. Solar parallax was determined by obtaining Mars' parallax, which Flamsteed indicated was "never greater than 25 seconds of arc"[182] – although he later changed it to 26 seconds of arc.[183] Cassini was delighted that his figures agreed so closely to Flamsteed's:

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[178] 1/11 August 1673, FC, I, 232.
[179] Cassini mentioned in a letter to Flamsteed that perhaps their observations did not agree because of differences in their "methods of observation" (29 October/8 November 1673, FC, I, 258).
[180] 7 July 1673, FC, I, 219.
[181] 1/11 August 1673, FC, I, 231.
[182] 7 July 1673, FC, I, 219.
[183] Flamsteed to Cassini, 5 September 1673, FC, I, 251.

> As far as the parallax of Mars is concerned, it is remarkable how far we agree in our definitions of it. . . . I found the parallax of Mars to be 25 seconds of time. And. . . I defined the mean solar distance from Earth as 22 thousand earth-radii. . . . *Certainly by so close an agreement of observations great authority is conferred upon the ratios determined by us between the radius of the Earth and the distances of all the planets*.[184]

Cassini added that he was eager for Flamsteed to send him his measurements of Mars' diameter, which Cassini found to be equal with unity. On this matter, however, Flamsteed's ratios did not agree – he found the ratio between its diameter and parallax to be 5:4, "a little different from what [Cassini] stated."[185] "As concerns the diameter of Mars," Cassini concluded, "I do not know whether the difference of a few seconds between our observations is to be attributed to the residue of rays from which it is with difficulty freed in less perfect telescopes, or to the method of observation." Undaunted, Cassini concluded that "this discrepancy will urge me on to make further observations."[186]

Flamsteed and Cassini also discovered that their observations of Jupiter's shape differed. The latter mentioned that the disc of Jupiter was not round, but "broader in the direction of the digressions of his satellites."[187] Jupiter had always appeared round, according to Flamsteed, and the work of other astronomers never indicated that Jupiter was oval or bulged more at the equator than towards the poles.[188] Cassini proposed in the following letter that he would make further observations of Jupiter's shape as soon as it can be observed.[189]

When Flamsteed discovered discrepancies between his and Cassini's observations of the lunar eclipse of June 26, 1675, he became determined to find the meridians between London and Paris. Flamsteed used observations of the solar eclipse of June 1666 to determine the temporal difference in meridians between Danzig and London ("not greater than 1 hour 16 minutes"), and between London and Paris ("not less than 12 minutes").[190] A few years later, Flamsteed

[184] (Emphasis mine) 1/11 August 1673, FC, I, 232.
[185] 5 September 1673, FC, I, 251.
[186] 29 October/8 November 1673, FC, I, 258.
[187] 1/11 August 1673, FC, I, 231.
[188] 5 September 1673, FC, I, 251.
[189] 29 October/8 November 1673, FC, I, 258. After further observations with his telescopes, Flamsteed accepted Cassini's view that Jupiter was oval-shaped (FC, I, 259 n. 1).
[190] 19 September 1675, FC, I, 370.

indicated to Cassini that he had determined the difference between the Paris and London meridians to be 9¼ minutes.[191]

Flamsteed also discovered that the French astronomers (like Newton) believed that the comets seen in 1680 and 1681 were two distinct comets. Flamsteed believed them to be same – a theory he could not "answere. . . as yet without unpresidented suppositions."[192] When Cassini sent Flamsteed a printed treatise on the 1680-81 comet,[193] entitled *Observations sur la cométe qui a paru au mois de Décembre 1680. et en Janvier 1681. présentées au Roi* (Paris, 1681), Flamsteed unhesitatingly informed Cassini that he found errors in Cassini's positions of the comet and indicated that he sees no reason why he should "depart from [the] opinion" that the two comets were the same.[194]

Despite their differences, Cassini and Flamsteed were both proponents of the new instrumentation, especially telescopic sights and micrometers. Clearly, they had a high degree of respect for one another and a certain sense of trust in each other's observations:

> when one compares the time-differences of the phases both with our own, and with each other, they are far from showing the same agreement as can be observed in those which I have compared here, and which usually occurs between your observations and ours. *And so your studies and mine will always be the most acceptable to me.*[195]

Flamsteed even tried to engage Cassini in the controversy over the use of "bare" or naked-eye sights by referring to Hevelius repeatedly. On one occasion, he wrote to Cassini that Hevelius was planning a star catalog of his own, but because Hevelius still used bare sights, "it is scarcely permissible to expect any greater precision from him than we find in Tycho." If anyone should attempt to undertake such a labor, it should be Cassini and his colleagues at the Paris Observatory because it "will be undertaken by yourselves in a better way with telescopes properly applied to instruments."[196] On another occasion, Flamsteed even indicated to Cassini that of those who do not use "lenses," a "screw," or a "moving device," "nothing is to be expected but that the observations will conflict with each other and be as unreliable as the

[191] 8 January 1678/9, FC, I, 668. The current value is 9 minutes 23 seconds (FC, I, 672 n. 3).
[192] Flamsteed to [Caswell], 4 February 180/1, FC, I, 753.
[193] Enclosed with a letter dated 20 June 1681, FC, I, 791.
[194] 1 November 1681, FC, I, 836. It does not appear as though Cassini ever discussed the one-comet theory with Flamsteed.
[195] (Emphasis mine) Cassini to Flamsteed, 5 June 1682, FC, II, 4.
[196] 5 September 1673, FC, I, 253.

instruments."[197] Cassini, however, refused to either directly address the issue of plain sights, or comment on Flamsteed's criticisms of Hevelius.[198] In fact, the first time Cassini mentioned Hevelius in a letter to Flamsteed was by positively referring to Hevelius' lunar maps.[199]

Years before he began his work at Greenwich, Flamsteed listed what he believed to be the necessary characteristics of the successful astronomer: "indefatigable industry, some watchful nights, careful days, curious calculations, a Lynceus' eyes, Apelles' hands, and Kepler's ingenuity" – "truly Herculean labours."[200] Irrespective of their differences, the meticulous observational programme embraced by Flamsteed was clearly espoused by Cassini who also shared his enthusiasm for the study and promotion of astronomy.

Conclusions

In his *Machina coelestis pars prior*, Hevelius wrote that the well-being of astronomy depended as much on well-constructed instruments as it did on proper accommodations. But above all, he argued that the decisive components for the proper promotion of astronomy were diligence and effort on the part of experienced observers.[201] Cassini and Flamsteed shared Hevelius' sentiments of a successful research programme in astronomy. Their belief in the patient gathering of meticulous observations with continuity and regularity, was not only preferred, but required – this was part of Tycho's legacy. The Dane had emphasized the necessity of using multiple instruments over a long period of time so as to gather the most precise measurements and eliminate as many errors as possible. But the era of multiple instruments was coming to an end, and it was to be replaced by the era of the meridian.[202]

Hevelius, Cassini, and Flamsteed also insisted on the essential symbiosis between observer and instruments – a careful balance between an experienced astronomer and the most

[197] 19 September 1675, FC, I, 371.
[198] To my knowledge, Cassini *never* addressed the issue of sights in any letters to Flamsteed.
[199] 1 February 1675/6, FC, I, 426.
[200] Flamsteed to Brouncker, 24 November 1669, FC, I, 17.
[201] Quandoquisdem negotium Astronomicarum Observationum non unicè ab elato, pollucibili, atque; omnimodè magnificè exstructo Observatorio; sed à probè accommodato, ab Instrumentis benè justis, æneis, maximis & volubilis, tum ab apparatu convenienti, atque; Peritorum Observatorum diligentiâ dependet (Hevelius, *Machina coelestis pars prior*, 444).
[202] Bennett, "Flamsteed's Career in Astronomy," in Willmoth, ed., Flamsteed's Stars, 27.

advanced instruments he could use. Certainly, Hevelius believed that instruments were only as good as the astronomers who used them but, unlike Cassini and Flamsteed, he did not acknowledge the advantages afforded by the new instrumentation of the 1660's. In contrast, Cassini and Flamsteed maintained that *if* there were any new astronomical improvements or devices, they *should* be added.

Working out of a properly-constructed observatory was also crucial according to all three astronomers. Views conducive to proper observations usually required both a clear horizon and an unobstructed zenith. Moreover, the observatory needed to be comfortable, especially if the astronomer (and his assistants) lived there. However, only Hevelius was able to dictate the design of his observatory. At Paris, Perrault determined the design before Cassini arrived there, while Greenwich observatory was built over an older edifice that was not properly aligned in a north-south axis. Cassini and Flamsteed were also more restricted in their astronomical activities since the Paris and Greenwich Observatories belonged to the state. Consequently, they had to answer to higher authorities – though Cassini seemed to have greater autonomy.

Flamsteed remained focused on the goal of elevating the status of practical and observational astronomy throughout his life. When his efforts to promote the "right" type of astronomy were not sufficiently appreciated, he became defensive, suspicious of others, and determined to be recognized for his contributions. He appreciated, too, Cassini's efforts to compute, reduce, and apply the data to his own astronomical work. The function, purpose, and integrity of the Greenwich observatory depended on these conditions and not on the short-term agendas of opportunistic individuals.

Conclusions

Summary and Analysis of the Work

By the end of the seventeenth century, astronomical practitioners were emphasizing the empirical approach to astronomical research and instrumentation was continuously being improved upon and refined. As explained in Chapter 1, micrometers and telescopic sights immediately impressed the astronomical community when they were initially added to the arsenal of traditional measuring instruments, and astronomers quickly recognized their worth and usefulness in raising the level of precision in measurement. Moreover, unlike the telescope which only after decades was integrated into the everyday practice of positional astronomers, the new instrumentation that was reinvented in the late 1660's became commonly-used by the majority of the astronomical community almost immediately (with micrometers and telescopic sights, the telescope became a different instrument). The fact that there could not be a consensus on measurements if astronomers were using different types of sights was a significant factor in the sights' endorsement.

In addition to new instrumentation, methods of observation also included the way astronomical knowledge was accumulated. Tycho Brahe believed that astronomy was not an Aristotelian science, but rather, more like alchemy – "its knowledge was private, acquired through personal experience and endeavor."[1] Knowledge about the heavens was not a part of daily common experience – that is, astronomical phenomena were not evident and were usually accumulated through discrete singular experiences. These accumulated experiences were gathered over time and eventually deposited into the astronomers' general knowledge.[2] The observations of one individual were not always sufficient, however, in producing knowledge about the heavens. Because astronomical knowledge was "historically-situated" and involved

[1] Dear, Discipline & Experience, 50.

the observations of "unrepeatable" singulars, "a long time frame, and the attendant necessity for trustworthy records" was necessary in astronomical practice.[3] By the end of the seventeenth century, astronomers believed that only through consistent observations could astronomical knowledge be collected, analyzed, and interpreted. Neither principles nor *a priori* philosophizing taught one how to "do" astronomy – only daily experience could provide that knowledge. Ironically, it was *a priori* philosophizing that contributed to a new disciplinary awareness in astronomy beginning in the sixteenth century.

As discussed in Chapter 2, natural philosophy and mathematics were separate university subjects with relatively no overlap until the publication of Copernicus' *De revolutionibus*. Natural philosophy was the search for either physical or natural causes, whereas the mathematical subjects, including astronomy, were descriptive rather than causal. The disciplinary boundaries that existed between the two were so entrenched among scholars that challenges to these boundaries persisted well into the seventeenth century. Moreover, natural philosophy was clearly more prominent and important than mathematics. The publication of Copernicus' *De revolutionibus* contributed to making the astronomer's role more dynamic, as mathematicians and astronomers attempted to redefine the boundaries of natural philosophy and astronomy. At the same time, they introduced the "new science" to the universities by actively teaching both the Tychonic and Copernican systems alongside the Aristotelian-Ptolemaic system.

Momentous transformations were taking place in the universities in the sixteenth and seventeenth centuries, despite the fact that the role played by European universities in the new learning has been de-emphasized by some scholars. At the universities, individuals were exposed to the new learning and made contacts with others that were usually maintained even after they left the universities. Moreover, many former students contributed back to the "system" by becoming university instructors, outside tutors, promoters of practical mathematical and astronomical knowledge, or even active correspondents. As such, these individuals served as true savants because they assumed multiple roles and multiple "cognitive roles" – one could be

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[2] Dear, Discipline & Experience, 46, 47.
[3] Dear, Discipline & Experience, 93.

an ontologist, methodologist, or epistemologist in astronomy.[4] Moreover, their education contributed to the rise of networks and the active exchange of ideas.

By the seventeenth century, astronomers were working within a variety of overlapping social settings as illustrated by Chapter 3's discussion of correspondence and networks *vis a vis* the rise of scientific groups and circles. These settings were international in scope. There were no boundaries in the promotion of astronomy, and in fact, there was a sense of "fluidity" across geographical borders as well as across settings as practitioners moved in and out of both universities and scientific circles with relative ease. Collaboration entailed the exchange of ideas, mainly through correspondence. Work done in secrecy or seclusion was discouraged in order to promote the gathering of astronomers' "experiences." In fact, correspondence was not an issue of what individuals contributed individually, but was rather, a means of creating networks from the accumulation of individual letters. As a result, we see the rise of networks (which most belonged to) that offered a certain sense of protection, cohesion, and solidarity to astronomical practitioners and their work. Consequently, astronomy was promoted in all "spaces" with certain expectations regarding the adoption of new instruments, the sharing of results, and the participation of individuals as active members in the astronomical community. When opinions clashed with respect to these expectations, controversy ensued.

The controversy over the relative merits of naked-eye versus telescopic sights discussed in Chapter 4 was the result of a number of both epistemological and social factors. In terms of the technical aspects of the debate, the ease and seemingly blind faith by which these new technological enhancements were adopted into the astronomical practitioner's arsenal point to the trust that practitioners had for instruments that had been only recently invented (or reinvented). Moreover, although practitioners were aware of the differences between the theory behind optical instruments and the practice or use of such instruments, there did not seem to be any serious concern for an understanding of theoretical explanations prior to the new instruments' use and the establishment of their reliability. But the relative omission of theoretical explanations also occurred with the invention and development of the telescope – the theory behind the optics had to eventually catch up with the practice.

[4] Westman, "Humanism and Scientific Roles," in Schmitz and Krafft, eds., Humanismus und Naturwissenschaften, 84. Westman argues that we see a rise in the epistemological-methodological role of astronomers in the sixteenth century and that Tycho worked within this context (Ibid., 96-97, 98).

As the controversy reveals, this blind faith toward the new instrumentation of the late seventeenth century did not mean the same for Hooke as it did for others. Blind faith for Hooke meant placing measurements provided by the new instruments ahead of any measurements made without the use of micrometers and telescopic sights. Astronomical observation did not go far beyond the implicit trust of these new technologies, and Hooke did not have a reputation for being a patient and dedicated observer. Moreover, he was not known for his powers of persuading others of the merits of his perspective and his acerbic personality only exacerbated his relationship with his peers. While astronomers like Cassini and Flamsteed also placed a great deal of trust in the new instrumentation of the 1660's, their preferred method of observing was characterized by patient and meticulous observations performed over the course of years, even decades. It was not enough for them to blindly accept the new instrumentation – they also firmly believed in *using it repeatedly* and promoting observation as much as possible, a sentiment they shared with Hevelius. Moreover, Flamsteed, Cassini, and Hevelius were all devoted exclusively to astronomy, whereas Hooke lacked the "specialization" in the subject.

If there was a "Hevelian programme" based on Hevelius' observations and measurements, it was short-lived. Certainly, Royal Society members continued being polite to Hevelius and receptive to his correspondence and work, but it appears that this was due more to Hevelius' old age and short life expectancy rather than their agreement with his views. His catalog, published after his death in 1690, was eventually superseded by Flamsteed's catalog in 1725. While the controversy technically ended with Hevelius' death (although Hooke continued to rail against the use of open sights), in reality it died long before Hevelius did, though it appears that he was not aware of this fact.

Hevelius may have had the prestige and Hooke the better instruments, but naked-eye sights were ultimately abandoned for epistemological (technical) reasons – this was the main driving force of the dispute. Astronomical practitioners other than Hooke provided independent epistemological justification for the claims made concerning precision and measurement, especially Flamsteed who (though years later) empirically provided the raw data. Hooke did not provide the data, and the only direct evidence between one school and the other was Halley's testimonial letter, which is the reason why the letter carried so much weight. The technical arguments may suggest that we should not bother with the social context of this controversy since the determining factors are purely epistemological. However, my aim is to provide a more complete reconstruction and interpretation of the events surrounding the controversy by recognizing the interplay between the science and the social processes that influenced it (without necessarily determining it).

Combined epistemological and sociological components abruptly forced astronomical practitioners to define themselves, their methods, and on a larger scale, the discipline of astronomy. The participants of the controversy were forced to take sides, but they also had to define what it meant to be a "user" of naked-eye sights (Hevelius' reasons differed from Wallis') or a "user" of telescopic sights (Hooke's reasons differed from Flamsteed's). Certainly not everyone was in agreement even if they were on the same side, and there was often disagreement among the ranks of Royal Society members.[5] Nevertheless, the strength of this debate rests on the diversity of opinions and views – without them and without the controversy, the discipline of astronomy would not have evolved in the same manner and time that it did.

The consequences of the controversy are apparent in the work of Cassini at the Paris Observatory and Flamsteed at the Greenwich Observatory, as discussed in Chapter 5. Hevelius, Cassini, and Flamsteed believed in the symbiosis between the observer and his instruments. The astronomy espoused by Cassini and Flamsteed entailed the best of both worlds – their astronomy was based on meticulous observations that were computed, reduced, and applied (Hevelius' genuine advantage) together with the best and latest instruments (Hooke's theoretical advantage). In addition to the controversy's direct influence on Flamsteed's views of astronomical practice, his connections to and admiration of the three northern astronomers are also significant for understanding the English astronomical tradition of the seventeenth century.

Relying on instruments, emphasizing multiple observers, rejecting past accomplishments, and believing that knowledge of the heavens was produced only through fresh observations, point to a continuous tradition eventually adopted by Flamsteed. Despite his admiration for the work of his countrymen, however, he believed that their work was outdated, and his concern that Horrocks' work was going to be published along with Hevelius' *Mercurius sub sole visa* is indicative of his trepidation. He realized that the bound publication of these works promoted the idea that this was the most current knowledge about the heavens. In the Preface of the 1725

[5] See Michael Hunter, "Latitudinarianism and the 'Ideology' of the Early Royal Society: Thomas Sprat's *History of the Royal Society* (1667) Reconsidered," Establishing the New Science. The Experience of the Early Royal Society (Woodbridge, Suffolk, UK: The Boydell Press, 1989), 45-71; and his "Introduction," Science and the Shape of

edition of his *Historia coelestis Britannica,* Flamsteed barely mentioned any of the northern astronomers, but moreover, he left out other prominent names such as Descartes, Kepler, Riccioli, and Hevelius. This gave the appearance that Flamsteed believed that he did not owe anything to his more immediate predecessors, and so "no figure of any stature is admitted to interrupt or divert the line of direct transmission from Copernicus through Tycho to Flamsteed."[6] Clearly, Flamsteed was careful in setting limits to the amount of credit given to his predecessors regardless of their influence.

Overall, the rise of the Paris and Greenwich Observatories and the birth of modern astronomy in the late seventeenth century were the result of the convergence of several parallel and overlapping forces that included the changing nature of instruments, mathematical and astronomical education, the assimilation of new ideas and systems, and the rise of networks – all of which contributed to laying the foundation for astronomy in the late seventeenth century. The Hevelius-Hooke controversy and ensuing debate served as a catalyst that transformed that foundation into an official discipline. It was the means through which astronomers debated and resolved what they believed were the defining characteristics of astronomy and appropriate activities of the astronomer. Ultimately, the development of the Paris and Greenwich Observatories catapulted astronomy from an activity pursued privately and independently to an institutionalized science.

The Controversy Within the Historiography of the "Scientific Revolution"

The Scientific Revolution, traditionally seen as covering the period between Copernicus and Newton, "is probably the single most important unifying concept in the history of science."[7] In the past, scholars focused on the Scientific Revolution by exploring the contributions made by the "canonical individuals" whose contributions shaped our modern conceptions of science. Copernicus, Tycho, Kepler, and Galileo – all of whom are mentioned in this study, are on the

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Orthodoxy: Intellectual Change in Late Seventeenth-Century Britain (Woodbridge, Suffolk, UK: The Boydell Press, 1995), 1-18.

[6] Willmoth, "Models for the Practice of Astronomy," in Willmoth, ed., Flamsteed's Stars, 74-75.

[7] Margaret J. Osler, "The Canonical Imperative: Rethinking the Scientific Revolution," in Osler, ed., Rethinking the Scientific Revolution, 3. I do not intend to go into the recent debates of the term "Scientific Revolution," but rather, discuss specific points as they relate to this study.

list. However, the "middle tier" of individuals such as Hooke and Flamsteed, do not count as part of the canon despite their importance. Moreover, Hevelius, who according to traditional accounts would be best characterized as a minor figure in the history of science who was in the wrong, played one of the most important roles of all. Not only can we claim that by persisting in his point of view in the controversy, he forced subsequent astronomers to identify their beliefs and values in astronomical practice, but we can also state that certain aspects of his astronomical practice were, in fact, admired and adopted. Therefore, it is a mistake to dismiss Hevelius as someone who does not fit into the historical record of significant individuals in this time period.

The list of "canonical individuals" is often related to the fact that they were somehow "winners" – that their work triumphed over the competition. Therefore, in controversies, we may ask who the "winners" and "losers" are, and why we even bother examining the roles played by the losers. However, I would argue that in this case, there were no definite winners or losers. Perhaps on the surface, Hooke appears to be the winner and Hevelius the loser since naked-eye sights were ultimately abandoned, along with the Tychonic observational program. But Hooke was far from being perceived as a "winner" because of the way he conducted himself in the controversy. And although Hevelius never adopted the very instruments that would have saved his life's work, he was perhaps the best observer in Europe at the time. If there was a winner in the controversy between Hevelius and Hooke, it was the astronomical research program.

Furthermore, some studies on the Scientific Revolution have focused on the event as a sudden, radical, and complete break with past scientific accomplishments and world-views including the seminal work by Thomas Kuhn, The Structure of Scientific Revolutions and I.B. Cohen's Revolution in Science.[8] The Copernican Revolution is usually the model case study for making arguments in support of revolutions in science. However, as indicated in earlier parts of this study, the acceptance and assimilation of the Copernican system was anything but immediate, and instead, was a highly complex process that scholars are currently still evaluating. Likewise, the foundations for the astronomical research programme in the seventeenth century are also complex and are not easily encapsulated in the "revolution" of the new instrumentation.

[8] Thomas S. Kuhn, The Structure of Scientific Revolutions, 2nd ed. (Chicago: The University of Chicago Press, 1970); I.B. Cohen, Revolution in Science (Cambridge, MA: The Belknap Press of Harvard University Press, 1985). Betty Jo Teeter Dobbs' response is that the Scientific Revolution was *not* sudden, radical, or complete (see her chapter, "Newton as Final Cause and First Mover," in Osler, ed., Rethinking the Scientific Revolution, 25-39).

Neither was Hevelius' "programme" replaced by Hooke's "programme," nor were there sharp, radical breaks between the two. As I have argued, the Hevelius-Hooke debate was a highly-complex affair, and the ideas espoused by each side were not "incommensurable."

The debate over "internalist" versus "externalist" approaches in the historiography of the Scientific Revolution has reached a point in which most scholars argue that some sort of combination of both approaches is necessary in grasping the complexities of the Scientific Revolution.[9] The Hevelius-Hooke controversy does not lie altogether within issues of "science," and therefore, was provoked and stimulated by many influences. Only by understanding the dynamic interplay between science and society, rather than seeing them as disparate, self-contained categories, can we more fully begin to comprehend these complexities. Beyond my claims that tying epistemological and sociological factors together is ideal in the reconstruction of historical events, I would also argue that I could not have made the connections without looking at the combined interplay of all factors involved.

The Sociology of Scientific Knowledge and the Controversy

Studies in the sociology of scientific knowledge focus on how knowledge is shaped and the extent to which social, political, cultural, economic, and ideological factors have contributed to science. This includes analyzing how concepts such as reputation, authority, alliances, and different "forms of life" shape how science gets "done" or "constructed," what kinds of questions get asked, and which ones are actually answered. The history of science is driven by non-scientific factors that in the end determine the outcomes of controversies. Issues of authority and reputation are invoked by participants in controversies serving as additional guiding forces in their outcomes.

Two works in the sociology of scientific knowledge that have direct bearing on this study are Shapin and Schaffer's Leviathan and the Air-Pump and Shapin's A Social History of Truth.[10] In Leviathan and the Air-Pump, the authors attempt to show three things: "that the solution to the

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[9] See H. Floris Cohen, "The New Science in its Social Setting," in The Scientific Revolution: A Historiographical Inquiry (Chicago and London: The University of Chicago Press, 1994), 198-208.

problem of knowledge is political;" "that the knowledge thus produced and authenticated becomes an element in political action in the wider polity;" and "that the contest among alternative forms of life and their characteristic forms of intellectual product depends upon the political success of the various candidates in insinuating themselves into the activities of other institutions and other interest groups."[11] The authors pit Boyle, a proponent of experimental method, against Hobbes, a mathematician who argued for deduction and paid little attention to experiment and observation. The authors use Hobbes and Boyle to conclude that disputes are resolved when one "candidate" or group defeats the other and as a result, the individual or group's "forms of life" prevail. Controversy, according to their argument, is a clash between different forms of life, and is resolved by political and social means by focusing on issues of authority, reputation and the enlistment of allies.

Shapin further develops this argument in The Social History of Truth. More specifically, he links science with gentlemanly conduct and argues that seventeenth-century empirical knowledge in England was guided and determined by gentlemanly practices and that this was an indication of credibility and trust – whose testimony was ultimately reliable.[12] The reliable truth-tellers were the "gentlemen" who could be trusted to speak the truth – all others were either liars or base and could not be trusted.[13] Gentlemanly norms came out of the Italian Renaissance, and specifically, from court manuals. The Royal Society eventually adopted these norms and trust in the gentleman's word was decisive in their acceptance or rejection of knowledge.

In his chapter "Knowing about People and Knowing about Things: A Moral History of Scientific Credibility," Shapin applies his model to the controversy between Hevelius and Auzout in the mid-1660's over cometary positions, and claims that his aim is to "show how knowledge about the heavens and knowledge about people-reporting-about-the-heavens were juxtaposed and evaluated so as to produce new knowledge of both comets and cometary observers."[14] 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

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[10] Steven Shapin and Simon Schaffer, Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life (Princeton: Princeton University Press, 1985); Steven Shapin, A Social History of Truth: Civility and Science in Seventeenth-Century England (Chicago: The University of Chicago Press, 1994).
[11] Shapin and Schaffer, Leviathan and the Air-Pump, 342.
[12] Shapin, A Social History of Truth, xxi.
[13] Shapin, A Social History of Truth, xxvi.
[14] Shapin, A Social History of Truth, 268. For the controversy, see Noriss S. Hetherington, "The Hevelius-Auzout Controversy," Notes and Records of the Royal Society of London 27 (1972): 103-106.

a regularly recurrent star."[15] The major issue that became the focus 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 mistaken since he and others had seen the comet 1°17' distant from Hevelius' position.[16] Pierre Petit "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.[17] 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.[18] As arbiter, the Royal Society hoped to compare Auzout and Hevelius' observations with those made by Englishmen. The matter was decided by June 28, 1665, when astronomers of the Royal Society endorsed Auzout's position.[19]

Shapin's conclusions on this debate are that it was about "things" as well as about "people," and that Hevelius and Auzout

> were not gentlemen nor did they obviously pattern their behavior upon the Boylean program for civic philosophy then being disseminated by the Royal Society. Yet several of the men who judged their dispute *were* gentlemen-philosophers, and all were members of a society which had pledged itself to producing reliable knowledge in and through a moral economy patterned upon the conventions of gentlemanly conversation.[20]

Based on Shapin's arguments, Hevelius was not a "gentleman" and this cast doubt on his testimony. To make matters worse, Hevelius was a foreigner and outsider despite his membership in the Royal Society, and his different "forms of belief" were a hindrance to his reliability. Hevelius' reputation was barely tarnished in the affair, however, and the dispute neither prevented astronomers from admiring his work and methods, nor cast doubt on his observations and measurements.

Applying Shapin's arguments to the Hevelius-Hooke controversy reveals even more problems in his thesis. As I have argued throughout this study, each side of the controversy between Hevelius and Hooke points to the complexities of this debate and the heterogeneity of

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[15] OC, II, 407-408 n. 8.
[16] See Hetherington, "The Hevelius-Auzout Controversy," 104.
[17] Oldenburg to Boyle, c. 16-18 June 1665, OC, II, 406.
[18] Auzout to Oldenburg, 22 June 1665, OC, II, 425-426.
[19] Birch, The History of the Royal Society of London, 63.
[20] Shapin, A Social History of Truth, 287, 290.

those involved. Certainly, the social status of many of the Royal Society's members was ambiguous, and foreigners involved in the controversy like Cassini complicate arguments that Hevelius was up against a unified front. Regardless, whether certain members considered Hevelius a "gentleman" does not have bearing on the controversy – the controversy was not about social status.[21] Hevelius' status as an astronomer was significantly more important, although as I have already indicated, part of that included his reputation and authority that astronomers like Flamsteed and Cassini admired. Hooke, on the contrary, was (according to Shapin) an employed technician who was not of "gentlemanly" status. Certainly, Hooke's social status was less eminent, and certainly not superior, to Hevelius'. Yet the Royal Society, while disagreeing with Hooke's rhetoric, sided with him rather than Hevelius, in effect actually generating the controversy. Moreover, the Royal Society frequently turned to Hooke as a reliable witness of the multiple experiments he performed at the Royal Society's request.

Sociologists of scientific knowledge would argue that the Hevelius-Hooke controversy was about a clash of different cultures, ideologies, and social ranks, in that Hevelius' "form of life" clashed with the Royal Society's (or Hooke's) "form of life." When Hevelius was presented with an option to "convert," he refused, and in doing so, rejected the Royal Society's "form of life." Under such conditions, according to Shapin, there should not have been any understanding between the two opposing sides. However, as I discussed in Chapter 4 and in this conclusion, Royal Society members and Hevelius did in fact understand each other's arguments even if they did not agree with them. Part of the Tychonic programme (that Hevelius espoused) was even carried forward into the work of Hevelius' opponents.

While social standing and the reliability of testimony should not be ruled out entirely as two of many influences in this case, they are not the driving forces of the controversy. As the actual arguments made by the controversy's participants demonstrate, expertise, experience, and abilities were much more important to those involved in the controversy and to those responsible for the success of the Paris and Greenwich Observatories. Moreover, arguments based on social status are even harder to apply in cases involving tangible, working instrumentation. In the end, astronomers adopted, defined, and refined a programme of systematic and consistent observations and measurements that had been provoked in large part by the controversy itself.

[21] If that were the case for either the Hevelius-Auzout controversy or the Hevelius-Hooke controversy, Hevelius should have won both!

In sum, the new instrumentation and ensuing controversy set new standards of accuracy and precision. Beyond this, however, the controversy helped astronomers reach a new consensus by defining the astronomical community and the daily activities and agendas of the astronomers at the observatories. Cumulatively, I have tried to show that if we are to understand the very nature of this science – the guiding principles and objectives of the astronomical practitioner in the late seventeenth century – we cannot do so without the multiple components that contributed to the controversy and beyond. The consideration of all these factors including the education and qualifications of astronomers, technological innovations, methods of observation, modes of communication, and the rise of community-based science not only enriches, but also contextualizes our understanding of disciplinary astronomy in the late seventeenth century.