Priority and Nationalism:
The Royal Society's International Priority Disputes, 1660-1700

by

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Abstract

The Royal Society of London, the English scientific society founded in 1660, was involved in a number of disputes in the seventeenth century concerning who was the first person to make an invention or discovery. These priority disputes had a significant effect on the careers of most of the prominent figures in the early Royal Society, including Newton, Boyle and Hooke. Inventions and discoveries were the foundation of the Royal Society's reputation, and thus needed to be claimed and protected in priority disputes. The subjects of these disputes ranged from solutions to mathematical problems to high-profile experiments. Such disputes frequently pitted Fellows of the Royal Society against intellectuals from the Continent. They were occasions for polemics framed in nationalistic terms, despite the collaborative spirit with which the transnational Republic of Letters purported to operate. This thesis examines how the Royal Society's priority disputes began, how they functioned once underway, and how they concluded. It focuses on disputes between the Royal Society and its continental rivals, seeking to determine the extent to which nationalism was a factor. It argues that Society members, who were always guided by multiple loyalties, valued their loyalties to themselves, to the Society and to the English nation more than their loyalty to the Republic of Letters. Other social factors that motivated the disputants are also explored, including honour, credibility, and the Society's ideal of aversion to conflict.

This thesis highlights patterns in the behaviour of the participants of seventeenth-century priority disputes. It draws on methodology used in the sociology of science to analyze these patterns, examining the social construction involved in invention and discovery. Case studies are used to illustrate how the participants in priority disputes redefined several entities in ways that suited their own claims to priority: the invention or discovery being disputed, the etiquette of the Republic of Letters, the distinction between invention and innovation, and priority itself. Particular attention is paid to the activities of Henry Oldenburg, Secretary of the Royal Society, who communicated on behalf of the Royal Society through his correspondence network and the journal he edited, the *Philosophical Transactions*. This thesis argues that the Royal Society valued Oldenburg in part for his role in instigating priority disputes with non-English intellectuals, a role to which he was well-suited on account of his many contacts in England and on the Continent, his rhetorical skills, and his experience as a diplomat. It also analyzes the roles of experts like John Wallis and Timothy Clarke in priority disputes, arguing that Oldenburg could call upon them to defend English priority. However, it is noted that these figures (especially Wallis) sometimes abandoned the façade of English unity in favour of causes that affected them more personally, including their own priority claims. Accordingly, they employed the same polemical style in domestic priority disputes that they did in international ones. This study concludes with the suggestion that the polemics of figures like Oldenburg, Clarke and Wallis were crucial to the program of the seventeenth-century Royal Society because conflict, the idea of aversion to conflict notwithstanding, was an acknowledged and valued part of early Royal Society culture.

List of Abbreviations Used

CSM	Rigaud, Stephen Peter, ed. *Correspondence of Scientific Men of the Seventeenth Century, Including Letters of Barrow, Flamsteed, Wallis, and Newton, Printed from the Originals in the Collection of the Right Honourable Earl of Macclesfield*, 2 vols. Oxford: Oxford University Press, 1965. Originally published in 1841.
HRS	Birch, Thomas. *The History of the Royal Society of London for Improving of Natural Knowledge*, 4 vols. London: A. Millar, 1756-1757. Reprint, Hildesheim: Georg Olms Verlag, 1968.
NC	Newton, Isaac. *The Correspondence of Isaac Newton*, 7 vols. Edited by H. W. Turnbull., J. F. Scott, A. Rupert Hall and Laura Tilling. Cambridge: Cambridge University Press, 1959-1977.
OC	Oldenburg, Henry. *The Correspondence of Henry Oldenburg*, 10 vols. Edited by A. Rupert Hall, Marie Boas Hall and Eberhard Reichmann. Madison: University of Wisconsin Press, 1965-1975.
ODNB	Matthew, H. C. G and Brian Howard Harrison, eds. *Oxford Dictionary of National Biography*, online ed. Oxford: Oxford University Press, 2004. Available from http://www.oxforddnb.com.ezproxy.library.dal.ca.
OED	Simpson, John and Edmund Weiner. *Oxford English Dictionary*, online ed. Available from http://www.oed.com.ezproxy.library.dal.ca.
Phil. Trans.	Oldenburg, Henry, ed. *Philosophical Transactions (1665-1678)*. Available from http://www.jstor.org.ezproxy.library.dal.ca/action/showPublication?journalCode=philtran1665167.
WB	Boyle, Robert. *The Works of the Honourable Robert Boyle*, 6 vols. Edited by Thomas Birch. London: W. Johnston, et al., 1772. Reprint, Hildesheim: Georg Olms Verlag, 1965-1966.
WC	Wallis, John. *The Correspondence of John Wallis (1616-1703)*, 2 vols. Edited by Christoph J. Scriba and Philip Beeley. Oxford: Oxford University Press, 2003-2005.

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Chapter 1: Introduction

In 1663, the French intellectual Samuel de Sorbière visited the Royal Society of London during a three-month visit to England and was elected as a Fellow. A 'trumpeter' of the cause of natural philosophy, as he referred to himself,[1] Sorbière was historiographer royal to Louis XIV and the Secretary of the Montmor Academy, a group of scholars who met in Paris and were advocates of the burgeoning field of experimental philosophy. He was amazed at the organization and civility of the Society's weekly meetings, and "took special notice of this Conduct, in a Body consisting of so many Persons, and of such different Nations." Sorbière was honoured that the Fellows asked him to join and pleased that the Society demanded very little from its members, asking only that they attend meetings when they could, and that they "honour and promote all they can the Interests of the [Society], as long as they are willing to continue Members of it".[2] Perhaps Sorbière thought that the Society's tolerance of foreigners which he found so remarkable was responsible for how smoothly its meetings ran; after all, it ensured that the Society's actions were not entirely governed by Englishmen. For Sorbière, the English had "a natural inclination . . . to Laziness, Presumption, and a sort of Extravagance of Thought".[3] The compliments he paid the Society were in stark contrast to every comment he made about the English people, except natural philosophical figures like Bacon and Harvey whom he greatly admired.[4] Sorbière felt that English intellectuals (apart from the Royal Society) produced little of value, and made up for their lack of creativity with plagiarism: "they frequently never cite the Books from whence they Borrow, and so their Copies are taken for Originals."[5]

What Sorbière underestimated was the extent to which the members of the Royal Society considered themselves English. After Sorbière had an account of his visit printed, a furious Thomas Sprat, promoter of the Royal Society and author of its *History* (1667),

1 Samuel de Sorbière, *A Voyage to England Concerning Many Things Related to the State of Learning, Religion, and Other Curiosities of that Kingdom* (London: J. Woodward, 1709), sig. a3r.
2 Sorbière, *Voyage to England*, 37.
3 Sorbière, *Voyage to England*, 4.
4 Sorbière, *Voyage to England*, 31-32.
5 Sorbière, *Voyage to England*, 70.

wrote a response refuting every point that Sorbière made about the English people. Sprat explained that one of the main goals of the Royal Society was "to be Advantageous to the Glory of *England*", a goal which would be threatened "if my Countrymen shall know that one who calls himself a Member of that *Assembly* has escaped unanswered in the publick Disgraces which he has cast on our whole Nation."[6] If the welcoming spirit of the Royal Society had led Sorbière to believe that it was not as dedicated to England as it was to experimental philosophy, Sprat showed him his mistake. As for Sorbière's claim that the English were plagiarists, Sprat dismissed this as the result of the shared delusion of people in France, Spain and Italy who thought "that all Wit is to be sought for nowhere but amongst themselves."[7] Sorbière had made a critical mistake in doubting that Englishmen were capable of two things on which the Fellows of the early Royal Society prided themselves: their ingenuity to make original intellectual contributions, and their respect for such contributions that were made by others in the European intellectual community.

From its foundation in 1660, the Royal Society valued very little as much as it valued invention and discovery; these were the measure of its contribution to England and the world. The sense of international cooperation that Sorbière encountered during his visit was expendable in cases when foreigners denied that an English member had made the invention or discovery that he had claimed. Frequently during its seventeenth-century history, the Royal Society's anger came down upon foreign rivals who denied English inventions and discoveries by claiming that someone else had made them first. Historians have long accepted that priority disputes – conflicts about who first invented or discovered something – are common in the history of science, and there are dozens of examples from early modern Europe. However, while there has been much historical scholarship on individual priority disputes, especially those that concern what historians consider a landmark discovery or invention like infinitesimal calculus, there have been surprisingly few historical studies examining priority disputes as a recurrent historical phenomenon. Certainly some historians, especially in the last three decades, have sought by examining a priority dispute to reveal what motivated the participants, how they

6 Thomas Sprat, "Observations on Mons. de Sorbiere's Voyage into England" in Sorbière *Voyage to England*, 102.
7 Sprat, "Observation on Sorbière's Voyage," 171.

treated each other, how they came to make the same discovery or invention, what lengths they were willing to go to in order to be given credit, and whether the dispute had any impact on how the entity in question and its disputants were remembered.[8] Yet such studies have almost always limited themselves to a discussion of one priority dispute and made conclusions about the history of a single entity being disputed or about the particular individuals involved in the dispute. At best these studies make some speculations about how the factors that played a role in that particular dispute might do the same in others. Few historians have looked at priority disputes as a pattern of behaviour, as an expression of ever-present concerns in the mind of natural philosophers; this is curious considering the frequency of early modern priority disputes.

This thesis offers an analysis of the behaviour of the participants in priority disputes involving the Royal Society, primarily during the 1660s, the first decade of its existence. This was a time when the Royal Society sought to become an exemplar for the practice of experimental philosophy and thus gain the admiration of continental scholars. In this the Fellows were quite successful, at least during the seventeenth century, and I suggest that their agenda of claiming and protecting priority of invention and discovery for England was a crucial factor in that endeavour, even though it occasionally alienated their opponents. This thesis will consist of a series of case studies detailing the course of priority disputes from the perspective of the Royal Society with a focus on the efforts of a few key individuals. Through these case studies I will seek to answer a number of questions: What was at stake during priority disputes between English and continental natural philosophers in the latter half of the seventeenth century, at the level of

8 See, for example, A. D. Farr, "The First Human Blood Transfusion," *Medical History* 24 (1980): 143-162; Galileo Galilei, *On Sunspots*, trans. Eileen Reeves and Albert van Helden (Chicago: University of Chicago Press, 2010); A. Rupert Hall, *Philosophers at War: The Quarrel between Newton and Leibniz* (New York: Cambridge University Press, 1980); A. Rupert Hall and Marie Boas Hall, "The First Human Blood Transfusion: Priority Disputes," *Medical History* 24 (1980): 461-465; Katherine Hill, "'Juglers or Schollers?': Negotiating the Role of a Mathematical Practitioner," *The British Journal for the History of Science* 31 (1998): 253-274; Rob Iliffe, "'In The Warehouse': Privacy, Property and Priority in the Early Royal Society," *History of Science* 30 (1992): 29-68; N. 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); Domenico Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz* (Oxford: Oxford University Press, 1996); Holly Tucker, *Blood Work: A Tale of Medicine and Murder in the Scientific Revolution* (New York: W. W. Norton, 2011); A. J. Turner, "William Oughtred, Richard Delamain and the Horizontal Instrument in Seventeenth Century England," *Annali Dell'Istituto e Museo di Storia della Scienza di Firenze* 6 (1981): 99-125.

individuals, scientific societies, and nations? Which historical actors within the Royal Society were involved in causing these disputes and what motivated them and others to participate? What did Royal Society members with different functions and from different social strata stand to gain? Who settled priority disputes and how, and was there ever consensus? Did individuals participate in these disputes for their own gain, for that of the Royal Society (or in some cases another collective of intellectuals), for their nation, or for some combination thereof? Perhaps most importantly, what socio-cultural factors – what patterns of behaviour – influenced the course of priority disputes and in whose favour? To answer these questions I trace the course of priority disputes through personal correspondence, especially that of Henry Oldenburg and John Wallis, and printed texts ranging from books and pamphlets to scholarly journals like the *Philosophical Transactions of the Royal Society* and the *Journal des sçavans*.

Chapter 2 examines the role of Henry Oldenburg, who as Secretary of the Royal Society managed a vast correspondence network and edited the Society's journal, the *Philosophical Transactions*. Oldenburg's two domains were frequently the site of priority disputes as continental natural philosophers and mathematicians routinely communicated with the Royal Society through letters to Oldenburg, and learned about the Society's activities through his replies (and in some cases through the *Transactions*). Oldenburg used skills from his experience as a diplomat to maintain a valuable flow of information into the Royal Society from abroad. Yet at the same time he catalyzed conflicts between English and continental scholars to give the English a chance to claim intellectual accomplishments and amplify the reputation of the Royal Society. Chapter 3 examines Oldenburg's priority-defending strategies in action in a case of particular importance to the Royal Society, the priority dispute over the invention of blood transfusion. In addition to highlighting Oldenburg's facilitation of the blood transfusion dispute, this chapter introduces two other important roles in the Society's defence of English priority: that of innovators like Richard Lower, the English claimant to priority who evidently performed the first blood transfusion between two animals, and that of experts such as the physician and original Society member Timothy Clarke to whose authority Oldenburg could refer important matters of priority. Chapter 3 also raises the question of whether priority

disputes between English and continental rivals can be considered "nationalist conflicts" and seeks to answer that question through an analysis of the motives of the participants. Chapter 4 examines more closely the role of experts in priority disputes through a series of cases in the career of the Oxford Professor John Wallis who promoted and defended English achievements in mathematics for half a century. It revisits and complicates the notion of nationalism with a case study of a priority dispute between Wallis and his fellow Englishman William Holder who both claimed priority for teaching a mute deaf person to speak.

In my effort to understand the social behaviour of natural philosophers I have benefitted greatly from the work of sociologists of science, especially Simon Schaffer. Schaffer suggests a reason why priority disputes have been under-studied. Historians, he claims, have been embarrassed by priority disputes; it does not reflect well on the major figures in the history of science to highlight their petty, selfish rivalries.[9] Hence historians have largely dismissed them as unfortunate and unpleasant moments in the otherwise instructive history of discovery and invention. For example, for A. Rupert Hall, the dispute in which John Wallis accused François Dulaurens of plagiarizing English mathematicians (see Chapter 4) was "typical of seventeenth-century mathematical controversy, but more sterile even than usual."[10] Similarly, after discussing Wallis's dispute with William Holder, Jonathan Rée writes, "This kind of petty rivalry was to occur over and over again amongst educators of the deaf. But behind it all there lay one of the greatest scientific advances of the entire seventeenth century, and one of the most beneficial medical and educational discoveries of all time."[11] Thus Rée tries to rescue the

9 Simon Schaffer, "Making up Discovery" in *Dimensions of Creativity*, ed. Margaret A. Boden (Cambridge, MA: The MIT Press, 1994), 31-32, 34. Similarly, Niccolò Guicciardini suggests that historians' embarrassment is to blame for a number of unanswered questions about the dispute between Newton and Leibniz and the history of the development of infinitesimal calculus in general (Niccolò Guicciardini, *Isaac Newton on Mathematical Certainty and Method* [Cambridge, MA: The MIT Press, 2009], xv).

10 OC IV xx.

11 Jonathan Rée, *I See a Voice: Deafness, Language and the Senses – A Philosophical History* (New York: Metropolitan Books, 1999), 109. For other examples of dismissals of disputes among natural philosophers and mathematicians as unfortunate and petty and assurances that they do not affect the quality of the science, see Stephen Jay Gould, *Ontogeny and Phylogeny* (Cambridge, MA: The Belknap Press of Harvard University Press, 1977), 35; A. Rupert Hall and Marie Boas Hall, "Why Blame Oldenburg?," *Isis* 53 (1962): 484; Joe Jackson, *A World on Fire: A Heretic, an Aristocrat, and the Race to Discover Oxygen* (New York: Viking, 2005), 336; D. and S. M. Easton, *Multiple Discovery: The Pattern of Scientific Progress* (Aldershot: Avebury, 1984), 150; J. F. Scott, "The Reverend John Wallis, F. R. S. (1616-1703),"

valuable science from the petty dispute which was a waste of Wallis and Holder's energy.[12] Evidently writers like Hall and Rée dismiss the role of the self-serving actions of individuals in the process of discovery and invention because they want to separate these actions from valuable intellectual production and maintain the supposed purity of science. This attitude is reflected in the rhetoric of the historical actors themselves who, for reasons that will be explored below, expended much effort in convincing their contemporaries of their distaste for conflict; the adoption of this attitude by historians has not been fruitful.

In 1994, Schaffer suggested that historians were just beginning to understand that they cannot afford to let this embarrassment or desire to protect the image of science deter them from studying the processes of discovery and invention. He considers the disputes involved in those processes to be "crucial sites for investigating how scientists work." Conflicts, for Schaffer, are occasions when the normally buried social conventions of scientific practitioners come to the surface: "What must invisibly have underlain this conduct now becomes visible. Contests about discovery are excellent examples of this, because in such 'priority disputes' the conventions of authorship and authority are most lucidly disputed and constructed."[13] Priority disputes, Schaffer implies, are not simply ugly moments in the history of science that historians should be glad to dismiss as incidental to the development of scientific ideas. Rather, they are crucial moments in the establishment of scientific fact and uncommon opportunities to see how the social conventions of scientists and natural philosophers factor into the processes of invention and discovery.

Schaffer's work is largely sociological and reflects the fact that the majority of scholarly works attempting to explain the recurring phenomenon of priority disputes have

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Notes and Records of the Royal Society of London 15 (1960): 60; Turner, "Oughtred, Delamain and the Horizontal Instrument," 101-106.

12 Richard Westfall adopts a similar position in his analysis of Galileo's Machiavellian efforts to use telescopic discoveries to earn the patronage of Cosimo II de' Medici, writing, "It appears to me that some degree of disillusionment is likely to accompany most examinations of the social settings of science. By their nature, they lead us out of the context of justification and into the context of discovery, where we see the play of human motives instead of the finished products of reason." However, Westfall assures the reader, this is irrelevant to the quality of the science; Galileo's strengthening of the Copernican position remains an "enormous achievement" (Richard S. Westfall, "Science and Patronage: Galileo and the Telescope," *Isis* 76 [1985]: 28).

13 Schaffer, "Making up Discovery," 31-32.

been written from a sociological perspective rather than a primarily historical perspective. One of the earliest and best-known examples is Robert Merton who lays out a sociological theory for the cause of priority disputes in his "Priorities in Scientific Discovery" (1957). For Merton, priority disputes are the result of two conflicting norms inherent in the institutions of modern science that guide scientists' actions: the norm of humility and the norm of originality. Priority disputes are inevitable in Merton's scheme because the reward system in science favours originality, and in striving for originality scientists are bound to violate the expectation that they remain humble, disinterested, and dedicated only to the utilitarian goal of science.[14] It is very likely that similar research programs will yield the same result, and in such cases the norm of originality forces each group to claim originality at the expense of the others, thus violating the norm of humility; this tends to offend the members of the scientific community who then pick sides in the ensuing priority dispute.[15]

Merton's explanation of priority disputes relies on the Zeitgeist theory of multiple discovery which posits that natural philosophers and scientists will inevitably happen upon the same inventions and discoveries because, in any time period, they are working within the same cultural and intellectual context, with the same background and goals. The Zeitgeist, as the theory goes, is the reason for the apparent abundance of coincidence in discovery and invention called "multiple discovery/invention" or "convergence".[16] Under the Zeitgeist theory, the conditions are always right for multiple individuals (or multiple groups) to make the same invention or discovery. According to Edwin G. Boring, the Zeitgeist makes multiple discovery much more common than single

14 Merton's fellow sociologist of science Michael Strevens calls this reward system "the *priority rule*, the system of rewards which accords all credit, and so all the personal benefits that go along with credit, to the first research program to discover a particular fact or procedure, and none to the other programs pursuing the same goal" (Michael Strevens, "The Role of the Priority Rule in Science," *The Journal of Philosophy* 100 [2003]: 55-56).

15 Robert K. Merton, "Priorities in Scientific Discovery: A Chapter in the Sociology of Science," *American Sociological Review* 22 (1957): 635-659.

16 The term "convergence" appears in Hall's account of the Newton-Leibniz dispute. References to "multiple discovery" and "multiple invention" are more common, as in the works of Augustine Brannigan and Richard A. Wanner, and Edward W. Constant II (Augustine Brannigan and Richard A. Wanner, "Historical Distribution of Multiple Discoveries and Theories of Scientific Change," *Social Studies of Science* 13 [1983]: 417-435; Hall, *Philosophers at War*, 5; Constant, "The Diversity and Co-Evolution of Technological Multiples," 183-210).

discovery.[17] Yet Schaffer is skeptical of the Zeitgeist theory, as well as other competing theories that seek to explain why multiple discovery happens. In Schaffer's view, they do not happen; they are made. Discovery and invention, whether single or multiple, happen when authorities in the scientific community say they happen, and they make this decision retroactively. Certainly, a shared intellectual backdrop leads to the development of similar research programs. As Edward W. Constant II argues specifically regarding technology, forms of scientific research "co-evolve"; they develop in response to each other.[18] Yet Schaffer points out that there are inevitably some differences between any two entities that historians or historical actors might consider to be the same invention or discovery based on certain shared characteristics; people decide based on arbitrary criteria whether these differences are enough to require the separation of entities that are otherwise the same. Schaffer's view of multiple discoveries and inventions as social constructions has been a key influence on my research. Schaffer's work implies that the individuals involved in the negotiation of the classification of inventions and discoveries as the same or different had remarkable agency in instigating and guiding seventeenth-century priority disputes; in short, these disputes were fundamentally social activities.[19]

These sociological analyses of priority disputes, while valuable, are not sufficient for an understanding of what motivated priority disputes in a specific historical context. Schaffer's point that multiple discoveries are the constructions of historical actors seems to apply to the entire history of priority disputes, but it reveals the need for separate historical studies of priority disputes that occurred in different periods. If those who decide what constitutes discovery and invention impress their values onto the process of invention and discovery, the fact that values inevitably change implies that future studies of the process of discovery would benefit from a focus on a narrow period of time. In the

17 Edwin G. Boring, "Cognitive Dissonance: Its Use in Science." *Science*, New Series 145 (1964): 680-685. See also Brannigan and Wanner, "Historical Distribution of Multiple Discoveries," 417-435; Dean Keith Simonton, *Scientific Genius: A Psychology of Science* (Cambridge: Cambridge University Press, 1988); Schaffer, "Making Up Discovery," 33-35.

18 Edward W. Constant II, "On the Diversity and Co-Evolution of Technological Multiples: Steam Turbines and Pelton Water Wheels," *Social Studies of Science* 8 [1978]: 184, 197, 202-205.

19 Schaffer, "Making Up Discovery," 34-36. Constant makes a similar argument about multiple invention. However, while he is skeptical about most cases of multiple invention and largely rejects the determinism of the Mertonian Zeitgeist theory, he does refer to the invention of the Pelton water wheel as a rare "unequivocal instance" of multiple invention, and takes issue mainly with the use of determinism to explain multiple invention. (Constant, "The Diversity and Co-Evolution of Technological Multiples," 183-210. The quotation is from p. 198).

Royal Society of the seventeenth century, the relationship between honour and social standing on one hand, and credibility and authority on the other, was inescapable in the production of scientific knowledge.[20] The ideal cases with which to study this relationship are priority disputes in which the various parties defined what constituted an invention or a discovery in a way that promoted and protected what was important to them: typically, among seventeenth-century members of the Royal Society, what was important was the honour of their nation, the advancement of their career, and their public image.

A rare example of a historiographical work that has examined priority disputes as a recurring phenomenon is an article by Rob Iliffe. Iliffe explores the role of priority in the early Royal Society, especially as it relates to Robert Hooke and the patent system. In the coming chapters I draw repeatedly on Iliffe's concept of redefinition in priority disputes. Iliffe explains that the label applied to the subject of a dispute can never refer to just one entity. His main case study is the priority dispute between Robert Hooke and Christiaan Huygens over the invention of a watch that kept time using the isochronous vibrations of a spring. Iliffe explains, "it is clear that there is no one instrument which corresponded to the 'balance-spring watch', and therefore there can be no one moment of invention of the balance-spring watch.'"[21] In Iliffe's view, disputants on opposing sides defined an entity like the balance-spring watch in different ways to suit their own needs. One could defend himself against a charge of plagiarism or stealing an idea by suggesting that his invention or discovery was not the same as the one that preceded it; in this case one's opponent might reply that the entities are the same, except his own is better.[22] Iliffe emphasizes that the criteria for making an original invention were subjective and that self-serving individuals manipulated the areas of ambiguity in the process of invention to promote their own causes. My thesis builds on Iliffe's notion of definition and redefinition in priority disputes by suggesting that in addition to redefining the entity being disputed, the participants redefined any other factor they could, such as how priority was established, what distinguished invention from innovation, and what rules of etiquette governed the actions of natural philosophers and mathematicians when they discussed inventions and discoveries. My aim is to show that, when contemporaries

20 Steven Shapin, *A Social History of Truth* (Chicago: University of Chicago Press, 1994), xxv-xxxi.
21 Iliffe, "In the Warehouse," 53. Cited in full in n. 8 above.
22 Iliffe, "In the Warehouse," 52.

tested who deserved to be recognized for priority, there were many independent variables and essentially no dependent variables; priority disputes could drag on for years as disputants argued over every criterion for assigning priority.

Another valuable study that touches on priority disputes is Allan Chapman's *England's Leonardo* which focuses on the career of Robert Hooke and makes insightful reflections based on his frequent involvement in priority disputes. Chapman suggests that seventeenth-century priority disputes were so bitterly fought because original invention and discovery were crucial to the identity of intellectuals in that period; making inventions and discoveries was "germane to the very world-view of the seventeenth-century Virtuosi." In Hooke's view in particular, Chapman explains, producing useful inventions was the *sine qua non* of an experimental philosopher. Accordingly Hooke claimed to have made literally hundreds of inventions.[23] In short, threats to a natural philosopher's priority could be taken as threats to his identity. Adrian Johns has made similar observations about the role that the concepts of authorship and ownership of intellectual property played in the early Royal Society, concepts which he argues the Society helped to transform. For Johns, the perpetual concern about owning one's texts serves as a way to explain the highly defensive attitudes of figures like Hooke, Newton and Boyle toward the content of their printed and unprinted texts.[24]

Mario Biagioli and Nick Jardine provide further evidence about the socio-cultural factors that gave intellectuals occasion to engage in priority disputes. Biagioli has shown that natural philosophers and mathematicians (especially Galileo) used discoveries to gain and secure patronage by using these discoveries to enhance their patrons' image.[25] In *Galileo's Instruments of Credit*, Biagioli explains that discoveries became the subject of priority disputes because of the lack of clear rules about what constituted discovery.[26] Elsewhere, Biagioli writes that priority disputes were like duels in that they coincided with perceived slights against one's honour. In the case of the priority dispute between

23 Allan Chapman, *England's Leonardo: Robert Hooke and the Seventeenth-Century Scientific Revolution* (Bristol: Institute of Physics Publishing, 2005), 170.
24 Adrian Johns, *The Nature of the Book: Print and Knowledge in the Making* (Chicago: University of Chicago Press, 1998), 444-542.
25 Mario Biagioli, "The Social Status of Italian Mathematicians, 1450-1600," *History of Science* 27 (1989): 41-95. See also Westfall, "Science and Patronage," 11-30.
26 Mario Biagioli, *Galileo's Instruments of Credit* (Chicago: University of Chicago Press, 2006), 97.

Tycho Brahe and Nicolaus Ursus over the geoheliocentric model of the universe, "Tycho did not perceive clear boundaries between personal challenges and scientific disputes."[27] This is the dispute that Jardine examines in his *Birth of History and Philosophy of Science*. For Jardine, third parties joined in priority disputes in order to impress the person whose side they took; thus Kepler advanced his career by writing in favour of Tycho.[28]

The work of Steven Shapin has also guided my research through its analysis of the role of social conventions in the Royal Society in the seventeenth century. Shapin's *Social History of Truth* provides a useful analysis of the culture of honour in which the early members of the Royal Society operated. For Shapin, honour was proportional to credibility in seventeenth-century natural philosophy, and it consisted largely in gentlemanly behaviour such as telling the truth. Shapin takes Robert Boyle as a paradigm of such gentlemanly behaviour who fashioned an identity for himself as a "Christian Virtuoso" which allowed him to speak with more authority than the other Fellows.[29] Shapin's work has been particularly valuable to my research because it is rooted in a belief that knowledge production is a social process. Elsewhere, Shapin argues "that the making and warranting of scientific knowledge are *performances*, that those producing scientific knowledge can and do use a full range of cultural resources to produce these performances, and that these include displaying the marks of integrity and entitlement: expertise, to be sure, but also the signs of dedication and selflessness."[30] The letters and printed texts written during priority disputes are replete with evidence of such performances. As disputants argued that they or their nation deserved priority, they constantly had to reinforce the credibility that allowed them to make such a claim. Hence the frequent reminders of their gentlemanly qualities such as a distaste for conflict and respect for the truth. Shapin has also written an article about Robert Hooke that analyzes his troubled relationship with Henry Oldenburg, who particularly frustrated Hooke when

27 Mario Biagioli, *Galileo, Courtier: The Practice of Science in the Culture of Absolutism* (Chicago: University of Chicago Press, 1993), 60.
28 Jardine, *The Birth of History and Philosophy of Science*, 16-20. Cited in full in n. 8 above.
29 Shapin, *Social History of Truth*, xxv-xxxi. Cited in full in n. 20 above.
30 Steven Shapin, *Never Pure: Historical Studies of Science as if It Was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority* (Baltimore: The Johns Hopkins University Press, 2010).

he became Huygens' ally during the priority dispute over the balance-spring watch. Shapin points out that Hooke and Oldenburg were competing for the attention of their mutual patron, Robert Boyle, and that Oldenburg and the other Fellows viewed themselves as Hooke's social superiors.[31] In short, Shapin's work functions as a reminder that participants in priority disputes were not simply fellow natural philosophers or mathematicians; each individual played several social roles at once that were never entirely separate from their intellectual careers.

Before engaging in case studies, we must problematize what the word "priority" means when historians use it to refer to the subjects of contests between seventeenth-century intellectuals claiming credit for a discovery or invention. Instead of "priority", contemporaries generally referred to the "honour of", "glory of" or "credit for" an invention or discovery; "priority" is a historiographical term. Priority disputes more often than not merged with debates over the quality of the work of the disputants, in terms of both research and presentation. As such, it was not sufficient to be first in order to be recognized for priority by one's peers; one also had to convince his peers that he was right – and, often, not simply right, but more right than one's rival. Priority, then, was not just the state of having made an invention or discovery first, but rather a prize awarded by members of the intellectual community. This prize was sometimes worth money, but it was seemingly always worth honour and social credit. Priority constituted a legitimation of one's work by one's peers, so it conferred respect. What individuals claiming priority wanted to be recognized for was not being the first to contribute something of value to the intellectual community, but doing so without the suggestion that this contribution relied heavily on the work of someone else; priority was not awarded for redundant contributions or ones based on stolen or borrowed work. This is why a priority dispute could occasionally be defused by the parties agreeing that it was a case of independent multiple discovery; it is also why, more often, disputants portrayed the actions of their rivals as theft of intellectual property.[32] As nationalistic individuals, natural philosophers

31 Steven Shapin, "Who Was Robert Hooke?" in *Never Pure*, 190-196.

32 For information about the development of the concept of intellectual property, its distinctive early modern manifestations, and its role in priority disputes, see Pamela O. Long, "Invention, Authorship, 'Intellectual Property,' and the Origin of Patents: Notes toward a Conceptual History," *Technology and Culture* 32 (1991): 846-884, especially 881-884. See also Kevin Dunn, "Milton among the Monopolists: *Areopagitica*, Intellectual Property and the Hartlib Circle" in *Samuel Hartlib and the Universal*

and mathematicians sought to eliminate any suggestion that they were indebted to foreign rivals. Priority, as a sign that one produced original work of high quality, was a measure of an individual's worth as a natural philosopher or mathematician, which was why honour was awarded in proportion to priority.[33] Accordingly, I will use the word "priority" to refer at once to the recognition of being first and to an award from the intellectual community for success as a natural philosopher or mathematician.

* * *

This thesis explores the link between the defence of English priority and the defence of English nationalistic pride. There has been much recent debate about whether nationalism existed prior to the second half of the eighteenth century, with the modernist side arguing that it was only then that a national consciousness reached the masses, and their medievalist and early modernist opponents arguing for continuity between the eighteenth and earlier centuries. Before the mid-eighteenth century, the modernist argument goes, those expressing seemingly nationalist sentiments were confined to a group of social elites whose national consciousness, as Philip Schwyzer writes, constitutes "not the nation *per se* so much as the nation *in potentia*"; theirs is a sort of proto-nationalism that predates the process of "social diffusion" of national consciousness during the Enlightenment.[34] Yet Schwyzer points out that the term "nationalism" is still a useful shorthand for writers on the modernist side of the debate. It signifies the pride in one's ethnicity, the sense of a continuum between past, present and future generations, and the creation of a mythologized history, all of which characterize both the proto-nationalism of early modern elites and the nationalism *per se* of modern societies as a whole.[35] This thesis primarily examines Fellows of the Royal Society who were cultural elites, so the modernists should have no quibble with the application of this shorthand to the present discourse.

Reformation: Studies in Intellectual Communication, eds. Mark Greengrass, Michael Leslie and Timothy Raylor (Cambridge: Cambridge University Press, 1994), 177-192; Christopher May, "The Hypocrisy of Forgetfulness: The Contemporary Significance of Early Innovations in Intellectual Property," *Review of International Political Economy* 14 (2007): 1-25.

33 Of course, as Shapin's *Social History of Truth* indicates, priority was far from the only source of honour for a seventeenth-century intellectual. See pp. 11-12 above.

34 Philip Schwyzer, *Literature, Nationalism, and Memory in Early Modern England and Wales* (Cambridge: Cambridge University Press, 2004), 8-9. The quotation is from p. 9.

35 Schwyzer, *Literature, Nationalism, and Memory*, 2, 6-9.

Apart from this practical reason for discussing early modern nationalism, the main reason for applying the term is that the early modernist side of the debate has shown that tracing the effects of nationalism *qua* nationalism in the early modern period can be quite fruitful, especially in studies of England where nationalism seems to have first developed. For instance, David Loewenstein and Paul Stevens, the editors of *Early Modern Nationalism and Milton's England* identify nationalism in the writings of John Milton that took on many forms: it could be ethnic, religious, political, or literary, depending on the sort of commentary that Milton was making.[36] Additionally, Mark Stoyle cogently argues for the active role of nationalism in determining the loyalty of people throughout the British Isles during the Civil War, and he does not restrict his analysis to a particular portion of the socio-economic gamut. Stoyle sides with the historiographic trend that views England as "the very cradle of 'nationalism' as early as the tenth century", taking the exceptional "pride and self-confidence" of early modern English people as evidence that they "formed a precociously homogenous whole".[37] In Stoyle's account the parliamentary party appealed to the nationalism of many English people, especially in southern and eastern England, employing the association of Parliament with traditional English liberties and Protestantism at a time of high paranoia about foreign invasions and Popish plots. Conversely, Welsh and Cornish people tended to have royalist sympathies because the monarchy, while subjecting these regions to its rule, had fostered their national pride with token gestures that at least ostensibly showed respect for their heritage and autonomy. In time both sides risked alienating their supporters by relying on help from non-English troops, and each side's approach to this issue was decisive. Charles I blundered by failing to distance himself from Irish and Scottish supporters. Meanwhile, the parliamentarians formed the New Model Army which capitalized on English nationalism through its strict exclusion of non-English soldiers. Parliament then undermined the support for Charles in Cornwall and Wales by toning down its xenophobic rhetoric and reassuring the Cornish and Welsh people of the safety of their

36 David Loewenstein and Paul Stevens, eds., *Early Modern Nationalism and Milton's England* (Toronto: University of Toronto Press, 2008). 3-4.

37 Mark Stoyle, "English 'Nationalism', Celtic Particularism, and the English Civil War." *The Historical Journal* 43 (2000): 1113-1115. The quotations are from p. 1115.

national identities.[38]

Admittedly, it is doubtful that Stoyle's identification of nationalism is a self-sufficient explanation of the loyalties of British people during the Civil War period, but Stoyle does demonstrate the power of the sense of cultural uniqueness that evidently was developing more quickly on the British Isles than elsewhere in the seventeenth century. In England, as Stoyle's account makes clear, this power relied on English people viewing themselves in opposition to an "other." Many studies of early modern nationalism have suggested that the early modern English identity was largely defined by the differences that the English perceived between themselves and others. This was a time of a remarkable sense of particularism in England. As Liah Greenfeld explains, writers in Elizabeth's reign like John Foxe thought of God Himself as English; Foxe encouraged readers to thank God seven times each day for making them English rather than French, Italian, or German. As God was English, He favoured England with natural resources, protection from foreign invasion, and stable government, but He would take these favours away if the English abandoned Protestantism, which would be tantamount to ceasing to be English. This English particularism reflected a conception of the Protestant English nation that largely defined itself in opposition to continental Catholic nations, a definition which thus excluded English Catholics from the nation. Greenfeld emphasizes that this conflation did not mean that these writers were promoting the cause of Protestantism rather than the English nation; they simply lacked a vocabulary and framework in which to discuss these novel nationalist ideas and Protestantism provided a convenient vehicle for them. For Greenfeld it is clear that pro-English Elizabethan writers like Foxe viewed the English people as fundamentally better than others, and that religion was the tool that these writers used to express this superiority, particularly in the sixteenth century but into the seventeenth as well.[39]

As Greenfeld's analysis moves into the seventeenth century, it becomes particularly relevant to my research as she insists that, over the course of that century, science gradually supplanted religion as the feature of society that the English used to define their nation (although Greenfeld constructs something of a false dichotomy

38 Stoyle, "English Nationalism," 1113-1128.

39 Liah Greenfeld, *Nationalism: Five Roads to Modernity* (Cambridge, MA: Harvard University Press, 1992), 60-66.

between science and religion). Science, at the expense of Protestant fervour, became "the essence of the English national identity." Consequently science "acquired great authority and assumed a central place in the national consciousness."[40] Science not only helped the English to defend their nascent nationalism, but also allowed them to refine it, and thus became one of the major factors in the precocious development of English nationalism. For Greenfeld, the English came to associate science with their national identity because they found themselves to be the best at it by about 1660. Thus Sprat's *History of the Royal Society*, as the leading example of scientific propaganda, insisted that science was the true measure of the success of a nation, and downplayed the value of literature and humanistic scholarship, fields in which the English knew they could not compete with their continental rivals. Science, like Protestant religion before it, became the guarantor of England's continued prosperity. Crucially, the English distanced themselves both from the ancients and from the Continent by embracing science; the ancients had none, and the rest of Europe neglected science in favour of the same scholarly pursuits as the ancients.[41]

This need for the English to define themselves in opposition to outsiders to their nation, which is critical for an understanding of how English natural philosophers viewed their opponents in priority disputes, is well expressed by Schwyzer who writes that "Englishness is not a self-generated but rather a relational identity, a matter of complex and often bitter negotiation among the nations of the Atlantic archipelago (England, Ireland, Scotland, and Wales)."[42] Among the studies that demonstrate the prominence of this need for the English to define themselves in opposition to an "other" are two studies that examine English notions about Jews in the early modern period by James Shapiro and Mary Janell Metzger, respectively. As these studies explain, there was a debate about the Jews in early modern England concerning whether they were too different to be reintegrated into English society. Although there were few of them in England, Jews, as Shapiro explains, occupied an unusual position in England as Christian writers suggested that Christians and Jews had insurmountable biological or racial differences, and at the same time expressed a desire to convert the Jews. Shapiro identifies a conflict between the fact that identities inevitably overlap – one can be both English and Jewish – and the

40 Greenfeld, *Nationalism*, 85-86.
41 Greenfeld, *Nationalism*, 78-86.
42 Schwyzer, *Literature, Nationalism, and Memory*, 3.

fact that these identities only make sense in opposition to each other – Englishness was largely defined in opposition to Jewishness.[43] As Metzger explains, this yielded a notion of two different types of Jews: those like Shylock in *The Merchant of Venice* who was of a separate race and could only be converted by force; and those like his daughter Jessica whom the other characters describe as "white" and who is willingly converted through marriage. Such distinctions were useful: they allowed the English to integrate people from outside the English nation when it suited them, and still left them with their "requisite other, the alien."[44] Thus, integration into the English nation was possible but difficult, and entirely at the discretion of those already within it, which is probably why Henry Oldenburg – the German-born Secretary of the Royal Society and a key figure in the coming chapters – was such an ardent defender of English priority: he constantly needed to make a case for being an Englishman.

It is inherent in this view of early modern nationalism that nations are a social construct; they are a collective identity to which people assign themselves.[45] This is why these examinations of the English-versus-other mentality are so valuable to a study of priority disputes that occurred between intellectuals who assigned themselves to several collective identities at the same time. Figures like John Wallis and Henry Oldenburg at once considered themselves members of the English nation,[46] of the Royal Society, and of the transnational Republic of Letters.[47] This bred multiple loyalties as natural philosophers and mathematicians struggled to account for people whose interests partly overlapped with their own and partly did not. In much the same way that the English wondered whether Jews could be integrated into Christian society and, if so, which ones, the Royal Society wondered about which foreigners (and which Englishmen who did not fit the mould of an experimental philosopher) could be integrated into the Royal Society, either as Fellows or as contributors through Oldenburg's correspondence network. Generally the solution was to integrate these foreigners inasmuch as it was useful to the

43 James Shapiro, *Shakespeare and the Jews* (New York: Columbia University Press, 1996) 1-11.
44 Mary Janell Metzger, "'Now by My Hood, a Gentle and No Jew': Jessica, *The Merchant of Venice*, and the Discourse of Early Modern English Identity," *PMLA* 113 (1998): 52-63. The quotation is from p. 55.
45 See Schwyzer, *Literature, Nationalism, and Memory*, 3; Patrick Wormald, "*Engla Lond*: The Making of an Allegiance," *Journal of Historical Sociology* 7 (1994): 1.
46 This must be qualified in the case of the German Oldenburg, as Chapters 2 and 5 will discuss.
47 The concept of this transnational "Republic" in which intellectuals from across Europe took part will be addressed in Chapter 2.

Society; it was only useful to them as long as these foreigners did not infringe on English priority.

The above consideration touches on the limitations of the nationalist discourse in the study of priority disputes. The link between natural philosophy and (especially English) nationalism, though it was often a driving force behind priority disputes, has led a number of commentators to oversimplify priority disputes as nationalist conflicts, to treat nationalism alone as a sufficient explanation of the occurrence of these disputes. For instance, Greenfeld writes, "National prestige was . . . the main issue in the controversies over priority in scientific discoveries." She cites the examples of the two protagonists of my study: John Wallis and Henry Oldenburg.[48] Similarly the immunologist Charles T. Ambrose cites the Swedo-Danish nationalist rivalry as the main impetus for the priority dispute over the discovery of the lymphatic system, which occurred between the Swedish anatomist Olof Rudbeck and the Danish anatomist Thomas Bartholin.[49] Yet one needs to keep in mind that natural philosophers were not only concerned about the collective identities to which they assigned themselves: such impulses had to compete with these philosophers' loyalties to themselves and to their personal friends. Greenfeld makes a comment similarly reductive to the one quoted above in asserting that "[t]he glory of the nation was also an important reason – probably the only one that competed with the personal inclinations of scientists – to proceed with scientific endeavor at all."[50] She perceives two dominant factors in the motivation of natural philosophers: national glory and their interest in the natural world. In that perception I agree with her, but not at the expense of considerations about self-fashioning,[51] nepotism, and personal rivalry. The

48 Greenfeld, *Nationalism*, 80.

49 Charles T. Ambrose, "Immunology's First Priority Dispute: An Account of the 17th-Century Rudbeck-Bartholin Feud," *Cellular Immunology* 242 (2006): 1, 6-7; Charles T. Ambrose, "Rudbeck's Complaint: A 17th-Century Latin Letter Relating to Basic Immunology," *Scandinavian Journal of Immunology* 66 (2007): 486-493

50 Greenfeld, *Nationalism*, 81.

51 Throughout this thesis I have employed Stephen Greenblatt's definition Renaissance self-fashioning. For Greenblatt, self-fashioning refers to "the forming of a self . . . it is linked to manners and demeanor, particularly that of the elite; it may suggest hypocrisy or deception, an adherence to mere outward ceremony; it suggests representation of one's nature or intention in speech or actions." In Greenblatt's account, self-fashioning was frequently a method of advancing one's career; he examines how it was used by "talented middle-class men" who "embody, in one form or another, a profound mobility." It is this sense of self-fashioning that I have attempted to highlight in the careers of natural philosophers and mathematicians in the early Royal Society who sought to project a number of desirable gentlemanly qualities, which will be discussed below, in order to build a strong reputation and earn the admiration of

prominence of national pride in the program of the Royal Society did not prove mutually exclusive with the social advancement of individuals who were operating within a patronage system. In addition to being reductive, Greenfeld's account of the rise of English nationalism is also overly deterministic; she writes, "It seemed as if all the important factors in English history of the time conspired to favor this growth [of nationalism]" and explains that, consequently, the English people had no choice but to embrace it: "nationalism was the basis of people's identity, and it was no more possible at this point to stop thinking in national terms than to cease being oneself."[52] By describing religion and, in turn, science as in a state of bilateral reinforcement with English nationalism, she neglects the role of individuals – generally self-serving ones – who both contributed to and exploited the link between natural philosophy and English nationalism.

In many (but not all) cases the multiple loyalties of members of the Royal Society organized themselves into a hierarchy, with the transnational Republic of Letters on the bottom, England above the Republic, the Royal Society above England,[53] and oneself or one's friends within the Society above the Royal Society as a whole. Thus Royal Society members sometimes sacrificed international cooperation when they perceived a threat to English inventions and discoveries; they denounced Englishmen like Thomas Hobbes who opposed experimental philosophy and had dangerous political views, and thus threatened the Society's contributions to England in another way; and they abandoned the façade of unity within the Royal Society to defend their honour and their own priority. Of course, the same actions could – and usually did – satisfy multiple loyalties simultaneously: for example, defending the priority of a compatriot or another Fellow, especially a prominent one, earned honour for the defender. Additionally, the Republic of Letters both inspired international cooperation and provided valuable information to members of the Royal Society that allowed them to advance their own intellectual

powerful intellectual figures (Stephen Greenblatt, *Renaissance Self-Fashioning: From More to Shakespeare* [Chicago: University of Chicago Press, 1980], 2, 3, 7).

52 Greenfeld, *Nationalism*, 87.

53 A possible explanation for this could be that Fellows considered the Royal Society to have a powerful force in shaping English society. Boyle in particular conceived of the Royal Society as an exemplar of virtue and as a source of useful inventions, political stability, morality, and support for the Church of England. Thus the Royal Society became crucial to England's continued prosperity (Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life* [Princeton: Princeton University Press, 1985], especially 283-331).

endeavours and keep pace with continental invention and discovery; this, it seems, was what justified Oldenburg's extensive participation in the Republic of Letters. In short, just as the identification of the limits of nationhood in early modern England is messy and problematic, so is the identification of the limits of Royal Society Fellowship. Priority disputes were a manifestation of the tension between the competing loyalties inherent in membership in the Royal Society. My systematic examination of many volumes of the *Philosophical Transactions* and *Oldenburg Correspondence* has revealed that most priority disputes involved a breakdown between the Fellows' loyalties to the Society and to England and one hand, and to the Republic of Letters on the other. Thus, while there were cases of tension between the Fellows' other loyalties, most priority disputes involving the early Royal Society spanned the English channel and invoked nationalistic sentiments. It appears that, of the aforementioned loyalties, the Fellows valued their loyalty to cooperation among international natural philosopher the least; it also appears that the loyalties to England and to the Society were most easily unified, although they were not identical. Hence priority disputes between English fellows and foreign rivals framed in nationalist terms were inevitable and frequent.

It is not my purpose here, however, to determine whether the disputants were motivated by true nationalism or a form of proto-nationalism; that would require much more than an analysis of priority disputes. Nor will I engage more than is necessary in the several other debates on which my identification of multiple loyalties touches, including how seventeenth-century English people conceived of the self, how loyal they felt themselves to be to the crown before and after the Glorious Revolution, how class shaped their relationships, and whether they participated in a public sphere. The complex definitions and analyses required for such debates are beyond the scope of this study. What is relevant here is not what concepts like the self and the nation meant to England in the latter half of the seventeenth century; what is relevant here is the impact of these concepts on the minds of natural philosophers and mathematicians, particularly when both the individuals and the concepts came into conflict. One of the primary goals of this study is to determine the role of conflict in the Royal Society's program: How might it have been useful to the individual, to the Society, to the nation, and to the Republic of

Letters? Which of these entities did it serve best? This study aims to convey that Society members valued conflict, but they acknowledged this value tacitly rather than explicitly in order to make it more palatable to the intellectual community. This is the context in which Henry Oldenburg, a protagonist in the coming discussion, thrived.

A Note about Dating Conventions and Orthography

As this study concentrates on events in England, I have not converted the dates of texts to New Style if they were written, received, or printed by people in England. Within the text, I have instead given split dates for all letters involving English correspondents and texts printed in England, with the Old Style date followed by the New Style date (for example, 1/11 January, or 31 January/10 February). I have also used split years for dates up to 25 March O.S. (Lady Day) such as 1660/1, as many English writers did. I have used New Style for the dates of letters neither written nor received by a person in England and texts printed on the Continent, as continental writers would generally have used exclusively New Style dates. In citations, I have kept dates as they appear in the source being cited.

When quoting from a primary source, I have maintained the spelling, capitalization and punctuation used in the source as much as possible, except that I have changed “i” to “j” and “u” to “v” (and vice-versa) where appropriate in the interest of clarity. I have likewise changed “VV” to “W”. In all quotations, where I have preserved original punctuation at the end of a quotation, I have included that punctuation within the quotation marks. Where I have changed it, I have added the punctuation after the quotation marks. When quoting from documents that are not in English, I have provided my translation within the text and supplied the text in the original language in a footnote.

In many cases, I have used masculine pronouns to refer to an unspecified person; this reflects the fact that the historical actors discussed in this thesis generally belonged to a number of social categories from which women were excluded.

Chapter 2: Definitions and Redefinitions: Henry Oldenburg Defends English Priority

2.1: Oldenburg's Tools: The Republic of Letters and the *Philosophical Transactions*

Even before the foundation of the Académie des Sciences, essentially the French equivalent to the Royal Society, in late 1666, there were signs of competition between French natural philosophers and members of the Society. A controversy began after Christiaan Huygens wrote a letter to the French astronomer Adrien Auzout describing a design for a machine for grinding telescope lenses. The new method of grinding had been proposed by Robert Hooke, the Royal Society's Curator of Experiments, and conveyed to Huygens by Sir Robert Moray, a founding member of the Society. Shortly thereafter, Hooke's proposed machine was printed in his *Micrographia*. Auzout was not impressed by Hooke's design and told Huygens so in a letter written 6 March 1665: "I am quite surprised that the Society that wants to be so sure and so reserved allowed the printing of a machine without having proven it since it would have taken so little time and expense to prove it."[1]

Before long, Auzout had put this criticism in print, and members of the Royal Society took offence. The fourth issue of the *Philosophical Transactions of the Royal Society*, the journal edited and published by Henry Oldenburg, a German-born Fellow and one of two Secretaries of the Royal Society,[2] included a letter from Hooke to Oldenburg responding to Auzout's criticisms. Hooke defended his invention, but he was more vehement in defending the reputation of the Royal Society. Hooke wrote:

1 The original text, as quoted by McKeon from the Huygens Correspondence V 257, reads, "Je m'étonne fort que la Société qui veut être si sûre et si réservée ait laissé imprimer une machine sans l'avoir éprouvée puisqu'il faillait si peu de temps et de dépense pour l'éprouver" (Robert M. McKeon, "Établissement de l'astronomie de précision et œuvre d'Adrien Auzout" [PhD diss., University of Paris, 1965], 213. All translations from McKeon's dissertation, which was written in French, are my own).

2 While relevant biographical information about Oldenburg will be given throughout this chapter, valuable sources for further biographical information include Marie Boas Hall's biography of Oldenburg, the *Oldenburg Correspondence* which she edited along with A. Rupert Hall and Eberhard Reichmann (abbreviated "OC" throughout this thesis), Steven Shapin's essay review of the *Correspondence*, and Iordan Avramov's examination of Oldenburg's early career (Iordan Avramov, "An Apprenticeship in Scientific Communication: The Early Correspondence of Henry Oldenburg [1656-63]," *Notes and Records of the Royal Society of London* 53 [1999]: 187-201; Marie Boas Hall, *Henry Oldenburg: Shaping the Royal Society* [Oxford: Oxford University Press, 2002]; Steven Shapin, "O Henry," review of *The Correspondence of Henry Oldenburg*, eds. A. Rupert Hall and Marie Boas Hall, *Isis* 78 [1987]: 417-424).

> nothing surprised me so much, as, that he [Auzout] is pleased (after he had declared it a fault, to write this *Theory*, without having reduced it to practice) to lay it, as he seems to do, in one place of his book, *p.* 23 upon the *Royal Society*. Truly, *Sir*, I should think my self most injurious to the *Noble Company*, had I not endeavoured, even in the beginning of my Book, to prevent such a misconstruction. . . . [If Auzout had] read my *Dedication* to the *Royal Society* . . . he would have found, how careful I was, that the *Illustrious Society* should not be prejudiced by my *Errors*, that could be so little advantaged by my *Actions*.[3]

These texts sparked a conflict between Hooke and Auzout that dragged on for months. The most comprehensive historical analysis of Auzout and of his conflict with Hooke is found in Robert McKeon's 1965 PhD dissertation on the under-studied French natural philosopher.[4] McKeon found that the most active figure in causing and perpetuating this conflict was Oldenburg. McKeon succinctly explains, "One notices the important role of Oldenburg in this controversy. In effect it was he who aroused it". Oldenburg encouraged Hooke and Auzout to engage in a debate about grinding lenses "without offending or provoking each other".[5] It was Oldenburg who offered to serve as the intermediary between Hooke and Auzout as they exchanged arguments and counterarguments, as neither of them could read the language of the other.[6] Of course, as McKeon notes, Auzout had taken a controversial approach by criticizing not only Hooke, but also the Royal Society as a whole for allowing an unproven invention to be described in print.[7] Nevertheless, McKeon concludes that the primary catalyst for the dispute was neither Auzout nor Hooke, but Oldenburg.

Apart from it being a conflict between natural scientists, the Hooke-Auzout dispute is relevant to a study of priority disputes for several reasons. Firstly, it reveals the precarious nature of a new invention, and the lengths to which a natural philosopher might have to go in defending it. Secondly, it reveals the pride an inventor took in his

3 *Phil. Trans.* 4 (5 June 1665) 64-65.

4 McKeon describes the conflict on pp. 209-223 of his dissertation, which also examines a conflict between Auzout and Giuseppe Campani over whose telescope design was better. Additionally, N. S. Hetherington addresses a dispute between Auzout and Johannes Hevelius, a German member of the Royal Society, over their conflicting observations of a comet (N. S. Hetherington, "The Hevelius-Auzout Controversy," *Notes and Records of the Royal Society of London* 27 [1972]: 103-106; McKeon, "Adrien Auzout," 209-233. McKeon's dissertation is cited in full in n. 1 above).

5 The original text reads, "On constate le rôle important de Oldenburg dans cette controverse. Ce fut en effet lui qui la suscita" . . . "sans s'offenser ou se piquer" (McKeon, "Adrien Auzout," 216-217).

6 McKeon, "Adrien Auzout," 214-216.

7 McKeon, "Adrien Auzout," 217.

work and how defensive he could become about it. McKeon notes that Hooke's response to Auzout's criticisms highlighted the novelty of Hooke's method of grinding glasses.[8] Auzout threatened the credit that Hooke was due for this invention, not, in this case, by challenging Hooke's priority, but rather by doubting the quality of the invention.[9] Hooke was not willing to let Auzout belittle his contribution. Thirdly, this case shows the incentive an inventor or discoverer might have to delay announcing his invention or discovery until it had been perfected and its utility had been proven.[10] This delay, however, could be an inadvertent invitation to confusion about priority; an inventor or discoverer could find that, while he was fine-tuning, someone else made what the natural philosophical community would view as the same invention or discovery, announced it in print, and received credit for it. Finally, the Hooke-Auzout case shows that, although elsewhere Oldenburg denounced polemical disputes between natural philosophers, he could be an agent of conflict, and could openly endorse at least certain kinds of debate. As such, Oldenburg's *Philosophical Transactions* and his very extensive correspondence are logical sources on which to focus to understand priority disputes between the Royal Society and natural philosophers and mathematicians on the Continent.

Much of Oldenburg's correspondence in the late 1660s was concerned with building a network of contacts for the Royal Society by drawing on the community to which European intellectuals considered themselves to belong, the Republic of Letters. As John Marshall explains, natural philosophers both helped to form and benefitted from this imagined community. Indeed, Oldenburg was instrumental in encouraging the Republic to carry on a scientific dialogue through correspondence and journals.[11] Accordingly his correspondence is filled with entreaties to natural philosophers, in some cases ones whom he already knew and in other cases ones whom he had only heard of, to send him whatever natural knowledge they could as a contribution to the Royal Society and to put him in contact with other like-minded people. These correspondents included

8 McKeon, "Adrien Auzout," 222.

9 As will be discussed below, these two ways of challenging an invention or discovery were not always unrelated. See n. 93 below; Chapter 5, p. 168 below.

10 See Rob Iliffe, "'In the Warehouse': Privacy, Property and Priority in the Early Royal Society," *History of Science* 30 (1992): 38-39.

11 John Marshall, *John Locke, Toleration and Early Enlightenment Culture: Religious Intolerance and Arguments for Religious Toleration in Early Modern and 'Early Enlightenment' Europe* (Cambridge: Cambridge University Press, 2006), 501-535, especially 502-503.

European natural philosophers, whose efforts to establish their own scientific communities Oldenburg encouraged, as well as inhabitants of and visitors to English colonies who could send Oldenburg useful data from exotic locations.[12] Oldenburg used the growing reputation of the Royal Society to recruit these contacts and also used his letters to them to promote that reputation. He sent the *Philosophical Transactions* and Thomas Sprat's *History of the Royal Society* to many of his correspondents and received other fledgling scientific journals, primarily the French *Journal des sçavans*, in return.[13] The contents of the *Transactions*, as Adrian Johns notes, came to be measure of the Royal Society's success, and continental readers sought copies both to gauge the Society's productivity and to indicate their own membership in the Republic of Letters.[14]

12 It will suffice here to list a few examples of Oldenburg's efforts to build a wide geographic network. Henry Howard's letter of 3/13 July 1666 discussed Howard's efforts to put Oldenburg in touch with Petrus Lambecius, "ye learned bibliotecario at Vienna." Oldenburg appreciated Howard's efforts and encouraged him to continue helping the Society to "obtaine some Philosophicall Correspondents in ye chief Citty's of Italy, and particularly in Florence, Pisa, Bologna, Milan, Venice, Naples, Rome." While he used contacts like Howard as instruments in developing his network, Oldenburg did much of the work himself. He made his goal explicit in a letter of 23 July/2 August 1666 to the Polish astronomer Stanislas Lubienietzki: "You are well aware that the elements of human knowledge . . . are scattered and dispersed throughout the globe. I think that there is no better way of assembling them than by constantly bringing together the zealous and inventive in regular correspondence." He also asked Lubienietzki to report any scientific news he heard from Germany and Poland. Oldenburg did not only seek contacts who could inform him about Europe; he asked for contributions from Richard Norwood who had been to America and Bermuda, and from Sir George Oxenden, the governor of Bombay and the President of the East India Company (OC *Henry Howard to Oldenburg 3 July 1666* III 173, *Oldenburg to Lubienietzki 23 July 1666* III 192, *Oldenburg to Howard 27 July 1666* III 200, *Oldenburg to Norwood 24 October 1666* III 277, *Oldenburg to Sir George Oxenden* III 384-385, *Norwood to Oldenburg 18 June 1667* III 442-444).

13 A notable example of such an exchange is Oldenburg's correspondence with his French friend Henri Justel. In addition to political news that Oldenburg relayed to powerful English figures like Joseph Williamson, Justel provided Oldenburg with updates about the state of natural philosophy in France, before, during and after the formation of the Académie des Sciences in late 1666. Justel repeatedly asked Oldenburg to send him copies of the *Philosophical Transactions* and sent Oldenburg the *Journal des sçavans* in return. He also conveyed letters to Oldenburg from scholars like Auzout and Huygens. Auzout in particular was enthusiastic about the correspondence that he developed with Oldenburg, and repeatedly begged Oldenburg to send him issues of the *Transactions.* See, for example, OC *Justel to Oldenburg c. 14 January 1665/6* III 12, *Auzout to Justel 17 January 1665/6* III 27-28, *Auzout to Oldenburg 2 February 1665/6* III 38-39l *Auzout to Oldenburg 4 April 1666* III 84, *Auzout to Oldenburg 8 May 1666* III 115, *Justel to Oldenburg 16 May 1666* III 134, *Justel to Oldenburg 3 October 1666* III 241. In many cases Oldenburg assumed that the Society's reputation was enough to convince those who received his letters to contribute their knowledge. For example, in a letter introducing himself to Manfred Settala on 13/23 June 1667 he wrote, "I imagine that the repute of the Royal Society founded at London by the most august King of Great Britain has reached your native Italy". Oldenburg then attempted to use the Society's cachet to convince Settala to send him valuable philosophical information. In other cases he sent Sprat's *History* as a tool to explain the noble history and goals of the Society. For example, he sent a copy to Leopold de' Medici. Leopold was politically powerful as the brother of the Duke of Florence, and was also a patron, founder, and member of the Accademia del Cimento, Florence's scientific society (OC *Oldenburg to Manfred Settala 13 June 1667* III 440-441, *Oldenburg to Leopold de' Medici 26 November 1667* III 620-622).

14 Adrian Johns, *The Nature of the Book: Print and Knowledgeable in the Making* (Chicago: University

Oldenburg's work in exchanging information using his correspondence network and the *Philosophical Transactions* was innovative, but he benefitted from the examples of other communicators of natural knowledge within England and elsewhere in the Republic of Letters. Iordan Avramov argues that chief among these influences were his patron Robert Boyle and, especially, Samuel Hartlib.[15] Hartlib, whom contemporaries in Europe and North America knew as "the great intelligencer of Europe",[16] was the nucleus of what historians have called the "Hartlib circle". This group of scholars with both natural philosophical and theological interests was driven by an ideal of unity in both Protestant religion and natural knowledge. Accordingly, in the early and mid-seventeenth century, Hartlib and his circle became "key intellectual brokers" in support of his utilitarian ideal of shared knowledge, and believed that experimentation was an ideal source for the valuable information they wished to share.[17] The Hartlib circle had clear thoughts on intellectual property: they were anti-monopolists who viewed knowledge "as a commodity, publicly open yet privately owned." Knowledge could be owned and traded, but the system should be one of free trade that has been established for public benefit, rather than private.[18] This view of knowledge which the Hartlib circle professed in the 1640s and 1650s is reminiscent of the attitude that would be implicit in Oldenburg's actions in the 1660s: knowledge could be traded for mutual benefit, but it belonged to individuals. For Oldenburg and the rest of the Royal Society, it was those who generated knowledge who owned it, and while the Society encouraged such people to share their knowledge, it was of the utmost importance that they be recognized for their priority in producing it.[19]

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of Chicago Press, 1998), 514.

15 Avramov, "Apprenticeship in Scientific Communication," 187-194, especially 190. Cited in full in n. 2 above.

16 Mark Greengrass, Michael Leslie and Timothy Raylor, eds., *Samuel Hartlib and Universal Reformation: Studies in Intellectual Communication* (Cambridge: Cambridge University Press, 1994), 1, 16; G. H. Turnbull, "Some Correspondence of John Winthrop, Jr., and Samuel Hartlib," *Proceedings of the Massachusetts Historical Society*, Third Series, 72 (1957-1960): 36, n. 1.

17 Greengrass, Leslie and Raylor, *Samuel Hartlib and Universal Reformation*, 2-3, 13-22. The quotation is from p. 2.

18 Kevin Dunn, "Milton among the Monopolists: *Areopagitica*, Intellectual Property and the Hartlib Circle" in Greengrass, Leslie and Raylor, *Samuel Hartlib and Universal Reformation*, 178-183. The quotation is from p. 182. Hartlib imagined that towns could have an "Office of Address", a variation on the House of Salomon in Bacon's *New Atlantis* where an "aristocratic Brethren [would] distribute information to people and state alike, but according to their discretion" (Dunn, "Milton among the Monopolists," 186).

19 It has long been known that Hartlib was active in the group that met in Oxford in the 1640s and became

Avramov explains that Oldenburg's interest in natural philosophy was sparked by his meeting the likes of Boyle and Hartlib while visiting Interregnum England in the 1650s.[20] From them, especially Hartlib, Oldenburg learned about "merchandising in knowledge", which he began to do when he accompanied his pupil Richard Jones (the nephew of Boyle, Oldenburg's new patron) on a tour of the Continent in the late 1650s.[21] In his letters back to England, which were mostly written to Hartlib, Boyle, and Boyle's sister (and Jones's mother) Lady Ranelagh, Avramov identifies many of the skills Oldenburg would later employ in his extensive correspondence as Secretary of the Royal Society. For instance, Oldenburg learned how to describe the natural philosophical developments of the various scholarly groups he visited and communicated with, using a combination of direct quotations and his own comments. He also added to his fledgling correspondence network, seeking out competent natural philosophers to become his contacts so that they could provide him with useful information in the future. He was equally skillful in communicating with his English friends; he wrote to Boyle in a way that was sure to arouse his interest in the knowledge Oldenburg was gathering. By this point, Oldenburg was an apologist for natural philosophy and was honing his rhetorical skills by promoting this cause. According to Avramov, Oldenburg used a distinctly Hartlibian approach to the scientific news that he dispensed and collected: "the exchange of scientific news was conceived quite literally as a 'trade', where availability of some specific 'commodities' at the communicator's disposal was of utmost importance for business."[22] Oldenburg put these skills to use when he returned to England in 1660, and fulfilled a vital need in the new Royal Society as he invented "the new profession of

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the Royal Society, although Hartlib died in 1662 and thus had little opportunity to take part in the activities of the Society itself; G. H. Turnbull wrote a paper on this subject in 1953. In Turnbull's estimation, Hartlib's main role in this group was to transmit information to other members about each other's work and that of outsiders. For Hartlib the foundation of the Royal Society was an important step toward "universal reformation", the term he used to describe his goal of reforming not only religion, but the rest of society as well. As Greengrass, Leslie and Raylor, put it, universal reformation was a sort of societal "alkahest" (Greengrass, Leslie and Raylor, *Samuel Hartlib and Universal Reformation*, 14; G. H. Turnbull, "Samuel Harlib's Influence on the Early Royal Society," *Notes and Records of the Royal Society of London* 10 [1953]: 101-130, especially 129-130).

20 Avramov, "Apprenticeship in Scientific Communication," 187. Oldenburg's visits to England will be addressed below. Marie Boas Hall suggests that Hartlib instilled in Oldenburg the Baconian ideal of collecting a mass of natural and technological information from across the world (Boas Hall, *Henry Oldenburg*, 19).

21 Avramov, "Apprenticeship in Scientific Communication," 187.

22 Avramov, "Apprenticeship in Scientific Communication," 188-193. The quotation is from p. 194.

scientific administration."[23]

Avramov notes that Oldenburg's enthusiasm for reporting natural philosophical news increased when he arrived in Paris in 1659.[24] Perhaps while he was there he met some friends of another exemplar in intellectual communication who had died just over a decade earlier: the Minim friar Marin Mersenne. Mersenne, who was himself knowledgeable in music, philosophy and mechanics, was known for relaying information from a wide array of fields between intellectuals in France, England and Italy[25] and was a close friend of Descartes. As well as keeping Descartes involved in natural philosophy and frequently asking him natural philosophical questions in letters, Mersenne acted as Descartes' trusted ally at times when his priority was threatened. He communicated Descartes anger to Isaac Beeckman when Beeckman tried to take credit for Descartes' *Compendium musicae.* Furthermore, Mersenne was among the few with whom Descartes would discuss matters that he wished to keep secret before publishing them.[26] Mersenne was also among the French scholars who promoted the work of Galileo and supported him after his condemnation in 1633. In fact, Mersenne translated Galileo's *Two New Sciences* and *Mechanics* into French and had them published in France.[27] Historians have repeatedly compared Mersenne's activities as an intellectual intelligencer to those of Oldenburg. A. Rupert Hall and Marie Boas Hall note that both Mersenne and Oldenburg at times escalated conflicts between natural philosophers by relaying their arguments and counterarguments,[28] and elsewhere Boas Hall identifies Mersenne as a precursor to Oldenburg and to the Royal Society as a whole in both the collection and dissemination of natural knowledge.[29] For Boas Hall, the Society adopted Mersenne's role as an international trader in information "probably unconsciously", but no evidence precludes the possibility that Oldenburg found out about Mersenne's correspondence network,

23 Avramov, "Apprenticeship in Scientific Communication," 187.
24 Avramov, "Apprenticeship in Scientific Communication," 193.
25 A. Rupert Hall, "Concluding a Correspondence," review of *Correspondance du P. Marin Mersenne* vol. 15, ed. Cornelis de Waard and Armand Beaulieu, *Isis* 76 (1985): 77.
26 Geneviève Rodis-Lewis, *Descartes: His Life and Thought* trans. Jane Marie Todd (Ithaca: Cornell University Press, 1998), 83, 85-87, 98-99.
27 Frederic J. Baumgartner, "Galileo's French Correspondents," *Annals of Science* 45 (1988): 169, 177, 179.
28 A. Rupert Hall and Marie Boas Hall, "Why Blame Oldenburg?," *Isis* 53 (1962): 488.
29 Marie Boas Hall, "The Royal Society's Role in the Diffusion of Information in the Seventeenth Century," *Notes and Records of the Royal Society of London* 29 (1975): 173-192.

either through one of Mersenne's contacts in England or through someone Oldenburg met in France, and learned from him just as much as he did from Hartlib.

Another precedent in scholarly communication was Kenelm Digby, an English Catholic who lived in France from the mid-1630s to the end of the 1650s and transmitted information back and forth from England. As Jacqueline Stedall notes, Digby was a key figure in the exchange of mathematical news in this period. For instance, it was Digby who showed John Wallis's *Arithmetica Infinitorum* to Pierre de Fermat in 1656.[30] Before Digby moved to Paris where the religious climate suited him better, he spent much time at Gresham College in the 1630s. Digby had a strong sense of honour; he once killed a Frenchman in a duel who had insulted Charles II. The episode was recorded in *Sr. Kenelme Digbyes Honour Maintained* which, according to Michael Martin, earned him "notoriety and prestige (at least in England)".[31] Digby's interests spanned a range of theological, mathematical and natural philosophical issues, but his biographer R. T. Petersson considers Digby's greatest accomplishment to be that he took up a much needed task of connecting the work of geographically distant natural philosophers through his correspondence. Petersson explains, "Perhaps as well as any man of the period, either in England or out of it, Digby succeeded in understanding, organizing, and communicating the discoveries of science."[32] Digby met Mersenne and Descartes while on the Continent,[33] and had enough connections in England that he was on the initial list of men asked to join the Royal Society. Having returned to England by the time of the foundation of the Society, Digby became not only a Fellow but also one of the first twenty-one Council members along with Boyle, Oldenburg and others.[34]

Digby's natural philosophical contributions to the Society were rare and his involvement in general was very limited,[35] but perhaps he had something to teach

30 Jacqueline A. Stedall, "The Collaborations of Wallis and Brouncker II. Number Problems," *Notes and Records of the Royal Society of London* 54 (2000), 317.

31 Michael Martin, "Love's Alchemist: Science and Resurrection in the Writing of Sir Kenelm Digby," *Prose Studies* 32 (2010): 221-222. The quotation is from p. 222.

32 R. T. Petersson, *Sir Kenelm Digby: The Ornament of England, 1603-1665* (Cambridge, MA: Harvard University Press, 1956), 107, 119. The quotation is from p. 119.

33 Petersson, *Sir Kenelm Digby*, 123-128

34 Petersson, *Sir Kenelm Digby*, 294-295. The list of those who made up the rest of the first Council can be found in the Society's "Charta Prima" which was printed in the *Philosophical Transactions* in 1781. The list included Timothy Clarke and John Wallis, whose extensive involvement in priority disputes will be discussed below (*Phil. Trans.* 71.3 [1781] 3-4).

35 Petersson explains that Digby's "last and best scientific offering" was his *Discourse Concerning the*

Oldenburg with whom he shared not only an interest in natural philosophical communication, but also a history of diplomatic service. Digby, whose father was James I's chief foreign ambassador, accompanied his father and the king's son Charles to Spain where the prince courted Maria Anna, Infanta of Spain.[36] Similarly, Oldenburg, a native of the German free city of Bremen,[37] secured for himself the position of envoy to England and was sent to meet Oliver Cromwell.[38] Neither of them had much diplomatic training prior to these appointments,[39] but this was not altogether uncommon. Diplomacy was generally not a career in and of itself in the seventeenth-century. Ambassadors were chosen for factors like family connections and gentlemanly behaviour and learned diplomatic skills through hands-on experience, as many did during the Congress of Westphalia in 1648. As Anuschka Tischer explains, "a mission as envoy or ambassador was part of a broader political or administrator career."[40] Oldenburg, who acted as a sort of diplomat for the Royal Society but had many other responsibilities, constantly employed his diplomatic skills when writing to foreign correspondents as the Society's spokesman and when disseminating the natural knowledge of the Royal Society and its foreign contributors through the *Philosophical Transactions*.[41] This was by no means simply a matter of smoothing over conflicts; as I intend to demonstrate, Oldenburg was

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Vegetation of Plants (1661) in which he described his discovery that some substance in the air (which we now know to be carbon dioxide) sustains plants. This was Digby's only notable contribution as a Fellow of the Royal Society; he stopped attending meetings in 1664 and died in 1665 (Petersson, *Sir Kenelm Digby*, 296-301. The quotation is from p. 297).

36 Petersson, *Sir Kenelm Digby*, 57-65.

37 Bremen was a self-governing member of the Hanseatic League whose independence was reinforced by the Peace of Westphalia in 1648, although Sweden attempted to make Bremen submit to its rule in the 1650s (Boas Hall, *Henry Oldenburg*, 3, 12-13).

38 Boas Hall, *Henry Oldenburg*, 11. This was probably not Oldenburg's first visit to England. Marie Boas Hall has concluded based on comments in his letters and the quality of his English that he visited England in the 1640s and met pupils with whom he traveled around western Europe. This, as noted above, is precisely what he did in the late 1650s with Richard Jones, Boyle's nephew. In his travels, particularly in the Netherlands, Oldenburg met a number of English people who had been exiled during the Civil War and the Interregnum. After about a decade of work as a tutor, Oldenburg returned to Bremen in 1653. Months later, having been appointed as an envoy, he was back in England to meet with Cromwell to secure recognition of Bremen's neutrality in the Anglo-Dutch war, which Bremen's government felt had been threatened by the seizure of French ships carrying brandy and wine bound for Bremen (Boas Hall, *Henry Oldenburg*, 7-12).

39 In Oldenburg's case, a rival used this point to argue against his appointment as an envoy (Boas Hall, *Henry Oldenburg*, 11-12).

40 Anuschka Tischer, "Claude de Mesmes, Count d'Avaux (1595-1650): The Perfect Ambassador of the Early 17th Century," *International Negotiation* 13 (2008): 197-200. The quotation is from p. 197.

41 For the diplomatic tactics Oldenburg employed as editor of the *Transactions*, see Johns, *Nature of the Book*, 499.

very capable of doing so but he also had a talent for doing just the opposite when it suited the Society's needs. Oldenburg was a masterful negotiator in the trade of natural philosophical information. He knew the importance of maintaining friendly relationships with intellectuals on the Continent, but also knew, despite the rhetoric of the Republic of Letters, that this was not an end in itself. The information provided by foreign correspondents was valuable, but, for reasons that will become clear below, conflict was sometimes necessary and often useful.

As Liah Greenfeld argues, the promoters and researchers within the Society worked toward a common goal of justifying their natural philosophical program by linking it to the glory of the nation. Thus Boyle asked Oldenburg to translate English natural philosophical works into Latin so that foreign rivals would not make unauthorized translations and claim the contents of these works for themselves.[42] Apart from projects like this, Oldenburg's principal way of promoting the Society's cause was by gathering a mass of disparate natural philosophical and mathematical information from across Europe and beyond so that he could share it at Society meetings and through the *Transactions*. It seems that Oldenburg, who did not contribute experiments, inventions or discoveries of his own to the Society, viewed these tools as his personal ways to contribute to the improvement of natural knowledge and to the Society's role in that improvement. In this daunting task Oldenburg needed help; he told Boyle in a letter of 12/22 November 1667, "Any competent Assistance would, by Gods blessing, enable me to bring from all parts of ye world into England, as to a Center, whatever Ingenuities, Discoveries, Observations and Experiments lye scatter'd up and downe everywhere. But no man, yt I see, does effectually consider this, wch must needs overwhelme and oppresse [me]". When he found that his network was nevertheless proving useful, he proudly wrote to Boyle about its success: "I have lately received very many letters and papers, stored with philosophicall matters, from Anwerp [*sic*], Dantzick, Sueden, Poland, ye Bermudas, and Holland."[43] Evidently Oldenburg viewed his extensive letter-writing as one of his primary contributions to the Society, and he wrote letters vigorously as a "philosophicall

42 Liah Greenfeld, *Nationalism: Five Roads to Modernity* (Cambridge, MA: Harvard University Press, 1992), 81.
43 OC *Oldenburg to Boyle 12 November 1667* III 593, *Oldenburg to Boyle 3 December 1667* IV 6.

merchant"[44] on behalf of the Royal Society until his death in 1677.

Of course the letters that Oldenburg received were not for him alone. He frequently copied or described them in letters to his other contacts, and presented their contents at the meetings of the Royal Society. Indeed, one could view such letters as scribal publications. Harold Love explains that early modern publishing was not restricted to printing; it included any act of making a text public. The contents of a manuscript or letter could often be made public even if the writer wanted them to remain private; all it took to publish a letter was to make copies of it. For Love, scribal publication was a fundamentally "social activity", and Oldenburg engaged in this activity as a member of a social group.[45] In a sense, natural philosophers who wrote to Oldenburg were writing to the Royal Society.

Oldenburg's correspondence network frequently allowed the exchange of a currency of both information and honour. In this scheme, Johns argues, the *Transactions* and other Royal Society publications served as "diplomatic gifts, instruments of patronage, and status symbols as much as neutral conveyors of knowledge."[46] While it was not a perfect system – priority disputes show that it did not eliminate tension – this exchange helped to keep rivalries in check because it was mutually beneficial. Chief among the honours that the Royal Society could exchange for information was election to the Society as a Fellow. Oldenburg knew this honour was taken seriously and could facilitate ongoing dialogue with natural philosophers on the Continent. He wrote to Auzout on 24 May/3 June 1666 to inform him the Society had elected him as a Fellow. He told Auzout, "By this you see, Sir, that this company holds the opinion that political and national differences ought not to impede the philosophical exchange nor shut the door upon the appreciation of the sciences and of virtue." Oldenburg went on to write that the Royal Society hoped the French would soon form a scientific academy of their own so that the French and the English might inspire "other nations, however little civilized," to join them in "a philosophical alliance".[47] Auzout replied on 6/16 July 1666 to thank the

44 Avramov, "Apprenticeship in Scientific Communication," 187, 188, 190, 194, 198.
45 Harold Love, *Scribal Publication in Seventeenth-Century England* (Oxford: Oxford University Press, 1993), 36-43.
46 Johns, *Nature of the Book*, 541.
47 OC *Oldenburg to Auzout 24 May 1668* III 141-142.

Society for "the honor they have done me." He, too, realized that this was a chance for a valuable reciprocity of information. He wrote, "One of these days you will see our observations of the eclipse of the sun and you will do us the kindness to send us yours." He then made the friendly gesture of describing the additions that the French had made to Hooke's method of sounding depths and asked Oldenburg to tell Hooke about this "so that he can recommence his observations."[48] This is just one of many such exchanges that can be found in Oldenburg's correspondence; they show his skill in exchanging honours for information, and the willingness of his correspondents to take part in that exchange.[49]

Oldenburg was excited by the prospect of other scientific societies being founded throughout Europe, presumably because he hoped that such societies would foment natural philosophical research and thought from which the Society could eventually benefit.[50] Marshall notes that, for the participants in the Republic of Letters, "criticism

48 OC *Auzout to Oldenburg 6 July 1668* III 176-177.

49 Similarly, Oldenburg wrote to Ismael Boulliaud on 22 April/2 May 1667 to tell him that he had been unanimously elected to the Society whose members "believ[e] you to have the warmest possible fervor towards the advancement of their designs." Boulliaud replied on 6/16 May that he was extremely appreciative of this honour, "there being nothing that affects me more keenly than good repute and the approbation of those who are celebrated for their merits and virtues." The editors of the *Oldenburg Correspondence* note that Boulliaud also wrote a letter of thanks directly to the Royal Society, which is extant in manuscript form. On 25 March/4 April 1669, Oldenburg wrote to Marcello Malpighi to inform him of his election to the Society, but not without reminding Malpighi of his promise to continue to contribute to the Society. Samuel de Sorbière referred to his election as a Fellow as "an inestimable Honour" (OC *Oldenburg to Boulliaud 22 April 1667* III 399, *Boulliaud to Oldenburg 6 May 1667* III 410-411, *Oldenburg to Malpighi 25 March 1669* V 459; Royal Society MS. B 1, no. 123; Samuel de Sorbière, *A Voyage to England Concerning Many Things Related to the State of Learning, Religion, and Other Curiosities of that Kingdom* [London: J. Woodward, 1709], 49). As Greenfeld contends, foreign natural philosophers who praised the Royal Society like this "mixed" this praise "with a dose of envy"; certainly they admired the Royal Society, but they also seem to have wished in many cases that their own nation could keep up with England and the Society in natural philosophical production (Greenfeld, *Nationalism*, 83). Oldenburg himself received a similar honour when he was elected to the Académie des Sciences, and he thanked Pierre de Carcavy for it in a letter written on 2/12 January 1667/8. The two continued to exchange letters in early 1667/8 expressing their praise for each other and their desire to work together to foster the growth of European natural philosophy. Oldenburg also wrote to Boyle on 25 February/March 6 telling him of his plans to correspond and exchange information with Carcavy. Oldenburg, in short, was not the only person to use this tool to ensure the exchange of information (OC *Oldenburg to Carcavy 2 January 1667/8* IV 101, *Carcavy to Oldenburg 8 February 1667/8* IV 158, *Oldenburg to Carcavy 24 February 1667/*8 IV 200, *Oldenburg to Boyle 25 February 1667/8* IV 206-207).

50 Oldenburg hoped that the Society would serve as an exemplar to other European states with ambitions to form a scientific society. He wrote to Boyle in June 1666 about rumours he had heard about plans to found such societies in France and Denmark. (In France, these plans led to the formation of the Académie des Sciences.) Oldenburg hoped that "our Society will in time ferment all Europe, at least; I wish only, we had a litle more Zeale, and great deal more assistance, to doe our work thoroughly; as, I am apt to believe, the French will study to doe theirs (they being like to be endow'd) were it but out of Emulation." Similarly, Oldenburg indicated his optimism in January 1666/7 that "the societies newly established here, in France, and in Italy will serve as a stimulus, within a few years, to incite all the other nations of Europe to take up

and constructively critical conversation was pleasurable and exciting",[51] but there was always a fine line between friendly constructive criticism and defensive, polemical rivalry. Accordingly, Oldenburg had to manage conflict delicately and the exchange of information was useful in smoothing it over. Oldenburg was not the only one who understood this process. During the Hooke-Auzout dispute, Hooke communicated his ideas about grinding lenses to Auzout through Oldenburg. After describing his method, Hooke wrote, "This, when you have an opportunity, you may please to communicate, with my respects, to M. Auz[out]: and if he think fit to communicate his observations about the last two comets, I shall be very ready to return him an account of any thing of that kind, or of any other experiment which I have made."[52] Hooke, it seems, was trying to use this offer of information to keep the dispute from escalating. Auzout wrote back, "I wish I had something to communicate to Mr. Hooke now to requite him for his good will"; he had appreciated Hooke's gesture and wanted to reciprocate.[53]

As Secretary of the Royal Society and publisher of the *Philosophical Transactions*, Oldenburg had to maintain a balance. On one hand, he had to protect the Society's secrets so that members could proclaim their inventions and discoveries and bolster the Society's reputation; on the other, he had to share enough of the information and accolades at the Society's disposal to ensure that more information would flow in which would help the Society to make those discoveries and inventions. If the incoming information contained a threat to the honour of the Society, this balance was upset. For Oldenburg in particular, the reputation of the Society was not only a matter of pride; it was useful as an entry point into valuable scholarly correspondence. If one of these correspondents should threaten that reputation in a letter, it is no surprise that Oldenburg would "publish" the letter at a Royal Society meeting or via letters to some of the Fellows; nor is it surprising that he would cooperate in the Society's efforts to put that correspondent in his place. However, it will be demonstrated in this and the coming chapters that such rebukes had to be approached delicately and according to the code of conduct of the Republic of Letters, especially when the dispute concerned a high-profile

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the same studies" (OC *Oldenburg to Boyle 8 June 1666* III 155, *Oldenburg to Pierre de Carcavy* IV 101).

51 Marshall, "John Locke," 504.

52 OC *Hooke to Oldenburg February 1665/6* III 43.

53 OC *Auzout to Oldenburg 4 April 1665/6* III 84.

invention like blood transfusion.

The polemical texts of the Royal Society's priority disputes were by no means exclusively funnelled through Oldenburg's letters and the *Transactions*.[54] However, Oldenburg facilitated priority disputes through his correspondence network and his willing cooperation with English disputants (regardless of how much he denied the latter)[55] in a way that few of his contemporaries could do. Oldenburg's connections in England and abroad allowed him to participate in each of the types of acts of redefinition that disputants used as rhetorical strategies to make evidence favour their own claims to priority. Disputants redefined four things whose ambiguity and malleability could help their case: priority itself, the entity for which priority was being contested, the difference between invention and innovation, and the gentlemanly etiquette of the Republic of Letters. The priority disputes discussed in this thesis show how and why multiple discoveries were constructed and deconstructed; the participants in these disputes redefined both the entity that was discovered or invented and the criteria for making a discovery or invention, in a way that suited the needs of their claim of priority on behalf of themselves or their countrymen.

2.2: Oldenburg and Boyle on Priority

Some of the comments about priority from the 1660s that seem the most honest are those expressed by Oldenburg and Boyle during their correspondence while Boyle was in Oxford. Like Boyle, Oldenburg was always sensitive to matters of priority. As Rob Iliffe explains, Oldenburg was in a precarious position as the editor of the *Philosophical Transactions.* He was responsible for publicizing new inventions but he always needed to make sure he did not betray any of his many loyalties when he did so. This problem was amplified in priority matters that were not restricted to England; in such cases his loyalty

54 Alan Cook explains that while Oldenburg was instrumental in establishing the Society's network of foreign correspondence, other Fellows took an active interest in writing to contacts on the Continent or overseas and reporting what they had learned at the Society's meetings. It was often Oldenburg who put members of the Society in direct contact with these foreign contacts (Alan Cook, "Our Foreign Correspondence," *Notes and Records of the Royal Society of London* 53 [1999]: 179-182).

55 Steven Shapin explains that Oldenburg was very skilled at appearing impartial, which his role as international intelligencer and editor of the *Philosophical Transactions* required, but he had subtle ways of favouring his friends and allies (Shapin, "O Henry," 418, 424).

to England and to the Royal Society were at stake.[56] Oldenburg kept Boyle informed about the Society's activities and news he received from his contacts on the Continent, and, in addition to reports about his own experiments, Boyle responded with any concerns he had about what Oldenburg told him. In a letter from 17/27 March 1665/6 concerning Hooke's work on sounding depths, which he had described in the *Philosophical Transactions*, Oldenburg showed how he distinguished between an invention and an innovation. Oldenburg told Boyle that he did not credit Hooke with inventing this method of sounding depths, but rather with only

> a new contrivance of a way already started; since 'tis said, there, [in *Philosophical Transactions* 9] yt ye *following wayes were contrived by M. H.* [Hooke]; which cannot well be otherwise interpreted, then that ye wayes, as they follow, were contrived by him; not, yt he first invented ye notion of this practise: And he assured me, yt yt way of sounding wth a round leaden or stone ball, he borrowed from no Author.[57]

This distinction between invention and innovation, which he and other Society members would use about a year later in the transfusion dispute, was shifting and versatile; the decision where to draw the line between invention and innovation was subjective. In this letter to Boyle, Oldenburg made this distinction in a manner that would benefit Boyle's priority. He assured Boyle that "I was in my thoughts as far from derogating any thing from another, as any person alive, whatsoever, can be."[58]

Still, the *Philosophical Transactions* functioned as a record of Hooke's priority in making this innovation, and this was one of many times that Oldenburg would insist that the journal did so.[59] He wrote to Boyle on 8/18 June 1666 about Cassini's observation of "ye Conversion of Mars about his Axe" and remarked that "yesame was observ'd here in England yesame months, witnesse one of our Transactions, Num. 11; wch, I am now very glad, tooke timely notice of it in publick."[60] As far as Oldenburg was concerned, among the uses of the *Philosophical Transactions* was protection of priority, sometimes in

56 Iliffe, "In the Warehouse," 34-35.
57 Original emphasis. OC *Oldenburg to Boyle 17 March 1665/6* III 61.
58 OC *Oldenburg to Boyle 17 March 1665/6* III 61.
59 As Johns explains, contemporaries identified Oldenburg's role as the keeper of the register books, which were an official catalogue of the priority of Fellows and contributors for their inventions and discoveries, with his role as editor of the journal, which itself became a de facto register that was in fact more reliable than the register books (Johns, Nature of the Book, 501-504).
60 OC *Oldenburg to Boyle 8 June 1666* III 155.

defence of one Englishman from another, and often in defence of the English from a foreigner.

Like many Fellows and Royal Society contributors, Boyle trusted Oldenburg with knowledge of discoveries and theories that he wanted to keep secret. Oldenburg wrote to Boyle on 1/11 October 1667 to thank Boyle for entrusting him with several "Paradoxes" concerning fire, and he assured Boyle that he would conceal this information until Boyle thought fit to reveal it: "If they were not forbid to be made publick, they would certainly kindle many of ye Curiousest of Philosophers to try conclusions. But you may be sure, I shall keep them secret, as long I receive no leave to divulge them. The like you may be confident off [*sic*], whenever you shall please to impart [secrets] to me".[61] As Iliffe notes, natural philosophers and mathematicians were to be granted time to perfect their inventions and discoveries before publicizing them; otherwise, someone else could perfect the work and steal the honour and financial benefits due to the inventor. In many cases, the solution was to publish a vague description of the invention or discovery, so that one could protect his priority without revealing information clear enough for someone else to perfect his work. It became increasingly common, however, for Royal Society members and correspondents to send these discoveries in letters to Oldenburg who would record and date them in the Royal Society's register books, and keep them secret until the inventor or discoverer wanted to reveal them.[62] Clearly, Oldenburg had a great deal of responsibility regarding matters of priority.

Fittingly, it was to Oldenburg that Boyle complained when he felt his priority had not been respected. He wrote to Oldenburg on 17/27 October 1667 about an item printed in the latest *Philosophical Transactions* that discussed Carlo Fracassati's experiments on the injection of acids into the veins of animals.[63] Boyle had performed what he considered

61 OC *Oldenburg to Boyle 1 October 1666* III 503.

62 liffe, "In the Warehouse," 37-38; Giuliano Pancaldi, "Priority" in *The Oxford Companion to the History of Modern Science*, ed. J. L. Heilbron, et al. (Oxford: Oxford University Press, 2003), 676. Iliffe explains that the register books were intended as a method of eliminating ambiguous priority so that priority disputes could be avoided, but in this the registry was unsuccessful. For instance, Robert Hooke searched the register books for evidence of his invention of the balance-spring watch during his priority dispute with Christiaan Huygens, but found that Oldenburg had made little mention of it. He thus suspected that Oldenburg had consciously tried to undermine his invention. The register books were only useful in establishing priority inasmuch as one trusted Oldenburg, which Hooke certainly did not (Iliffe, "In the Warehouse," 37, 43, 46).

63 *Phil. Trans.* 27 (July-Sept. 1667) 492.

the same experiments, and had made them public, he contended, by announcing them at a meeting of the Royal Society.[64] Boyle did not accuse Fracassati of knowingly neglecting his priority, but rather suspected that "he may have had some imperfect Rumor of our Experiment without knowing whence it came, and so may, without any disingenuity have thence taken a hint to make and publish, what you have English'd for him" in the *Transactions.* Boyle encouraged Oldenburg to look through the register books for any mention of his experiments with injection, and promised to send proof in the event that Oldenburg did not find any.[65] Boyle gave permission for the Society to improve on his experiments once this proof had been found; yet Boyle, perhaps bearing in mind the confusion about Hooke's innovations on the method of sounding depths, suggested that Oldenburg keep track of what he initially contributed and what other members of the Society would subsequently add.[66] Boyle concluded by ensuring that Oldenburg would protect his priority in the future in order to avoid such confusion: "You will oblige me to put, from time to time, in a distinct sheet of paper, the dates of those letters of mine, wherein I mention to you my own experiments made or proposed."[67] Boyle left no doubt that his priority mattered to him, and that he considered its maintenance to be Oldenburg's responsibility.

Boyle's concern for priority did not, however, translate into enthusiasm for defending priority in disputes. In his letter of 26 October/5 November, Boyle maintained a diplomatic attitude toward Fracassati: "I am still of the opinion, that Signior *Fracassati* be tenderly dealt with." Nevertheless, he insisted that Oldenburg endorse his letters upon receiving them in case Boyle forgot to date them. Boyle's reason for this was twofold: "because frequent experience shows us how much our *English* have lost, for want of being so [careful to protect their priority]; and (which is more considerable) how difficult it is to avoid the occasions of personal disputes or reflections, which, for my part, I

64 Boyle claimed repeatedly that the meetings of the Royal Society were public, although opponents like Thomas Hobbes pointed out the Society excluded most people from its meetings.. Shapin and Schaffer refer to the places where the Society met as "restricted public place[s]" (Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* [Princeton:Princeton University Press, 1985], 37, 57, 69, 78. The quotation is from p. 39).

65 OC *Boyle to Oldenburg 17 October 1667* III 532-533.

66 OC *Boyle to Oldenburg 17 October 1667* III 533.

67 OC *Boyle to Oldenburg 17 October 1667* III 534. For more on Oldenburg's efforts to protect Boyle's priority, see Johns, *Nature of the Book*, 508-510.

heartily desire to shun."[68] Boyle insisted that the primary goal of keeping clear records of priority was to avoid disputes; Shapin and Schaffer explain that unity was always among Boyle's chief concerns, and that he strove to eliminate anyone or anything that he considered divisive from the Royal Society.[69] Here, Boyle revealed the balancing act that his approach to matters of priority required: one had to set these matters straight so that everyone received the credit he deserved, but one also had to avoid outright conflict in doing so. Just over two months later, he wrote to Oldenburg about a possible case of plagiarism: someone had apparently printed two of Boyle's books in Holland without giving him credit. He asked Oldenburg to use his continental connections to find out whether this was true, and, if so, to make it known that Boyle had published both of these works in 1661 "because that if they should be supposed to be published but this present year, I may be suspected to have taken divers things from others, which they indeed borrowed from me." Here, Boyle expressed his anxiety about not being recognized for his priority, but he framed this concern in such a way to suggest that his main concern was being protected from charges of plagiarism himself.[70] Boyle, in short, was masterful in ensuring the protection of his priority without provoking a dispute.

Through an analysis of priority disputes involving the Royal Society in the 1660s, it will become clear that Boyle's strategy of valuing priority but being disinclined to defend it in a dispute was an influential, if imperfectly followed, exemplar. Boyle was, after all, considered the society's most credible witness to scientific experiments; he developed an identity as a "Christian Virtuoso" by fusing Christian, scholarly and gentlemanly qualities including an unfailing dedication to the truth.[71] Even those Fellows

68 OC *Boyle to Oldenburg 26 October 1667* III 540.

69 See Shapin and Schaffer, *Leviathan and the Air-Pump*, especially 71-72.

70 WB *Boyle to Oldenburg 29 December 1667* VI 71. These examples should be sufficient to illustrate the nature of Oldenburg and Boyle's discussions concerning priority, but the Oldenburg correspondence is peppered with further examples until Boyle's return to London in the spring of 1668. See, for example, Oldenburg's letter to Boyle on 3/13 December 1667, in which the two negotiated the extent to which Oldenburg was free to reveal the results of Boyle's experiments to the Royal Society (OC *Oldenburg to Boyle 3 December 1667* IV 5-6).

71 Shapin explains Boyle's importance in establishing a standard by which a natural philosopher could be measured: "Boyle did not *take on* the identity of experimental philosopher, he was a major force in *making* that identity. . . . if one wished to be an experimental philosopher, then the identity of Robert Boyle offered a paradigm of the new role." Boyle projected a decidedly "gentlemanly identity." Since Boyle played this role, Shapin explains, his honour was useful to legitimize his own actions and those of other Englishmen. He founded this honour upon his noble birth, being the son of an earl, but also upon gentlemanly qualities like integrity and Christian virtue. Boyle actively encouraged others to adopt this lifestyle. Many of Boyle's

who were involved in the most polemical disputes, perhaps consciously modelling themselves after the singularly influential Boyle, prefaced the evidence for their priority with a number of excuses for their supposedly reluctant involvement in the dispute. These excuses frequently included loyalty to respected natural philosophers and a belief in the nobility of the truth. In some cases such excuses seem to have been an afterthought, but the more skilled and experienced rhetoricians made cogent arguments protecting themselves against the charge of seeking the glory of discovery at the expense of unity. This is not to say that these rhetoricians were not as concerned about priority as others; they were simply better at hiding this concern. Whereas Boyle seems genuinely to have wanted to avoid outright dispute while he defended his priority, others exploited his example to adopt a defensible position while protecting their priority with vicious polemics.

In a way, this approach to the actions of natural philosophers in priority disputes resembles Merton's classic explanation of priority disputes. Merton writes that they result from a conflict between two institutional features of science: the norms of originality and humility, both of which Merton traces back to the time of Galileo.[72] A natural philosopher or scientist might do nearly anything, including in some cases falsifying evidence, to support his or her claim to originality in spite of the norm of humility which is supposed to preserve science's "commitment to the disinterested pursuit of truth."[73] If other practitioners perceive that someone has breeched the norm of humility in this way, thereby failing adequately to respect one of their peers, they will come to their peer's defence and a priority dispute will ensue.[74] I argue that, while both of these ideals were ostensibly present and in conflict in early modern priority disputes, the conflict between them did not cause the disputes. Rather, a desire for the honour of priority (along with the

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contemporaries who were involved in priority disputes seem to have tried to portray themselves as having these same qualities in order to counterbalance their passionate polemics (Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England* [Chicago: University of Chicago Press, 1994), xxviii, 127, 130-131, 156, 160-161, 166-167]). See also the second chapter of *Leviathan and the Air-Pump* for Shapin and Schaffer's analysis of how Boyle established the importance of witnessing in experimental philosophy and made himself the ideal witness (Shapin and Schaffer, *Leviathan and the Air-Pump*, 23-79).

72 Robert K. Merton, "Priorities in Scientific Discovery: A Chapter in the Sociology of Science," *American Sociological Review* 22 (1957): 635, 639, 646.

73 Merton, "Priorities in Scientific Discovery," 653-654, 658.

74 Merton, "Priorities in Scientific Discovery," 649

closely related goal of self-fashioning) was a common cause of priority disputes, and the norm of humility was an obstacle to one's performance in them; one had to be sure neither to ignore that norm nor to let it overwhelm his argument. A disputant could overcome this obstacle with language that suggested that he was chiefly motivated by the noble goal of finding the truth. Rather than being guided in part by humility, disputants seem to have feigned it. Merton identifies the conflict between the two norms in early modern priority disputes in an effort to find precursors for his assessment of priority disputes in his own time. Yet the actions of disputants in the seventeenth century suggest that humility was institutionalized in science to a far lesser degree in that period than in the mid-twentieth century. Accordingly, Merton's analysis functions much better as a sociological analysis of scientists of his own time than as an historical analysis of early modern natural philosophers.[75]

Merton claims that egotism is not a sufficient explanation for priority disputes because the disputants are often mild-mannered outside of priority disputes and do not engage in conflict otherwise; furthermore, the most ardent participants in these disputes are often not the discoverers and inventors themselves, but rather their supporters who "stand to gain little or nothing" by defending someone else's priority.[76] This explanation is not tenable regarding the early modern period for several reasons. Firstly, a natural philosopher could gain considerable social credit from coming to the defence of a prominent person, especially if that person's friendship could advance one's career. This is what Nick Jardine suggests was Kepler's motivation when he defended Tycho Brahe against Nicolaus Ursus during their priority dispute over the invention of geoheliocentrism. For Kepler, this was an opportunity for self-fashioning and for saving face with Tycho who had been offended because he thought that Kepler had written a

75 Merton also claims that scientists were forced to violate the institutionalized norm of humility by arguing for their own priority so that their originality would be recognized; consequently scientists "come to despise themselves for wanting that which the institutional values of science have led them to want." Not only is this hypothesis much easier to test with present-day scientists than with early modern natural philosophers, it also fits better with the modern material-based reward system that, as Merton himself notes, has largely supplanted the early modern reward system that was chiefly based on honour. While the production of original work was central to a natural philosopher or mathematician's identity in the seventeenth century, it was still only one of many sources of honour, which itself was only one factor contributing to material wealth (Merton, "Priorities in Scientific Discovery," 642, 647).

76 Merton, "Priorities in Scientific Discovery," 638-639.

letter in support of Ursus.[77] Secondly, while viewing priority disputes as simply a consequence of egotism would be an oversimplification, many seventeenth-century figures were involved in not one but several protracted priority disputes throughout their lives. Furthermore, Merton neglects the fact that, while these disputes typically began as contests over priority, they quickly became more generalized disputes in which the participants argued over who had made the best observations or collected the best data, whose explanations of these data was the most logical, and who had unjustly insulted whom so far in the dispute. If many natural philosophers spent years of their life engaged in such multi-faceted debates, one wonders whether they can be considered "men of ordinarily modest disposition";[78] at what point does it become evident that a natural philosopher had no real aversion to conflict?

2.3: The Clarke-De Graaf Dispute

Certainly Oldenburg showed little aversion to conflict when he served as the intermediary between the English physician Timothy Clarke[79] and the younger Dutch anatomist Regnier De Graaf during their priority dispute beginning in 1668, in which Oldenburg did everything he could to help Clarke make his case.[80] This dispute reveals both Oldenburg's

77 N. 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), 15-20.

78 Merton, "Priorities in Scientific Discovery," 638.

79 Clarke was a member of the Royal Society since its beginning and sat on its first Council. A Royalist during the Civil War and Interregnum, he served as a Royal Physician to Charles II after the Restoration and was well liked by the King (Gordon Goodwin, "Clarke, Timothy [*d.* 1672]", rev. Michael Bevan in ODNB, accessed 1 July 2011).

80 Although it has not received as much historiographical attention as cases such as the blood transfusion dispute, this dispute and the rest of De Graaf's career have been discussed in several articles by writers in medical and scientific professions. For example, an article by Clark T. Sawin gives an overview of the Clarke-De Graaf dispute, as well as biographical details about De Graaf. Born in Schoonhoven in 1641, De Graaf studied medicine at Utrecht and Leiden in the early 1660s. He was thus still a young anatomist trying to make a name for himself when he ran afoul of Timothy Clarke. However, he had by then published his *Disputatio medica de natura et usu succi pancreatici*, a notable work with original observations of the pancreas. He then spent much of 1665 working, studying and making contacts in France before returning to the Netherlands where he both practiced medicine and continued to publish about his research. Venita Jay adds that De Graaf was refused a professorship at Leiden because he was Catholic. Sawin explains that De Graaf began to study the testis, the subject of his dispute with Clarke, around 1665 (Venita Jay, "A Portrait in History: The Legacy of Reinier De Graaf," *Archives of Pathology & Laboratory Medicine* 124 [2000] 1115; Clark T. Sawin, "Historical Note: Regnier De Graaf and the Graafian Follicle," *The Endocrinologist* 7 [1997]: 415-417, especially 415. See also W. M. Ankum, H. L. Houtzager and O. P. Bleker, "Reinier De Graaf [1641-1674] and the Fallopian Tube," *Human Reproduction Update* 2 (1996): 365-369; Irvin Modlin,

behind-the-scenes activities and his public participation in the Society's priority disputes. Oldenburg's role in this dispute was mainly that of a facilitator. He used his communicative tools, his correspondence network and the *Philosophical Transactions*, to foster the dispute between Clarke and De Graaf. Yet Oldenburg also showed that he was willing to stimulate the continuation of the argument with his own occasional comments. Another skill he employed in this case was his knowledge of when to draw a priority dispute to a close; after over a year of Clarke and De Graaf exchanging polemical letters through Oldenburg, the Secretary, perhaps recognizing that no English claim to priority would withstand much scrutiny, helped Clarke to end the dispute when De Graaf had been sufficiently humbled.

The dispute began when Clarke denied that De Graaf was the first to suggest that the testes were composed of nothing but small vessels in his *Tractatus de virorum organis generationi inservientibus*. After detailing how other anatomists had described the substance of the testes, De Graaf declared boldly, "Certainly they ought to have waited longer before claiming that they knew the truth. I hope that they will pardon us for saying that no one has yet elucidated in writing the real substance of the testicles, or even indeed touched the shadow of what is really the case." De Graaf explained that those who claimed that the testes were glands or something similar to marrow were mistaken; he had observed that they were "simply a collection of minute vessels or tubules".[81] De Graaf's claim of priority for this discovery was unambiguous.

On 10/20 July 1668, De Graaf wrote to Oldenburg, having heard of his "very great courtesy towards foreigners", and asked for his opinion of the *Tractatus*.[82] De Graaf

"Regnier De Graaf: Paris, Purging, and the Pancreas," *Journal of Clinical Gastroenterology* 30 [2000]: 109-113).

81 Regnier De Graaf, "Tractatus de virorum organis generationi inservientibus" in *Regnier De Graaf on the Human Reproductive Organs: An Annotated Translation of* Tractatus de virorum organis generationi inservientibus *(1668) and* De Mulierum organis generationi inservientibus tractatus novus *(1672)*, trans. H. D. Jocelyn and B. P. Setchell (Oxford: Blackwell Scientific Publications, 1972), 23-24.

82 OC *De Graaf to Oldenburg 10 July 1668* IV 524. Only five months earlier, De Graaf was involved in another case in which multiple discovery was identified, but in which a dispute was deftly avoided. The *Philosophical Transactions* of April reviewed De Graaf's *Epistola de nonnullis circa partes genitales inventis novis* alongside a similar treatise written by Johannes van Horne entitled *Observationum suarum circa partes genitales in utroque sexu prodromus.* The review states, "It seemes, that the two Authors in these two Papers have met with almost the very same Observations; which they account New, about the *Genitals* in both Sexes". De Graaf's treatise was published first and, when van Horne saw it, he "thought himself obliged . . . to declare in this his *Prodromus*, that, though he knowes not, whether the Observations of [De Graaf] be *altogether* the same with his, yet, to avoid dispute hereafter, he thought fit, in this *Paper* of

was not pleased to find that Clarke had claimed that De Graaf's observations had been anticipated by several anatomists: in addition to giving credit to Johann van Horne as De Graaf's collaborator in observing that the testes were composed of vessels, Clarke explained that "this was not only known to us long ago, but even before that to the famous [Jean] Riolan and others."[83] De Graaf wrote to Oldenburg on 25 September/10 October, "I am indeed astonished that the learned and inquisitive men of all England should have disposed so freely of my discoveries, assigning to this person and to that the renown of this or that discovery, which I had boldly attributed to myself, especially as I could find nothing similar in any other anatomist." He claimed that a number of French and German scholars had already confirmed that his discovery was original.[84] Evidently, De Graaf saw this discovery as an opportunity to gain honour and advance his name in the network of natural philosophers connected through the Republic of Letters. He had already received a number of compliments for his work from these French and German men and now sought additional recognition from the Royal Society. Whether or not De Graaf intended, as he claimed, to make his observations of the testes "for the advancement of the science of anatomy", he reacted in this letter as if he had been personally attacked and his work "set at naught" by the Royal Society.[85]

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his, to represent the short of his own un-borrowed Observations, concerning that Subject, till he should be able to publish a *full* History of the structure of those parts." Van Horne thus recognized that the overlap was coincidental and took steps to avoid charges of plagiarism in the future. Apparently van Horne was right to suspect that these observations could lead to a priority dispute, because similar observations became the subject of De Graaf's dispute with Clarke later that year (*Phil. Trans.* 34 [13 April 1668] 663-664). For more on De Graaf's troubled relationship with van Horne, see n. 138 below.

83 OC *Clarke to Oldenburg April/May 1668* IV 367; *Phil. Trans.* 35 (18 May 1668) 672-682. This use of the word "others" seems deliberate; Clarke was undermining De Graaf's priority with a reference to Riolan, but not giving Riolan, who was French, complete credit either. Clarke remained vague during this dispute about who deserved priority instead of De Graaf, perhaps because he realized that there was no evidence of an Englishman having viewed the vessels in the testes that predated Riolan's *Anthropographia* of 1618. Clarke made repeated references to English anatomists having observed the threads that make up the testes – usually the ones he named were Francis Glisson and Thomas Wharton – but at the same time he cited examples of French and Italians making similar observations. It seems that for Clarke this dispute was more about taking priority away from De Graaf, which he could do with reasonable certainty, than about providing proof for English priority, which was much less certain. Still, Clarke was sure to pepper the letters he wrote in this dispute with references to English anatomists observing the vessels in the testes so that readers would associate the English with priority for this discovery without Clarke having to claim it outright. (See OC V *Clarke to Oldenburg 20 December 1668* 270).

84 OC *De Graaf to Oldenburg 25 September 1668* V 69.

85 De Graaf, who evidently did not know that Oldenburg was originally German, lumped Oldenburg in with the English as he framed the dispute in nationalist terms (OC *De Graaf to Oldenburg 25 September 1668* V 69).

De Graaf suggested that, to liken Riolan's observations to his own, Clarke had "twisted" Riolan's words "like a waxen nose". He also claimed, however, that there would not have been any confusion if "Riolan had given a sufficiently clear description of the substance of the testes."[86] De Graaf's defence of his priority, then, rested not on denying that Riolan had made the same observations as him, but rather on claiming that Riolan had not expressed his findings clearly enough for others to be certain that he deserved priority for discovering the composition of the testes. De Graaf, as if recognizing that the identification of multiple discoveries is social and somewhat arbitrary, noted that precursors to any natural philosophical work could be found if one looked hard enough; he provocatively suggested, for example, that one could find what sounds like William Harvey's notion of circulation in Hippocrates.[87] De Graaf adopted the position that a discovery was not officially made until it had been explicitly stated. This definition of priority was not compatible with some of the methods that De Graaf's contemporaries used to establish and defend priority – such as hinting at a discovery in a publication, or encoding it in an anagram and revealing the solution later – and it seems to have been designed expressly to support De Graaf's claim. As was often the case, the one claiming priority defined it in a way that suited his needs. Nevertheless, De Graaf knew better than to make this remark about Harvey in a way that would challenge Harvey's honour:

> I do not say this in order to detract in the slightest form the *glory of discovery* belonging to these men [Harvey and Jean Pecquet], who are worthy of the highest praise, for without their extreme diligence in dissecting bodies these remarkable discoveries would perhaps still lie hid in deepest darkness, for no one would have interpreted those passages in Hippocrates in that sense . . . I only mean to say the same of Riolan's words in the present context, and the more so as he did not put forward the true substance of the testes.[88]

De Graaf then reiterated his belief that being the first to make a discovery was not the only factor in deciding who deserved credit for it: "I am glad that like myself your countrymen discovered the communication between the *vasa deferentia* and the seminal vessels, but who depicted it best will be seen when the seminal vessels which the learned

86 OC *De Graaf to Oldenburg 25 September 1668* V 69.
87 OC *De Graaf to Oldenburg 25 September 1668* V 69.
88 My emphasis (OC *De Graaf to Oldenburg 25 September 1668* V 69-70).

Mr. Clarke has kept by him dried and inflated are opened."[89] De Graaf concluded in a rather accusatory tone toward not only Clarke but also toward English natural philosophers in general, and made it clear that what was at stake for him here was the honour of the discovery: "When all these matters are well considered I feel sure that your countrymen will allow to other nations as much renown for their discoveries as they in turn desire to receive from others for their own."[90]

These remarks succeeded in inciting the anger of the Royal Society, especially Clarke. His reply, through Oldenburg, of 20/30 December 1668 first took issue with the young De Graaf's excessive enthusiasm for seeking the honour of discovery; De Graaf's letter was "too filled with that petty ambition and youthful heat which renders young men obnoxious because the desire for honor makes them intolerant of small achievements."[91] Of course, seeking credit for the discovery was exactly what Clarke did more than once on behalf of the English. In this case he set out his own criteria for assigning credit for a discovery, with an emphasis on novelty. According to Clarke, one must "first diligently and carefully consider whether what is claimed as a discovery is really new, and then philosophize freely upon the usefulness of that invention".[92] For Clarke, originality was as significant a factor as utility in determining the worth of a natural philosopher's contribution. As was typical, the challenge to De Graaf's priority ran parallel to a critique of the quality of his work.[93] Clarke suggested that the Royal Society was pursuing the

89 OC *De Graaf to Oldenburg 25 September 1668* V 70.
90 OC *De Graaf to Oldenburg 25 September 1668* V 70.
91 OC *Clarke to Oldenburg 20 December 1668* V 270.
92 OC *Clarke to Oldenburg 20 December 1668* V 270.
93 We again see an attempt to use the poor quality of an opponent's work as a way to weaken his priority claim in the Newton-Hooke dispute regarding the discovery of universal gravitation. In a letter to Edmund Halley of 1686, Newton denied Hooke's accusation that Newton had taken the inverse-square law from Hooke without acknowledging him. Newton responded to the suggestion that he should acknowledge Hooke in the *Principia* with a claim that everything that Hooke had sent him about laws of planetary motion was either false or unoriginal: "why should I record a man for an Invention who founds his claim upon an error therein & on that score gives me trouble? He imagins he obliged me by telling me his Theory, but I thought my self disobliged by being upon his own mistake corrected magisterially & taught a Theory wch every body knew & I had a truer notion of then [*sic*] himself." Newton agreed with "Hevelius and others" that Hooke was "a man of a strange unsociable temper" (NC *Newton to Halley* II 437-439. The quotations are from p. 439). The link between what is considered insufficiently original and what is considered bad is also found in the realm of Renaissance literature. As some mid-seventeenth-century Dutch writers indicated, *imitatio* was an important feature of literature and proved one's wit when he performed it correctly, but this required him to imitate with creativity and subtlety; obvious and "servile" imitation was a sign of bad writing. On the other hand, other Dutch writers pointed out that obviousness of an imitation could also be a sign of literary skill. For instance, it could signal a likeness between the subject matter of the text that imitates and the text that is imitated. A sufficiently educated reader should notice the

truth of the matter rather than the honour of the discovery, and thus would respect De Graaf's achievement if he turned out to be right about the substance of the testes. Clarke could not refrain from belittling De Graaf as he made this point:

> since he [De Graaf] seeks so avidly to gain the glory of discovery you will take care to inform him that as soon as it is evident to us that the whole substance of the testicles of any animal at all is nothing but a mass of very minute vessels for preparing the semen we will assign the glory of this discovery to him alone; even though we may know how different the discoveries of intellect are from those of good fortune.[94]

Clarke's point was that, if De Graaf was right, he would merely have gotten lucky. In light of these polemics, Clarke's concluding promise that "we English will honor him with due zeal and concern" seems to be little more than lip service.[95]

Clarke's letter insisted on his and on the Society's respect for priority. According to Clarke the Fellows were generally "noble philosophers suffering from neither jealousy nor vice who gladly assign to each man his own".[96] Clarke suggested that those who strove to be recognized for priority with as much enthusiasm as De Graaf did so at the expense of the truth and would steal priority that belonged to others; this implied that one who took on the Royal Society in a priority dispute was acting out of jealousy. This is how Clarke manifested the conflict between the compulsion to defend one's priority and the pressure not to dispute priority; he turned the subject of the dispute from priority itself to *respect* for priority. Clarke was appealing here to what Steven Shapin identifies as the paradigm of the gentleman in his *Social History of Truth.* A gentleman had "no inducement to misrepresent the case, no forces working on him that would shift his utterances out of correspondence with reality." He was thus able to speak with authority and as a credible witness. It is this paradigm that lets Shapin describe the establishment of scientific truth in early modern England as a social practice within an honour culture.[97] Clarke's letter projected the intimidating notion that to engage in a priority dispute with

imitation and appreciate the comparison. In short, in the Netherlands in the seventeenth century, writers differed regarding how a text of high quality struck a balance between imitation and originality (Ronny Spaans, "*Imitatio* and Plagiarism in the Dutch Renaissance: An Interpretation of Joannes Six van Chandelier's 'Teegen.'t lasterschrift op Gerrit Brand" in *Borrwed Feathers: Plagiarism and the Limits of Imitation in Early Modern Europe*, ed. Hall Bjørnstad [Oslo: Unipub, 2008]: 203-206).

94 OC *Clarke to Oldenburg 20 December 1668* V 271.

95 OC *Clarke to Oldenburg 20 December 1668* V 271.

96 OC *Clarke to Oldenburg 20 December 1668* V 270.

97 Shapin, *Social History of Truth*, xxvi-xxvii.

members of the Royal Society – that is, to question whether the priority they assigned was truthful – was to threaten their honour as gentlemen.[98] He defined the etiquette of the Republic of Letters in a manner that protected English priority interests. Clarke asserted that some English natural philosophers had known that the testes were composed of "nothing but masses of vessels" twenty-five years earlier, and he insisted that De Graaf only denied this because "no one gladly listens to or sees what may deprive him of any part of the glory of his discovery".[99] Having made this accusation soon after claiming that the Royal Society had a deep concern for assigning priority correctly, Clarke put De Graaf in an awkward position: if he continued to argue that his discovery was new, he would be taking on the Royal Society as a whole and challenging the gentlemanly status of its members.

De Graaf's reply through Oldenburg of 12/22 February 1668/9 was less accusatory than his previous letter. He insisted that his intention was to set the record straight rather than "to speak ill of your illustrious Society out of a desire for premature distinction."[100] Yet De Graaf was not willing to concede to the Society. Continuing to consider the establishment of priority to be a social activity, he maintained that it must be done publicly and clearly. Clarke had proven that English anatomists had observed the composition of the testes with evidence from the records of the Royal Society: evidence, in other words, that was not public. As further proof, De Graaf closely examined each point of evidence that Clarke had raised and highlighted the details that distinguished his own work from that of the anatomists who supposedly preceded his discovery.[101] As

98 In his discussion of perceptions of plagiarism in the second half of the eighteenth century, Richard Terry explains that in the 1750s an accusation of plagiarism was as serious an insult as plagiarism itself. He accredits this in part to an episode in which William Lauder's accusation that John Milton had been a plagiarist turned out to be a fraudulent attempt to slander Milton. For a brief period after Lauder's fabrication, to charge someone with plagiarism was to associate oneself with Lauder's deception, so charges of plagiarism were perceived as a sign of disrespect and were met with outrage. What Terry illustrates is the notion that, in the right context, a disputant could deflect a complaint that he was depriving his opponent of due credit by portraying that complaint as slanderous. This may have been what Clarke was trying to do in setting De Graaf's priority claim in opposition to the honesty of the Royal Society; perhaps he hoped that the reputation of the Society would cast doubt on the character of anyone who challenged its authority (Richard Terry, "'In Pleasing Memory of All He Stole': Plagiarism and Literary Detraction, 1747-1785" in *Plagiarism in Early Modern England*, ed. Paulina Kewes [New York: Palgrave Macmillan, 2003], 183-194).

99 OC *Clarke to Oldenburg 20 December 1668* V 270.

100 OC *De Graaf to Oldenburg 12 February 1668/9* V 398.

101 OC *De Graaf to Oldenburg 12 February 1668/9* V 398-399.

Jardine shows in his analysis of the priority dispute between Tycho and Ursus over the geoheliocentric model of the universe, early modern natural philosophers could claim priority not only for an entire discovery, invention, or theory as a present-day observer would conceive of them; early modern priority disputes can be "seen in terms of isolated items", and a disputant could contest the priority of each of these.[102] As such, De Graaf continued to build his case on these minor differences and, crucially, dismissed the similarities by pointing out that they did not lead these other anatomists explicitly to conclude what De Graaf had concluded: that the the testes consist *only* of tiny vessels. In De Graaf's view, a natural philosopher could claim priority only if he expressed his point clearly.[103] Oldenburg had reproduced a 1658 broadsheet entitled *Testis examinatus*, written by Claude Aubery, in the *Philosophical Transactions* of December 1668.[104] The broadsheet included diagrams (Figure 2.1) and explained that "the entire testis is the entire vessel."[105] Still, De Graaf maintained that, if the members of the Royal Society were to compare his text with Aubery's, "they will readily learn how much more and how much more clearly I have addressed the learned world on the subject of the testes than he did."[106]

Priority disputes sometimes overlapped with contests over the right to interpret the results of an observation or experiment, as the opinions of the inventor or discoverer himself were seen as the most credible, and his data the most reliable.[107] In this case, the

102 Jardine, *Birth of History and Philosophy of Science*, 33-34.

103 It is worth noting the similarity between De Graaf's position and the style that Shapin and Schaffer identify in Boyle's writing in *Leviathan and the Air-Pump*. To describe experiments, Boyle adopted (and encouraged the rest of the Royal Society to adopt) a "plain, ascetic, unadorned (yet convoluted) style [that] was identified as functional." De Graaf may have insisted on the clarity in his writing style in an effort to appeal to the conventions that the Royal Society had adopted (Shapin and Schaffer, *Leviathan and the Air-Pump*, 66-67).

104 Oldenburg also took this opportunity to show that he had sided with Clarke. He appended the following comment to *Testis examinatus*: "Besides what *D. de Graef* hath since publisht upon this Subject, the same hath been lately examin'd by the *R. Society* with so much care and exactness, that now there remains but little doubt of what is conceived, and hath been so many years agoe, by able Anatomists here in *England*, of the structure of the *Testicles, viz.* that they are a *Congeries*, or heap of very fine vessels, that may be drawn out ike [*sic*] thred, and distinctly expos'd to the Eye." (*Phil. Trans.* 42 [14 December 1668] 844).

105 That is, "totus sit, totum vas, testis" (*Phil. Trans.* 42 [14 December 1668] 843-845. My translation).

106 OC *De Graaf to Oldenburg 12 February 1668/9* V 399.

107 For example, Albert van Helden explains that Christiaan Huygens took his discovery of Saturn's moon Titan to be proof that he had better telescopes than anyone else, and thus considered himself qualified to evaluate other astronomers' telescopic observations. Huygens used this assumed interpretive authority to support his theory that Saturn was surrounded by a ring. Domenico Bertoloni Meli makes a similar point in his analysis of the Newton-Leibniz dispute, arguing that priority "had a subtle relation with the prestige and credibility of an interpretation: the ingenuity of the 'first inventor' went hand in hand with the favourable

Figure 2.1: Diagrams of a Testis from *Testis examinatus*.

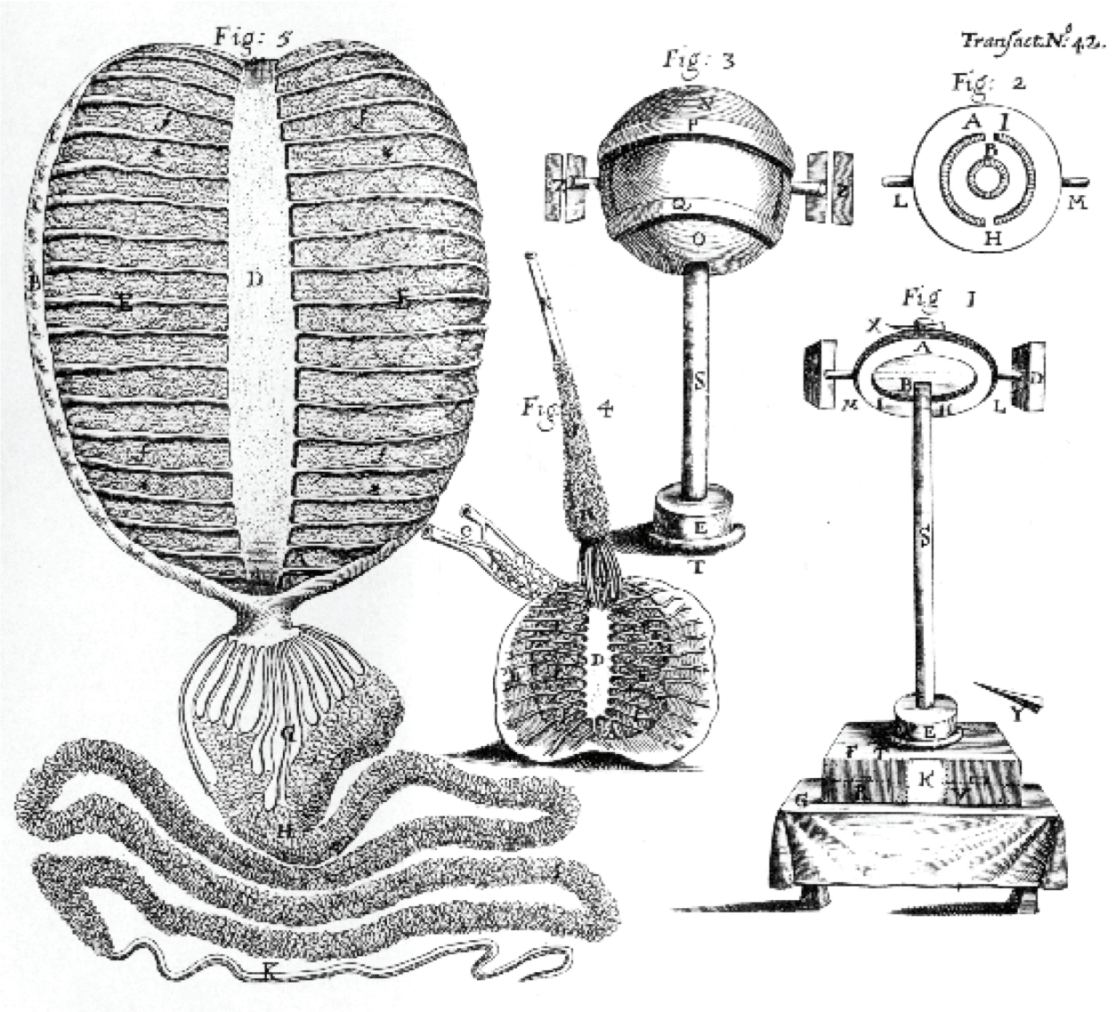

Image from *Phil. Trans*. 42 (14 December 1668) 845.

scope of a dispute over priority expanded to include differences of opinion over the anatomy of the testes. Jardine's point that even minor observations were subject to priority disputes helps to explain how Clarke was able to adopt the seemingly contradictory position of suggesting that England deserved a share of priority for a discovery and denying the accuracy of the work that announced it. Clarke believed that De Graaf's main point, that the testes are composed of vessels, was at least true in part –

reception of his work. The 'second inventor', by contrast, had to protect himself from the shade of doubt about his integrity and from the suspicion of having stolen a secret not from Nature, but from a colleague" (Domenico Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz* [Oxford: Oxford University Press, 1996], 4; Albert van Helden, "'Annulo Cingitur': The Solution to the Problem of Saturn," *Journal for the History of Astronomy* 5 [1974]: 161-162).

the testes may have been composed partly of vessels, and partly of another substance[108] – but he doubted many of De Graaf's details and analyses. Accordingly, De Graaf's letter was quite defensive about the conclusions he had drawn from his observations. Clarke had implied not only that De Graaf's discovery was unoriginal; it was also probably inaccurate and, if not, then De Graaf had been lucky. Having addressed the first criticism, De Graaf moved on to the others. Rather than luck, De Graaf claimed, the source of his discovery was "my own labor and industry."[109] Furthermore, in response to Clarke's doubts about the accuracy of his observations, he offered to send Oldenburg specimens he had collected that would support his findings.[110]

Yet De Graaf needed to downplay the intensity with which he sought the honour of priority. He told Oldenburg that he resented Clarke's claim that "ambition and youthful ardor shone forth in my letter" and he added that he could have written back with similar polemics "did I not venerate the age of that famous man, and did I not prefer to demonstrate from the facts themselves that I am not such a man as his prejudiced imagination feigns me to be."[111] However, De Graaf's previous letter, while it did not name Clarke personally, had made implied accusations and criticisms of Clarke. As such, this show of respect and appeal to "the facts themselves" seem more strategic than genuine. De Graaf was coming to understand that it was preferable to make priority claims under the guise of seeking the truth. He also now had firsthand experience of the fact that, if one criticized the Royal Society outright in a dispute, the actual substance of one's points could be overshadowed by the Society's complaints of disrespect.

In May 1669 Oldenburg took a more active role in the dispute. Rather than simply letting Clarke speak for himself as before, he sent Clarke's next letter to De Graaf enclosed with his own in which he showed solidarity with the Society and definitively adopted Clarke's position. He explicitly and tersely scolded De Graaf for apparently challenging the entire Royal Society:

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108 The Royal Society repeated the experiments that led to De Graaf's theory, and Edmund King was able to replicate his results, but Clarke remained unconvinced that the testes were composed exclusively of vessels (Sawin, "Regnier De Graaf," 416).

109 OC *De Graaf to Oldenburg 12 February 1668/9* V 399.

110 OC *De Graaf to Oldenburg 12 February 1668/9* V 399.

111 OC *De Graaf to Oldenburg 12 February 1668/9* V 400.

> I think it will appear from the answer he [Clarke] makes to your latest letter, firstly, that you have made an accusation against the Royal Society itself, and not against any individual physician or anatomist, in your preceding letter. Next, it is by no means difficult to gather from it that you were neither the sole nor the first discover of that texture of the testes for which you contend, although no one, so far as I know, denies that you have elucidated that business much more plainly than anyone, perhaps, has done before and furnished opportunity to investigate that part more accurately.[112]

Oldenburg thus conceded that De Graaf expressed the theory better than anyone, but denied that this was decisive in the priority dispute; it did not fit the definition of priority that he and Clarke had adopted. Far from trying to defuse the conflict, Oldenburg accepted De Graaf's offer to send specimens to support his theory; perhaps he thought he was calling De Graaf's bluff. As he concluded his letter, Oldenburg told De Graaf that, in recommending "propriety" to him, the Society was acting out of concern for De Graaf's honour. Oldenburg thus illustrated the contradiction inherent in such disputes, that is, the conflict between the honour of priority and the dishonour of striving for priority too earnestly. At the same time that he provoked De Graaf to prolong the conflict, Oldenburg provided De Graaf with a less than ideal way out of it: he essentially recommended that De Graaf cut his losses by giving up the honour of the discovery, and spare himself the loss of honour that would result from any further rebuttal to the Society's ruling.[113]

Clarke began his next letter, written on 10/20 May 1669, by telling Oldenburg that he appreciated his agreement that De Graaf was out of line in questioning the integrity of the entire Royal Society.[114] Nevertheless, the bulk of this letter consisted in Clarke's outrage at what he considered personal insults. In this letter, Clarke mixed his identification of De Graaf's factual errors with his reaction to perceived slights against his honour: "He mistook my 'angle' for 'ring' in my first letter, but more unworthily, I say, he has charged me with the offense of libel when he asserts that in my former letter I called him a calumniator." Clarke explained that he preferred to refer to De Graaf as a

112 OC *Oldenburg to De Graaf 8 May 1669* 530.
113 OC *Oldenburg to De Graaf 8 May 1669* 530.
114 OC *Clarke to Oldenburg 10 May 1669* V 534-535. In the passage that Clarke quoted as evidence, De Graaf addressed the Royal Society as a whole rather than naming Clarke, who wrote the letter that originally denied De Graaf's priority. One wonders if the alternative would have served De Graaf any better, or if this would simply have given Clarke a reason to take offence at having been personally insulted.

"quibbler" [*cavillatorem*] than as someone who libels [*calumniari*].[115] This point is significant for three reasons. Firstly, Clarke revealed that the dispute had expanded to be about much more than priority. Secondly, he showed that he considered De Graaf's accusatory word choice to have been dishonourable; again, Clarke implied that the act of pursuing the honour of discovery at the expense of the Royal Society's (or Clarke's own) reputation was dishonourable in and of itself. Finally, this was part of Clarke's ongoing effort to distance himself from his obvious desire to claim a share of priority for the English. Further to this point, he assured Oldenburg that he made this distinction between "quibbler" and "libeller" to show that "I never willingly intended to insult any learned man, much less provoke him by my words to anger"; conversely, De Graaf's letters were full of "threats".[116] Clarke claimed to be indifferent to the honour of priority: "If it happens that I attain to a truth in the studies which it is proper for me to pursue, whether through my own diligence or through that of others, I shall freely allow the honor of discovery to the others, whoever they be." Clarke thus set himself up in opposition to De Graaf, for whom he condescendingly wished "all those honors which he seeks so eagerly."[117]

By calling De Graaf a quibbler, Clark again redefined the entity over which he and De Graaf were arguing. While De Graaf relied on the principle that Jardine identifies – that the most minor of details can be the subject of a priority dispute – Clark dismissed all such arguments as quibbles and insisted that what was really at stake was the priority for discovering the general structure of the testes. This was why Clarke considered it an acceptable concession to admit that the other natural philosophers he mentioned "have not, I confess, spoken of the vessels preparing the semen in the very same way that he did".[118] Clarke's redefinition which deemphasized the details of De Graaf's observations allowed him to claim at least some English priority by pointing out that the English anatomists Francis Glisson (1597-1677) and Thomas Wharton (1614-1673) "had at least discovered something of the same kind as the vessels of Mr. De Graaf, although they did

115 OC *Clarke to Oldenburg 10 May 1669* V 535.
116 OC *Clarke to Oldenburg 10 May 1669* V 535
117 OC *Clarke to Oldenburg 10 May 1669* V 536, 538.
118 OC *Clarke to Oldenburg 10 May 1669* V 535.

not call them vessels, etc."[119] De Graaf had identified a number of minor differences between his findings and those of the anatomists who were said to have anticipated his discovery, and had thus narrowed the scope of the priority dispute to a point that suited this evidence. In turn, in this letter Clarke widened the scope in order to favour the evidence he had originally raised against De Graaf: a general similarity in the way other anatomists had described the substance of the testes. In any case, Clarke remarked, De Graaf's nitpicks were inaccurate, unless "the *vasa deferentia* of Hollanders are, perhaps, different from those of Englishmen."[120]

As the conflict dragged on, De Graaf showed that he had learned how important it was to deny that he was motivated by receiving credit for priority; he also showed that he was learning how to show indifference even while introducing new evidence for his priority. His letter of 15/25 July began in an apologetic tone: De Graaf had carefully read the letters he had received from Oldenburg and Clarke and "silently rendered my thanks to you, because having at your urging laid aside my youthful ardor" (a quality he had denied having before) "and shaped my style modestly I have in some measure won your goodwill." He had not meant to accuse the entire Royal Society, he claimed, but rather only one or two members of the Society to which most of "the learned men of all England" belonged.[121] De Graaf, in the style of Clarke, filled this letter with suggestions that what was important to him was setting the record straight, rather than being given credit for a discovery. He explained that a fellow Dutchman, Dulijst van Voorhout, had been present at the Royal Society meeting when some of the Fellows had questioned his priority, and had told De Graaf about it. Then, Oldenburg had confirmed van Voorhout's report in his letter of 28 July/7 August 1668.[122] Thus, De Graaf implied, the impetus for the dispute came not from himself but from those who had told him that members of the Society had doubted his priority. It was more important to him, he wrote, to gain Clarke's friendship than to earn "renown as a discoverer."[123]

However, De Graaf neither excused all of Clarke's insults nor gave up on his

119 OC *Clarke to Oldenburg 10 May 1669* V 536.
120 OC *Clarke to Oldenburg 10 May 1669* V 537.
121 OC *De Graaf to Oldenburg 15 July 1669* VI 121.
122 OC *De Graaf to Oldenburg 15 July 1669* VI 121. The letter from Oldenburg is apparently not extant. See *Oldenburg to De Graaf 28 July 1668* IV 572.
123 OC *De Graaf to Oldenburg 15 July 1669* VI 121.

effort to prove his priority. He accused Clarke of denying him the "right which individuals enjoy of interpreting their own words"; again, he suggested, Clarke's argument rested more on clever manipulation of De Graaf's words than on evidence.[124] De Graaf added more evidence to his case for priority by arguing that Wharton believed the testes to be "glandular" rather than composed of vessels.[125] Although by now De Graaf had learned how better to hide his polemics, they remained close to the surface. He did in fact send Oldenburg a specimen he had taken from a dormouse, and he returned Clarke's sarcasm when he predicted that the specimen would prove his theory to be superior to Clarke's, "unless, as he suggested in his letter, possibly the *vasa deferentia* of Englishmen are differently constructed from those of the Dutch." He encouraged Clarke to send back a specimen proving his opinion about the seminal vesicles and promised that he would reply by sending him "ten seminal vesicles . . . out of men of different nationalities" in favour of his theory.[126] Nevertheless, De Graaf concluded by implying that it was Clarke who was concerned about being granted the honour of discovery: "our various opinions may be confirmed by dissection and the glory of discovery which he declares to be so greedily sought by myself may rest unimpaired with the celebrated Mr. Clarke."[127] Evidently De Graaf had picked up on some of Clarke's rhetorical strategies: making one's criticisms implicit but still obvious, shifting the blame for initiating the priority dispute, suggesting that one's opponent was overzealous in his pursuit of priority, and introducing evidence for one's priority claim subtly under the guise of determining the truth.

If De Graaf's latest letter showed that he was starting to grasp these skills, Clarke's reply of 20/30 December showed that he had mastered them. His tactic in this case was not to blame De Graaf, but rather Oldenburg, for perpetuating the dispute. He wrote, "You urge me too much, friend Oldenburg, and overcome me by that friendship with which you always bind me to yourself, so that I am again dragged into distasteful wrangling." What Clarke was suggesting was true – Oldenburg was certainly a catalyst for the dispute with De Graaf – but Clarke seems to have pointed this out in an effort to

124 OC *De Graaf to Oldenburg 15 July 1669* VI 121.
125 OC *De Graaf to Oldenburg 15 July 1669* VI 121-122.
126 OC *De Graaf to Oldenburg 15 July 1669* VI 122-124.
127 OC *De Graaf to Oldenburg 15 July 1669* VI 124.

gloss over his own enthusiasm for taking up the cause of English priority. Although, Clarke stated, he enjoyed debating difficult matters in order to come to the truth, it was not worthwhile to argue over matters such as this, whose resolution should be obvious. He insisted that "quarrelling [was] greatly opposed to my nature" and that he did not have time for it; evidently neither of these claims was true. In any case he was willing to "humor [Oldenburg] this time" by responding to De Graaf, with the intention of ending the dispute.[128] In the spirit of this claim he withdrew his accusation that De Graaf had accused the entire Royal Society of injustice (not without reminding Oldenburg that he had been of this opinion too) and admitted, at least, that the lack of clarity in his previous letters may have fostered the dispute.[129]

Of course, the main purpose of Clarke's latest letter was not to apologize. Instead he showed the subtlety required to deny De Graaf's priority while upholding the etiquette of the Republic of Letters. Clarke admitted that, although other anatomists had anticipated De Graaf's discovery, "there is no way of discerning whether they had provided a perfected and in all respects complete description of the testes".[130] Here, Clarke redefined De Graaf's accomplishment, limiting it to the perfection of what others had started – it was innovation rather than discovery – perhaps in an effort to placate De Graaf by giving him credit for *something*. Clarke, however, then made an interesting move by ostensibly granting priority to De Graaf. He wrote, "leaving all this aside (for I have not leisure to argue about much) let the whole glory of the discovery belong to Mr. De Graaf, for that learned man does not know me well when he thinks that I have like himself, a keen desire for this honor."[131] Clarke's reason for making this concession appears to be twofold. Firstly, it further distanced him from people like De Graaf who sought priority too eagerly. Secondly, Clarke probably did not expect anyone to take this as an honest admission of defeat because he positioned it both before and after remarks that suggested that De Graaf's only accomplishment was synthesizing and improving on the discoveries of other anatomists, including English ones. He followed this gesture with a rather condescending remark: "our very accurate anatomist, [Richard] Lower, happened

128 OC *Clarke to Oldenburg 20 December 1669* VI 385.
129 OC *Clarke to Oldenburg 20 December 1669* VI 386.
130 OC *Clarke to Oldenburg 20 December 1669* VI 386.
131 OC *Clarke to Oldenburg 20 December 1669* VI 389.

by chance upon the same discovery as myself, long before Mr. De Graaf had been heard of."[132] When combined with Clarke's repeated insistence that he was not motivated by the honour of discovery, this dismissive attitude toward recognition functioned as reminder that Clarke had enough honour already; he would let De Graaf have it in the end because it did not mean as much to Clarke. Nevertheless, Clarke accepted De Graaf's offer of friendship, and the conflict began to settle down. The editors of the *Oldenburg Correspondence* note that Oldenburg evidently told De Graaf about the contents of Clarke's letter in early 1669/70, but the letter in which he did so is apparently missing.[133]

Whether it was genuine or not, Clarke's offer to let De Graaf have the credit for the discovery seems to have pacified him. In his next letter to De Graaf, written on 5/15 March 1669/70, Oldenburg encouraged De Graaf to continue his experiments on the testes; his tone implied that it was time to bring the dispute to an end.[134] Evidently Oldenburg had gotten what he wanted out of the dispute. Clarke had gotten the last word and ostensibly yielded priority to De Graaf, but suggested that he was simply letting De Graaf have his way in the interest of civility and international cooperation. Clarke thus both saved face and let the Royal Society maintain its sense of self-satisfaction; the implication was that the natural philosophical community would always know that English anatomists may not have been first in observing vessels in the testes, but at least they did so before De Graaf. Meanwhile, De Graaf, perhaps overwhelmed by the determination of the Royal Society in matters of priority, was seemingly satisfied with this token gesture that let him save face as well. In any case, his reply to Oldenburg of 19/29 June 1670 focuses on his gratification that the Society members had found his dormouse specimen to be convincing evidence that the testes were composed of vessels. He noted that "I could say much more in answer to the famous Dr. Clarke's letter but that I prefer to satisfy his wish that we should put an end to our disputations."[135] Thus, the conflict had come to a close at an opportune moment for Oldenburg and the Royal Society; Clarke could revel in his victory and De Graaf would not be sufficiently discouraged or offended to stop contributing to the Society. After all, while the Royal

132 OC *Clarke to Oldenburg 20 December 1669* VI 389.
133 OC *Oldenburg to De Graaf 5 March 1669/70* VI 535.
134 OC *Oldenburg to De Graaf 5 March 1669/70* VI 534-535.
135 OC *De Graaf to Oldenburg's 19 June 1670* VII 38-39. The quotation is from p. 39.

Society remained adamant in denying De Graaf's priority, they still valued his work highly; De Graaf may not have been the first to suggest that the testes were made of vessels, but in the estimation of the Society he had proven it.[136] To the Society, it seems, De Graaf was a valuable contributor despite his lapse in judgment when he strove for priority at the expense of the reputations of others.

The conflict had no negative impact on De Graaf's career; he went on to make several important anatomical discoveries before his early death at the age of thirty-two in 1673. In the same year as the publication of the *Tractatus de virorum organis*, he published papers about two inventions: a "surprisingly modern" syringe and a device for performing enemas on oneself.[137] In 1672 he published a book giving a comprehensive description of the human female reproductive system, complete with observations of the stages of fetal development. This work, *De mulierum organis generationi inservientibus tractatus novus*, identified ovaries in human females, reversing the commonly held belief that human females did not produce eggs.[138] De Graaf continued to submit information to Oldenburg until shortly before De Graaf's death. He was last mentioned for a contribution in the *Philosophical Transactions* in No. 94 of May 1673, in which Oldenburg described the contents of a letter from De Graaf written on 18/28 April. De Graaf's letter brought to the attention of the Society the observations of "one Mr. [Antonie van] *Leewenhoeck* [who] hath lately contrived Microscopes excelling those that have been hitherto made by *Eustachio Divini* and others".[139] De Graaf attached a letter

136 Sawin, "Regnier De Graaf," 416.

137 Sawin, "Regnier De Graaf," 415.

138 Sawin, "Regnier De Graaf," 417-418. Additionally, Ankum, Houtzager and Bleker argue De Graaf was probably the first to understand the purpose of the fallopian tubes, and *De mulierum* was the first text to demonstrate it (Ankum, Houtzager and Bleker, "Renier De Graaf," 365-369). As Sawin explains, in this work De Graaf also identified what is now known to be an ovarian follicle, which is referred to as the "Graafian follicle". *De mulierum* involved De Graaf in another priority dispute with fellow Dutch natural philosopher Jan Swammerdam who claimed that De Graaf had plagiarized his and Johannes van Horne's observations of the female reproductive organs. The disputants called upon the Royal Society to arbitrate, but the three Fellows appointed for this task failed to make a decisive ruling. De Graaf died before he heard back from the Society. The reason for his death is unclear. Venita Jay claims that De Graaf's "brooding" during the conflict with Swammerdam factored into his death, but Jay cites no sources in support of this claim. Other secondary sources attribute his death to plague or dysentery (Jay, "Reinier De Graaf," 1116; Modlin, "Regnier De Graaf," 109; Sawin, "Regnier De Graaf," 420). Sawin himself weighs in on the priority dispute between De Graaf and Swammerdam. He determines, based on De Graaf's earlier publication date, superior experimentation skills and more complete illustrations that "[i]t seems right to remember de Graaf as the innovator." (Sawin, "Regnier De Graaf," 420).

139 *Phil. Trans.* 94 (19 May 1673) 6037-6038. The quotation is from p. 6037. Additionally, in 1671, Oldenburg gave a favourable review of De Graaf's *De succo pancreatico* (*Phil. Trans.* 79 [22 January

written by Leeuwenhoek himself which, as Sawin points out, became Leewenhoek's first publication and the first of 308 letters he would write to the Society.[140] Despite the priority dispute with Clarke, De Graaf remained an important (if indirect) source of natural knowledge even after his death.

As for Clarke, he died in February 1671/2, about a year and a half before De Graaf. Although he was an original Fellow and Council member of the Royal Society, his main contribution remained his letter in the *Philosophical Transactions* arguing for English priority for identifying the composition of the testes, as well as for discovering the lymphatic system and inventing blood transfusion, as will be discussed in Chapter 3. Apart from his involvement in these priority disputes, he figured in Oldenburg's correspondence only occasionally (although he did appear more often in the minutes of the Royal Society's meetings). Yet Clarke was still in a position to command authority. Apart from his Royal Society credentials, he was also a Royal Physician, and in a letter to Hevelius written in January 1667/8 Oldenburg introduced him as "the celebrated Timothy Clarke, Physician to the King".[141] While his production of natural knowledge was limited, Clarke was still useful; he was someone to whom Oldenburg and the rest of the Society could refer matters of priority, who had the experience and connections to speak from a position of authority. No doubt Oldenburg appreciated Clarke's pro-English position on the matter of blood transfusion, a case which, for reasons that will become clear in the next chapter, Oldenburg had reason to take personally.

1671/2] 3066).
140 Sawin, "Regnier De Graaf", 420-421.
141 OC *Oldenburg to Hevelius 31 January 1667/8* IV 137.

Chapter 3: The Blood Transfusion Dispute: A Case Study in the Defence of English Priority

3.1: Blood Transfusion Experiments at Oxford and the Royal Society

The best known priority dispute involving the Royal Society in the 1660s is the dispute over the invention of blood transfusion. It is likely that historians have discussed it so much because historical actors did; the Royal Society and their opponents in the French Académie des Sciences showed more excitement about the possible applications of blood transfusion than about the subjects of the conflicts discussed in the previous chapter. The reason for the attention to blood transfusion among natural philosophers seems to have been twofold. Firstly, as Richard Lower, one of the English disputants, wrote, the transmission of an animal's blood into a human – transfusion experiments in the seventeenth century generally involved sheep, calves and dogs – was expected to "be employed with great profit for the human race" by curing diseases thought to result from corrupted blood, or by replacing blood lost because of an injury.[1] Secondly, those who experimented on blood transfusion wanted credit for solving a notoriously difficult natural philosophical problem. By the mid- to late 1660s, the time of the dispute, blood transfusion had a reputation as a "most difficult experiment"; this is how it was described by Boyle, who himself had tried and failed to transfuse blood from one animal into another, as will be discussed below.[2] Experimenters like Boyle and Lower attempted blood transfusion with the dual goals of improving the human condition and enhancing their own reputations. In addition to this self-fashioning, several other features of priority disputes involving the Royal Society that I have identified will become clearer through a case study of the dispute over blood transfusion, a dispute whose prize was particularly important to the Royal Society and its opponents. We see Oldenburg employing all of the resources at his disposal to secure English priority; we see other Fellows helping him

1 Richard Lower, "Tractatus de corde item de motu & colore sanguinis et chyli in eum transitu" in *Early Science in Oxford* vol. 9, trans. K. J. Franklin (London: Dawsons of Pall Mall, 1968; orig. pub. 1932), 189. See also Jean-Baptiste Denis, "A Letter Concerning a New Way of Curing Sundry Diseases by Transfusion of Blood, Written to Monsieur de Montmor, Counsellor to the French King, and Master of Requests" in A. D. Farr, "The First Human Blood Transfusion," *Medical History* 24 (1980): 158-159.

2 *Boyle to Lower 26 June 1666*, quoted in Lower, *Tractatus*, 177.

willingly and joining him in the dismissal of the evidence presented by their opponents; and we see the disputants interpreting evidence in whichever way will support their own claim.

Two useful historical accounts of the blood transfusion dispute were both published in 1980. Firstly, A. D. Farr gives a general description of the dispute and focuses on the role of a particular document: an English translation of a letter written by the French physician Jean-Baptiste Denis, who seems to have performed the first transfusion involving a human patient and who claimed priority for blood transfusion in general for the French.[3] Secondly, A. Rupert Hall and Marie Boas Hall describe the dispute through the perspective of Oldenburg and analyze the nationalist attitudes of the participants.[4] Neither of these accounts gives much analysis of what social circumstances apart from nationalism may have caused English and French natural philosophers to dispute the priority of blood transfusion so vehemently.[5] The nationalist rivalry is also at the forefront of Holly Tucker's recent book on the invention of blood transfusion; Tucker describes the affair as "a showdown between France and England in the fight for scientific dominance."[6]

3 Farr, "First Human Blood Transfusion," 143-154. Cited in full in n. 1 above.

4 A. Rupert Hall and Marie Boas Hall, "The First Human Blood Transfusion: Priority Disputes," *Medical History* 24 (1980): 461-465.

5 There are several other scholarly works that discuss the transfusion dispute but they provide little analysis. For example, in "A Short History of Transfusion Medicine" T. J. Greenwalt describes the dispute, but he seems to want to settle the question of priority rather than to analyze the dispute. The same can be said for M. T. Walton's "The First Blood Transfusion: French or English?", except for a concluding remark that suggests reasons for the dispute: "it is clear that nationalism and pride in one's own scientific society as well as individual ambition motivated the transfusion debate." Certainly Walton is correct to identify nationalism and ambition in this dispute, but a simple acknowledgment of the presence of these factors does not constitute an explanation for how they shaped the events of this dispute in particular or how it became so hotly contested. As Merton writes concerning the notion that priority disputes are the result of multiple discovery, these factors provide "only an *occasion* for disputes over priority, not their *cause* or their *grounds*" (T. J. Greenwalt, "A Short History of Transfusion Medicine," *Transfusion* 37 [1997]: 550-553; Robert K. Merton, "Priorities in Scientific Discovery: A Chapter in the Sociology of Science," *American Sociological Review* 22 [1957]: 637; M. T. Walton, "The First Blood Transfusion: French or English?" *Medical History* 18 [1974]: 360-364. The quotation is from p. 362).

6 Holly Tucker, *Blood Work: A Tale of Medicine and Murder in the Scientific Revolution* (New York: W. W. Norton, 2011), xxi. This is an example of how Tucker excessively dramatizes the history of blood transfusion throughout her book. Her work is useful as a general account of seventeenth-century blood transfusion, but only if one recognizes its several factual errors, its ignorance of recent historiography of science, and its obvious agenda. In an effort to compare debates over the safety and utility of blood transfusion to criticism of modern biomedical research, Tucker portrays these debates as "clashes between science and society" brought on by superstition and "high-pitched social fears". Apparently she intends the book to be a warning that society should not "set limits on its science" (Tucker, *Blood Work*, xvii-xxix, especially xxvii-xxix).

Historians, like contemporaries, have generally viewed blood transfusion as an outgrowth of experiments on intravenous injection, which in turn have been viewed as an extension of Harvey's discovery of circulation.[7] Oldenburg mentioned transfusion and injection experiments together in letter to Boyle of 17/27 March 1665/6, reporting that the Royal Society was currently engaged in both. In this letter Oldenburg also reported that a familiar figure in priority matters, Timothy Clarke, had mentioned at a meeting of the Society that he had attempted transfusion two years earlier, which prompted Robert Moray to add that Boyle was optimistic about "mastering ye difficulties, yt are met with in yt Experiment."[8] The Royal Society apparently viewed transfusion as the continuation of English accomplishments in anatomy; the Fellows thought they had the ability to succeed in this endeavour, although it was fraught with difficulty.

Boyle's experiments on transfusion were apparently unsuccessful, and he was understandably interested when John Wallis told him that Richard Lower, an up-and-coming Oxford-educated physician who had moved to London in 1666,[9] had managed to transfuse the blood of one dog into another. Boyle made it clear that the Royal Society was impressed that Lower had succeeded in a difficult operation where other notable anatomists had failed, and that the honour of this accomplishment could accelerate Lower's career. He wrote in a letter to Lower of 26 June/6 July 1666:

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7 Farr notes that Geoffrey Keynes made this connection as early as 1949 (Farr, "First Human Blood Transfusion," 143. See also J. H. Felts, "Richard Lower: Anatomist and Physiologist," *Annals of Internal Medicine* 132 [2000]: 421; R. Shane Tubbs, et al., "Richard Lower [1631-1691] and His Early Contributions to Cardiology," *International Journal of Cardiology* 128 [2008]: 17). As will be discussed below, Timothy Clarke and Richard Lower both placed the accomplishment of transfusion in the tradition of Harvey. The Royal Society was also sensitive about their priority for injecting liquids into the veins of animals. In the *Philosophical Transactions* of December 1664, Oldenburg printed "*An Account of the Rise and Attempts*, *of a Way to conveigh Liquors immediately into the Mass of Blood*" to supply evidence against works from "beyond the Seas" that failed to credit Christopher Wren with inventing these experiments. Oldenburg claimed that it was well known that Wren had come up with this idea six years earlier and "could easily contrive a Way to conveigh any liquid thing immediately into the Mass of Blood". The experiment was first performed on a dog (just as the English first tried transfusion on a dog, which was a key point in their priority claim) and eventually on human patients. Oldenburg made it clear that naming witnesses and publishing an item in the *Transcations* both contributed to the establishment of priority. He wrote, "the Reader may securely take this Narrative, as the naked real Matter of Fact, whereby 'tis as clear, as Noon day (both from the Time, and irrefragable Testimony of very many considerable Persons in the University, who can jointly attest it; as well as from that particular unquestionable one of Mr. *Boyle* and his worthy Company, who were the first Eye-witnesses of the Tryals made, that to *Oxford*, and in it, to Dr. *Christopher Wren*, this Invention is due" (*Phil. Trans.* 7 [4 December 1665] 128-130).

8 OC *Oldenburg to Boyle 17 March 1665/6* III 61-62.

9 Tubbs, et al., "Richard Lower," 17.

> I heard from Dr. [John] *Wallis* that you had at last (in his presence) successfully accomplished that most difficult experiment on the transference of blood from one to the other of a pair of dogs. I judged the matter clearly worthy of being communicated to that very celebrated assembly. I therefore proposed that they should ask that distinguished gentleman for an account of the way in which it had been performed. His description of it was such as to increase not a little our opinion of your reputation.[10]

Coming from the authoritative Boyle, this promise of honour must have been exciting for Lower. Boyle's letter suggested that this was a rare opportunity for Lower to establish a reputation: "I thought it would be to your advantage, if this celebrated assembly became acquainted with you at this propitious moment."[11] As it turns out, Lower was elected as a member of the Royal Society at the height of the transfusion dispute, in October 1667.[12] By this time, Lower had shown the Royal Society other impressive anatomical experiments, such as his way of "*making a* Dog *draw his Breath exactly like a* Wind-broken Horse".[13] Still, the invention of blood transfusion was the basis of his reputation as a talented anatomist and experimental philosopher. It is no wonder that Lower felt threatened when the French challenged his priority in inventing blood transfusion.

Lower was happy to reply to Boyle with a full description of his method of transfusion in a letter of 6/16 July. He explained that he had drawn blood from the donor by attaching a tube to its carotid artery, which pumped blood through the tube and into the jugular vein of the receiver. Meanwhile, another tube pumped the receiver's blood into a dish. Draining its blood killed the donor dog, but the receiving dog immediately appeared as healthy as before.[14] Lower was concerned to show that transfusion was harmless and beneficial to the receiver. He wrote that, after the procedure, the receiver "shakes itself a little, as though just aroused from sleep, and runs away lively and strong, more active and vigorous, perhaps, with the blood of its fellow than with its own."[15] Evidently, Lower felt that he would be able to take pride in the safety and utility of the operation in addition to its originality and difficulty.

Boyle sent Lower's letter to Oldenburg so that he could relate its contents to the

10 Lower, *Tractatus*, 177-178.
11 Lower, *Tractatus*, 179.
12 See OC *Boyle to Oldenburg 26 October 1667* III 540
13 *Phil. Trans.* 29 (11 November 1667) 544-546. The quotation is from p. 544.
14 Lower, *Tractatus*, 174-176; Tucker, *Blood Work*, 43-44.
15 Lower, *Tractatus*, 183.

Royal Society. Oldenburg replied that he was sure the Society would be excited about Lower's achievement, and he quickly took steps to announce it publicly and secure Lower's priority. He told Boyle that he assumed that Lower would "give me leave to make his way of bleeding one dog into another publick, to invite others to ye like Experiments."[16] The next day, Oldenburg announced the news at a meeting of the Royal Society. The experiment was immediately recorded in the Society's register books, and a team of "curators" was appointed to try to replicate the experiment; if they were successful, they were to repeat the experiment in front of credible witnesses at a Society meeting.[17] The Society was moving quickly to confirm Lower's success and establish his priority. By late October the Society was making plans to have the curators perform the experiment at a Society meeting, which they did on 13/23 November, according to Oldenburg's minutes, "with very good success".[18] Three days later, Oldenburg wrote to Boyle to report that the Society had "conquered ye difficulty" of transfusion.[19] After the procedure, the team of curators was asked to bring a detailed description of the procedure to the next meeting so that it could be registered; one of the curators, Edmund King, did so on 21 November/1 December. At this meeting, the Fellows were optimistic and excited about transfusion. The minutes report that "[t]he spaniel was next morning [14/24 November] very well and brisk, and so continues." This was to be just the first of the Society's many experiments with transfusion: "It was order'd, that the experiment of exchanging the blood of animals be prosecuted and improved by bleeding a sheep into a mastiff, and a young healthy dog into an old and sick one, *& vice versa*; and that Mr. King be desired to continue his assistance therein."[20]

Oldenburg made the news of the transfusion experiments as public as he could. On 19/29 November, Oldenburg announced in the *Philosophical Transactions* that transfusion, which was "hitherto look'd upon to be an almost unsurmountable difficulty," had been achieved in Oxford by Lower and in London by the Royal Society. Oldenburg's report concluded with a promise to describe the procedure in full as soon as possible.[21]

16 OC *Oldenburg to Boyle 25 September 1666* III 233-235.
17 HRS II 115.
18 HRS II 117-118, 123.
19 OC *Oldenburg to Boyle 16 November 1666* III 281-282.
20 HRS II 123, 125.
21 *Phil. Trans.* 19 (19 November 1666) 352.

Then, in the *Transactions* of 17/27 December, Oldenburg reproduced Lower's letter. He also affirmed Lower's priority and described how the Society had expanded on Lower's work. Oldenburg added that the Society had plans for more experiments for the purpose of "knowledge, or use, or both" and speculated that "[t]he most probable use of this Experiment may be conjectured to be, that one Animal may live with the bloud of another; and consequently, that those Animals, that want bloud, or have corrupt bloud, may be supplyed from others with a sufficient quantity."[22] In the second half of 1666, the Royal Society had heard a report of Lower's success, registered it to ensure that he be recognized for his priority, successfully replicated the experiment, registered their own experiment, announced the accomplishment in print in a manner that linked the Royal Society's success to that of Lower, and announced the Society's intention to expand on these experiments. When the Fellows were excited about a discovery or invention, they moved quickly to confirm it, protect English priority, and proclaim their success.

The Society's enthusiasm for transfusion continued into 1666/7, and Oldenburg began to mention it to foreign correspondents who would probably not have read the *Transactions.* He continued to do so as the Society performed more elaborate transfusions.[23] Meanwhile, the *Transactions* continued publicly to establish the likes of Lower and King as authorities on transfusion; in February Oldenburg published a series of "*Tryals proposed by Mr.* Boyle *to Dr.* Lower".[24] The first three items of the May issue concerned transfusion, the first of which described King's "*easier and safer Way of* Transfusing Blood *out of one Animal into another*".[25]

22 *Phil. Trans.* 20 (17 December 1666) 353-358. The quotations are from pp. 357, 358. Oldenburg reported that the Society had already succeeded in transfusing sheep's blood into a dog. He also announced the Fellows' intention to try "exchanging the bloud of Old and Young, Sick and Healthy, Hot and Cold, Fierce and Fearful, Lame and Wild Animals, &c. And that not only of the same, but also of different kinds" (*Phil. Trans.* 20 [17 December 1666] 356-357. The quotation is from p. 357).

23 Oldenburg wrote to Stanislas Lubienietzki on 3/13 January 1666/7 that "[o]ur Society is now busy with the *experiment of transfusing the blood of one animal into another*, either of the same species or of a different one." This interested Lubienietzki, who asked to know more about it in his letter of 13/23 April. Toward the end of the year he was still mentioning English achievements in transfusion, as he did in a letter to René François de Sluse on 25 November/5 December (OC *Oldenburg to Lubienietzki 3 January 1666/7* III 304, *Lubienietzki to Oldenburg 13 April 1667* III 392, *Oldenburg to Sluse 25 November 1667* III 617). For information on some of the Society's more elaborate transfusion experiments, see n. 22 above.

24 *Phil. Trans.* 22 (11 February 1666) 385-388.

25 *Phil. Trans.* 25 (6 May 1667) 449-451.

3.2: The French Claim to Priority for Blood Transfusion

The third of these items, however, was not about transfusion experiments in England, but rather in France. Oldenburg translated part of a letter written by the French physician Jean-Baptiste Denis which had been printed in the *Journal des sçavans* on 25 April.[26] At least a month earlier, members of the Royal Society were concerned that the French, who had started to perform their own transfusions, would threaten their priority. It is clear from another letter from Denis that appeared in the *Journal* on 14 March that he was aware of the English success with transfusion and wanted to exceed it. He wrote, "We proposed to do not only what is marked down in the extract from the English journal, which is to pass blood from one animal into another, while causing the one who donates to die in order to save the other who receives; rather, we wanted to save both".[27] Nevertheless, John Wallis' letter to Oldenburg of 21/31 March described the French experiments as if they were merely derivatives of Lower's work; he mentioned "Dr Lowers Operation of transmitting Bloud from one Animall to another, confirmed by ye French operation in imitation of his". Although Wallis suggested that it was useful that the French had added more credibility to the transfusion experiment, he still wished that English natural philosophers would be "a little more forward . . . in timely publishing their own Discoveries, & not let strangers reape ye glory of what those amongst ourselves are ye Authors."[28]

The French not only succeeded in replicating the transfusion experiment, but also claimed that Emmerez, the surgeon who performed the experiment with Denis, had improved it. Oldenburg's translation of Denis' letter read, "We have found new wayes of making this Transfusion with so much faculty, that M. *Emmerez* undertakes to perform it *without* any *Ligature*, onely by pricking, like that, which is used in *Letting of blood*."[29] In fact, this was the second such letter from Denis to be printed in the *Journal* in as many months. Denis' letter in the issue of 14 March indicated that he considered transfusion to

26 *Phil. Trans.* 25 (6 May 1667) 453; *Journal des sçavans* (25 April 1667) 96.

27 That is, "Nous nous proposasmes de faire non seulement ce qui estoit marqué dans l'extrait du Journal d'Angleterre, qui est de faire passer le sang d'un animal dans l'autre, en faisant mourir celuy qui le communique pour conserver l'autre qui le reçoit; mais nos voulusmes les conserves tous deux" (*Journal des sçavans* [14 March 1667] 69). All translations from the *Journal des sçavans* are my own.

28 OC *Wallis to Oldenburg 21 March 1666/7* III 373. As will be discussed in Chapter 4, Wallis, who was always concerned about protecting English priority, made this complaint about his countrymen repeatedly.

29 *Phil. Trans.* 25 (6 May 1667) 453.

be both new and significant. He called it "a new proof [which is] entirely convincing for confirming the opinion of those who support circulation".[30] Denis, then, felt that much was at stake, and presumably felt that the honour of making original transfusion experiments would be valuable. Accordingly, he described the experiment and its success in detail. In fact, Denis wrote, he and Emmerez were so successful that they thought the receiving dog was well enough to be the donor in another transfusion experiment less than a week later.[31]

In many ways, Denis' account resembled those of Lower and Oldenburg. Denis emphasized the utility and low risk of transfusion, which he believed to be "not as dangerous as many imagine for he who receives new blood".[32] Furthermore, Denis wrote in a manner that suggested that he and Emmerez were authorities on transfusion. He remarked, for example, "We have indeed seen that if he who prepares the animals has much skill & diligence so that they are not left to languish for a long time, the transfusion is done much better and with more success."[33] Clearly, Denis viewed this improvement on the English transfusion experiments as a chance for honour and advancement. He not only named those who had witnessed the experiment but also emphasized how impressed they had been. Denis set a time and date for his and Emmerez's next feat, transfusing blood from a young dog into an old and mangy dog, which they would perform at the Quai des Grands-Augustins as the first of many "public proofs" of their success with transfusion.[34] It seems that Denis sought to associate himself with this impressive and popular new type of experiment as publicly as possible.

In the summer of 1667 Denis became bolder in his claims about French priority in accordance with the significance of his achievements. He wrote a letter to Henri de Montmor, who is known for holding scholarly meetings at his home before and after the foundation of the Académie des Sciences, on 25 June. Denis subsequently had the letter

30 That is, "une prevue nouvelle & tout à fait convainquante pour confirmer le sent-iment de ceux qui soûtiennent la circulation" (*Journal des sçavans* [14 March 1667] 69).
31 *Journal des sçavans* (14 March 1667), 70-71.
32 That is, "pas si dangereuse que plusieurs se l'imaginent pour celuy qui reçoit un nouveau sang" (*Journal des sçavans* [14 March 1667] 72).
33 That is, "Nous avons bien vue que si celuy qui prepare les animaux a beaucoup d'adresse & de diligence pour ne les pas laisser languir long-temps, la transfusion s'en fait bien mieux & avec plus de succès" (*Journal des sçavans* [14 March 1667] 72).
34 That is, "épreuves publiques" (*Journal des sçavans* [14 March 1667] 72).

printed and a copy was sent to the Royal Society. Farr suspects that Denis wrote his account in the form of a letter as "little more than a device"; he wanted to announce that he had successfully performed transfused blood into a human patient.[35] Denis' letter not only claimed that French anatomists had come up with the idea of transfusion, but also suggested that the practice of transfusion had far outgrown the initial experiments by Lower and the Royal Society; in short, Denis left little room for the English to receive any honour for their priority.[36] Denis stated outright that transfusion was "conceived *about ten years agoe*, in the illustrious Society of *Virtuosi* which assembles at your [Montmor's] house".[37] Apparently because Montmor held this meeting, Denis was willing to share the credit with this politically powerful figure; he made no similar gesture to the English for their contributions.[38]

Sharing credit with Montmor was one of the ways in which Denis tried to reduce the degree to which he seemed to strive for priority. He also did so by insisting that there was demand for him and Emmerez to expand on their earlier successes with transfusion. The March issue of the *Journal des sçavans* reported that the experiment "at the first

35 Farr, "First Human Blood Transfusion," 144-145. Farr's claim is supported by the fact that Denis told Montmor about his transfusion experiments with dogs in the letter. This was presumably unnecessary because, as Denis noted, Montmor had been present at these experiments. It also seems strange that Denis withheld the details of the animal-to-human transfusion, claiming that Montmor would understand better if he saw it for himself, since Denis went into great detail in describing his earlier experiments on dogs. Perhaps Denis wanted to announce the accomplishment to the English (and others) but was not ready to let them duplicate it (Denis, "Letter to Montmor," 155).

36 Although there is little doubt that Lower performed that first successful transfusion on dogs, and Denis the first on a human – accounts such as those of the Halls and M. T. Walton supplied the evidence for this in the 1970s and 1980s – there is less consensus among historians (as well as among contemporaries) about the origin of the *idea* of transfusion. Much of Greenwalt's "History of Transfusion Medicine" explores early suggestions to attempt transfusion. S. S. B. Gilder describes Francesco Folli's claim to have invented transfusion, although Folli never actually performed the procedure. Walton notes some early references to transfusion, such as that in a work by Thomas Coxe (Hall and Hall, "First Human Blood Transfusion," 465; S. S. B. Gilder, "Francesco Folli and Blood Transfusion," *Canadian Medical Association Journal* 71 [1954]: 172; Greenwalt, "History of Transfusion Medicine," 550-551; Walton, "First Blood Transfusion," 360-364).

37 Denis, "Letter to Montmor," 154. Denis was referring here to the work of Robert Desgabets. Desgabets described a method for transfusion and suggested that it would be beneficial and unlikely to cause harm in his *Discourse de la communication ou transfusion du sang*. Although the *Discourse* grew out of a lecture about transfusion Desbagets gave in 1658, it was not published until 1668 when Denis had it printed with a letter that he wrote to support his case in the priority dispute (Patricia Easton, "The Father of Cartesian Empiricism: Robert Desgabets on the Physics and Metaphysics of Blood Transfusion," [HOPOS 2010: The International Society for the History of Philosophy of Science, 24-27 June 2010, Budapest], 1, 7).

38 Although Denis did not share credit with the English, his letter cited a report by Thomas Coxe in *Philosophical Transactions* 25 about using a transfusion to cure mange (Denis, "Letter to Montmor," 158; *Phil. Trans.* 25 [6 May 1667] 451-452).

attempt succeeded as happily as we could wish", so Denis and Emmerez "were encouraged to repeat the same several times both in publick and private."[39] Furthermore, Denis claimed, they had the results of these experiments published only in order "that we might vent nothing unadvisedly"; they wanted to hear other natural philosophers' opinions of their work so that they could proceed prudently. Denis remained unwilling to admit to any desire to be recognized for priority.[40] While the feedback they received contained a number of concerns about the dangers and difficulties of human-to-animal transfusion,[41] Denis explained that many people had reacted favourably to the idea and suggested that they try the experiment on a condemned criminal.[42] In effect, Denis was again implying that his motivation for experimenting with transfusion was not the desire for recognition, but rather the enthusiasm of his peers.

Toward the end of the letter, Denis finally described his and Emmerez's human transfusion experiments. They had chosen as a patient a teenage boy who had been suffering from fevers for two months. Physicians had bled him twenty times to no avail, and lately he had been falling asleep at inappropriate times, such as at the dinner table. Denis claimed that the bloodletting had removed so much of the boy's blood that he could not stay awake, and he decided that a blood transfusion would be the perfect solution. Denis and Emmerez gave the boy lamb's blood and his health improved

39 Denis, "Letter to Montmor," 154.

40 Denis, "Letter to Montmor," 155.

41 Denis had a rebuttal for each of these concerns. For example, some believed that transfusion would prove to be "Chymerical & impossible" because blood varied too much between individuals, and especially between members of different species. As such it would be dangerous to transfuse an animal's blood into a human. Denis responded that, if this was the case, then the same problem would arise when a human ingested the meat of an animal. Obviously, Denis wrote, this was not the case; in fact, early modern medicine held that eating certain meats could cure disorders in the blood. For Denis, blood transfusion would have the same effect but would be more direct in that it would simply replace the diseased blood rather than having to alter it. Denis suggested that transfusion was both safe and superior to other medical treatments. As Patricia Easton notes, Robert Desgabets, the anatomist to whom Denis referred when he claimed that a Frenchman had conceived of transfusion a decade earlier, had already suggested a solution to the problem of dissimilar blood. The different "complexions" of blood could be overcome by choosing the donor carefully, just as one was careful about what he or she ingested. Denis even had several answers to people's moral qualms about introducing animal blood into a human. Firstly, he claimed, animal blood was in fact purer than human blood because it was not corrupted by excessive food and drink or by extreme emotion. Secondly, there were no such concerns about introducing an animal's milk or meat into the body, so there should be none about introducing an animal's blood. Finally, it was a more acceptable risk to open up an animal's artery to draw out blood than to do this to a human (Denis, "Letter to Montmor," 155-160; Easton, "Robert Desgabets," 12).

42 Denis replied that the criminal's fear could corrupt his blood; it would be better to perform the transfusion on someone who trusted him and Emmerez (Denis, "Letter to Montmor," 160).

immediately. Having been so successful, they soon performed another human transfusion, this time giving lamb's blood to a middle-aged man. The man felt so well afterward that, ignoring the doctors' recommendation to rest, he immediately went back to work and later drank with his friends. For Denis, the procedures were completely successful; his patients recovered because he had decided to give them lamb's blood.[43] Denis maintained as he concluded the letter to Montmor that he was not writing this to be given credit for his accomplishment. Rather, he felt obligated to report it: "This is an account of our Experiments, which indeed have not yet proceeded very far; nevertheless I could not longer conceale them from your curiosity, knowing well that from these few observations you will foresee consequences and advantages enough."[44] It is hard to believe this expression of obligation if one consider's that the letter was immediately printed and sent to the Royal Society.[45]

Farr gives an account of the events that followed in London. A translation of Denis' letter was printed in the *Philosophical Transactions*, but Oldenburg did not edit the issue in which it appeared. He was imprisoned in the Tower of London in late June, ostensibly because he was suspected of conveying sensitive information to one of his French contacts while the French were helping the Dutch in their war with England. Douglas McKie argues, however, that Charles II actually had Oldenburg arrested because he was offended by letters in which Oldenburg had criticized the actions of the English in the war.[46] With Oldenburg in the Tower, someone else – probably John Wilkins, the other

43 Denis, "Letter to Montmor," 160-161. For a discussion of why a lamb or another particular animal would be chosen, see n. 73 below.

44 Denis, "Letter to Montmor," 161.

45 Farr, "First Human Blood Transfusion," 145.

46 Douglas McKie, "The Arrest and Imprisonment of Henry Oldenburg," *Notes and Records of the Royal Society of London* 6 (1948): 31, 35, 47. Holly Tucker sees Oldenburg's imprisonment in the Tower as closely related to the blood transfusion dispute, but the connections she makes are supported by little evidence. Citing no source for her claim that Oldenburg's foreign correspondence put him "at the top of [Lord] Arlington's list of traitors", Tucker goes on to explain that the case against Oldenburg was confirmed by his receipt of Denis' letter claiming French priority for blood transfusion, which precipitated his imprisonment in the Tower. Tucker acknowledges the theory that John Wilkins published No. 27 of the *Transactions* with Denis' letter in it while Oldenburg was in the Tower, but supplies a "more likely" theory (which apparently is her own) that "the issue was a counterfeit created for financial gain or as an effort to construct additional evidence against Oldenburg and the treason charges he faced." This account of a conspiracy to defame Oldenburg because he was somehow responsible for the contents of one of the hundreds of letters he received makes a number of unwarranted logical leaps and ignores both important details and more likely explanations. There is no basis for the suggestion that promoting a French priority claim was a treasonable offence. Nor is there any reason to believe that Oldenburg, whose job included corresponding with foreigners and who never let this interfere with his efforts to promote English priority,

Secretary of the Royal Society – published issue 27 of the *Philosophical Transactions* on 22 July/1 August 1667 which included Denis' letter.[47]

Oldenburg was released from the Tower in late August and soon wrote a letter to Boyle in which he remarked, "The transfusing Experiment, I find, grows famous".[48] Yet if he sounded pleased with the attention this English experiment was receiving, he was not pleased about the issue of the *Transactions* that had been published without his permission. Oldenburg published his own version of No. 27 without Denis' letter on 23 September/3 October.[49] In the first item in this issue, Oldenburg responded to Denis' claim that the French had been the first to conceive of blood transfusion ten years earlier. He told the readers of the *Transcations* that, if he had published the letter, "he should then have taken notice, as he doth now, of what is affirmed in that *Letter* about the *time* and *place* of the *Conception* of that Transfusing design; and intimated to the Curious, that how long soever that Experiment may have been *conceived* in other parts (which is needless to contest) it is notorious, that it had its *birth* first of all in *England*". It was Richard Lower who deserved credit both for inventing a specific method of blood transfusion and for carrying it out.[50] In response to Denis' effort to claim priority for the French, Oldenburg rejected Denis' definition of priority in favour of his own. It was not enough to conceive of an experiment; the honour of invention was due to he who

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would be suspected of favouring the French simply because he received Denis' letter. Finally, the lack of sources cited as evidence for an anti-Oldenburg conspiracy within the Royal Society suggest that this is nothing more than a rather unlikely speculation. (Tucker, *Blood Work*, 146-152).

47 Farr, "First Human Blood Transfusion," 146; Hall and Hall, "First Human Blood Transfusion," 464.

48 Oldenburg expressed a similar sentiment in his letter to Boyle of 10/20 December, in which he wrote that a transfusion that the Royal Society would soon perform "will doubtlesse have a crowd of spectators, it being discoursed off [*sic*] in most companies, one resorts to." In another letter a week later, Oldenburg reported that they had performed the transfusion "in the presence of a strange crowd both of forrainers and domesticks". In fact, Oldenburg wrote, the Royal Society had spent so much time on the subject of transfusion lately that they had to put off a number of other experiments. According to Oldenburg, transfusion was receiving considerable attention both within the Royal Society and among the public (OC *Oldenburg to Boyle 12 September 1667* III 474, *Oldenburg to Boyle 10 December 1667* IV 27, *Oldenburg to Boyle 17 December 1667* IV 59).

49 Farr, "First Human Blood Transfusion," 146. Oldenburg wrote to Boyle on 24 September/4 October that his version of No. 27 would "containe in the beginning an Animadversion on the English'd letter of Monsr Denys concerning the Transfusion: wch translation I disowne for mine, inscribing this Tract with Numb. 27; wch the Stationer too boldly presumed to give to that translated Letter." Oldenburg was apparently embarrassed that a letter printed in his journal would weaken a priority claim that the English were taking so seriously. He claimed that he had planned to print the letter, but he would have printed it along with the animadversions that he included in his version of No. 27 (OC *Oldenburg to Boyle 24 September 1667* III 480).

50 *Phil. Trans.* 27 (July-September 1667) 489-490.

performed it successfully.

Oldenburg's definition was *ad hoc*, and he did not apply it to each case of unclear priority. Of course, to a certain extent, it had to be *ad hoc*; not all of the Royal Society's priority disputes concerned experiments, and it would have been difficult to apply the same standards to disputes that did not. However, it is possible to generalize about Oldenburg's approach to assigning priority based on an *idea* or a *performance*; in this he was inconsistent. In the letter where he assured Boyle that he had not meant to question his priority in sounding depths when he published Hooke's new method in the *Transactions*, he spoke of priority in terms of ideas. He insisted that he had meant "that ye wayes, as they follow, were *contrived* by [Hooke]; not, yt he first invented ye *notion* of this practise".[51] The Clarke-De Graaf dispute primarily concerned who had first expressed the *idea* that the testes were composed of vessels in a clear way; the disputants – Oldenburg included – did not argue about who had first unraveled a testis and viewed the vessels, but rather who had first come to this mental realization. Oldenburg's public declaration of English priority appended to the *Testis examinatus* in the *Transactions* claimed that the structure of the testes had been "conceived, and hath been so many years agoe, by able Anatomists here in *England*".[52] It is difficult to determine whether these *ad hoc* definitions were formed more by the needs of the disputant or the needs of the situation – Oldenburg could not easily have applied the same criteria for priority to, say, an experiment and a mathematical proof – but it seems clear that the notions of both *idea* and *performance* played some role each time priority was defined. Despite these different definitions, disputants behaved remarkably similarly in each of these cases, employing proof from earlier texts, appeals to witnesses, and complaints of insult. All of this is further evidence that early modern priority disputes were a fundamentally social phenomenon, based on the perceptions of multiple discovery, plagiarism and threats to the honour of historical actors in social settings.

Perhaps recognizing the instability of such definitions, Oldenburg also questioned Denis' claim to priority on Denis' own terms. Oldenburg remarked that the Royal Society had heard a number of reports that transfusion had been tried elsewhere after they had

51 My emphasis. OC *Oldenburg to Boyle 17 March 1665/6* III 61.
52 Original emphasis. *Phil. Trans.* 42 (14 December 1668) 844.

announced it, "without hearing any thing then of its having been *conceived ten years ago*."[53] Even if one were to accept Denis' definition of priority, Oldenburg suggested, his claim would still be unsubstantiated. This was the first indication that Oldenburg was prepared to diversify his strategy to downplay the accomplishment of the French, which he further showed in the October 1667 issue of the *Transactions*.

The title of the first item in this issue announced its intention not only to add "*a farther* Vindication *of this* Invention *from Userpers*" but also to criticize "*its circumspect practise on* Man".[54] Oldenburg was aware that there was a vigorous debate about the utility and safety of performing transfusions on people in Paris, and may have seen this as an opportunity to disparage the transfusions that the French were performing.[55] Oldenburg cited a letter written by Gasper de Gurye de Montpoly who considered transfusion to be "a very Ingenious Invention" but recommended more caution in practicing it.[56] Although Denis' early transfusions had been successful, Montpoly explained that his third patient, the sickly Swedish baron Gustaf Bonde who was living in Paris, had died after receiving two transfusions. Oldenburg noted that Montpoly did not indicate how long the doctors waited before performing the second transfusion, but Oldenburg still assumed that the baron died because the transfusions were too close together.[57] Montpoly also reported that a dog had died during a transfusion, apparently from being given too much blood. Still, Montpoly claimed that the reputation of transfusion could be saved if physicians would learn more from careful observations of transfusions performed on animals; they should do so "before any thing be hazarded, that may damnify the publick, and depreciate the Invention."[58]

Oldenburg too recommended caution in proceeding from animal transfusion to human transfusion, and shrewdly turned this point into an argument in favour of English priority. Oldenburg admitted that "the *French* have advanced this Invention so far, as to

53 Original emphasis. *Phil. Trans.* 27 (July-September 1667) 490.
54 *Phil. Trans.* 28 (21 October 1667) 517.
55 See OC *Oldenburg to Williamson 5 October 1667* III 507, *Oldenburg to Boyle 8 October 1667* III 510.
56 *Phil. Trans.* 28 (21 October 1667) 518.
57 *Phil. Trans.* 28 (21 October 1667) 519-520. Oldenburg had already told Boyle about these events in a letter of 8/18 October and mentioned that he would soon "give a fuller account in publick". Evidently, Oldenburg was eager to make the French failure known (OC *Oldenburg to Boyle 8 October 1667* III 510).
58 *Phil. Trans.* 28 (21 October 1667) 520-521.

try it upon *Men*, before any *English* did it, and that with good success."[59] Oldenburg's strategy here was twofold. Firstly, he defined human transfusion as only an improvement or innovation of animal transfusion, which was an English invention. Secondly, he claimed that the English could have performed human transfusions before the French but had chosen not to; unlike the French, the English were "tender in hazarding the Life of Man (which they take so much pain to preserve and relieve) [and] scrupulous to incur the Penalties of the Law, which in *England*, is more strict and nice in cases of this concernment, than those of many other Nations are."[60] Oldenburg made this distinction between philosophical and practical conditions in England and France to show that it was a desirable quality that had caused the Royal Society' hesitation to perform a transfusion on a human patient. If it had not been for the value that the English placed on the preservation of human health, both in law and according to the morality of the Royal Society, the English would have had priority for human transfusion. As proof, Oldenburg offered himself as a witness that the English had been ready to perform such a transfusion "several Moneths agoe". He also transcribed a letter from Edmund King describing how transfusion could be performed on a human patient. This method could not have been borrowed from the French, Oldenburg noted, because Denis' letter did not describe how he and Emmerez had done it.[61] King's letter explained that "[w]*e have been ready for this Experiment this six Months, and wait for nothing but good opportunities, and the removal of some considerations of a Moral nature.*"[62]

At this point Oldenburg had apparently abandoned his position that priority for an experiment should be awarded according to who first performed it. Accordingly, when he explicitly defined priority in this item, his definition was confusing. Oldenburg wrote, "Surely, all ingenius men will acknowledge, that the *certain* way of decidiug [*sic*] such Controversies as these, is a Publick Record, either written or printed, declaring the time and place of an Invention first proposed, the contrivance of the Method, to practise it, and

59 *Phil. Trans.* 28 (21 October 1667) 521-522.
60 Original emphasis. *Phil. Trans.* 28 (21 October 1667) 522.
61 Oldenburg did not date this letter, and there is no such letter in the *Oldenburg Correspondence* (*Phil. Trans.* 28 [21 October 1667] 522-523).
62 *Phil. Trans.* 28 (21 October 1667) 522-523. Regarding the matter of priority for human transfusion, Walton claims that Oldenburg "reluctantly conceded to the French", but because this concession was part of Oldenburg's rhetorical strategy, it seems more likely that Oldenburg considered this a perfectly acceptable loss (Walton, "First Blood Transfusion," 361).

the instances of the success in the Execution. All this appears in the field for *England*." Unlike the French, Oldenburg noted, the English had a record in their register books of transfusion experiments being proposed in 1665; they had simply lacked a way to carry it out until Lower brought one to their attention in 1666.[63] Oldenburg made it clear that a textual record should be used to establish priority, but in this case, Oldenburg required that a record of both the idea and the performance of the experiment be used to make this decision. This does not allow for the possibility that an idea could be first proposed in one place and first executed in another; his way of establishing priority did not need such flexibility in this case because Oldenburg had defined human transfusion as nothing more than an extension of animal transfusion, for which the English clearly had priority. Oldenburg's shifting and imprecise definition of priority was customized to suit the particular needs of his claim of English priority.[64]

Of course, Oldenburg did not forget to downplay the importance he placed on securing English priority for transfusion. Oldenburg suggested that the Royal Society had a responsibility to reveal the truth of the matter: "We would have said no more of this Argument at this time, were we not obliged to remove a mistake found in one of the late *French* Journals, affirming with confidence, that 'tis *certain* the *French* have given the *English* the first *thought* or notion of this Experiment."[65] The reason for the continued escalation of the dispute, then, was the carelessness or dishonesty of the French in failing to recognize English priority, rather than the offence the English took as a result. Oldenburg concluded the item with a suggestion that the question of priority was

63 Oldenburg was suspicious of the claim that the French had made "so surprizing an Invention" but had failed to mention it until the Royal Society announced that they had made the same invention ten years later. He also pointed to a discrepancy between two French sources about which French anatomist had conceived of blood transfusion. Oldenburg, it seems, stopped just short of calling the French liars (*Phil. Trans.* 28 [21 October 1667] 524).

64 This lends credence to the claim of Mario Biagioli, who examines a dispute much earlier in the seventeenth century, before there was any practice comparable to recording discoveries and inventions in Royal Society's register books: the dispute between Galileo and Chrisoph Scheiner over the identification of sunspots. For Biagioli, "the absence of established protocols for the evaluation of priority claims – some saying that verbal communication and third-party witnesses were sufficient, others linking priority to publication – fueled bitter disputes and added fragmentation to an already fragmented natural philosophical field." In cases involving the Royal Society in the 1660s, it seems that the inconsistency of such protocols, despite the steps that Boyle and others took to standardize them, allowed priority disputes to drag on for months or years as disputants continued to insist on different ways to establish priority (Mario Biagioli, *Galileo's Instruments of Credit: Telescopes, Images, Secrecy* [Chicago: University of Chicago Press, 2006], 97).

65 *Phil. Trans.* 28 (21 October 1667) 523.

immaterial to the Royal Society; all parties should pool their efforts "for the service and relief of humane [*sic*] life, if [transfusion] be capable of it; and this is the main thing aimed at and sollicited in this Discourse; not written to offend or injure any, but to give every one his due".[66] Oldenburg's language was ambiguous here; he claimed that "this" was the purpose of this item, but "this" can be taken to refer back to the utilitarian advancement of medicine or forward to the proper distribution of credit. Whether or not this ambiguity was intentional, it reflects the contradiction in Oldenburg's position. His noble appeal for the French and English to work together to save lives seems insincere after pages of disparaging the French and giving credit to the English.[67]

At this point, Oldenburg's claim of English priority depended on the notion that the French had been reckless in proceeding too quickly from animal transfusion to human transfusion. Oldenburg had to continue to profess this position of caution in the *Transactions*, but he did not recommend as much caution in his correspondence. He also had not indicated any moral or legal reservations about human transfusions before his most recent vindication of English priority in the *Transactions*. He wrote to Boyle on 12/22 September, just after he was released from the Tower, and mentioned that John Beale had recommended some human patients for transfusion. He asked Boyle to consult with people who knew how the experiments had turned out in Paris, and to make a recommendation about Beale's proposal accordingly.[68] It seems, then, that the Royal Society's decision whether to try human transfusion depended not on moral and legal concerns, but rather on the results of the French experiments. On 23 November/3 December, in fact, Lower and King successfully performed a transfusion on a human, and Oldenburg showed no sense of caution in reporting it to Boyle. On the contrary, it was a source of pride:

> On Thursday next, God willing, a report will be made of ye good success of the first Tryall of Transfusion practised on a Man, wch was by order of ye Society, and the approbation of a number of Physitians, performed on Saturday last in Arundel house, in the presence of many spectators, among whom were Mr

66 *Phil. Trans.* 28 (21 October 1667) 524-525.
67 Walton writes that here Oldenburg "turned quickly from criticism to conciliation", but this interpretation seems somewhat credulous. It does not seem that Oldenburg was trying "to soothe the waters he had troubled"; rather, it seems that he was saving face with an empty gesture at the end of a blatantly and purposefully provocative article (Walton, "First Blood Transfusion," 362).
68 OC *Oldenburg to Boyle 12 September 1667* III 474.

> Howard and his sons, the Bp of Salisbury, 4. or 5. physitians, some Parlement men, etc. by ye management and operation of Dr Lower and Dr King, ye latter of whom performed the chief part wth great dexterity, and wth so much ease to the patient, that he made not the least complaint, nor so much as any grimace during the whole time of ye operation[69]

The patient was willing, the doctors were skillful, and the experiment was successful. The Society paid the patient, a madman named Arthur Coga, a guinea for his participation, and Oldenburg suspected that the Fellows could persuade him with another guinea to receive a second transfusion "before the end of this very week, if circumstances shall persuade it."[70] Evidently Oldenburg's tone of reproach in October's *Transactions*, in which he speculated that the Swedish baron had died because his second transfusion was too soon after the first, was merely rhetorical.[71]

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69 OC *Oldenburg to Boyle 25 November 1667* III 611.

70 OC *Oldenburg to Boyle 25 November 1667* III 611. King also wrote to Boyle on the same day to tell him about the transfusion, and noted that Coga "is very willing to have it repeated (we gave him a guinea for his willingness) for his arm, he says, is well" (WB *King to Boyle 25 November 1667* VI 646-647). Coga indeed received another transfusion in front of a crowd of spectators who, as Tucker notes, complained that King did not transfuse as much blood as he said he did. Tucker explains that the crowd may have been correct, citing several reasons why there might have been little or no blood transfused into Coga's body, as well as the bodies of the other English and French transfusion patients. She adds that this could explain why many transfusion patients remained healthy after undergoing the risky procedure, for whom modern medicine would predict serious complications. The small amount of blood they did receive could account for the mild reactions such as fevers that these patients who emerged relatively healthy from their transfusions experienced. Tucker explains that Coga was a valuable patient because, although he was mentally ill, he had a Cambridge education and thus could give his surgeons a useful description of what happened to him as a result of receiving blood transfusions. However, after surviving two transfusions without serious complications, Coga refused to receive a third transfusion because he was concerned that he was turning into a sheep. Tucker suggests that Coga made this claim "at the dictation of adversaries of the Royal Society", citing evidence of efforts to turn Coga against the Society. However, Tucker suggests no reason why the Society's enemies would supply Coga with this peculiar and very specific reason for refusing another transfusion, and it should be remembered that he was being given transfusions to treat his madness (Tucker, *Blood Work*, 162-169. The quotation is from p. 169).

71 Oldenburg's subsequent letters to his contacts also reflect a lack of caution. For example, when he mentioned the Society's experiments with human transfusion to René Sluse, he wrote, "Surely this experiment may be most beneficial to humanity". He wrote again to Boyle on 3/13 December and told him that Coga had not only recovered from the transfusion but was in better health than before. Oldenburg added that although transfusion "met with so much difficulty and contradiction at first . . . [it] may at last prove very beneficiall to the Health of Men." Other members of the Royal Society, like William Neile, were just as eager to proceed with human transfusion. Neile suggested that it could be beneficial to transfuse chyle as well. However, Oldenburg did occasionally express the need for caution when writing to French correspondents such as Henri Justel. Justel replied to a letter from Oldenburg on 18/28 March 1667/8, writing, "You are right in saying that Mr. Denis was a little too hasty and insufficiently circumspect." It seems that Oldenburg felt compelled to convey the Society's attitude of caution only when writing to one of his French contacts (OC *Oldenburg to Sluse 25 November 1667* III 617, *Oldenburg to Boyle 3 December 1667* IV 6, *Neile to Oldenburg 5 December 1667* IV 9, *Neile to Oldenburg 15 December 1667* IV 54-55 *Justel to Oldenburg 18 March 1667/8* IV 257. See also OC *Oldenburg to Boyle 10 December 1667* IV 27, *Oldenburg to Boyle 17 December 1667* IV 59).

In the *Transactions*, however, Oldenburg continued to stress the need for caution, even when the English had started to perform transfusions on human patients. As he described the transfusion performed on Coga in the December issue, Oldenburg claimed that Coga "urged us to have the Experiment repeated on him within 3 or 4 days after this; but it was thought advisable, to put it off somewhat longer."[72] Whereas his letter to Boyle indicated that the Society would be able to convince Coga to undergo another transfusion if they were so inclined, now Oldenburg suggested that they had refused Coga's offer in the interest of caution. In the next item in the *Transactions*, Oldenburg gave a fuller account of the death of Gustaf Bonde, the Swedish baron who had received two calf's blood transfusions in Paris. Denis himself sent this account to Oldenburg, perhaps in an effort to explain why he and Emmerez should not be blamed, but in Oldenburg's relation of the events, the French doctors still appeared somewhat reckless. At first, Oldenburg explained, they were reluctant to give Bonde a transfusion because he seemed too sick for it, but Denis and Emmerez were convinced by promises that they would not be held accountable: "They, being overcome by this importunity, and having secured their honour and safety, by the declaration above mentioned of the Physicians, and by their consent to the trial of this Experiment, transfused into his Veins a small quantity of *Calves* blood". The patient initially seemed better, but his condition worsened about twenty-four hours later. Denis and Emmerez agreed to perform a second transfusion the next morning, after which the patient died at around 5:00 in the evening.[73] The details of this account (as Oldenburg reported them) do suggest that Denis and Emmerez had learned to tone down

72 *Phil. Trans.* 30 (9 December 1667) 557-558.

73 *Phil. Trans*. 30 (9 December 1667) 559, 563. Calf's blood was chosen for its supposed calming qualities. Denis wrote that he and Emmerez thought that "the blood of a Calf by its mildness and freshness might possibly allay the heat and ebullition of his blood of his blood, being mixed therewith." Evidently Denis and Emmerez believed that a calf was calm because of its blood, and the influence of this blood could therefore cure madness in a human. Similarly, Oldenburg wrote to Denis that calf's blood might have been a better choice for Arthur Coga than the lamb's blood he was given "since it is the more benign." Yet lamb's blood had its advantages: Coga himself said that he appreciated being given the blood of an animal that represented Christ (OC Oldenburg to Denis 23 December 1667 IV 77; *Phil. Trans.* 32 [10 February 1667/8] 620; Tucker, *Blood Work*, 165. See also Greenwalt, "History of Transfusion Medicine," 553). Tucker gives examples of the tradition in which Denis and his contemporaries were operating which associated cures to particular diseases with the bodily fluids or flesh of particular animals. For instance, some seventeenth-century sources claimed that the organs of a rooster would cure consumption, and that fox livers would improve the health of people with diabetes. One French commentator, Guillaume Lamy, feared that animal blood would transfer unwanted qualities to transfusion patients. Just as it was something in their blood that gave some animals horns, it was something in their blood that made cows stupid, so a person transfused with calf's blood might become stupid as well (Tucker, *Blood Work*, 133-134, 180-181).

their enthusiasm for transfusion, at least in a retrospective account about a patient who had died. They noted that they had performed the experiment at the request of the patient's relatives and the insistence of his physicians.[74] Yet the juxtaposition of this account, in which French doctors had agreed to perform the transfusion against their better judgment, and the previous account, in which English doctors had stuck to their decision to deny a patient a transfusion because they considered it unsafe, does not seem accidental. Oldenburg concluded his account of the death of the French patient with a reference to his warning about performing human transfusions rashly in No. 28. If the reader was to read this account and then look back at the comments Oldenburg had made two issues earlier, the implication would be that Oldenburg was right to urge caution, as opposed to the reckless French doctors.

Oldenburg gave Denis and Emmerez a chance to redeem themselves by printing a letter with Denis' account of the transfusions performed on a madman named Antoine Mauroy who had survived two transfusions.[75] There were few signs of caution regarding human transfusion in Denis' account; the French had "let slip no occasion to improve this Operation".[76] When he and Emmerez were approached about giving the patient a transfusion of calf's blood to cure his madness, they replied that they were not sure if transfusion could cure him, but it was unlikely to cause any harm as "this Operation was in it self not capable to cause the death of any one, if discreetly managed."[77] When the first transfusion briefly made the patient feel better, Denis decided that "by reiterating the Transfusion once or twice, we might find a more remarkable change in him."[78] During the operation the patient felt heat and pain and asked for the operation to stop; he then immediately vomited and later began to pass black urine. However, three days later he had recovered and his madness seemed to have been cured. Denis felt that the patient's condition would improve even more if they performed a third transfusion, but he decided that this was unnecessary when he saw how much the patient's health continued to improve.[79] He concluded the letter with a promise to perform more human transfusions

74 *Phil. Trans.* 30 (9 December 1667) 562-563.
75 *Phil. Trans.* 32 (10 February 1667/8) 618.
76 *Phil. Trans.* 32 (10 February 1667/8) 617.
77 *Phil. Trans.* 32 (10 February 1667/8) 620.
78 *Phil. Trans.* 32 (10 February 1667/8) 620-621.
79 *Phil. Trans.* 32 (10 February 1667/8) 621-622.

and to report what happened to Oldenburg. In Denis' view, there was still no evidence that transfusion was dangerous if it was performed carefully and the patient was properly prepared.[80] Neither the patient's initial adverse reaction nor the death of Gustaf Bonde deterred Denis' advocacy of human transfusion. Oldenburg made only slight modifications to the letter before printing it in the *Transactions*,[81] and he appended none of his own comments to the publication. Perhaps he felt that Denis' recklessness spoke for itself.

When Oldenburg wrote to Denis about this experiment he explicitly reversed the position on human transfusion he had taken in the *Transactions*. He wrote that Denis and Emmerez

> are indeed gallants, to woo your mistress with such boldness and importunity, being well aware that this is the surest and most powerful way to win her. In this fashion, it seems to me, you succeed by caressing nature, where many others proceed with *too much hesitation and scruple*. We have only twice performed a blood transfusion on a man, who is still very well in body although the wildness of his mind remains unchanged . . . For the rest, his complexion is fresher and handsomer since these two operations than it was before them. Possibly calf's blood would have been more appropriate to this man's condition than sheep's blood, since it is the more benign.[82]

Oldenburg went on explain that the Society would attempt transfusion on many more human patients in an effort to cure a variety of diseases. He wished that they could reduce the risk to human life by infecting animals with these diseases and then attempting to cure them with transfusions, but this was impossible.[83] Here Oldenburg praised the French for the same quality for which he had criticized them in the *Transactions*: an unwavering dedication to a procedure that many considered dangerous. When the scope of the dispute expanded to include more than issues of priority, Oldenburg belied his impatience with the slow progress of human transfusion in England, apparently as a rhetorical strategy to diminish the reputation of the French doctors who were performing transfusions. Oldenburg may have felt that the cautious position he expressed publicly was no longer tenable anyway, since the Royal Society had begun to perform their own transfusions on

80 *Phil. Trans.* 32 (10 February 1667/8) 623.
81 See OC *Denis to Oldenburg 13 December 1667* IV 48.
82 My emphasis. OC *Oldenburg to Denis 23 December 1667* IV 77.
83 OC *Oldenburg to Denis 23 December 1667* IV 77.

humans; to continue to condemn the procedure entirely would have appeared contradictory. Alternatively, Oldenburg may have sought to exchange the praise he gave in this letter for information: he concluded by asking Denis to keep him apprised about the transfusion patient discussed in Denis' letter.[84] Oldenburg's letters generally reveal that he was genuinely excited when doctors on the Continent performed transfusions because it would allow the Royal Society to learn from their results. As usual, the line between competition and collaboration was thin because natural philosophers valued both the honour of priority and information from abroad. At this point, Oldenburg had not yet found out that the transfusion had failed to cure the patient's madness; if it had been successful, the Royal Society would have considered it a significant accomplishment.[85] In either case, Oldenburg showed that he could adapt his opinion of human transfusion to suit the needs of a particular situation.

As transfusion continued to receive more attention across Europe, its reception straddled fame and infamy. Oldenburg told Boyle on 17/27 December that the recent human transfusion the Society had performed was attended by "a strange crowd both of forrainers and domesticks".[86] Sluse wrote to Oldenburg on 30 January/9 February that transfusion "is now practised almost everywhere, and I have heard there was lately an English physician prescribing it in two cases of illness; but the results were negative or, rather, adverse."[87] Oldenburg also received reports of transfusions performed in Rome.[88] Henri Justel told Oldenburg in a letter of 5/15 February that Denis' patient Mauroy was dead; he blamed Denis' impatience.[89] Denis was tried for murder over the death of this

84 OC *Oldenburg to Denis 23 December 1667* IV 77.

85 After Oldenburg wrote to Boyle to tell him about the "surprising success" of the experiment reported by Denis, Boyle replied, "The experiment of transfusion, made upon a mad man, I shall look upon, if the cure hold, as a very considerable thing, and capable of improvements." However, Oldenburg found out from letters from Justel written on 25 December/4 January and 29 January/8 February that the transfusion had failed to cure the man's madness. It is curious that Oldenburg did not include this information when he printed his translation of Denis' letter in the *Transactions* in February. As stated above, at this point he may no longer have considered it prudent to criticize human transfusions, as the English had started to perform them as well (OC *Oldenburg to Boyle 24 December 1667* IV 78-79. *Justel to Oldenburg 25 December 1667* IV 86, *Boyle to Oldenburg 29 December 1667* IV 94, *Justel to Oldenburg 29 January 1667/8* IV 129).

86 OC *Oldenburg to Boyle 17 December 1667* IV 59.

87 OC *Sluse to Oldenburg 30 January 1667/8* IV 132.

88 Oldenburg heard this from Justel, and later reported it to Sluse, remarking that he "anxiously await[ed] news of its success" (OC *Justel to Oldenburg 12 February 1667/8* IV 175, *Oldenburg to Sluse 26 February 1667/8* IV 212, *Oldenburg to Sluse 19 March 1667/8* 266).

89 OC *Justel to Oldenburg 5 February 1667/8* IV 149. See also OC *Justel to Oldenburg 15 February* IV 177, *Justel to Oldenburg 19 February* IV 190, *Justel to Oldenburg 21 February* IV 195-196.

patient, and although the court found that he had died because he was poisoned by his wife, the ruling stipulated that transfusion would thereafter only be legal with the permission of the Medical Faculty of Paris. By 1679, the court had prohibited transfusion entirely.[90]

This did not prevent either English or French natural philosophers from continuing to claim priority for their countrymen. In the same letter, Justel told Oldenburg that the *Journal des sçavans* would soon publish an item attributing transfusion to the Cartesian Benedictine monk Robert Desgabets whose lecture on transfusion in 1658 was the basis for Denis' claim of French priority in his printed letter of 25 June 1667.[91] The editor of the *Journal* had also just published a long item about transfusion but it focused on the safety and utility of the experiment rather than on priority.[92] Denis soon published a letter to Samuel de Sorbière that was printed as a pamphlet, which, as Patricia Easton notes, included the first publication of Desgabets' lecture.[93] Justel and Denis each sent copies of this pamphlet to Oldenburg on 11/21 and 12/22 March, respectively. Denis claimed that the letter described "the Origin of Transfusion, as we understand it in France."[94] Meanwhile, as Oldenburg advertised England's transfusion experiments to his correspondents, he made sure to continue to emphasize English priority.[95]

90 Tucker, *Blood Work*, xix, 205-210; Walton, "First Blood Transfusion," 362, 364, n. 18. Denis spent most of the rest of his career lecturing in Paris, but he did make a notable medical invention, styptic, before his death in 1704. Again, Denis had to defend both his priority and the utility of his invention (*Phil. Trans.* 94 [19 May 1673] 6039, 95 [23 June 1673] 6052-6059, 96 [21 July 1673] 6078-6079, 97 [6 October 1673] 6115; Tucker, *Blood Work*, 221-222).

91 OC *Justel to Oldenburg 5 February 1667/8* IV 149.

92 The item transcribed and discussed a number of letters expressing opinions both for and against human transfusion, as well as a report from the *Philosophical Transactions* about the Royal Society's experiments (*Journal des sçavans* [6 February 1667/8]).

93 Easton, "Robert Desgabets," 1, 7.

94 While Denis defended French priority, he also had to defend transfusion itself from the criticism that followed the deaths of his patients; he had heard a rumour that transfusion was in as much trouble in England as it was in France, and asked Oldenburg whether this was true. Despite the priority dispute, he may have been looking for English allies in the defence of the practice of human transfusion (OC *Justel to Oldenburg 11 March 1667/8* IV 245, *Denis to Oldenburg 12 March 1667/8* IV 247).

95 For example, in a letter to Johann Christoph Beckman of 30 March/9 April, Oldenburg subtly emphasized English priority: "No doubt the report of that remarkable experiment in which blood is transfused from one animal into another through the arteries and veins, *which we first thought* of, has already reached you. It was attempted several times on animals; afterwards there was one attempt on a man here in London, and more in Paris which met with varying success, many of them being not merely harmless, however, but actually beneficial" (my emphasis). Significantly, Oldenburg described transfusion as a transfer of blood between two animals, and not an animal and a human, because this definition favoured Lower's priority (OC *Oldenburg to Beckman 30 March 1668* IV 280. See also OC *Travagino to Oldenburg 21 April 1668* IV 328-329, in which Francisco Travagino described transfusion as an English

The next full-scale argument for English priority came not from Oldenburg, but from Timothy Clarke. Clarke wrote a letter to Oldenburg in the spring of 1668 which Oldenburg published in Latin in the May issue of the *Philosophical Transactions.* This was the same letter which upset De Graaf by denying his priority, and the style resembles that with which Clarke wrote throughout the dispute over the substance of the testes.[96] From the outset Clarke wrote in his usual polemical style denouncing those who "disparage the praise given to others, blacken their names, and dare to seize for themselves others' property as though it were their own." These people, he wrote, were motivated by anger and jealousy of others' fame. Clarke insisted that natural philosophers should seek a reputation based on "the consciousness of right-doing" and a commitment to knowledge rather than on recognition for priority.[97] Clarke seems to have barely concealed an accusation of the French when he sarcastically condemned those who, "when they happen to hear of some new discovery, straightaway flourishing their fluent pens . . . try to make the world believe that they alone were its authors or at least that they first had some idea of if, but could not bring it to light because of the hindrance of other business (of great moment, no doubt)."[98]

For Clarke, multiple discoveries and inventions were possible and resulted from luck, but luck generally favoured those who worked the hardest, so it was proper to recognize well-deserved priority. As an example of multiple discovery, Clarke cited the discovery of the lymphatic system by Olof Rudbeck and Thomas Bartholin, who were Swedish and Danish anatomists respectively. He neglected to mention that this case of multiple discovery led to a nasty priority dispute between Rudbeck and Bartholin in the 1650s.[99] Clarke pointed out that neither of them had realized that they had been preceded

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discovery of which they should be proud).

96 Clarke's letter was a generalized complaint about several recent threats to what he considered obvious cases of English priority. In the *Transactions* it was entitled "A Letter, written to the Publisher by the Learned and Experienced Dr *Timothy Clarck* one of His Majesties Physitians in Ordinary, concerning some Anatmoical Inventions and Obervations, particularly the Origin of the Injection into Veins, the Transfusion of Bloud, and the Parts of Generation" (OC *Clarke to Oldenburg April/May 1668* IV 360-367; *Phil. Trans.* 35 [18 May 1668] 672-682).

97 OC *Clarke to Oldenburg April/May 1668* IV 360.

98 OC *Clarke to Oldenburg April/May 1668* IV 361.

99 See Charles T. Ambrose, "Immunology's First Priority Dispute: An Account of the 17th-Century Rudbeck-Bartholin Feud," *Celluluar Immunology* 242 (2006): 1-8; Charles T. Ambrose, "Rudbeck's Complaint: A 17th-Century Latin Letter Relating to Basic Immunology," *Scandinavian Journal of Immunology* 66 (2007): 486-493.

by an Englishman, George Joyliffe, in this discovery. Joyliffe was a great example of how one should conduct himself in cases of multiple discovery:

> He himself, so long as he lived, considered it unworthy in either a prudent man or a learned physician to dispute over chance discoveries, feeling no envy because such learned men were partners in his good fortune. He freely acknowledged that their industry and luck were deserving of praise from all sides, especially as there could be no breath of suspicion that either of them would have secretly appropriated another's discovery.[100]

The beginning of Clarke's letter functioned as a set of instructions for avoiding priority disputes by respecting the priority of others and not seeking undue priority for oneself. As in the dispute with De Graaf, this was a handy argument for Clarke as it allowed him to insist that anyone who challenged the priority of a member of the Royal Society was simply failing to adhere to these two rules.

Evidently Clarke applied these rules selectively, which became apparent when he began to defend English priority for blood transfusion. Because of the inherent credibility of the Royal Society's members, Clarke suggested, they were champions of knowledge and true priority whereas their opponents were vainglorious opportunists, by whom the English, as pioneers in natural philosophy, had been most often victimized. Jealousy also motivated those who tried to deprive the English of discoveries in other ways, such as the people who denied circulation when Harvey had clearly shown that it exists.[101] Such people were a danger to the credibility of the "the world of learning" itself.[102] Clarke's insistence on the importance of collaboration in natural philosophy, as opposed to the self-serving actions of these opportunists, recalls Oldenburg's preference for cooperation among natural philosophers across Europe as long as it did not infringe on English priority.

Clarke claimed that the English cleric Francis Potter had first proposed transfusion thirty years earlier as a logical consequence of Harvey's work on circulation.[103] It will be seen below that Richard Lower also traced the influences of his invention of transfusion back to Harvey. By placing transfusion in this tradition, Clarke

100 OC *Clarke to Oldenburg April/May 1668* IV 361.
101 Clarke referred here to Martin Bogdan who, as Ambrose explains, was instrumental in setting off the Rudbeck-Bartholin priority dispute (Ambrose, "Immunology's First Priority Dispute," 6).
102 OC *Clarke to Oldenburg April/May 1668* IV 362-363.
103 OC *Clarke to Oldenburg April/May 1668* IV 364.

and Lower implied complete English priority for transfusion: it was an English experiment, which was based on an English idea, which in turn was based on a major English discovery. For Clarke the steps in between were also English: Christopher Wren conceived of and performed the first injections of liquids into animals' veins around 1656. He told Clarke about these experiments in 1657 while Clarke was working on blood; this inspired Clarke to consider the idea of blood transfusion. Clarke's attempts at transfusion were unsuccessful, as was every subsequent attempt until Lower's, the priority for which was established in the *Philosophical Transactions* in December 1666. Then, in 1667, he and the rest of the Royal Society heard that the French had performed transfusions on humans.[104]

In Clarke's selective history of transfusion, it was an exclusively English invention to which the French made an innovation that the English could have made, if not for their "timidity or sloth." Denis and Emmerez's innovation was welcome, then, but an innovation was all it was. Clarke claimed to be surprised that "that learned Frenchman [Denis] disputes so vigorously and so warmly over the origin of blood transfusion"; it should be obvious that the French only added to a body of English work.[105] According to Clarke's definition, blood transfusion was simply the logical conclusion of intravenous injection. The confusion over priority, he suggested, had resulted from an error in the *Philosophical Transactions*, which placed the first experiment with intravenous injection in 1659. This let the French claim priority through Desgabets' 1658 lecture on transfusion. The *Transactions* were mistaken, however; Clarke explained that the idea of intravenous injection was decades old and that injection experiments had begun in England in 1657, a year before Desgabets' lecture.[106] In any case, Lower's transfusion was announced in publication before any such French publication. Essentially, whether his opponents defined priority in terms of ideas, performance, or publication, Clarke claimed it for the English.

104 OC *Clarke to Oldenburg April/May 1668* IV 364-365.
105 OC *Clarke to Oldenburg April/May 1668* IV 365.
106 Clarke said that this was Oldenburg's error, but the editors of the *Correspondence* note that the words from the *Transactions* to which Clarke referred were not, in fact, Oldenburg's. Clarke did Oldenburg another disservice when he claimed to be "the first to vindicate our claim against theirs." He may have been the first to do so in the wake of Denis' latest publication, but this claim neglected Oldenburg's earlier arguments for English priority in the *Transactions* (OC *Clarke to Oldenburg April/May 1668* IV 365-366).

Clarke concluded with a familiarly contradictory attitude toward the honour of priority. He set himself up as an exemplar of indifference: "I am so little inclined to snatch praises due to others, or to seize for myself or my fellow-countrymen what belong to foreigners, that I would rather yield my own to others. But if I had in my possession some brilliant discovery made by someone now dead and known to no one, I would surely render the dead man his due, and set appropriate honors upon his name." Nevertheless, he followed this with a "bold assertion" that transfusion was an English invention and declared that he would never change his mind on the matter.[107] Evidently Clarke considered the authority he commanded as an important member of the Royal Society to be proof that his priority claims were accurate and those of his opponents were lies or errors.

3.3: The Resolution of the Blood Transfusion Dispute

Denis wrote to Oldenburg about transfusion again on 5/15 May 1668, and his letter was printed in the *Transactions* in June, but at this point Denis apparently had little time to debate priority; this letter described his efforts to keep transfusion legal in France after the death of Antoine Mauroy.[108] As Walton notes, Denis' legal troubles essentially brought the priority dispute to an end; his letters from this point on were more concerned with winning allies in his defence of transfusion than with claiming French priority.[109]

107 OC *Clarke to Oldenburg April/May 1668* IV 366.

108 OC *Denis to Oldenburg 5 May 1668* IV 385-387; *Phil. Trans.* 36 (15 June 1668) 710-715. See also OC *Denis to Oldenburg 18 February 1668/9* V 415.

109 Walton suggests that another reason why the dispute settled down was that Pauli Manfredi had revealed that the German natural philosopher Andreas Libavius had described transfusion as early as 1614. This suggestion is dubious. Walton neglects the fact that there was no consensus that conceiving of transfusion was sufficient to claim priority for it and, as Greenwalt notes, it is unlikely that Libavius ever attempted an actual transfusion. Additionally, in England at least, the suggestion that Libavius should be given credit did not prove to be persuasive. In the *Philosophical Transactions* of July 1668 Oldenburg admitted that Libavius had clearly conceived of transfusion in his *Defensione syntagmatis* (1615), but according to Oldenburg, "*Libavius* did not propose it but only to mock at it (which is the common fate of new Inventions, in their Cradle;) besides that he contrives it with great danger, both to the *Recipient* and E*mittent*, by proposing to open *Arteries* in both; which indeed may be practised upon *Brutes*, but ought by no means upon *Man.*" For Oldenburg, Libavius did not propose it seriously and his was not a viable method; as such it was not really a precursor to Lower's success with transfusion. In a letter to Oldenburg of 3/13 August, Wallis agreed with Oldenburg that Libavius' idea was nothing more than "a ridiculous fancy; not as if any such thing were then practised, or attempted; nor proposed in order to any experiment to be made." In short, the evidence of Libavius' idea was not the deterrent to English claims to priority that Walton assumes they were, at least among vocal proponents of English priority in the late 1660s like

Perhaps the notoriety that transfusion had earned caused other French natural philosophers to admit English priority. A letter from one of Oldenburg's French correspondents, Jean Pierre de Martel, in April 1669 indicated that transfusion was "first proposed in England".[110] The resolution of the transfusion dispute may in the end have been a pragmatic one.[111]

Another factor could have been that the Royal Society's excitement about blood transfusion had declined in favour of newer subjects. After Denis' letter that was printed in June 1668 and another short item about priority for transfusion in July,[112] the *Philosophical Transactions* did not mention transfusion again until December. There were only two items that mentioned transfusion in the *Transactions* in 1669.[113] Certainly, the Society continued to experiment with transfusion, but it did not seem to command the attention of the Fellows to the same extent as before by the end of the 1660s. In the minutes of the Royal Society meetings from the first half of 1669, which Oldenburg recorded, transfusion was consistently present, but so was another subject for which Oldenburg's letters began to express as much enthusiasm as they had about transfusion in 1667 and early 1668: laws of motion.[114]

Four Fellows – Christopher Wren, John Wallis, William Neile, and Christiaan Huygens – contributed their theories concerning laws of motion at the request of the Society. Oldenburg wrote to Wren on 6/16 November 1668 to tell him to have his theory registered in order "to insure to yourself & the nation ye honour of yt invention."[115] Less than two weeks later Oldenburg wrote to Huygens to ask for his theory, assuring him that "everything you are good enough to communicate to us upon this subject will be at once

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Oldenburg and Wallis (Greenwalt, "History of Transfusion Medicine," 551; OC *Wallis to Oldenburg 3 August 1668* V 4; *Phil. Trans.* 37 [13 July 1668] 731-732; Walton, "First Blood Transfusion," 362).

110 For Martel the issue of priority was apparently resolved, and he did not mention Libavius as Walton's claim (see n. 109 above) would lead one to expect (OC *Martel to Oldenburg 11 April 1669* V 482-483).

111 However, Tucker notes that blood transfusion had critics in England as well. For example, Thomas Shadwell's *Virtuoso*, a play that satirized the Royal Society (to which Robert Hooke took personal offence), joked that the transfusion experiments would turn people into sheep. Still, Tucker concludes that the moral backlash to blood transfusion was not nearly as strong in England as it was in France (Tucker, *Blood Work*, 169-170. See also Everett L. Jones, "Robert Hooke and the Virtuoso." *Modern Language Notes* 66 [1951]: 180-182).

112 This was the item about Libavius. See n. 109 above.

113 *Phil. Trans.* 42 (14 December 1668) 840-841, 45 (25 March 1669) 897, 54 (13 December 1669) 1075-1077.

114 See HRS 347-362.

115 OC *Oldenburg to Wren 6 November 1668* V 134-135.

inserted in our registers, by my own particular attention, so that you will not be deprived of the honor which is due to you."[116] Wallis and Neile also repeatedly exchanged letters about motion with Oldenburg in late 1668 and early 1669.[117] Motion appears to have risen to the forefront in Oldenburg's mind by 1669. In mentioning it to Sluse, he wrote that the Society members "eagerly await[ed]" the results of their experiments testing the various theories of motion; in other letters he referred to "yt important subject of motion" which he thought was "as delicate as it is important."[118]

It was not long before those who had contributed their theories, especially Huygens, became concerned about priority, a subject about which Huygens was always sensitive. Finding the security of the registry system to be insufficient, he sent ideas to Oldenburg in the form of anagrams or ciphers whose solutions he would reveal when he was ready to announce a discovery.[119] He became upset when Oldenburg published Wallis's and Wren's theories of motion in the *Transactions* without mentioning his.[120] Fearing that his work would be considered derivative if he waited to publish it, Huygens quickly submitted a letter describing his theory to the *Journal des sçavans*.[121] In the letter

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116 OC *Oldenburg to Huygens 18 November 1668* V 178. See also *Oldenburg to Huygens 11 January 1668/9* V 332.

117 See, for example, OC *Wallis to Oldenburg 15 Novermber 1668* V 170, *Wallis to Oldenburg 19 November 1668* V 193, *Wallis to Oldenburg 5 December 1668* V 220-221, *Neile to Oldenburg 18 December 1668* V 263-264, *Wallis to Oldenburg 21 December 1668* V 272-273, *Neile to Oldenburg 28 December* V 286-287, *Oldenburg to Wallis 29 December 1668* V 287, *Wallis to Oldenburg 31 December 1668* V 287-288.

118 OC *Oldenburg to Finch 14 January 1669* V 339, *Oldenburg to Sluse 26 January 1668/9* V 359-360, *Oldenburg to Huygens 4 February 1668/9* V 373.

119 Huygens sent Oldenburg the following cipher on 27 Janaury 1669:

a b c d e h i l m n o p r s t u y
5 2 2 1 4 1 2 3 3 1 3 2 2 3 2 4 1

The editors of the *Oldenburg Correspondence* explain that the solution is: "*Lens e duabus composita hyperbolicam aemulatur*", which means "A lens made from two [elements] rivals a hyperbolic [lens]" (this translation is theirs). Jan A. van Maanen notes that Hendrick van Heuraet, a Dutch mathematican with whom Huygens narrowly avoided a priority dispute, made fun of Huygens' use of anagrams in 1658. Nevertheless, most of the Royal Society approved of Huygens' method of adding a layer of security to the register books, and Wren created his own cipher which Oldenburg sent to Huygens:

a b c d e f g h i l m n o p r s t u x y
7 4 4 4 12 1 3 6 10 7 1 7 9 2 6 2 7 4 1 1

The editors of the *Correspondence* do not know of a solution to Wren's cipher (OC *Huygens to Oldenburg 27 January 1668/9* V 362-363, *Oldenburg to Huygens 4 February 1668/9* V 375; Jan A. van Maanen, "Hendrick van Heuraet [1634-1660?]: His Life and Mathematical Work," *Centaurus* 27.3 [1984], 240). The use of ciphers and anagrams to protect priority will be addressed further in Chapter 4 below.

120 *Phil. Trans.* 43 (11 January 1668/9) 864-868.

121 The editors of the *Oldenburg Correspondence* explain this in a note of OC *Oldenburg to Huygens 4 February 1668/9* V 375. See also *Huygens to Oldenburg 20 March 1668/9* V 452-453.

Huygens explained that the similarities between his theory and those printed in the *Transactions* were coincidental; none of his work was "borrowed".[122] Huygens came close to turning the matter into a priority dispute and might have done if Oldenburg had not printed Huygens' theory in a later issue of the *Transactions* and explained that he had received it around the same time as Wallis's and Wren's.[123] Like transfusion in the previous two years, the laws of motion became a highly valued invention within the Royal Society, and the parties involved began to struggle to maintain their priority; as such, transfusion was largely supplanted.

Yet one participant in the transfusion dispute still felt that he needed to defend his priority. In terms of publications, Richard Lower had been quiet in the preceding years, but he asserted his priority vehemently when the publication of his *Tractatus de corde* gave him an opportunity in 1669. This was not the first time that Lower had engaged in polemics in a dispute between English and non-English natural philosophers. Much like Kepler advanced his career by defending Tycho in his priority dispute with Ursus,[124] Lower wrote his *Vindicatio* (1665) in support of his friend, the highly respected physician Thomas Willis,[125] against the Irish Galenist Edmund O'Meara.[126] For J. H. Felts, the *Vindicatio* is evidence that Lower was "an expert polemicist in an age when the diatribe approached art."[127] In the dedicatory letter to Boyle, Lower adopted a familiar rhetorical strategy regarding disputes. He wrote, "I am well aware that you little relish these public disputes and I think you know how unwelcome to me too this task has fallen, even how much my spirit abhors abuse and quarrels. . . . For I do not launch, but only answer, the

122 That is, "emprunté". Huygens also offered to produce proof of this if there was any doubt (*Journal des sçavans* 18 March 1669 22-24).

123 *Phil. Trans.* 46 (12 April 1669) 925-928.

124 See Chapter 2, pp. 41-42 above.

125 Felts explains that Lower inherited a practice and a position at court from Willis; presumably Lower valued Willis' favour highly. Tubbs, et al. note that Lower, an outspoken Whig, lost this court position during the reign of James II (Felts, "Richard Lower," 423; Tubbs, et al., "Richard Lower," 17).

126 According to Kenneth Dewhurst, the editor and translator of the *Vindicatio*, Willis was popular in part because he ignored the rule at Oxford that professors had to give two lectures on Aristotle every week. Willis was a proponent of the experimental method who based his innovative ideas on Harvey's work on circulation. None of this appealed to O'Meara who insisted on the primacy of ancient medical authorities and rejected what he felt to be dangerous innovations like experimentation, Harvey's notion of circulation, and anything derived from the latter. As such, O'Meara criticized Willis with what Dewhurst calls "a sarcastic, and occasionally outright offensive, attitude" (Kenneth Dewhurst, trans. *Richard Lower's* Vindicatio*: A Defence of the Experimental Method* [Oxford: Sandford Publications, 1983], xiii-xxvii).

127 Felts, "Richard Lower," 421.

attack, and with no weapons other than O'Meara's own, turned back on him". Like Boyle himself, Lower claimed to be an advocate of unity and he stressed the obligation he felt to defend Willis. Indeed, he suggested that Boyle would have done the same: "I defend the experiments, and the self-same studies, in which you yourself are every day engaged".[128] It is difficult to believe Lower's claim that he was writing only to Boyle, without any concern about "the censures of other men, [and being] little concerned what noisy barking the pettifoggers and notion-smiths set up against me"; the *Vindicatio* seems to be a reaction to someone who, to Lower, was exactly that sort of person.[129]

The main text of the *Vindicatio* picked up where the polemics of the dedication left off; Lower denounced those who stubbornly rejected innovation in their unfailing adherence to ancient authorities. Lower claimed that anyone who introduced a new experiment or made a discovery would be attacked by "some cynic from the old bearded pack, stirred by disease of mind or envy".[130] For Lower, then, even those who were ostensibly opposed to discoveries were jealous of those who made them. In a sense, what Lower was defending here was the honour that was due to natural philosophers who advanced their fields with inventions and discoveries; that honour was precisely what was at stake during a priority dispute. In a way, the *Vindicatio* was practice for Lower's defence of the honour of his own invention four years later.

Lower discussed O'Meara again in 1669 in the dedication of the *Tractatus de corde.* O'Meara, Lower wrote, was one of many people whose jealousy made them stand in the way of those who sought to improve the human condition; among them, O'Meara himself "takes first prize for sheer perversity and stupidity."[131] Much of the *Tractatus* was a response to O'Meara's errors, but Lower also devoted a chapter to transfusion "so that the credit for the discovery of this celebrated experiment may be given to the Author, to whom it is rightly due."[132] Although Lower claimed to be motivated by the noble goal of

128 Lower, *Vindicatio*, 198. Dewhurst notes that O'Meara took a similar approach at the end of his *Examen Diatribae Thomae Willisii*. Although it seems that O'Meara went out of his way to instigate the dispute, in Dewhurst's words he "excused himself on account of the gross provocation of Williis's teachings, and the arrogant attempts of his followers to overthrow traditional medicine" (Dewhurst, *Vindicatio*, xxiii-xxiv).

129 Richard Lower, "Diatribae Thomas Willisii doct. med. & profess. oxon. de febribus vindicatio adversus de meara ormoniensem Hibernum M.D." in Lower, *Vindicatio*, 198.

130 Lower, "Diatribae Thomas Willisii," 200.

131 Lower, *Tractatus*, vi-viii.

132 Lower, *Tractatus*, viii-xi.

improving human health, the dedication of the *Tractatus* revealed that his purpose in defending his priority was to secure the honour of inventing transfusion, and he implied that those who contested his priority were of O'Meara's ilk.

The chapter that gave evidence for Lower's priority began with a step-by-step account of how he "first *reasoned it out* and *undertook it*"; Lower not only took credit for performing the first transfusion but, ignoring both English and French arguments to the contrary, suggested that no one had seriously considered performing a blood transfusion until three years earlier, which was about the time that he performed his first transfusion.[133] He also included plates with diagrams of the equipment he used in his transfusion experiments (Figure 3.1). Lower adopted a two-pronged (if somewhat contradictory) approach to defending his priority, emphasizing both the originality of his work and the English tradition out of which it grew. He portrayed himself as a successor of Harvey and noted his own involvement in the experiments on intravenous injection at Oxford. Yet Lower also claimed that when he realized that the best method for transfusion would be to transfer blood from the artery of one animal into the vein of another, he was "guided, as it were, by nature herself".[134] Lower explained that his transfusion experiments had been successful and that he had performed one at Oxford in February 1665/6. Boyle had then asked him to send a letter to the Royal Society to inform the Fellows. After Oldenburg had reported Lower's accomplishment in the *Philosophical Transactions*, the French, "attracted by the novelty of the thing, . . . soon began to follow it up more thoroughly, to extend and embellish it by other further experiments, and to apply to the use of man that which I had only accomplished in animals." Lower stated that he was willing to give the French credit for those additions to his work, but that was all they were; like Clarke, he defined human transfusion as simply an innovation on animal transfusion.[135]

However, Lower continued, Denis wanted to "deprive me of priority in the discovery of this experiment, and to claim it for himself".[136] In response, Lower included

133 Lower, *Tractatus*, 171-172. The quotation is from p. 172.
134 Lower, *Tractatus*, 172-174.
135 Lower also implied that he had human transfusion in mind all along; he added a plate showing the equipment he had made for the first transfusion including "the same apparatus for transfusion of blood from an animal to man" (Lower, *Tractatus*, 175-176, 188).
136 Although the word "priority" appears in Franklin's translation, it seems that he inferred it from the

Figure 3.1: Tubes Used During the First Successful Blood Transfusion

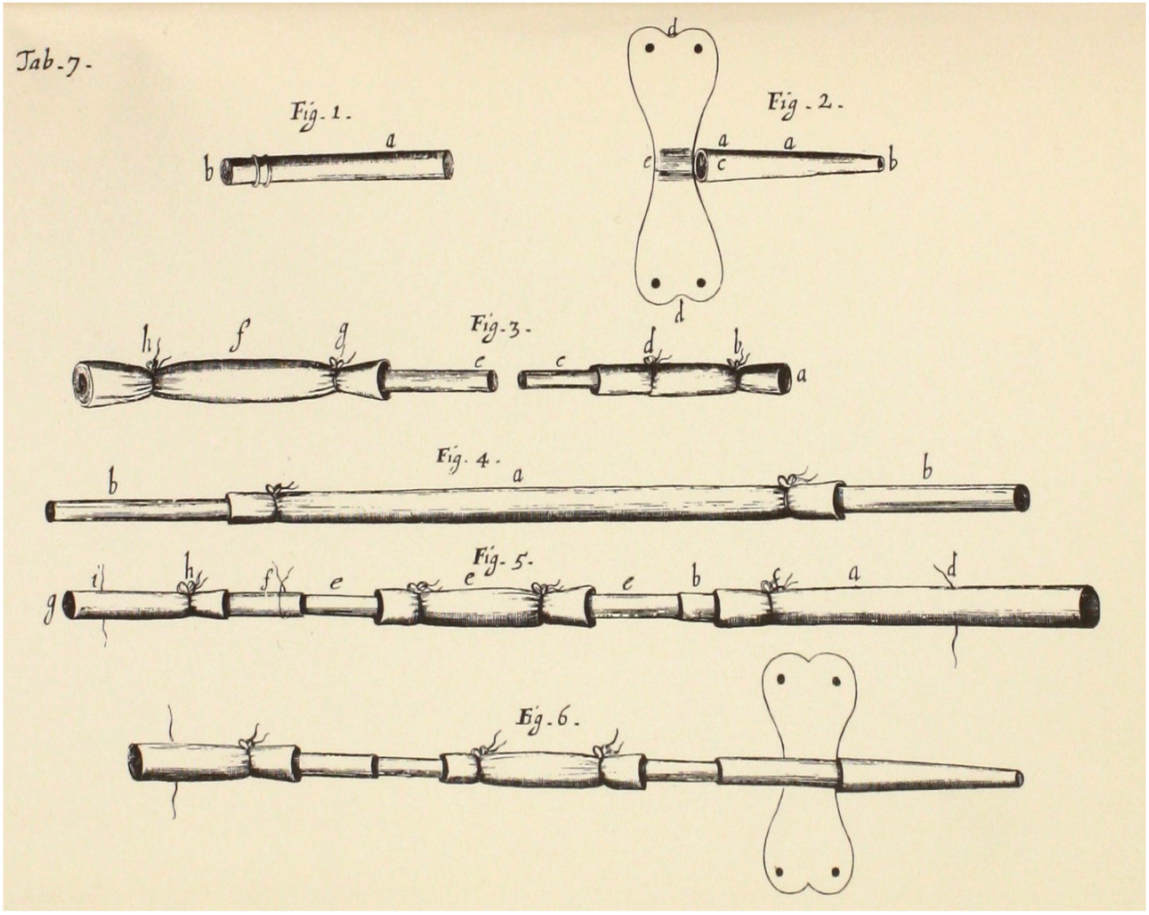


Image from Richard Lower's *Tractatus de corde* (1669).

in the *Tractatus* a transcription of a letter written to him by Boyle on 26 June/7 July 1666 and his reply of 6/16 July. He did this in an effort to attach particular dates to his invention of transfusion, but presumably he also wanted to use Boyle's authority in support of his claim. In any case, Lower evidently suspected that Denis would not be able to produce any documents showing that he had performed a transfusion before 1667.[137] Of course, Lower downplayed his need to defend his priority; he wrote that with every

Latin, in which the word does not appear. The Latin reads "*Dionysius* quidam Philosophiae & Mathematices Professor, in Epistolâ nuper editâ celeberrimi hujus experimenti inventionem mihi præripere & sibi arrogare conetur". Franklin evidently translates "inventionem" as "priority in the discovery". (Lower, *Tractatus*, 176-177).

137 Lower, *Tractatus*, 177-188. As noted above, Boyle expressed to Lower in this correspondence that transfusion had given Lower a rare opportunity to gain honour and to advance his career. See pp. 62-63 above.

significant invention or discovery came jealous people who tried to take credit for it, so "I shall not take it too much to heart".[138]

Next, Lower insisted on the utility and safety of transfusion, and recommended that doctors everywhere try it. He did, however, add a reminder that one needed to be cautious with transfusion, as "no treatment is so useful that its rash and unsuitable administration does not easily bring it into disrepute."[139] Considering that his readers would probably be familiar with Denis and Emmerez's legal trouble, this last point seems to be an oblique jab at the French doctors. Lower concluded his chapter on transfusion with a final reminder that transfusion was a wholly English invention rooted in an English tradition: "let it at least be attributed to the felicity of our Nation, or even to its praise, that, while *Harvey* first taught that the blood by its *Circulation* within its own vessels ensures life to the body, we also revealed that it could be *transferred* outside the confine of its own body for the health of a second."[140] The honour, then, was both his and England's, both for inventing transfusion and for practicing it more conscientiously than their rivals. Lower's *Tractatus* neatly draws a discussion of the transfusion dispute to a close as it contains many of the elements present in this and other priority disputes that involved the Royal Society in the late 1660s. Lower supplied evidence in favour of his priority while insisting that it was not important to him; he appealed to authorities as witnesses; he subtly expanded the scope of the dispute to allow a more robust criticism of his opponents; he identified only the precursors to his work who were from his own country; he defined the entity in question in a way that suited his priority claim; and he kept his definition of priority itself ambiguous so that he could claim it regardless of how his opponents defined it.

3.4: The Question of Nationalism

An important question remains: can the blood transfusion dispute be explained as simply a matter of nationalist (or proto-nationalist) rivalry? As discussed in Chapter 1, several works highlight the role of nationalism in the transfusion case and in other early modern

138 Lower, *Tractatus*, 188-189.
139 Lower, *Tractatus*, 189-192. The quotation is from p. 190.
140 Original emphasis. Lower, *Tractatus*, 192.

priority disputes. For example, Hall and Hall note that the transfusion dispute occurred when the English "were, to say the least, touchy about questions of national prestige and priority in scientific discovery".[141] Furthermore, Brendan Kane notes that nationalist sentiments could exacerbate conflicts about honour in early modern Europe.[142] Yet we cannot take for granted that nationalism was the driving force behind the dispute. Were Oldenburg, Clarke, Lower and Denis mainly protecting their nations' interests or their own? Was this a dispute that simply happened to involve natural philosophers inside and outside England that functioned in the same way as disputes that were confined to England? The answer to the latter question will be addressed in the next chapter. For now it will suffice to analyze the extent to which nationalism was a concern for the participants in this and similar priority disputes.

With respect to this question, the most interesting figure is, again, Henry Oldenburg. Oldenburg was German but came to England during the Interregnum to meet Oliver Cromwell on a diplomatic mission. He became friends with Boyle, and was elected as a member of the first Council, as well as one of two Secretaries of the Royal Society when it was given a Royal Charter in 1662. From then on, he functioned as a mouthpiece of the Society.[143] As a sort of a naturalized Englishman whom natural philosophers across Europe associated with the Royal Society, Oldenburg was sometimes mistakenly called an Englishman, and he made sure to correct this error when it happened. Denis called Oldenburg English in his letter of 5/15 May 1668 and in his printed letter to Sorbière; after he was made aware of his mistake, Denis wrote back to apologize on 22 June/2 July.[144] Nevertheless, Oldenburg considered England's natural philosophers to be vastly superior to their German counterparts. He wrote to Sluse,

141 Hall and Hall, "First Human Blood Transfusion," 461, 465. See also Ambrose, "Rudbeck's Complaint," 489; Merton, "Priorities in Scientific Discovery," 296; Walton, "First Blood Transfusion," 360, 362. See pp. 18-19 above.

142 Brendan Kane, *The Politics and Culture of Honour in Britain and Ireland, 1541-1641* (Cambridge: Cambridge University Press, 2010), 268-270.

143 Marie Boas Hall, *Henry Oldenburg: Shaping the Royal Society* (Oxford: Oxford University Press, 2002), xi, 69-70; Farr, "First Human Blood Transfusion," 145-146.

144 In the letter of 22 June/2 July, Denis wrote, "I was very angry at having been mistaken in giving you English nationality. What I have been able to do to repair the error is that, as soon as I learned of it, I went through all the printed sheets pasting the word 'German' over that of 'English.'" The editors of the *Correspondence* note that Oldenburg's letter to which Denis was replying here is missing, but presumably Oldenburg alerted Denis to the fact that he was mistaken in referring to Oldenburg as English (OC *Denis to Oldenburg 5 May 1668* IV 381, *Denis to Oldenburg 22 June 1668* IV 473-474).

"Much to my distress, since I am a German, Germany seems to lag from the start and to pay heed to scarcely any but scholastic studies, for the most part stale and useless."[145] By the time of the Auzout-Hooke dispute Oldenburg was, in the estimation of the publishers of the *Correspondence*, "unfailingly pro-English".[146] In a letter Oldenburg wrote from Paris in 1659, Avramov has detected in Oldenburg's writing a sense that the French had inherent qualities that prevented them from producing the high level of natural philosophical knowledge that they hoped to produce; he wrote that the French "have not yt required steddines" to be successful.[147] Avramov suggests that Oldenburg continued to feel this way after the foundation of the Royal Society, but generally considered it prudent to mask such beliefs with rhetoric about friendship and cooperation in his foreign correspondence.[148] It is not surprising, however, that Oldenburg was quite vocal in his support of English priority in the blood transfusion dispute after being released from the Tower of London for criticism of England's actions in the war with Holland. At that time more than ever, Oldenburg needed to prove his loyalty to England; a challenge to English priority for a high-profile experiment provided the perfect opportunity.

Some arguments about priority were also opportunities for Oldenburg to imply that the abilities of English natural philosophers were superior to those of their foreign rivals. For example, in an item in the *Philosophical Transactions* of 4/14 June 1666, Oldenburg responded to criticisms in the *Journal des sçavans* of Hooke's method of sounding depths that had been described in an earlier issue of the *Transactions*.[149] The French had claimed that "this way of Sounding Depths is no new invention"; Oldenburg responded by calming that he had only described Hooke's contribution as a new "manner of performing it".[150] Primarily, however, this item was a chance for Oldenburg to belittle French natural philosophers. The *Journal* had reported that the description of Hooke's method in the *Transactions* was unclear and difficult to follow. Oldenburg suggested in

145 OC *Oldenburg to Sluse 6 February 1666/7* III 339. See also OC *Oldenburg to Beckman 30 March 1668* IV 279.
146 OC III xxiv-xxv.
147 OC *Oldenburg to Hartlib 2 July 1659* I 278. Quoted in Avramov, "An Apprenticeship in Scientific Communication," 195.
148 Avramov, "An Apprenticeship in Scientific Communication," 195.
149 *Journal des sçavans* (24 May) 247-250; *Phil. Trans.* 9 (12 February 1665/6) 147-149. This is the same item from the *Transactions* that Oldenburg defended in the letter to Boyle of 17/27 March 1665/6 (OC *Oldenburg to Boyle 17 March 1665/6* III 61-62).
150 *Phil. Trans.* 13 (4 June 1665/6) 230.

turn that the editor of the *Journal* really meant that the description was "not well understood by *French* readers". Conversely, "*Englishmen* and such others, as are well versed in the *English* tongue" had no such difficulty.[151] Ostensibly this comment described a language barrier, but it can also be read as a jab at the ability of the French to understand the "plain, ascetic, unadorned (yet convoluted) style" of describing experiments that Boyle had introduced to the Royal Society.[152] Oldenburg went on to claim that the description was so straightforward "that tis some wonder here, that any difficulty of understanding [it] is pretended by any, that had but ordinary skill in *Cutts* and the *English* language."[153] In short, Oldenburg's response to the *Journal* questioning the clarity and originality of an item in the *Transactions* was to make his rebuttals personal.[154]

There were cases of tension and conflict in this period between Fellows of the Royal Society who lived in England and Fellows who lived on the Continent; an analysis of such cases can determine whether Oldenburg and the other members in England were guided more by their loyalty to their country or to their scientific society. When Huygens was concerned about being recognized for his laws of motion, Oldenburg refused to be held responsible for failing to publish Huygens' theory in the *Transactions* along with Wallis's and Wren's. When he first told Huygens about this item in the *Transactions* he wrote that he had decided not to publish Huygens' theory because he did not have Huygens' permission.[155] When Huygens complained, Oldenburg replied, "this (to speak frankly) is your own fault. You had the *Journal des sçavans* as a means for getting it printed, if you had wished to do so, as quickly as was done here for Messrs. Wallis and Wren in the *Philosophical Transactions.*" He then asked Huygens to apologize for accusing him of an injustice.[156]

Nevertheless, Oldenburg did reproduce Huygens' letter from the *Journal des*

151 Original emphasis. *Phil. Trans.* 13 (4 June 1665/6) 228.
152 Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985) 66.
153 *Phil. Trans.* 13 (4 June 1665/6) 229. The Oxford English Dictionary explains that the word "cutt" referred to a flat-bottomed boat (OED, "Cutt," accessed 31 March 2011).
154 These events occurred before an intense animosity developed between Hooke and Oldenburg. See Chapter 4, n. 14 below.
155 OC *Oldenburg to Huygens 4 February 1668/9* V 374.
156 OC *Oldenburg to Huygens 29 March 1669* V 465.

sçavans in the April 1669 issue of the *Transactions* with an apologetic introduction. In printing Huygens' laws of motion he wanted to be sure that "the worthy Author of them may receive what is unquestionably due to him, yet without derogating from others, with whom in substance he agreeth."[157] Oldenburg deftly mollified Huygens without reducing the credit he gave to Wallis and Wren. This episode reveals how Oldenburg carefully managed his split allegiance to England and to the Royal Society as a separate and international entity. The tone of his letter to Huygens was about halfway between his scolding of De Graaf and his failure to react to Clarke's and Wallis's criticisms of him during their respective disputes.[158] Additionally, Oldenburg consciously chose (or agreed) to publish Wren's and Wallis's theories of motion before getting permission from Huygens to mention his theory, but he tried to make up for this by publishing Huygens' theory later. In short, Oldenburg respected the priority concerns of a non-English member of the Royal Society like Huygens, but not quite as much as he respected the concerns of English members.

While loyalties during the priority disputes here were generally divided along national lines, there were exceptions. Notably, Oldenburg's French friend Henri Justel repeatedly showed his support for English priority in letters to Oldenburg during the transfusion case. Although he was often vocal in denouncing polemical priority disputes, Justel recognized that "[n]ew discoveries are difficult to make."[159] Justel apparently felt that it was most honourable to hold the opinion about a matter of priority that was most credible, whether or not it favoured his countrymen. He wrote to Oldenburg on 6/16 November 1667, "I must admit that the author in our *Journal* was too credulous in accepting the statements of those who said that transfusion was discovered in France rather than in England. I have told him that he ought to inform himself more carefully than he has done. All honorable men agree with your opinion."[160] Similarly, in December he defended English priority during one of Montmor's meetings: "I have told these

157 *Phil. Trans.* 46 (12 April 1669) 925-928.
158 Wallis's comments on Oldenburg's behaviour in his dispute with François Dulaurens will be addressed in Chapter 4. Oldenburg does not seem to have complained about such criticisms; perhaps he understood the need for disputants to shift the responsibility for causing conflict and allowed himself to take the blame, realizing that these comments would (apparently) not be taken seriously enough to threaten Oldenburg's reputation for impartiality.
159 OC *Justel to Oldenburg 20 March 1667* III 369.
160 OC *Justel to Oldenburg 6 November 1667* III 579.

gentlemen that it must be admitted that the English were the inventors of this and of many other fine things."[161] Justel was also critical of Denis' rashness in performing transfusions on humans; after telling Oldenburg that Antoine Mauroy had died, Justel remarked, "So Mr. Denis boasted inopportunely of his cure. He should have waited."[162] Evidently Justel linked Denis' lack of credibility in the priority dispute with the fact that his patient had died. In March 1667/8 he told Oldenburg that Denis "still wants to make the French the inventors of transfusion. You will see whether this is well founded or not. There is nothing truer than the fact that the madman on whom the transfusion was performed died on their hands."[163] Justel was happy to read Clarke's letter in support of English priority. He told Oldenburg that he was eager to show it to Denis and wished it would be printed in the *Journal des sçavans*.[164] Justel was also much more critical of François Dulaurens' polemics than those of John Wallis during the dispute between these two mathematicians which will be discussed in Chapter 4.[165]

Justel made a number of comments in his correspondence with Oldenburg that suggested his frustration with French natural philosophy and the Académie des Sciences. In a letter that expressed Justel's opinion that Europe's natural philosophers regarded the Royal Society highly, he told Oldenburg, "Our philosophers will soon produce something which is not very important."[166] In another letter Justel complained that French natural philosophers "don't work as hard and are not as precise as the English."[167] He repeatedly lamented that the Académie had accomplished nothing new or exciting, and that the *Journal des sçavans* was unimpressive.[168] The English, conversely, were "more suited to

161 OC *Justel to Oldenburg 28 December 1667* IV 89.

162 Justel repeated the news that Denis' patient had died in a letter two weeks later, and described what had happened in more detail in a letter written just two days after that. Evidently he wanted to be sure that Oldenburg understood Denis and Emmerez's failure (OC *Justel to Oldenburg 5 February 1667/8* IV 150, *Justel to Oldenburg 19 February 1667/8* IV 190, *Justel to Oldenburg 21 February 1667/8* IV 195-196. See also OC *Justel to Oldenburg 18 March 1667/8* IV 257).

163 OC *Justel to Oldenburg 11 March 1667/8* IV 245.

164 OC *Justel to Oldenburg 7 June 1668* IV 453.

165 See Chapter 4, n. 69 below. See also OC *Justel to Oldenburg c. 10 June 1668* IV 462, *Justel to Oldenburg 4 July 1668* IV 502. It should be noted that Justel showed some support for De Graaf during his dispute with Clarke, so he did not always favour the English, but De Graaf was of course not French. See OC *Justel to Oldenburg 28 November 1668* V 209.

166 OC *Justel to Oldenburg 20 March 1667* III 369.

167 OC *Justel to Oldenburg 15 July 1668* IV 545.

168 See, for example, OC *Justel to Oldenburg 14 October 1668* V 87, *Justel to Oldenburg c. 18 October 1668* V 93, *Justel to Oldenburg 28 November 1668* V 209.

discovery than any other people, being speculative and inventive."[169]

However, Justel's pro-English and anti-French sentiments may not only have been motivated by the abilities of the natural philosophers in the respective countries. As Oldenburg was aware, Justel was a Huguenot.[170] He was well-connected and moderate in the 1660s, and thus was under no particular threat of religious persecution, but Geoffrey Treasure suspects that he would have felt pressure to conform. It was to England that Justel came in 1681, a move prompted by strong anti-Huguenot sentiments and legislation which culminated in the revocation of the Edict of Nantes in 1685. He came at the invitation of Charles II and expected to receive help from his English friends and acquaintances like Locke, Pepys and Boyle.[171] Indeed, for Treasure, Justel was "a most important intermediary of English ideas to Frenchmen".[172] Perhaps, then, the personal reasons that may have made Justel favourable to English rather than French priority made him the exception that proves the rule of loyalty to like-minded countrymen.

Of course, this is just one example of the many possible reasons for which a natural philosopher could choose to support a foreigner rather than a countryman, or to engage in a dispute with a countryman. The next chapter will examine priority disputes in the career of John Wallis, who was among the most outspoken defenders of English priority but whose loyalty to other English natural philosophers in matters of priority was not absolute. As Chapter 4 will demonstrate, Wallis could abandon his attitude of solidarity among the English and within the Royal Society to advance his own interests. Nationalism was an ever-present factor in priority disputes that were not confined to a single country, but it would be simplistic to consider them nationalist conflicts.

169 Justel wrote this after he found out, to his relief, that a rumour about the decline of the Royal Society turned out not to be true. As he explained in subsequent letters, Justel then worked hard to restore the Society's reputation in France (OC *Justel to Oldenburg early September 1668* V 39, *Justel to Oldenburg 16 September 1668* V 45, *Justel to Oldenburg 23 September 1668* V 64).

170 See OC *Justel to Oldenburg 28 December 1667* IV 89, *Oldenburg to Boyle 7 January 1667/8* IV 104.

171 Geoffrey Treasure, "'That Great and Knowing Virtuoso': the French Background and English Refuge of Henri Justel" in *From Strangers to Citizens: The Integration of Immigrant Communities in Britain, Ireland and Colonial America, 1550-1750*, ed. Randolph Vigne and Charles Littleton (Brighton: Sussex Academic Press, 2001), 206-208. Justel also knew the Dutch Huygens and the German Leibnitz.

172 Treasure, "Great and Knowing Virtuoso," 212.

Chapter 4: The Merchant of Glory:
John Wallis and the Limits of English Nationalism in Priority Disputes

4.1: Wallis and English Priority before the Royal Society

On 3/13 June 1655, Christiaan Huygens[1] wrote to John Wallis, the Savilian Professor of Geometry at Oxford, future founding member of the Royal Society and member of its first Council,[2] to discuss mathematics at the request of their mutual friend, Frans van Schooten.[3] Huygens took this opportunity to entrust Wallis with an anagram that would indicate a discovery when its solution was revealed: Huygens had observed a satellite

1 Christiaan Huygens (1629-1695) was a Dutch-born natural philosopher, notable for numerous astronomical, mathematical and technological achievements, who spent much of his life living in Paris as a prominent member of the Académie des Sciences. Relevant biographical information about Huygens will be given throughout this chapter, but for additional information see D. Bierens de Haan, et al., eds., *Oeuvres complètes de Christiaan Huygens* (The Hague: Martinus Nijhoff, 1888-1950); H. J. M. Bos, et al., eds., *Studies on Christiaan Huygens: Invited Papers from the Symposium on the Life and Work of Christiaan Huygens, Amsterdam, 22-25 August 1979* (Lisse: Swets & Zeitlinger, 1980); *Huygens et la France*, Table ronde du Centre National de la Recherche Scientifique, Paris, 27-29 March 1979 (Paris: Librarie Philosophique J. Vrin, 1982).

2 John Wallis (1616-1703), born in Kent and educated at Cambridge, became one of the most well-known mathematicians in the Royal Society, but had little mathematical training in his early life. He was ordained in 1640 and served as a chaplain for much of the early 1640s. Wallis was noticed for his cryptanalytic skills in 1642 and was a Parliamentary codebreaker during the Civil War, although he opposed the execution of Charles I. In this period Wallis became interested in experimental philosophy and acquainted himself with the groups of scholars in London and Oxford who would later form the Royal Society. Despite his minimal mathematical production so far, Wallis was made Savilian professor of geometry at Oxford in 1649 to replace a Royalist, and he held this post for over fifty years. He served as keeper of the university archives from 1658 on and as a royal chaplain after the Restoration; he also continued his work as a codebreaker for the government. Wallis's mathematical production increased in the 1650s, and his *Arithmetica infinitorum* of 1655 became particularly influential for the likes of Isaac Newton. Wallis was a prolific contributor to the Royal Society, mainly in terms of mathematics but also occasionally in physics. Most of his works were included in his three-volume *Opera mathematica* (1693-1699). The 1690s were also a period of much theological production from Wallis; he published eleven texts opposing unitarianism (Domenico Bertoloni Meli, "Wallis, John [1616-1703]" in ODNB, accessed 1 July 2011]). There have been few modern biographies of Wallis, but he was the subject of a slew of biographical works in the eighteenth century (and an autobiographical work the he wrote in 1696/7) which are described (and, in the case of the autobiography, transcribed) in an article by Christoph J. Scriba. Other useful sources for information about Wallis's, apart from his *Correspondence* whose publication is in progress (abbreviated WC throughout this thesis) are listed here (J. F. Scott, *The Mathematical Work of John Wallis, D. D., F. R. S. [1616-1703]*, 2nd ed. [New York: Chelsea Publishing, 1981; orig. pub. 1938]; J. F. Scott, "The Reverend John Wallis, F. R. S. [1616-1703]," *Notes and Records of the Royal Society of London* 15 [1960]: 57-67; Christoph J. Scriba, "The Autobiography of John Wallis, F. R. S.," *Notes and Records of the Royal Society of London* 25 [1970]: 17-46. See also Christoph J. Scriba, "A Tentative Index of the Correspondence of John Wallis, F. R. S.," *Notes and Records of the Royal Society of London* 22 [1967]: 58-93).

3 WC *Huygens to Wallis [3]/13 June 1655* I 141-145.

revolving around Saturn, now called Titan. Anagrams, ciphers and other "concealed communication techniques" were a common way of protecting one's priority upon making a discovery or invention in early modern Europe,[4] and Huygens in particular was known to use this method frequently. Huygens' anagram, in its encoded form, was: "ADMOVERE OCULIS DISTANTIA SIDERA NOSTRIS, VVVVVVVCCCRR-HNBQX."[5] He also asked Wallis to communicate the anagram to Seth Ward, Wallis's Oxford colleague, "so that I have him too as witness that what is hidden with these letters was never seen by anyone before me."[6] Huygens had to protect this discovery carefully. It had been fashionable for astronomers to observe Saturn since Pierre Gassendi noticed that the characteristic handles or "anses" protruding from the sides of the planet had disappeared in 1642. The problem of Saturn's changing appearance would be a

4 Giuliano Pancaldi, "Priority" in *The Oxford Companion to the History of Modern Science*, ed. J. L. Heilbron, et al. (Oxford: Oxford University Press, 2003), 676. Sending an anagram or cipher to one's correspondent gave a natural philosopher or mathematician time to perfect his invention or confirm his discovery; when he had the proof he needed, he would reveal the solution to the anagram, thus proving that he had discovered or invented it by the time he sent the anagram. If someone else announced the same discovery or invention in the intervening period, he would reveal the solution ahead of schedule as proof that he had priority. Galileo used this method in 1610 to protect his discovery that Saturn was accompanied by two small bodies at its sides (which were later observed to have the appearance of handles protruding from Saturn) and, in the same year, to protect his discovery of the phases of Venus (Paolo Palmieri, "Galileo and the Discovery of the Phases of Venus," *Journal for the History of Astronomy* 32 [2001]: 109-129; Van Helden, "Saturn and His Anses," 105; Richard S. Westfall, "Science and Patronage; Galileo and the Telescope," *Isis* 76 [1985]: 12, 23-29). There has been much useful scholarship on early modern uses of anagrams and ciphers. Margaret Ferguson, for instance, discusses the use of ciphers by the novelist Aphra Behn, who used literal ciphers in her activities as a spy in Flanders, and "biographical ciphers" to encode her personality in her writing and in the image of herself that she projected. Peter Pesic notes that Francis Bacon was very familiar with ciphers: he had experience both creating and breaking coded letters, his brother was a spy working for Sir Francis Walsingham, and his friend Thomas Phelippes was involved in decoding the letters that revealed the Babington plot. Bacon applied this knowledge to develop a way of "deciphering" nature. Pesic argues that this cryptanalytical metaphor for studying nature was crucial to efforts to describe the novel aspects of seventeenth-century science (Margaret Ferguson, "The Authorial Ciphers of Aphra Behn" in *The Cambridge Companion to English Literature, 1650-1740*, ed. Steven N. Zwicker [Cambridge: Cambridge University Press, 1998], 225-249 [the quotation is from p. 238]; Peter Pesic, "The Clue to the Labyrinth: Francis Bacon and the Decryption of Nature," *Cryptologia* 24 [2000]: 193-211. See also Paul Lunde, ed. *The Book of Codes: Understanding the World of Hidden Messages* [Berkeley: University of California Press, 2009], 63-87).

5 WC *Huygens to Wallis [3]/13 June 1655* I 145. The intelligible part of the anagram means, roughly, "To direct our eyes to the distant stars." The concealed meaning, as Huygens later revealed, was SATURNO LUNA SUA CIRCUNDUCITUR DIEBUS SEXDECIM HORIS QUATUOR", or "Saturn's moon is led around it in sixteen days and four hours." In the letter revealing the meaning of the anagram, Huygens added that he had since removed the extra four hours from his calculation of the period of Saturn's satellite (WC *Huygens to Wallis [5]/15 March 1655/6* WC I 177. All translations from *Wallis Correspondence* are my own).

6 That is, "ut ipsum quoque si quando opus sit testem habeam nemini ante me id visum fuisse. (WC *Huygens to Wallis [3]/13 June 1655* I 145).

significant source of honour for whoever solved it – and it would inevitably be solved given the continual advances in telescopy and the constant attention the planet was receiving – which inspired a number of competing theories. This included the theory proposed by Huygens himself, who used another anagram in his *De saturni luna observatio nova* of 1656 to encode his belief that Saturn was surrounded by a ring, and then revealed the solution in his *Systema saturnium* of 1659 (see Figure 4.1).[7] Thus it was reasonable for Huygens to fear that someone else might observe Saturn's satellite before he was ready to announce his discovery.

Wallis's reply to Huygens of 21 June/1 July contained an anagram of his own.[8] When Huygens revealed the solution to his anagram in a letter to Wallis written in the following March (which accompanied a copy of *De saturni luna*, the work that announced the discovery of Titan), Wallis shocked and disappointed Huygens with his response: the English anagram concealed the same discovery, which Sir Paul Neile and Christopher Wren had made before Huygens.[9] Two and a half years later, Huygens announced to Wallis that he was ready to publish his long-awaited *Systema saturnium* which would explain his theory of Saturn's appearance in full. Wallis chose this moment to reveal that his anagram had been fake. It was a cleverly crafted pseudo-anagram with several possible solutions, one of which Wallis would choose to correspond to whatever Huygens's anagram turned out to mean. Evidently Huygens did not know about Wallis's history as a parliamentarian codebreaker during the English Civil War, and Wallis saw this as an opening to take advantage of him.[10] Wallis explained that the English astronomers actually had observed the satellite earlier than Huygens, but thought it was a

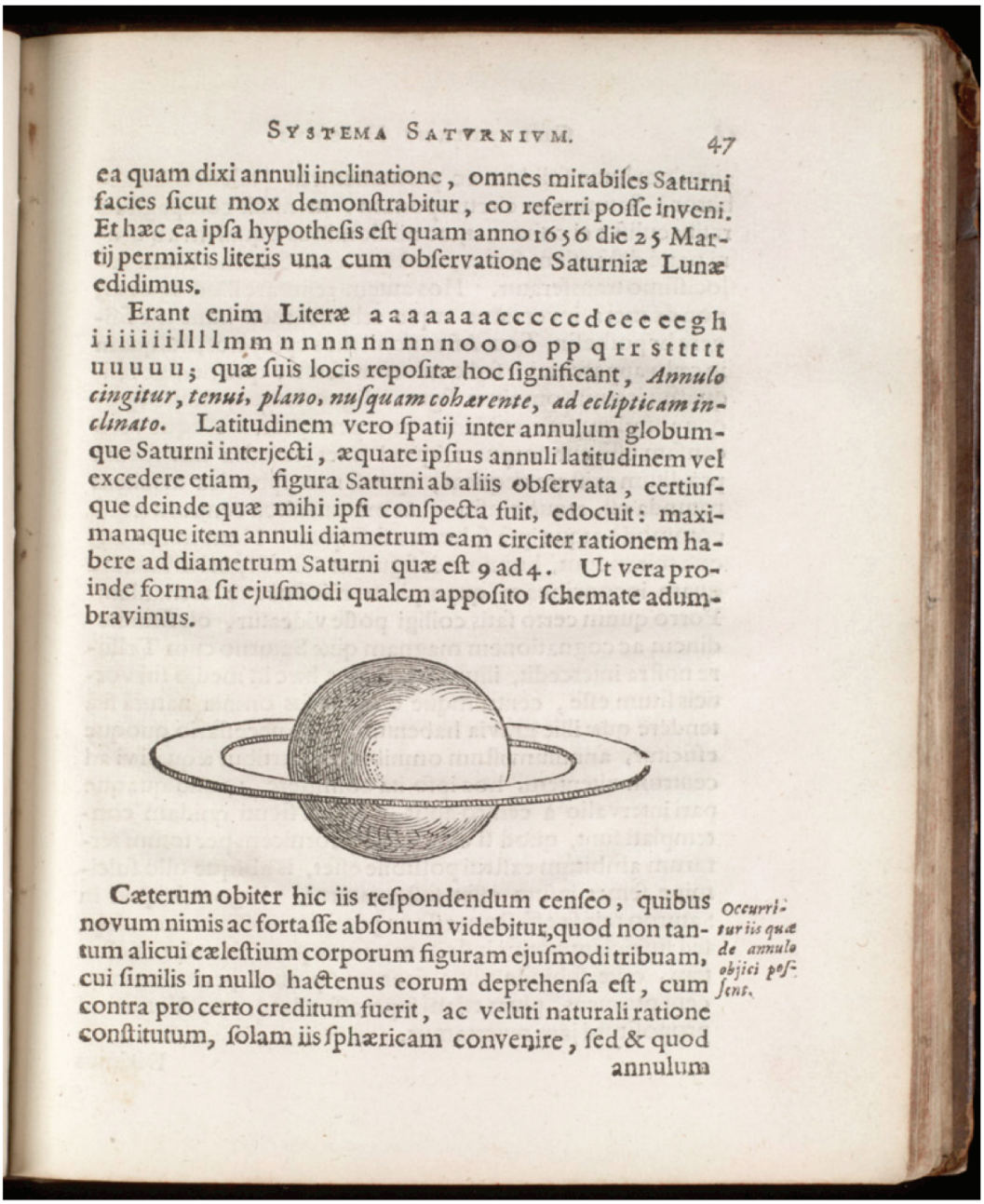
7 Albert van Helden, "'Annulo Cingitur': The Solution to the Problem of Saturn," *Journal for the History of Astronomy* 5 (1974): 155-174; Albert van Helden, "Saturn and His Anses," *Journal for the History of Astronomy* 5 (1974): 113-115.

8 WC *Wallis to Huygens 21 June/1 July 1655* I 148-149.

9 WC *Huygens to Wallis [5]/15 March 1655/6* I 177, *Wallis to Huygens 22 March [1655/6]/[April 1] 1656* I 178.

10 Wallis wrote with pride about his codebreaking history in his autobiography. He explained that he had been asked half-jokingly to solve a cipher contained in an intercepted letter in 1642, and so impressed people with his "unexpected success" that he quickly earned a reputation for his impressive cryptanalytical skills. He was subsequently sought out to break codes both during and after the war (John Wallis, "The Autobiographies" in Scriba, "The Autobiography of John Wallis," 37-38. See also Peter Pesic, "Secrets, Symbols, and Systems: Parallels between Cryptanalysis and Algebra, 1580-1700," *Isis* 88 [1997]: 674-692, especially 688-691).

Figure 4.1: The Solution to Huygens' Anagram Concerning the Ring of Saturn.



SYSTEMA SATVRNIVM. 47

ea quam dixi annuli inclinatione, omnes mirabiles Saturni facies ſicut mox demonſtrabitur, eo referri poſſe inveni. Et hæc ea ipſa hypotheſis eſt quam anno 1656 die 25 Martij permixtis literis una cum obſervatione Saturniæ Lunæ edidimus.

Erant enim Literæ aaaaaaaccccccdeeeeegh iiiiiiiillllmmnnnnnnnnnoooopp q rr ssttttt uuuuu; quæ ſuis locis repoſitæ hoc ſignificant, *Annulo cingitur, tenui, plano, nuſquam cohærente, ad eclipticam inclinato.* Latitudinem vero ſpatij inter annulum globumque Saturni interjecti, æquare ipſius annuli latitudinem vel excedere etiam, figura Saturni ab aliis obſervata, certiuſque deinde quæ mihi ipſi conſpecta fuit, edocuit: maximamque item annuli diametrum eam circiter rationem habere ad diametrum Saturni quæ eſt 9 ad 4. Ut vera proinde forma ſit ejuſmodi qualem appoſito ſchemate adumbravimus.

Cæterum obiter hic iis reſpondendum cenſeo, quibus novum nimis ac forta ſſe abſonum videbitur, quod non tantum alicui cæleſtium corporum figuram ejuſmodi tribuam, cui ſimilis in nullo hactenus eorum deprehenſa eſt, cum contra pro certo creditum fuerit, ac veluti naturali ratione conſtitutum, ſolam iis ſphæricam convenire, ſed & quod annulum

Occurritur iis quæ de annulo objici poſſent.

Image from Christiaan Huygens' *Systema saturnium* (1659).

The solution reads, "Annulo cingitur, tenui, plano, nusquam cohaerente, ad eclipticam inclinato" which translates to "It is girdled by a ring, thin, plane, nowhere attached, inclined to the ecliptic" (translation from Helen M. Davis, "Huygens Explains Saturn's Ring," *The Science News-Letter* 16 [1929]: 192).

fixed star.[11] He added that Huygens should be sure to mention this in his *Systema saturnium*. According to Wallis, this trick was not meant to diminish Huygens' honour in any way, but rather to show him that anagrams did not offer sufficient security for one's discoveries.[12] Why Wallis chose that moment to reveal the deceit to Huygens is not entirely clear, but it seems unlikely that the timing was an accident. Perhaps he thought that the truth had to be revealed before Huygens gave the English undue credit in the *Systema saturnium*; if it was later determined that they had lied, Wallis and his friends could be charged with failing to respect Huygens' priority. By admitting to the deceit just in time for the publication of the *Systema saturnium*, Wallis made sure that the English would at least be given due credit for observing the satellite first, albeit without recognizing it as such. In any case, it seems clear that Wallis tried to use his pseudo-anagram trick to trivialize what proved to be an important discovery for Huygens;[13] by making a game out of it, he suggested that he had little respect for Huygens' discovery. Huygens was finding out the hard way that Wallis would go to great lengths to promote the capacity of Englishmen to make important discoveries and inventions, and to deride that capacity in foreigners.

In the 1660s and beyond Huygens, who was greatly concerned about priority even by the standards of his time, continued to become involved in these episodes of priority-related tension with the English despite his election as a Fellow of the Royal Society in 1663.[14] This was also not Huygens' only run-in with John Wallis concerning priority. The

11 There is no evidence to suggest that Wallis was lying about Neile and Wren thinking the satellite was a fixed star, and this was an easy mistake to make; Galileo admitted in the *Sidereus nuncius* that he originally thought the Medicean Stars were fixed, and only took note of them because they all appeared parallel to the ecliptic. He observed these stars again the next day, "led by what, I do not know", and noticed that they had moved (Galileo Galilei, "The Starry Messenger" in *Discoveries and Opinions of Galileo*, trans. Stillman Drake [New York: Anchor Books, 1957], 51-52. The quotation is from p. 52).

12 WC *Wallis to Huygens 22 December 1658/[1 January 1659]* I 550-551.

13 As discussed in Chapter 2 n. 107 above, in the *Systema saturnium*, Huygens tried to legitimize his theory of Saturn by claiming to have better telescopes than those who proposed rival theories; he had proven this by discovering a satellite revolving around the planet that no one else had yet noticed. Huygens hoped that his telescopic superiority would translate into the credibility of a theory which sought to solve a famous question that not even Galileo had solved (Van Helden, "Annulo Cingitur," 161-162).

14 Huygens became one of the earliest foreigners to receive that honour. Bachrach suggests that the Society sought out Huygens, who was something of a celebrity, to help legitimize the fledgling Society (A. G. H. Bachrach, "Les Huygens entre la France et l'Angleterre" in *Huygens et la France*, 22). Huygens' dispute with Robert Hooke in the mid-1670s over the invention of the balance-spring watch, a watch that kept time via the isochronous vibrations of a spring, has received much historiographical attention. This was a rare case in which Oldenburg favoured a foreigner in a priority dispute with an Englishman. The episode intensified an animosity between Oldenburg and Hooke that persisted for the rest of Oldenburg's

two disputed whether William Neile, whom Wallis defended, or Hendrick van Heuraet, whom Huygens defended, had been the first to rectify the cubic parabola.[15] In the episode concerning Titan, Huygens stumbled upon what he considered an annoying quality in the Savilian Professor. Although gracious in his reply to the letter in which Wallis gave the false solution to his anagram, Huygens seemed frustrated when he wrote about the event to Schooten on 2 June 1656. He wrote that Wallis "is now jealous of me because of telescopes and because of the new planet that has been discovered, which he is trying to claim for his English if he can."[16] Similarly, after the revelation that Wallis's anagram was fake, Huygens expressed a mixture of condescending amusement and irritation at Wallis's efforts to claim discoveries for England. He wrote to Pierre de Carcavi that Wallis "indeed has a quick mind and it is enjoyable to see how he tries at all costs to

life, and in Hooke's case for much longer. See Robyn Adams and Lisa Jardine, "The Return of the Hooke Folio," *Notes and Records of the Royal Society of London* 60 (2006): 236; Allan Chapman, *England's Leonardo: Robert Hooke and the Seventeenth-Century Scientific Revolution* (Bristol: Institute of Physics Publishing, 2005), 177-181; Rob Iliffe, "'In the Warehouse': Privacy, Property and Priority in the Early Royal Society," *History of Science* 30 (1992): 29-62; Lisa Jardine, *The Curious Life of Robert Hooke: The Man Who Measured London* (New York: HarperCollins, 2004) 198-211, 214. See also Adrian Johns, *The Nature of the Book: Print and Knowledge in the Making* (Chicago: University of Chicago Press, 1998), 521-531. Watches were a source of multiple priority disputes for Huygens. During the conflict with Hooke he also faced a challenge to his priority for the balance-spring watch from French inventors and watch-makers. Hooke also challenged Huygens' priority for an earlier invention, a clock that kept time using the isochronous motion of a pendulum tracing out a cycloid (Christiaan Huygens, "Horologium" in *The Story of the Pendulum Clock* by Ernest L. Edwardes [Altrincham: John Sherratt and Sons, 1977], 67-69; Christaan Huygens, *The Pendulum Clock or Geometrical Demonstrations Concerning the Motion of Pendula as Applied to Clocks* trans. Richard J. Blackwell [Ames: The Iowa State University Press, 1986], 12-13; Iliffe, "In the Warehouse," 40-51; J. H. Leopold, "L'invention par Christiaan Huygens du ressort spiral régland pour les montres" in *Huygens et la France*, Table ronde du Centre National de la Recherches Scientifique, Paris, 27-29 March 1979 [Paris: Librarie Philosophique J. Vrin, 1982], 153-157. 153-157). John Aubrey, who was Hooke's friend, took Hooke's side in the dispute over the pendulum watch in his *Brief Lives*, writing, "'Twas Mr. Robert Hooke that invented the Pendulum-Watches, so much more usefull than the other watches." Aubrey did not mention Huygens. He also claimed priority for Hooke for the inverse-square law, explaining that Newton had "not at all own[ed] he receiv'd the first Intimation of it from Mr. Hooke. Aubrey also asserted that, "Likewise Mr. Newton haz in the same Booke [*Principia mathematica*] printed some other Theories and experiments of Mr. Hooke's, without acknowledgeing from whom he had them" (John Aubrey, *Aubrey's Brief Lives*, ed. Oliver Lawson Dick [London: Secker and Warburg, 1949], 165-166, 167).

15 To rectify a curve means to find a straight line of the same length so that that length can easily be measured. Jan A. van Maanen explains that Huygens himself claimed priority for this method of rectification against van Heuraet in the late 1650s, but admitted by 1673, the time of the dispute with Wallis, that the late van Heuraet had preceded him. Heuraet also poked fun at Huygens' use of anagrams with eight pseudo-anagrams of his own, but these seem to have had no meaning, rather than multiple meanings like Wallis's pseudo-anagram (Jan A. van Maanen, "Hendrick van Heuraet [1634-1660?]: His Life and Mathematical Work," *Centaurus* 27 [1984], 240, 249-250). The dispute of 1673 is discussed further in n. 90 below.

16 The Latin reads, "qui [Wallis] mihi nunc aemulus est telescopiorum causa et ob novi planetae repertum, quod suis si possit Anglis arrogare conatur" (Huygens, *Oeuvres complètes* I, 429. My translation).

maintain the honour of his nation." This does not suggest that Huygens did not take the matter seriously. In fact it made him wary of any discussion of inventions and discoveries in correspondence. He told Carcavi, "I see more than ever from this latest writing of Wallis the inconveniences and disputes that can arise when inventions of some consequence go from hand to hand before being published."[17]

The tone of Huygens' exasperated comments was echoed in the *Brief Lives* of John Aubrey, who was a friend of Wallis's longstanding rival, Thomas Hobbes.[18] Aubrey suggested that Wallis would lie to appropriate any discovery he could. However, Aubrey was not referring to Wallis stealing discoveries for England, but rather stealing the work of other Englishmen for himself. In Aubrey's words Wallis was "*so extremely greedy of glorie, that he steales feathers from others to adorne his own cap; e.g. he lies at watch, at Sir Christopher Wren's discourse, Mr. Robert Hooke's, Dr. William Holder, &c; putts downe their notions in his Note booke, and then prints it, without owneing the authors*." Wallis contributed to "*Learning*", but only by publishing material that he stole from more talented Englishmen that they did not have time to publish.[19] In an autobiography that Wallis wrote in January 1696/7 at the request of Royal Society member Thomas Smith,[20]

17 WC *Huygens to Carcavi [17]/27 March [1659]/1660* II 9-10. The French reads, "Ce Mr. Wallis tesmoigne certes d'avoir l'esprit prompt et il y a du plaisir a veoir comme il tasche a toute force de maintenir l'honneur de sa nation. . . . Je voy plus que jamais par ce dernier escrit de Wallisius les inconvenients et disputes qui en peuvent naistres lors que des inventions de quelque consequence vont de main en main devant que d'estre publiees." Wallis was not unique among his contemporaries as the object of such criticisms. Jesseph describes Hobbes's frustration at the French mathematician Gilles de Roberval, who participated in several priority disputes, for his tendency to respond to the announcement of any major mathematical discovery with a claim that he had preceded it (Douglas Michael Jesseph, "Descartes, Pascal and the Epistemology of Mathematics: The Case of the Cycloid," *Perspectives on Science* 15 [2007]: 417-418). Of course, as we have seen, Huygens did continue to convey his discoveries and inventions through letters – often using anagrams to do so – as he did in a letter to Oldenburg when called upon to contribute his theory of motion. See Chapter 3 pp. 87-89, 96-97 above.

18 The dispute between Wallis and Hobbes was multifaceted and spanned decades, ending only when Hobbes died in 1679. See Aubrey, *Brief Lives*, 158; Niccolò Guicciardini, *Isaac Newton on Mathematical Certainty and Method* (Cambridge, MA: The MIT Press, 2009), 144-148; Hal Hellman, *Great Feuds in Science: Ten of the Liveliest Disputes Ever* (New York: John Wiley & Sons, 1998), 21-37; Scott, "The Reverend John Wallis," 65; Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton:Princeton University Press, 1985) 131-132.

19 Aubrey, *Brief Lives*, xciii. By using the word "feathers", Aubrey was employing a trope that would have been familiar to contemporary readers. Intellectual property theft had been compared to a bird stealing feathers from another bird since Aesop alluded to plagiarism in one of his fables about a crow who steals feathers from more beautiful birds. Horace subsequently made this metaphor well known, and his use of it was adopted by early modern writers. (Hall Bjørnstad, ed., *Borrowed Feathers: Plagiarism and the Limits of Imitation in Early Modern Europe* [Oslo: Unipub, 2008], 10 [editor's introduction]).

20 See Scriba, "The Autobiography of John Wallis," 17-46.

he tacitly acknowledged his frequent participation in priority (and other) disputes, but he portrayed his role in them quite differently. Wallis suggested that he avoided conflict when he could and lived his life as a moderate, except when called upon to defend "Religion, Learning, and the publick good".[21] The emphasis that Wallis placed on his tendency toward moderation seems to have been a reaction to the reputation for belligerence that Wallis knew he had. He could hardly deny that he had frequently been involved in conflicts, but, like Henry Oldenburg and Timothy Clarke, he tried to justify this behaviour by claiming to have been drawn into disputes by his obligation to the truth and to the public good. In short, Wallis earned a reputation for stealing priority for himself from his countrymen in the same way that he stole priority for England from foreign rivals, and he strove to spin that behaviour into a reflection of positive personal qualities. This chapter shows that Wallis was a master of defending English priority – who understood the importance of masking one's efforts to do so – and that he was regularly sought out by the Royal Society for these skills. However, this chapter also emphasizes that Wallis was not afraid to use the same skills to support his own advancement and honour.

4.2: The Wallis-Dulaurens Dispute

Wallis's skills as a defender of English priority, who could do so while pretending not to be concerned about priority, were on display during a priority dispute with François Dulaurens, a French mathematician trying to make a name for himself, in 1668. Dulaurens sought out a correspondence with Oldenburg whom he had met while Oldenburg was in France in order to bring the Royal Society's attention to his new book, the *Specimina mathematica.*[22] In a letter probably written on 2/12 February 1667/8,[23]

21 Wallis, "Autobiographies," 42-43.

22 The editors of the *Oldenburg Correspondence* explain that Oldenburg and Dulaurens might have met in Paris or in Montpellier, the hometown of a Dulaurens family of which François was probably a member (OC *Dulaurens to Oldenburg 2 February 1666/7* III 336-337). For the editors' brief account of the Wallis-Dulaurens dispute, see OC IV xx.

23 The editors note that the letter is dated 2/12 February 1666/7, but Oldenburg endorsed it with the date February 1667/8. All subsequent letters between Oldenburg and Dulaurens are dated 1667/8 or 1668, and none of them mention a long delay in the delivery of Dulaurens' first letter. Thus, Dulaurens probably wrote the wrong year on the first letter (OC *Dulaurens to Oldenburg 2 February 1666/7* III 337).

Dulaurens wrote that he was anxiously waiting for Oldenburg to receive two copies of the *Specimina* that he had sent through Henri Justel. Justel had also communicated a mathematical problem from Dulaurens, which, as Oldenburg subsequently reported to Justel, a Royal Society member had solved.[24] Dulaurens was pleased to hear this and felt "obliged to this good man who has resolved it, for being willing to work at so slight a thing". Yet he reminded Oldenburg that "I expressly stated that I did not propose it because I considered the solution very difficult, but so that in that way some fine properties of the circle might be discovered, which no one has hitherto mentioned."[25] Dulaurens was being proactively defensive of his mathematical abilities by making it clear that he did not think such an easy problem would be a challenge for the mathematicians of the Royal Society. It was important for Dulaurens to maintain a reputation for mathematical skill as he tried to insinuate himself into the social circle of the famous Society. He asked Oldenburg to send the Society member's solution to the problem and promised to reply with his own solution (and one from another Frenchman) as well as "the consequences I derive from it."[26] Dulaurens displayed confidence in his mathematical skill; he did not yet know that it was his confidence that Wallis would find so bothersome.

The Society member who solved Dulaurens' problem was probably Wallis, although Oldenburg did send it to others. Wallis became aware of Dulaurens when Oldenburg expressed his optimism about the *Specimina* in a letter of 10/20 December 1667. Oldenburg wrote that he thought Wallis would be "well pleased" with the recently-published book when it arrived from Paris, and equally pleased with "a Probleme, sent from the Author of this Book, wch he recommends not for the difficulty of its Solution, but for the inferences, yt may be thence made, of some very fine properties of ye Circle, by him affirmed not to have been discovered hitherto."[27] Oldenburg then transcribed Dulaurens' problem and asked Wallis to "consider it, and what is intimated above of ye

24 See OC *Justel to Oldenburg 25 December 1667* IV 85-86, in which Justel mentions sending the copies of the *Specimina* to Oldenburg. In another letter written three days later, Justel assured Oldenburg that, although the problem he proposed was easy to solve, "Mr. Dulaurens has not behaved like Mr. Hobbes." According to the editors of the *Oldenburg Correspondence*, Justel meant that Dulaurens, unlike Hobbes, was a competent mathematician (OC *Justel to Oldenburg 28 December 1667* IV 89).

25 OC *Dulaurens to Oldenburg 2 February 1666/7* III 336-337.

26 OC *Dulaurens to Oldenburg 2 February 1666/7* III 336.

27 OC *Oldenburg to Wallis 10 December 1667* IV 23.

properties of ye Circle to be drawn from thence."[28] So far, Oldenburg seemed to be trying to help his French acquaintance and was hopeful that Dulaurens' work would be as fruitful as he suggested.

Wallis's reply written three days later showed that he was already skeptical about Dulaurens. He wrote that he would be willing to read Dulaurens' book, especially if it "do answere ye title to wch it doth pretend."[29] The full title of Dulaurens' book, as Oldenburg had written in his letter to Wallis, was:

> Francis Dulaurens, *Specimens of Mathematics*, contained in two books, of which the first, synthetic, treats of genuine principles of mathematics generally, but especially of the true elements of geometry, never before discussed; the second, analytic, deals at length with the method of composition and resolution, and it includes many new things which wonderfully advance the most subtle art of analysis.[30]

Evidently Wallis expected, based on how unimpressive Dulaurens' problem was, that the *Specimina* would not be sophisticated enough to deliver the original and important contributions that this title promised. He told Oldenburg his solution to the problem, "at least as I thought it was to be understood, constructed and demonstrated (for it was pretty obscurely expressed). . . . What those new properties of the circle are, to be discovered here, which cannot be found in any other way, I do not so far see."[31] Given Wallis's attitude toward the work of foreign mathematicians in general and his skepticism about Dulaurens in particular, it seems that Wallis was predisposed to find fault with the *Specimina* when he eventually read it.

It soon became clear that Wallis was not the only member of the Royal Society who was disappointed with Dulaurens' problem. As Oldenburg wrote to Wallis on 11/21 February 1667/8, William Brouncker and John Collins agreed with Wallis that the problem had been poorly expressed.[32] Wallis later discussed it with Collins directly,

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28 OC *Oldenburg to Wallis 10 December 1667* IV 24.

29 OC *Wallis to Oldenburg 10 December 1667* IV 36.

30 That is, "Francisci Du Laurens Specimina Mathematica, duobus libris comprehensa, quorum Primus, Syntheticus, agit de Genuinis Matheseos principiis in genere, in specie autem de veris Geometriae Elementis hucusque nondum traditis. Secundus, Analytics, de methodo Compositionis atque Resolutionis fuse disserit, et multa nova complecitur, quae subtilissimam Analyseos artem mirum in modum promovent." The translation is that of the editors of the *Oldenburg Correspondence* (OC *Oldenburg to Wallis 10 December 1667* IV 23-24).

31 OC *Wallis to Oldenburg 10 December 1667* IV 36-39.

32 OC *Oldenburg to Wallis 11 February 1667/8* IV 172. Brouncker also solved Dulaurens' problem. For

writing that Collins' opinion of Dulaurens "answers my expectation. For the . . . glorious Title which was represented; & the great weight layd upon a slight Probleme, & that so lamely proposed that it was hard to pick out what he meant: made mee think there was no great matter to be expected from him."[33] Evidently Dulaurens' reminder that the difficulty of the problem was unimportant had been ignored, but this was just one of the several factors that were beginning to make English mathematicians question Dulaurens' abilities; the lack of clarity with which he posed the question was also detrimental. The feelings of these mathematicians were evidently enough to make Oldenburg begin to revise his own opinion of Dulaurens as well. He suggested that perhaps Dulaurens had consciously made his wording of the problem obtuse in order "to embarasse the facility of it."[34] He reported what Dulaurens wrote in the letter of 2/12 February – that he would send his own solution to Oldenburg with an explanation of what can be learned about the circle from it – but he added nothing to suggest that he was hopeful that Dulaurens would produce something impressive. He was similarly lukewarm about the *Specimina* which he had earlier mentioned with excitement: "His new Book, I find, is already on ye way: When it coms, such persons as you will soon discover his abilities."[35]

Oldenburg may have felt that he needed to distance himself from the increasing evidence that Dulaurens was a disappointing mathematician. It would not reflect well on him if a foreign mathematician whom he brought to the attention of prominent Royal Society members was incompetent. Thus, it seems, Oldenburg began to follow the lead of Wallis, Brouncker and Collins in disparaging Dulaurens,[36] and he became fully invested in Wallis's later effort to show that Dulaurens had plagiarized English mathematicians. As the editors of the *Oldenburg Correspondence* note, this was an example of Wallis reciprocating Oldenburg's help in Wallis's personal battles, such as the one with Hobbes.[37] As Steven Shapin explains, Oldenburg had a remarkable ability to maintain a

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his solution, see WC *Brouncker: Solution to Dulaurens's Problem [December 1667/January 1667/8]* II 394-395.

33 WC *Wallis to Collins 27 February/[8 March] 1667/8* II 429-430.

34 OC *Oldenburg to Wallis 11 February 1667/8* IV 172.

35 OC *Oldenburg to Wallis 11 February 1667/8* IV 172.

36 However, as the editors of the *Correspondence* point out, this did not prevent Oldenburg from printing part of a letter from Dulaurens of 29 February/10 March 1667/8 in the *Transactions* (OC *Dulaurens to Oldenburg 29 Feb. 1667/8* IV 216).

37 OC III xxiv. For information about Wallis's feud with Hobbes, see note 18 above.

guise of impartiality as he promoted the causes of particular members of the Royal Society, which seems to have included Wallis.[38] Oldenburg and Wallis exchanged services: Oldenburg would call on Wallis's mathematical expertise to find fault with mathematical work that the Society wanted to deride, such as that of Hobbes,[39] and in turn Oldenburg let Wallis use the *Philosophical Transactions* for self-promotion, as well as self-defence when the disputes with Hobbes and others became personal. It was often in the interest of both of these Society members to disparage foreign mathematicians; Wallis provided the expertise and Oldenburg provided the venue, in the form of both his correspondence network and the *Transactions*. Like several other Society members, Marie Boas Hall explains, Wallis sometimes wrote letters to Oldenburg that were "thinly disguised papers". Oldenburg would then "touch [them] up" and print them in the *Transactions.*[40] An example of their collaboration is the sixteenth issue of the *Transactions*, dated 5/15 August 1666, which consisted of only three items. The first described Wallis's theory of tides and the second was Wallis's "*Answer to some Objections, made by several Persons*" about that theory. The third was a description of Wallis's animadversions on Hobbes's *De principiis et ratiocinatione geometrarum*.[41] In short, Oldenburg and Wallis played distinct and complementary roles in the Royal Society's efforts to promote the works of English mathematics, including Wallis himself.

After receiving a copy of the *Specimina*, Wallis gave his very candid opinion of it in a letter to Oldenburg of 30 March/9 April 1668, writing that he felt the book had, as expected, failed to live up to the promises of its title.[42] Oldenburg printed this letter in the *Transactions* of 13/23 April.[43] Yet the problem was not simply that Dulaurens was a mediocre mathematician: Wallis accused Dulaurens of extensive plagiarism of English mathematicians. Wallis complained that much of Dulaurens' book "seems to be taken out of Mr [William] *Oughtred* & my self, (though he doth not there so much as name either

38 Steven Shapin, "O Henry," review of *The Correspondence of Henry Oldenburg*, eds. A. Rupert Hall, Marie Boas Hall and Eberhard Reichmann, *Isis* 78 (1987): 421, 424. See also Johns, *Nature of the Book*, 498.

39 Shapin, "O Henry", 423. For information about Wallis's feud with Hobbes, see note 18 above.

40 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): 186.

41 *Phil. Trans.* 16 (6 August 1666) 263-294.

42 OC *Wallis to Oldenburg 30 March 1668* IV 285-286.

43 OC *Wallis to Oldenburg 30 March 1668* IV 285-292; *Phil. Trans.* 34 (13 April 1668) 654-655. As the dispute dragged on, Oldenburg published this letter again in *Phil. Trans.* 38 (17 August 1668) 748-750.

of us,) & that so evidently, that he doth many times not onely retain the peculiar phrases & forms of speaking, but ye very same Notes & Symbols."[44] As was typical of early modern priority disputes, there was no clear boundary between challenges to priority and to the quality of the work in question. Wallis showed this by moving into a rather polemical critique of Dulaurens' mathematics. For example, Wallis wondered "with what confidence (or gross negligence,) [Dulaurens] should publish, in ye face of all the world, a thing so false." Wallis went on to criticize Dulaurens' spelling, grammar and word choices and suggested that Dulaurens was led to such errors by a combination of negligence and ignorance.[45] Thus, while Wallis' letter opened with a complaint about priority, it became a systematic dismantling of Dulaurens' work. It seems likely that the threat to Wallis' priority and to that of his respected countryman Oughtred provoked this very thorough critique.

Dulaurens was eager to hear what Wallis thought of his work, and Wallis's attack came as quite a shock. Justel wrote a letter indicating Dulaurens' surprise that Wallis was apparently provoking a dispute.[46] Dulaurens himself wrote to Oldenburg on 13/23 May 1668 suggesting that Wallis had overreacted and denying the charge of plagiarism. He claimed never to have read Wallis' work, and he argued that what he had borrowed from Oughtred was "so commonplace that I did not think it was necessary to credit so celebrated an author with such a trifle."[47] If one takes Dulaurens at his word here, then he appears to have been a victim of the lack of guidelines governing whom one must cite and when. Indeed, what by modern standards would be the most egregious form of plagiarism – reproducing someone else's work verbatim without an explicit citation – was, according to Dulaurens, a way of giving credit to Oughtred: "I transcribed these

44 OC *Wallis to Oldenburg 30 March 1668* IV 286. William Oughtred (1574-1660) was an accomplished English mathematician who was himself involved in a passionate priority dispute with his fellow English mathematician, Richard Delamain (1600-1644), over the invention of the Horizontal Quadrant, which could be used for navigation or as a type of sundial, and the Circles of Proportion, a circular slide rule. See Jess Edwards, *Writing, Geometry and Space in Seventeenth-Century England and America: Circles in the Sand* (London: Routledge, 2006), 102, 116-118; Mordechai Feingold, "Science as a Calling? The Early Modern Dilemma," *Science in Context* 15 (2002): 101-103; Katherine Hill, "'Juglers or Schollers?': Negotiating the Role of a Mathematical Practitioner," *The British Journal for the History of Science* 31 (1998): 253-254; A. J. Turner, "William Oughtred, Richard Delamain and the Horizontal Instrument in Seventeenth Century England," *Annali Dell'Istituto e Museo di Storia della Scienza di Firenze* 6 (1981): 99-125.

45 OC *Wallis to Oldenburg 30 March 1668* IV 285-288.

46 OC *Justel to Oldenburg 2 May 1668* IV 370, *Justel to Oldenburg 13 May 1668* IV 405.

47 OC *Dulaurens to Oldenburg 13 May 1668* IV 400-401.

things word for word so that everyone could see what I had done". Dulaurens then contradicted himself, claiming that he had intended to cite all his sources, but ran out of room.[48] Dulaurens asked Oldenburg to arbitrate this dispute for them, writing, "I wish for no other judge than yourself (if you will take the trouble to read my book) to decide whether Mr. Wallis had cause to make such claims as he has."[49] Apparently Oldenburg's guise of impartiality was working. Dulaurens concluded by expressing equal surprise that Wallis had criticized the quality of his work, and he promised to respond to these points in a coming letter, which was later printed under the title *Responsio Francisci du Laurens ad Epistolam D. Wallisii ad Cl. V Oldenburg scriptam.*[50] Dulaurens used this latter work to defend himself as a mathematician, but his first concern had been to respond privately to the dishonourable charge of failing to recognize the priority of established mathematicians.

Because Wallis and Dulaurens expressed different criteria for determining what was plagiarism – what Wallis considered a deceitful act of intellectual property theft, Dulaurens considered an obvious allusion – it would be worthwhile to determine whom contemporaries would have agreed with, and if this was simply a misunderstanding based on divergent local interpretations of plagiarism. Did the English and their continental counterparts differ regarding what was considered plagiarism, or was Dulaurens' definition the result of panic because he knew he had been caught? Among Englishmen involved in many priority disputes, Wallis, Oldenburg, and Clarke, for instance, all used cognates of the word "plagiarism", but so did De Graaf; were they referring to the same thing?[51] While Paulina Kewes has rightly pointed out that early modern perceptions of plagiarism were often local, temporally specific and contingent on political and cultural developments,[52] this argument serves mainly to add nuance to an otherwise accurate belief among modern scholars that plagiarism was a stable concept at its core throughout the early modern period, a time of unprecedented concern about the security of

48 OC *Dulaurens to Oldenburg 13 May 1668* IV 400-401. The quotation is from p. 400.
49 OC *Dulaurens to Oldenburg 13 May 1668* IV 400-401.
50 OC *Dulaurens to Oldenburg 13 May 1668* IV 401.
51 Wallis, Clarke and De Graaf used the Latin word "plagii"; Oldenburg used the French word "plagiaire" (OC *Wallis's Animadversions on Dulaurens* IV 491, *Oldenburg to Huygens 29 March 1669* V 463, *Clarke to Oldenburg 10 May 1669* V 531).
52 Paulina Kewes, ed., *Plagiarism in Early Modern England* (New York: Palgrave Macmillan, 2003), 16-17 (editor's introduction).

intellectual property. Early modern conceptions of plagiarism must be set against the backdrop of the Renaissance notion for *imitatio*, a humanist approach to texts (especially literature) which lingered during the seventeenth century. The concept of *imitatio* not only tolerated but encouraged borrowing from both classical and contemporary writers; it was a sign of a writer's wit as long as he avoided "servile imitation." The idea was to take inspiration from other sources and make the content therein one's own; this was a sign of high-quality, creative literature, whereas simple appropriation was seen as a failure of originality and evidence of the writer's mediocrity.[53] Plagiarism, then, as Hall Bjørnstad argues, was a point on the spectrum of imitation, at the other end of which were perfectly acceptable forms of literary borrowing.[54]

This conception of early modern plagiarism is helpful in understanding how such borrowing functioned, but it reveals the difficulty of determining which historical actors were breaching the code of conduct of the intellectual community. As Bjørnstad suggests, the fact that certain forms of imitation were acceptable muddled the definition of plagiarism. All that separated acceptable immitation from plagiarism was a "blurred line".[55] If plagiarism was a reflection of mediocrity, then the identification of plagiarism was closely tied to the highly subjective identification of bad writing. Early modern writers exploited this ambiguity by deeming the same acts of appropriation acceptable when performed by a member of their social group, and unacceptable when performed by an outsider. For instance, Roy Eriksen argues that Christopher Marlowe tolerated Shakespeare's borrowing from his plays and taking inspiration from his style because Marlowe, although a member of the group of playwrights who received a humanist university education, also hobnobbed with the less well-educated social circle centred around the theatre of which Shakespeare was a member. He therefore viewed Shakespeare as both a friend and a rival, and permitted Shakespeare to borrow from his works and write about the same subjects in the spirit of friendly competition. However, Robert Greene, another university-educated dramatist, refused to associate with

53 Ronny Spaans, "*Imitatio* and Plagiarism in the Dutch Renaissance: An Interpretation of Joannes Six van Chandelier's 'Teegen 't lasterschrift op Gerrit Brand'" in Bjørnstad, *Borrowed Feathers*, 202-203. The quotation is from p. 203.
54 Bjørnstad, *Borrowed Feathers*, 5.
55 Bjørnstad, *Borrowed Feathers*, 5-6. The quotation is from p. 6.

Shakespeare's grammar-school crowd and warned Marlowe that Shakespeare was a plagiarist.[56] R. E. W. Maddison identifies a similar case of differing attitudes toward the same act of literary borrowing: the reader of an anonymous collection of *Moral Essays and Discourses* published in the 1690s recorded in his copy of the book that he was sure it was "a great plagiary" of Robert Boyle's *Free Discourse against Customary Swearing.* This reader was unaware that the anonymous writer was Boyle's brother Francis, who had evidently gotten permission from Robert to recycle passages from a treatise that Boyle had written decades earlier and was only printed posthumously; it seems that neither Francis nor Robert considered this plagiarism.[57] As early modern plagiarism was rarely a legal matter,[58] the tribunal in cases of perceived plagiarism truly was one's peers in the intellectual community, complete with their biases and multiple loyalties.

What this scholarship illustrates is that plagiarism – or the early modern English cognate "plagiary" which could refer both to the act of illicit literary borrowing and to the plagiarist himself[59] – was a concept that early modern figures acknowledged and conceived of in roughly the same way, but they applied different standards to different people depending on their social proximity to the person engaged in literary borrowing. When evaluating claims of plagiary, early modern figures considered the identity of the accused writer in conjunction with the content of his text; in such cases English men were likely to be suspicious of various kinds of "others" including women, non-elite men, and, as in this case, foreigners.[60] It is therefore appropriate to adopt Richard Terry's practice of regarding early modern charges of plagiarism as speech acts that said something about both the accuser and the accused.[61] What John Wallis considered plagiarism of William Oughtred and himself in Dularens' *Specimina*, he may not have considered plagiarism in a work by another Englishman, especially a member of the Royal Society. Adrian Johns examines how members of the Royal Society pooled their resources to reduce the risk of

56 Roy Eriksen, "Friends or Foes? Marlowe and Shakespeare as Rivals in the Republic of Letters" in Bjørnstad, *Borrowed Feathers*, 191-200.

57 R. E. W. Maddison, "The Plagiary of Francis Boyle," *Annals of Science* 17 (1961): 111-113. The reader's comments are quoted by Maddison on p. 111.

58 Laura J. Rosenthal, *Playwrights and Plagiarists in Early Modern England: Gender, Authorship, Literary Property* (Ithaca: Cornell University Press, 1996), 3.

59 OED, "Plagiary," accessed 13 July 2011.

60 See Rosenthal, *Playwrights and Plagiarists*, 3-4.

61 Richard Terry, "'In Pleasing Memory of All He Stole': Plagiarism and Literary Detraction, 1747-1785" in *Plagiarism in Early Modern England*, ed. Paulina Kewes [New York: Palgrave Macmillan, 2003], 182.

unauthorized borrowing and reproduction of their printed texts. Among the tools they employed were the *Philosophical Transactions*.[62] His brother's recycling of his discussion of swearing notwithstanding, Robert Boyle in particular was concerned about people stealing credit not only for his printed works, but also his hitherto unprinted works. Repeatedly, people such as visitors who had access to his manuscripts stole them and had them printed before Boyle had a chance. This made Boyle rush unfinished works into print and keep the pages of his manuscripts separate so they would be more difficult to steal. Reluctant as Boyle was to engage in polemical priority disputes, his paranoia about plagiarism, which was not entirely unwarranted, led him to seclude himself in his laboratory and avoid talking to people.[63] Yet at the same time, as William Newman points out, Boyle himself practised unacknowledged intellectual borrowing from alchemists, whose influence on him could have embarrassed Boyle if people knew about it. Newman does not suggest that Boyle should be regarded as a plagiarist; rather, he was operating within the context of an intellectual community that borrowed liberally but manipulated the perception of that borrowing to project the sort of image that suited their needs.[64] Considering that a single act could be seen as plagiarism by some and not by others, it seems that Dulaurens, in denying that his appropriation of Oughtred's words, was not appealing to a particularly French definition of plagiarism, but rather appealing to a transnational definition of plagiarism whose application to particular cases was highly subjective. Dulaurens hoped to suggest that he was a member of the same intellectual community as Oughtred, and Wallis maintained that he was not.

In May 1668, Justel warned Oldenburg that Dulaurens' *Responsio* to Wallis's accusations was "very salty" and noted that he had discouraged Dulaurens from sending the polemical text to Oldenburg by post.[65] Justel apparently felt, like Boyle, that it was more important to maintain decorum than to defend one's priority with polemics. In June he wrote a letter to Oldenburg that commented on the Wallis-Dulaurens dispute,

62 Johns, *Nature of the Book*, 445-446, 498-504. Indeed, Johns argues that despite the difficulty of keeping the *Transactions* afloat, the journal became a more useful guarantor of priority than even the register books: "In the wider world, it stood to become a register in its own right" (Johns, *Nature of the Book*, 501).
63 Johns, *Nature of the Book*, 504-508.
64 William R. Newman, "The Alchemical Sources of Robert Boyle's Corpuscular Philosophy," *Annals of Science* 53 (1996): 567-585, especially 585.
65 OC *Justel to Oldenburg 27 May 1668* IV 429.

lamenting that "literary men dispute and destroy one another, because that makes them look ridiculous."[66] Justel's next letter to Oldenburg a few days later expressed further displeasure with Dulaurens' and Wallis's polemics – he doubted that they would yield the truth of the matter – and suggested that Dulaurens, at least in Justel's opinion, was responding to a feeling that his honour has been publicly threatened: "If Mr. Wallis's letter had not been printed [Dulaurens] would not have cared about it."[67] Dulaurens had apparently not concealed how highly he valued the honour that came with making an original contribution to mathematics, and was therefore subject to the disapproval of his peers. While Justel found such disputes distasteful, he did not apparently find them very surprising. He told Oldenburg in a letter written on 4/14 July that he was sure that "Mr. Wallis will reply with some heat, and so the quarrel will grow hotter."[68] Justel implied that the intellectuals of his time had a need to defend their honour that often trumped their supposed aversion to conflict.[69]

Wallis continued to escalate the conflict with more polemics in his animadversions to the *Responsio*. He enclosed these with a letter to Oldenburg written on 2/12 July, and Oldenburg printed them in the *Philosophical Transactions* of 17/27 August.[70] Although he did not shy away from insulting Dulaurens, Wallis blamed Oldenburg for provoking this dispute. Not only did Oldenburg print what was supposed to be a private letter, he also printed only the part of the letter in which Wallis complained

66 OC *Justel to Oldenburg 7 June 1668* IV 453.

67 OC *Justel to Oldenburg c. 10 June 1668* IV 462.

68 OC *Justel to Oldenburg 4 July 1668* IV 502.

69 Although Justel did not approve of the dispute, he eventually took a side in it. He told Oldenburg in a letter of 15/25 July that he "found Mr. Dulaurens' reply too bitter. I tell him my opinion; but authors' passions rise so high that they are incapable of listening to reason. I do not think Mr. Wallis treats him badly; our people are certain of it." As discussed in Chapter 3, Justel was also sympathetic to the English cause during the transfusion dispute (OC *Justel to Oldenburg 15 July 1668* IV 545). In Newton's conflict with Hooke about the discovery of the inverse-square law, we see another example of a natural philosopher's supposed preference for reconciliation being abandoned when his opponent continued to challenge his priority. Newton wrote to Halley on 27 May/6 June 1686 in response to news of Hooke's priority claim. His letter began, "I thank you for wt you write concerning Mr Hook, for I desire that a good understanding may be kept between us." Newton then proceeded to explain why he thought Hooke had not really expressed the inverse-square law in a letter to him. Nevertheless, after learning that Hooke had refused to yield and had loudly insisted on his own priority at a Society meeting, Newton wrote a tirade against Hooke in his next letter to Halley of 20/30 June (NC *Newton to Halley 27 May 1686* II 433, *Newton to Halley 20 June 1686* II 437-440).

70 OC *Wallis's Animadversions on Dulaurens* IV 492; *Phil. Trans.* 38 (17 August 1668) 744-750. The editors of the *Oldenburg Correspondence* note that Wallis suggested minor changes to the wording of his animadversions in several letters before Oldenburg had them printed (OC *Wallis's Animadversions on Dulaurens* IV 495).

of the "injury done to me" and this was what had angered Dulaurens. It was Oldenburg who decided what to do with the letter, which, in any case, Wallis wrote as a favour to Oldenburg.[71] Dulaurens, however, was hardly blameless; his "injustice" was calling Wallis a "braggart" in the *Responsio*. Wallis wrote that he was willing to grant that Dulaurens had not read his works. However, he would not accept that the contents of the *Specimina mathematica* had "never as yet [been] discussed" as Dulaurens claimed. In short, what Wallis wanted most was that his priority (and that of Oughtred) be acknowledged.[72] He asserted that he had caught Dulaurens redhanded: "So obvious are the traces of this borrowing [from Oughtred] that now he cannot but confess it although he at first withheld the name."[73] After providing all of this evidence of Dulaurens' failure to respect priority, Wallis took further steps to deny his willing involvement in this dispute. He again admonished Oldenburg for publishing only part of the letter, and insisted that he only wanted to convey the truth of the matter as another favour to Oldenburg. He did not want

> to make a charge of plagiary on either account (which he [Dulaurens] tries to refute) but to let you know, as the case is, that his principles, in so far as they are rational, were both known to others before and discussed long ago by others . . . Nor are they now first discovered, nor 'never as yet discussed.' And I pointed out to you in what authors you might find the same matters treated and (to my mind) no less happily.[74]

It is striking how adeptly Wallis defended his and Oughtred's priority while denying that he was motivated by his desire to do so. Wallis balanced the need to protect the honour that Dulaurens had slighted by failing to recognize English priority (and insulting him in the *Responsio*) with Boyle's ideal of avoiding conflict.

Oldenburg shifted the responsibility for sparking the dispute onto Dulaurens when he printed Wallis's animadversions in the *Philosophical Transactions*. Oldenburg claimed that, in publishing Wallis's first letter about the *Specimina mathematica*, he had only done "what he thought Justice required of him".[75] Now, similarly, although he "wished very much, that he might not be necessitated to say any more of this subject", he was

71 OC *Wallis's Animadversions on Dulaurens* IV 492.
72 OC *Wallis's Animadversions on Dulaurens* IV 493.
73 OC *Wallis's Animadversions on Dulaurens* IV 494.
74 OC *Wallis's Animadversions on Dulaurens* IV 494.
75 *Phil. Trans.* 38 (17 August 1668) 747.

compelled not only to print Wallis's latest letter denouncing Dulaurens but also to reprint Wallis's first letter because Dulaurens had wondered in the *Responsio* why Wallis had decided to write against him. Oldenburg spared Wallis any blame here, noting that he had merely animadverted Dulaurens' book "*en passant*" because Oldenburg had asked him to do so. Oldenburg suggested that Dulaurens, conversely, had ensured that the conflict would continue; it was "unavoidable to comply with [Dulaurens] in that demand, and to publish, what (out of respect to the same) was supprest ever since that Vindication was printed".[76] Oldenburg, like Wallis, claimed to serve both the truth and the honour of English mathematicians.

Wallis was not finished animadverting on Dulaurens. Part of his next letter on the subject was printed in the *Philosophical Transactions* of the following month.[77] In that issue, Oldenburg made his supposed aversion to such conflicts explicit: he claimed to publish the letter "*rather from a desire, further to comply with the said* Du Laurens, *demanding the reasons of the Animadvertex's Censure, than from any propension to disputes.*" Oldenburg then fully endorsed Wallis's letter, remarking that he "*can* bonafide *assure*" that its contents were accurate.[78] Both he and Wallis, while they strove to ensure the recognition of English priority, glossed over their active roles in the dispute with Dulaurens.

Dulaurens and Wallis each wrote a subsequent letter to support their cases. Upon hearing that Dulaurens had written another letter, Wallis told Oldenburg that he might write "8 or 10 lines; and no more" in response.[79] Wallis was not willing to let Dulaurens have the last word, but he was careful to indicate that he was not very enthusiastic about continuing the conflict. Similarly, he wrote on 19/29 January that, in case Dulaurens wrote another letter, Oldenburg should add the following when he printed Wallis's recent letter in the *Transactions*: "This is said of his second paper; the same may be applied to his third or fourth (if he shall write more). For it will not be necessary to go over them

76 *Phil. Trans.* 38 (17 August 1668) 747-748.
77 Oldenburg, having run out of room, printed the remainder of this letter in November. *Phil. Trans.* 39 (21 September 1668) 775-779; 41 (16 November 1668) 825-832.
78 *Phil. Trans.* 41 (16 November 1668).
79 OC *Wallis to Oldenburg 7 November 1668* V 137. Wallis retained this attitude – continuing to argue with Dulaurens while acting as though he had no interest in the dispute – at least until a letter of 21/31 December in which he made changes to his argument against Dulaurens and remarked, "And then, I hope, I have done with him" (OC *Wallis to Oldenburg 21 December 1668* V 272-273).

word by word, any more than the book itself. I shall take no more notice of him."[80] Wallis wanted the readers of the *Philosophical Transactions* to understand that he was not tacitly conceding to Dulaurens by no longer replying to his letters; rather, he was ending the conflict on his own terms. However, Oldenburg, perhaps having come to regret his role in the conflict or feeling pressure to stop perpetuating it, decided to print neither Dulaurens' nor Wallis' next letter.[81] After all, Oldenburg needed at least to appear impartial and averse to conflict and may have realized that he was risking that image by perpetuating the dispute. This realization may have been prompted by a letter from Justel, written on 31 October/10 November 1668, denouncing Dulaurens' latest attack in a dispute of the sort "which produces nothing but quarrels and which discredits men of letters." He told Oldenburg, "You will do well not to put any of this kind of writing in your *Transactions* in the future."[82] This was not, of course, the last time that the *Philosophical Transactions* would play a central role in a priority dispute, but Oldenburg seems to have understood Justel's point and he did what he could to suppress this dispute. Justel's frequent objections to the dispute were a signal that it would no longer be honourable to allow the *Transactions* to serve as a public forum for the conflict, and Oldenburg responded accordingly. In any case, it seems that contemporaries considered Wallis the clear winner of the dispute as even mathematicians in France began to disparage Dulaurens' mathematics. Justel told Oldenburg that Huygens was examining Dulaurens' claim to have solved the difficult problem of finding mean proportionals because other French mathematicians doubted that he had been successful. Although Huygens supported Dulaurens at first, his examination showed that Dulaurens had failed, and he made this clear to Dulaurens.[83] Wallis was pleased to hear of this failure from Oldenburg, writing, "Certainly he is no competent undertaker of such designs: & but a very indifferent superficial mathematician."[84] There are few traces of Dulaurens after this dispute;[85] it

80 OC *Wallis to Oldenburg 19 and 21 January 1668/9* V 343-344.
81 The editors of the *Oldenburg Correspondence* explain this in the notes to *Justel to Oldenburg 31 October 1668* V 123-124. Wallis gave permission to Oldenburg to do as he saw fit with the letter in *Wallis to Oldenburg 30 November 1668* V 210.
82 OC *Justel to Oldenburg 31 October 1668* V 123.
83 OC *Justel to Oldenburg 13 May 1668* IV 405-406, *Justel to Oldenburg 27 June 1668* IV 479, *Justel to Oldenburg 1 July 1668* IV 486, *Justel to Oldenburg 15 July 1668* IV 545. A mean proportional is a value between a and b where $a/x = x/b$.
84 OC *Wallis to Oldenburg 20 July 1668* IV 560.
85 Scott, *Mathematical Work of John Wallis*, 188.

seems that the rest of the mathematical community agreed with Wallis that Dulaurens had failed in his effort to earn a reputation for mathematical innovation.

4.3: Wallis's Ongoing Work as a Defender of English Inventions and Discoveries

Oldenburg's refusal to publish any other letters related to the Wallis-Dulaurens dispute eventually succeeded in quelling Wallis's interest in the conflict, but Wallis continued to seize any opportunity to promote his own priority and that of his fellow Englishmen. In the same letter in which he hoped to "have done" with Dulaurens, he referred to natural philosophers, including himself, who had anticipated Hooke's work on physics which suggested that "*springyness is ye cause of rebounding*", and he listed Oldenburg as a witness that he had come to this conclusion before Hooke.[86] In another letter less than two weeks later he claimed that the English had anticipated Giovanni Borelli's recent work on the Medicean Stars in the early 1660s.[87] Despite his repeated insistence that his involvement in the dispute with Dulaurens was reluctant, Wallis would apparently take any opportunity to inform Oldenburg of any instance of plagiarism or multiple discovery involving Englishmen that he perceived.

Nor was Oldenburg the only Society member with whom Wallis corresponded about matters of priority and disagreements with foreign mathematicians. Occasionally he discussed priority with his friend and former collaborator, Viscount William Brouncker. In two papers, Jacqeuline Stedall describes how Wallis and Brouncker worked together on mathematical problems frequently in the 1650s before Brouncker became occupied with his duties as President of the Royal Society.[88] Associating himself with Lord Brouncker helped Wallis to advance his career, and the partnership was beneficial to

86 OC *Wallis to Oldenburg 21 Dec. 1668* V 274. Wallis evidently thought better of starting a dispute within the Society (although, as will be discussed later, he was sometimes quite willing to engage in such disputes) and he excused Hooke from any charge of plagiarism. He wrote, "I do not take ye notion to be very common: (and I am not sorry if Mr [Hooke] bee of my mind)" (OC *Wallis to Oldenburg 21 Dec. 1668* V 274-275).

87 OC *Wallis to Oldenburg 2 Jan. 1668/9* V 303.

88 Jacqueline A. Stedall, "The Collaborations of Wallis and Brouncker I. Squaring the Circle," *Notes and Records of the Royal Society of London* 54 (2000): 293-316; Jacqueline A. Stedall, "The Collaborations of Wallis and Brouncker II. Number Problems," *Notes and Records of the Royal Society of London* 54 (2000): 317-331. Stedall explains that their partnership thrived despite their very different political views (Stedall, "Wallis and Brouncker I", 295-296).

Brouncker as well: the only mathematical work by Brouncker that was published before he became President of the Royal Society was that which was included in Wallis's own texts. Wallis ensured that Brouncker's work was published (especially his notable discovery of continued fractions) because, as Stedall writes, Wallis was "[a]lways concerned that English mathematicians should be given their due".[89] After the Dutch mathematician Hendrick van Heuraet caught the attention of the likes of Huygens and Sluse for his rectification of the semicubical parabola, Brouncker wrote to Wallis, "Indeed Heuraets invention is perfectly equipollent to Mr [William] Neil's; &, for ought I know, he might have it from thence."[90] The two also worked together on mathematical problems proposed by Fermat in 1657, problems which Fermat "portrayed . . . as a duel between the English and French". Wallis was initially dismissive of Fermat's problems but was drawn in by Fermat's threats to the honour of English mathematics and of Wallis himself. Fermat criticized Wallis's *Arithmetica infinitorum* (1655), challenging Wallis's quadrature of the circle and denying his priority in the quadrature of certain curves. With Brouncker's help, Wallis produced solutions to the problems that Fermat himself accepted.[91]

Wallis also communicated regularly with John Collins, a mathematician and

89 Stedall, "Wallis and Brouncker I", 294-296. The quotation is from p. 296.

90 WC *Brouncker to Wallis 26 November/[6 December] 1659* I 589. Wallis, Brouncker and Christopher Wren again argued for Neile's priority for this rectification against Huygens, who argued in favour of the late Heuraet, when the dispute resurfaced in 1673. For accounts of the priority disputes over the rectification of the semicubical parabola see V. Frederick Rickey, "Isaac Newton: Man, Myth, and Mathematics," *College Mathematics Journal* 18 (1987), 362-389 in *Sherlock Holmes in Babylon and Other Tales of Mathematical History* eds. Marlow Anderson, Victor Katz and Robin Wilson (Washington: The Mathematical Association of America, 2004), 250; Iliffe, "In the Warehouse," 33; Stedall, "Wallis and Brouncker I," 310; van Maanen, "Hendrick van Heuraet," 218-279. In 1673 the dispute between Wallis and Huygens featured in the *Philosophical Transactions*. This included the publication of a "Letter of Doctor John Wallis about the FIRST discovery and demonstration of the rectification of the parabolic curve, in the year 1647, reporting that was done by Doctor William Neile" (the Latin reads, "Epistola Doct. *Johannis Wallisii,* PRIMAM Inventionem & Demonstrationem Æqualitatis Curvæ *Paraboloidis* cum Recta, anno 1657. factam, Dn. *Guilielmo Neile* p. m. Afferens". My translation. *Phil. Trans.* 98 [17 November 1673]: 6146-6150).

91 Stedall, "Wallis and Brouncker II," 318-326. To square or find the quadrature of a circle means to find a square with the same area so that that area can easily be calculated; likewise, the quadrature of a curve refers to the area of the space underneath that curve. The quadrature of the circle in particular had long been considered a nearly impossible mathematical problem, so Wallis took much pride in what he considered an accurate way of squaring the circle. See Scott, "The Reverend John Wallis," 58, 65 Stedall, "Wallis and Brouncker I," 298-299. Stedall suggests that Wallis embraced the role of promoting English mathematical achievements in this encounter with Fermat. She claims that Wallis's letters seem to have been intended for publication when he wrote them, adding that "[n]either brevity nor modesty were Wallis's style" (Stedall, "Wallis and Brouncker II," 325, 327).

librarian for the Royal Society known for his extensive correspondence.[92] Indeed, Wallis seems to have corresponded with Collins more often than with Oldenburg, perhaps because, as Marie Boas Hall explains, Collins had a better and more current understanding of mathematics.[93] It was to Collins that Wallis complained in 1677 when he feared that continental mathematicians would steal the discoveries of Englishmen like Isaac Newton:

> I suspect (as I have formerly intimated) that of such communication (of things not published) they make but ill use beyond sea. And particularly, yt [Paris's Professor of Mathematics Claude de] Comiers New Notion of Two means proportional &c was borrowed from those Papers, where you mention . . . Mr Newtons making use of *two movable Angles*. And no doubt but many other particulars therein imparted, will shortly be published as French inventions.[94]

In another letter to Collins of the previous year, Wallis recommended that correspondence with the French be carried out in print, "they being apt to be disingenuous, in claiming all for their own which they have from hence, without owning whence they have it."[95] Niccolò Guicciardini explains that Collins's correspondence network allowed Newton to enhance his reputation for mathematics among the likes of Brouncker, Oldenburg and Wallis. It was Collins who sent Wallis a transcription of a copy of Newton's *De analysi per aequationes numero terminorum infinitas* in 1677.[96] Both of them became strong supporters of Newton in matters of priority (in Wallis's case, particularly in the dispute over infinitesimal calculus with Leibniz)[97] and both of them encouraged Newton to

92 See A. Rupert Hall, "John Collins on Newton's Telescope," *Notes and Records of the Royal Society of London* 49 (1995): 71-78; Guicciardini, *Mathematical Certainty and Method*, 351.

93 Boas Hall, "Diffusion of Information," 182.

94 NC *Wallis to Collins 8 October 1677* II 238.

95 CSM *Wallis to Collins 16 September 1676* II 600.

96 Collins had received it from Isaac Barrow (Guicciardini, *Mathematical Certainty and Method*, 12, 347, 351-352).

97 A. Rupert Hall gives a thorough account of Wallis's role in Newton's career, particularly regarding Leibniz and calculus. Newton acknowledged Wallis as a major mathematical influence on him, and he retrospectively claimed that Wallis had been witness to Newton's discovery of calculus in the mid-1660s, a time when Wallis did not yet know who Newton was (NC *Collins to Wallis ? 1667/8* II 244; A. Rupert Hall, *Philosophers at War: The Quarrel between Newton and Leibniz* [Cambridge: Cambridge University Press, 1980], 8, 12-13, 16. See also Guicciardini *Mathematical Certainty and Method*, 4, 26, 112, 155, 165-166 for books by Wallis and others that Newton owned and annotated, as well as similarities between the mathematical styles of Newton and Wallis). It should be noted that, according to Hall, neither Collins nor Wallis (nor Oldenburg nor even Newton himself) seems to have felt much animosity toward Leibniz during the 1670s and 1680s. When he commented on the issue of priority for calculus in the mid 1690s, Wallis, true to form, admitted that he knew little of Leibniz's work on calculus but still insisted that Newton had preceded him in it. Yet this did not prevent Wallis and Leibniz from developing a friendly correspondence

publish his various works.[98]

In 1671/2 Wallis wrote to Collins about the inadequacy of ciphers for protecting English inventions.[99] In the same letter he openly discussed the possibility of adding the unpublished work of the French mathematician Antoine de Laloubère to an English publication, seemingly for the purpose of sharing part of the credit for this work. Of course, he recommended this only if "it do not hinder the printing those of our own nation, who I doubt not have things as considerable as theirs [i.e., those of the French], which lie by the wall for want of publishing."[100] Wallis added that he wished Hooke and Newton would publish something about their telescopic innovations so that foreigners would not do what he was proposing to do to Laloubère, that is, "steal from us what our nation invents, only for our neglect to publish them ourselves."[101]

Wallis and Collins frequently discussed the other way in which Wallis promoted English mathematical discoveries: by writing about them in his own publications. As Hall explains, Wallis was working on "a large narrative of British mathematics" in the late 1670s which grew into his *Treatise of Algebra* of 1685.[102] Collins spoke on behalf of the Society in directing Wallis to include the work of certain Englishmen in 1677/8:

> That tis the Designe of the Royall Societie to promote and encourage the printing of Mathematicks and other bookes of Art in our owne tounge [*sic*]
>
> Whereas you have given an Account of the learned paines of Englishmen namely of Harriot, your owne works, Mr Newton, you may yet adde one Stone

from 1696 until Wallis's death in 1703, even though Leibniz had noticed (and been amused by) Wallis's tendency to act as though every discovery or invention was English. To Leibniz this was simply "laudable patriotism". If anything, Wallis became more sympathetic to Leibniz, offering him at least a small share in the credit for calculus, toward the end of his life. Wallis remained an outspoken supporter of Newton and had a role in intensifying the conflict, but Hall suggests that he did so even though he knew that priority should have been awarded to Leibniz (Hall, *Philosophers at War*, 38, 46-47, 74, 76, 95-99, 111-113, 121, 129. The quotation is from p. 121. See also Guicciardini, *Mathematical Certainty and Method*, 365; NC *Collins to Wallis ? 1677/8* II 242). Collins died in 1683, and thus was not available to support Newton as the dispute with Leibniz escalated in the late 1690s and the early 18th century. Still, Newton referred to his correspondence with the late Collins in support of his own case during that dispute, and Collins emphasized the priority of Newton's discoveries in general when he was alive (Hall, *Philosophers at War*, 16-20, 50, 130, 169-170).

98 See Guicciardini *Mathematical Certainty and Method*, 364; Hall, *Philosophers at War*, 17, 50, 113; Christoph J. Scriba, "Mercator's Kinckhuysen-Translation in the Bodleain Library at Oxford." *The British Journal for the History of Science* 2 (1964): 53; NC *Newton to Wallis 27 August 1692* III 219-220.

99 CSM *Wallis to Collins 25 January 1671/2* II 529.

100 CSM *Wallis to Collins 25 January 1671/2* II 529. See also CSM *Wallis to Collins 19 January. 1668/9* II 508-509. Wallis thought that Laloubère was "a learned man and very good mathematician, only somewhat intricate, and not so clear in his delivery of his notions" (CSM *Wallis to Collins* II 529).

101 CSM *Wallis to Collins 25 January 1671/2* II 530.

102 Hall, *Philosophers at War*, 75.

more to the Monument of the fame of the English Writers namely
To Commend the deceased Dr [Isaac] Barrow for setting such a studious painfull and learned Successor as Mr Isaac Newton . . .[103]

In the early 1670s Wallis and Collins exchanged letters about what should be included in a work that Wallis edited, the *Opera posthuma* (1672) of Jeremiah Horrocks (c. 1619-1641), an English astronomer known for observing the transit of Venus.[104] Wallis had a clear sense of how the memory of an English scholar should be preserved. He argued with John Flamsteed who wanted to replace a problematic letter in which Horrocks

103 NC *Collins to Wallis ? 1677/8* II 241. Although he worked closely with Wallis to promote the work of English mathematicians, Collins was evidently not pleased with what he considered Wallis's breaches of the etiquette of the Republic of Letters in doing so. In the same letter, he indicated his reluctance to send Newton's work on infinite series to Wallis: "If I had been so minded I could about 9 yeares since namely at the beginning of 1669 have imparted to you a full treatise of his of that Argument but did not, in regard you lye under a censure from diverse for printing discourses that come to you in private Letters without permission or consent as is said of the parties concerned." Collins then proceeded to express his respect for Leibniz as a Royal Society member and member of the Privy Council in Hanover. In a manner reminiscent of Oldenburg's attitude concerning Huygens' laws of motion, Collins reminded Wallis that, while the Royal Society would be doing Leibniz a favour by "expalin[ing] his doctrine more largely than himselfe could have leisure to doe", this must be done "without incurring any censure for publishing what was occasioned by his private Letters." It seems that Collins was expressing the view that, while emphasizing the accomplishments of English mathematicians was important and Wallis was well equipped to do so, it would not be wise to alienate an increasingly prominent foreign member of the Society (NC *Collins to Wallis ? 1677/8* II 242).

104 For an account of Horrocks' work and how his *Opera posthuma* came to be published, see H. C. Plummer, "Jeremiah Horrocks and His 'Opera Posthuma'," *Notes and Records of the Royal Society of London* 3 (April 1940-September 1941): 39-52, especially 48-50. Horrocks was admired by many major figures in mathematics and astronomy apart from Wallis in the late seventeenth century, including Newton and James Gregory. In part this resulted from the publication of his *Opera posthuma*, but in part it also inspired its publication over three decades after his death. The early Royal Society treated Horrocks as a precedent to its new experimental philosophy and the first notable English astronomer on the European stage. Allan Chapman has identified the recurrence of "[h]eroic references" to Horrocks throughout the eighteenth and nineteenth centuries. Horrocks received attention from "Victorian scientific hagiographers" who viewed him as "a true Smilesean hero; a poor boy made good and a clergyman to boot . . . whose genius, Godliness and hard work had brought him to an early grave." (Although by the nineteenth century Horrocks was referred to as "Reverend", Chapman also doubts that he was a cleric.) Subsequently twentieth-century historians remembered Horrocks as an important figure in the development of astronomy – Plummer calls him and Hooke "links between Kepler and Newton" – and he has retained a sort of mythological status around Much Hoole, Lancashire, where he made many of his famous observations. Chapman concludes that, while Horrocks' place in history has been mythologized and his observation of the transit of Venus was based on a "lucky prediction", Horrocks was still quite significant in the history of astronomy. He helped to bring the work of continental figures like Kepler and Galileo to the attention of English astronomers, and subsequently improved on that work. According to Chapman he understood the burgeoning astronomical revolution like no Englishman before him, and his observations of the lunar orbit yielded a greatly improved understanding of the solar system. For Chapman, Horrocks' "documented contributions to astronomy were formidable by any standard" (Allan Chapman, "Jeremiah Horrocks, the Transit of Venus, and the 'New Astronomy' in Early Seventeenth-Century England," *Quarterly Journal of the Royal Astronomical Society* 31 [1990]: 333-334, 348-350 [the quotations are from pp. 333-334]; Michael Gainsford, "Jeremiah Horrocks," *Journal of the British Astronomical Association* 115 [2005]: 193; Plummer, "Jeremiah Horrocks," 43-52).

himself described his lunar theory with a corrected and updated secondhand description of the theory, which included additions made by Flamsteed himself.[105] Wallis argued that Horrocks' own letter met a criterion which he always considered crucial: it expressed the theory clearly. The mathematical mistakes in the letter were "not so bad that we need be ashamed of it" and, in any case, Horrocks had corrected them later. Above all, it was more appropriate and respectful to include what Horrocks himself wrote: "it is his own, and it is the text of what all this is but a comment."[106] Respecting Horrocks' writing was not merely important for its own sake: Wallis noted that both he and Flamsteed would be criticized if they failed adequately to honour Horrocks.[107] To Wallis's chagrin, he was overruled by Collins and Flamsteed in this matter.[108] When the book was published, Wallis maintained his position that the omission of Horrocks' letter, which was referred to throughout the book, "makes the thing lame."[109]

What Collins asked Wallis to write on behalf of the Society was not always limited to descriptions of the work of English mathematicians. In a letter of 27 March/April 6 1673 Collins relayed Brouncker's request that the work of a number of foreign mathematicians on equations "be digested and illustrated with schemes and numerical examples". These mathematicians were mainly Dutch, but Brouncker's list included François Dulaurens; evidently Brouncker did not share Wallis's animosity toward the Frenchman.[110] Wallis's reply of 29 March/8 April showed that he had not changed his belief that everything that Dulaurens produced was either mediocre or plagiarized: "I do not look upon him to have any great matter, but what he hath from others, and what notions he hath were but crude and undigested, and of which he was not at all master."[111]

As for the Dutch mathematicians, Wallis was willing to discuss their work, but he

105 Plummer, "Jeremiah Horrocks," 50.
106 CSM *Wallis to Collins 26 October 1672* II 550-551. The quotations are from p. 551. Evidently Wallis felt that Flamsteed's own contributions to the *Opera posthuma* were just as problematic as those of Horrocks anyway, and he recommended in a subsequent letter that Flamsteed "look over them and note the errata" (CSM *Wallis to Collins 14 November 1672* II 553). See also CSM *Collins to Wallis 12 August 1672* II 547-549, *Wallis to Collins 15 August 1672* II 549.
107 CSM *Wallis to Collins 26 October 1672* II 551-552.
108 Plummer, "Jeremiah Horrocks," 50.
109 CSM *Wallis to Collins 14 November 1672* II 553.
110 CSM *Collins to Wallis 27 March 1673* II 556.
111 CSM *Wallis to Collins 29 March 1673* II 559-560.

was much more enthusiastic about explaining that they had reproduced the work of Descartes who, in turn, had stolen ideas from Englishmen like Thomas Harriot (c. 1560-1621).[112] This is an example of the antipathy that Wallis would go on to express for Descartes with less and less restraint. Wallis's felt that nearly everything Descartes had done was redundant – being either unoriginal or subsequently done better by others – and as such he did not deserve the reputation he had. Wallis, presumably attempting to enhance his own reputation at Descartes' expense, repeatedly accused the Frenchman of plagiarism. As Bjørnstad notes, identifying plagiarism in the work of a dead writer, if others agreed that it was in fact plagiarism, showed one's own cleverness and perceptiveness in noticing what others had missed.[113] Wallis explained in the letter of 29 March/8 April that the Dutch mathematicians Brasser and Ferguson used the same rule as Descartes for finding cube roots of binomials, a rule which Wallis happened to develop independently in 1648 and reported in a letter to Thomas Smith. Wallis acknowledged that Descartes seemed to be the first to find the rule and had included it in his *Géometrie* (1637), but he chose a method of determining priority in this case that would suit his own discovery of the rule: as far as Wallis was concerned, Descartes had passed up his chance for priority for the rule by failing to provide a proof for it, which Wallis had done at Smith's request.[114]

Wallis continued to describe his experience with Descartes' *Géometrie* in several subsequent letters to Collins throughout 1673.[115] His opinion of Descartes had been quite high when he first read the text in 1648. He found that he and Descartes had happened to find similar rules and make similar observations, and he admired how Descartes would solve equations by bringing all of the elements to one side, making them equal to zero.

112 Harriot was known among his contemporaries chiefly as a mathematician, but he was recognized in the late 18th and 19th centuries for his astronomical observations. Harriot, it seems, was the first person to use the telescope for an astronomical observation; in 1609 he preceded Galileo in observing the moon with a telescope and noticing differences on its surface compared to naked-eye observations. The Count de Brühl, a German astronomer, examined Harriot's astronomical writings in 1785 and recommended that they be published, as they eventually were in the 1830s by Stephen Peter Rigaud (Allan Chapman, "Thomas Harriot: The First Telescopic Astronomer," *Journal of the British Astronomical Association* 118 [2008]: 315).

113 Bjørnstad, *Borrowed Feathers*, 7.

114 CSM *Wallis to Collins 29 March 1673* II 558.

115 CSM *Wallis to Collins 8 April 1673* II 562-564, *Wallis to Collins 12 April 1673* II 564-576, *Wallis to Collins 6 May 1673* II 578-580, *Wallis to Collins 27 September 1673* II 581-586. See also CSM *Wallis to Collins 11 September 1676* II 591.

Yet he eventually determined that this fundamental strategy in Descartes' work was not original: it was used in Harriot's posthumous work, *Artis analyticae praxis*, which had been published in 1631, six years before the publication of the *Géometrie*. Descartes had also used much of the same notation as Harriot. However, Wallis found to his dismay that Descartes never mentioned Harriot in the *Géometrie*. Not allowing for the possibility of multiple invention, the young Wallis assumed that Descartes was a plagiarist and wrote to Smith with the same account that he was giving to Collins here.[116]

The clearest expression of Wallis's opinion of Descartes came in his *Treatise of Algebra* of 1685. This work was Wallis's testament to every English accomplishment in the field of algebra of which he knew. The *Treatise* fits into a genre of large collections of inventions, for which there were many precedents by 1685. Allan Chapman points out examples of works by Bacon, Hooke and others that claimed one hundred inventions,[117] and the *Treatise* had one hundred chapters apart from the appendices. There were also precedents at least from the beginning of the early modern period of reference books cataloguing inventions and attributing them to inventors, like Polydore Virgil's popular work, *De inventoribus rerum* (1499), a text which synthesized lists of inventions compiled by ancient, patristic and medieval writers.[118] Although it contains some of Wallis's own work, the *Treatise* is primarily intended to commemorate the algebraic inventions of his notable countrymen. For example, as Hall explains, the *Treatise*

116 CSM *Wallis to Collins 12 April 1673* II 573-576. Around 1655, Wallis alluded to the similarity he had detected in the notation used by Descartes and Harriot in a letter to Seth Ward and Lawrence Rooke. He referred to the use of the notation of "Dr. Descartes (unless you would rather that I name our Dr. Harriot, who preceded Dr. Descartes in entirely the same way)" (the Latin reads, "D. Cartesium [nisi velitis potius ut D. Harriotum nostratem nominem, qui in eadem fere semita D. Cartesio praeivit]"). Yet in December 1656 he placed Descartes alongside Viète, Oughtred and Harriot and "other men of great names" (that is, "aliorumque magni nominis"). Apparently, either Wallis's retrospective account of 1673 exaggerated the suspicions he had had of Descartes' duplicity, or he did not consider his reputation to be secure enough in 1656 to denounce Descartes openly. In a letter to Collins of 1666, Wallis mentioned that Thomas Bartholin had studied under Frans van Schooten, who published a Latin translation of Descartes' *Géomtrie*, and added that Bartholin had learned "his way of (as he calls it, Cartesian,) Geometry." Apparently this required no elaboration; Wallis must have made Collins aware of his position that Descartes had plagiarized Harriot by then (WC *Wallis to Seth Ward and Lawrence Rooke 1655?* I 170, *Wallis to Gerard Langbain, Henry Wilkinson, John Wilkins, and Jonathan Goddard 20/[30] December 1656* I 264, *Wallis to Collins 7/[17] August 1666* II 279. See also WC *Wallis to Pierre Gassendi 31 August/[10 September] 1655* I 162, *Wallis to Brouncker 5/[15] December 1656* I 233).

117 Chapman, *England's Leonardo*, 170.

118 Brian P. Copenhaver, "The Historiography of Discovery in the Renaissance: The Sources and Composition of Polydore Vergil's *De inventoribus rerum*, I-III," *Journal of the Warburg and Courtauld Institutes* 41 (1978): 192-214, especially 202, 211.

"wave[d] the Newtonian flag" when it summarized Newton's two famous letters to Leibniz about calculus of 1676.[119] One of Wallis's main goals in writing the *Treatise* was to celebrate William Oughtred and his *Clavis mathematicae* (1631). As Stedall explains, Wallis had known the late Oughtred personally, he acknowledged the *Clavis* as a major influence on his own work, and he used the *Clavis* extensively while teaching at Oxford. Wallis was primarily responsible for keeping the *Clavis* relevant when others dismissed it as outdated. He remained the most stubborn supporter of the Oughtred's book in the 1660s and resented efforts to supplant it with Descartes' *Geométrie*, among other texts.[120] Wallis's *Arithmetica infinitorum* of 1655 began with a dedicatory letter to a grateful, elderly Oughtred in 1655, referring to him as an "honoured old gentleman."[121] Oughtred's reply, which was also included in the front matter of the *Arithmetica*, contained high praise for Wallis:

> I do first with thankfulness acknowledge to God, the Father of lights, the great light he hath given you; and next I gratulate you, even with admiration, the clearness and perspicacity of your understanding and genius, who have not only gone, but also opened a way into these profoundest mysteries of art, unknown and not thought of by the ancients.[122]

Later, in 1685, Wallis in a sense dedicated the *Treatise of Algebra* to Oughtred's memory; fifteen of Wallis's one hundred chapters which were supposed to contain a thorough history of algebra discussed the *Clavis* alone.[123]

119 Hall, *Philosophers at War*, 38, 94-95. The quotation is from p. 38.

120 Jacqueline Anne Stedall, "Ariadne's Thread: The Life and Times of Oughtred's *Clavis*," *Annals of Science* 57 (2000): 41-43, 46, 50, 52. Stedall explains that the *Clavis* was never a particularly impressive or groundbreaking text, but was still the leading elementary mathematical textbook in England until the 1650s because of a lack of competition. In the early 1650s Wallis worked with Oughtred to correct and revise the *Clavis* for the third edition, and in the 1660s it was Wallis who ensured that the *Clavis* survived the criticism of people like John Collins (whose complaints included that Oughtred had failed to cite all of his sources). With Oughtred having died in 1660, Wallis was responsible for the printing of a posthumous fourth edition of the *Clavis* in the late 1660s (Stedall, "Oughtred's *Clavis*," 41-52).

121 John Wallis, *The Arithmetic of Infinitesimals* trans. Jacqueline A. Stedall (New York: Springer, 2004), 8. Stedall explains that Wallis asked for Oughtred's input in 1655 while working on the *Arithmetica*, but Oughtred proved to be too out of touch with recent mathematics to be of much help. In the introduction to her translation of the *Arithmetica*, Stedall adds that Descartes also influenced the *Arithmetica*, albeit in a limited sense. Wallis, it seems, neglected to mention that influence in the *Treatise of Algebra* (Stedall, "Oughtred's *Clavis*," 53; Stedall, *The Arithmetic of Infinitesimals*," xiii-xiv).

122 William Oughtred, *The Arithmetic of Infinitesimals*, 11.

123 Stedall, "Oughtred's *Clavis*," 53. Wallis wrote that the *Treatise* "contains an Account of the Original, Progress, and Advancement of (what we now call) *Algebra*, from time to time; shewing its true Antiquity (as far as I have been able to trace it;) and by what Steps it hath attained to the Height at which it now is." Wallis traced what he considered ancient Greek, Indian and Arabic roots of algebra (Wallis, *Treatise of Algebra*, sig. a2v).

Apart from ensuring that Oughtred remained relevant, Wallis's agenda when writing the *Treatise* included vindicating Harriot from Descartes' plagiarism. In the preface Wallis suggested that Harriot's work was in even more need of commemoration than that of Oughtred, as Harriot's only mathematical publication was his posthumous work of 1631, the *Artis analyticae praxis*, and even that was obscure. Of course, Wallis contended, Harriot's work had been preserved, but not in a manner of which Wallis approved: "He hath taught (in a manner) all that which hath since passed for the *Cartesian* method of *Algebra*; there being scarce any thing of (pure) *Algebra* in *Des Cartes*, which was not before in *Harriot*; from whom *Des Cartes* seems to have taken what he hath (that is purely *Algebra*) but without naming him." Thus, Wallis gave a thorough account of Harriot's *Artis* to balance out the credit that Descartes had stolen from him.[124] Accordingly, most of the content from Chapter XXX to Chapter LIV of the *Treatise* was taken from Harriot, after which a single chapter discussed a contribution from Descartes.[125] Wallis allowed that Descartes' works, like those of Oughtred, Harriot, and others, were still worth consulting if one wanted more information,[126] but he wanted to make sure that readers would understand who had made the algebraic innovations that Descartes expressed and who therefore deserved credit for them. Contrasting himself with Descartes, Wallis portrayed himself as utterly respectful of the priority of mathematicians: "I have endeavoured, all along, to be just to every one: Ascribing, as near as I could, every Step of advance to its own Author; or at least to the most ancient of those in whom I found it." Furthermore, he encouraged anyone who had proof that he

124 John Wallis, *A Treatise of Algebra Both Historical and Practical* (London: John Playford, 1685), sig. a3v-a4r. The quotation is from sig. a4r. It is not my purpose here to assess the accuracy of Wallis's charge of plagiarism, but I. Robert Cohen considers it (and Wallis's efforts throughout the *Treatise* to appropriate discoveries for the English) to be among "the greatest distortions in the history of science" (I. Robert Cohen, Review of *The Mathematical Works of John Wallis, D. D., F. R. S. [1616-1703]* by J. F. Scott, *Isis* 30 [1939]: 530-531).

125 Wallis, *Treatise of Algebra*, sig. b3r. In the first chapter about Harriot, Wallis claimed that without Oughtred's enormous but unacknowledged influence, the "whole Superstructure of *Des Cartes* (I doubt [i.e., suspect]) had never been." (Wallis, *Treatise of Algebra*, 126). In the chapter on Descartes, Wallis conceded a single algebraic rule (for reducing a biquadratic equation to two quadratic equations) to Descartes, but reiterated his belief that Descartes was so impressed by Harriot that his *Géometrie* "doth perfectly follow *Harriot*, almost in every thing. And adds very little of his own, if anything (as to *Pure Algebra*,) to what we have been shewed out of *Harriot*." Even this rule, Wallis added, "follows naturally" from Harriot's work; Wallis implied that it was not important enough for Harriot to "descend" to it (Wallis, *Treatise of Algebra*, 208-209).

126 Wallis, *Treatise of Algebra*, sig. b1r.

was mistaken in assigning priority anywhere in the *Treatise* to inform him so that he could make corrections accordingly.[127]

Wallis consistently rejected or at least downplayed the mathematical contributions of Descartes and Dulaurens, but this cannot quite be generalized as a dismissal of the contributions of all French mathematicians. Wallis was always respectful of François Viète, the sixteenth-century French pioneer in algebra.[128] He gushed about Viète's contributions to algebra in a letter written in December 1656, describing Viète as a "man of most sharp and very quick intelligence".[129] It seems likely that Wallis inherited much of his respect for Viète from Oughtred and Harriot. Stedall has explained that Harriot read the works of Viète carefully and enthusiastically, making extensive notes which suggest that Harriot's reading of Viète was quite formative.[130] Elsewhere, Stedall traces similarities between Oughtred and Viète in both methodology and notation.[131]

127 Wallis, *Treatise of Algebra*, sig. b1r. As in the Horrocks episode, Wallis also showed respect for the way that the writers whose work he included in the *Treatise* chose to express the innovations – this was a right that came with priority – but he was willing occasionally to "help an incommodious expression" in a writer's original work by replacing it with "one (as at least appeared to me) more intelligible and better agreeing (or more fully) to their own meaning; (without reproaching them for want of such:)" (Wallis, *Treatise of Algebra*, sig. b1r). Wallis showed as always that he cared about clear expression, but he did not make these changes in the interest of "seeking opportunities of Cavilling, or greedily catching at" the work of the original writers. Yet "cavilling" was precisely what Wallis did when he became frustrated with the abstruse wording of the problem proposed by Dulaurens and when Flamsteed wanted to replace Horrocks' own account of his lunar theory with one that Wallis thought was poorly expressed. In a publication such as the *Treatise* that he hoped would be widely read, Wallis needed prudently to deemphasize his frustration with unclear expression so that he would seem reluctant to cause a dispute by "cavilling". Showing this frustration was evidently a more acceptable risk both in private correspondence with Collins about plans to publish Horrocks' *Opera posthuma*, and in writing that sought to diminish the reputation of a headstrong young French mathematician like Dulaurens. As much as he downplayed his clarification efforts in the *Treatise of Algebra*, Wallis showed that this need for clarity could supersede the need to honour English innovators in mathematics by reproducing their work as faithfully as possible. By portraying his adjustments as minor, Wallis was trying to show as much respect for priority as possible while he took the necessary steps to make the work of these English mathematicians more digestible and, as such, potentially better known and more influential in the mathematical community.

128 In his (slightly Whiggish) account of Viète's career, Carl B. Boyer describes Viète as the key figure in the transition from the mathematics of the Renaissance to that of modernity, especially in algebra where he introduced a robust new notation with a wide range of applications. Similarly, Jacqueline A. Stedall explains that Viète synthesized medieval Arabic techniques and early modern Italian ideas, and applied them to ancient Greek texts, a move that proved to be a major influence on Fermat, Descartes, Harriot and Oughtred (Carl B. Boyer, *A History of Mathematics*, 2nd ed., rev. Uta C. Merzbach [New York: John Wiley & Sons, 1991; orig. pub. 1968], 302-306; Jacqueline A. Stedall, *A Discourse Concerning Algebra: English Algebra to 1685* [Oxford: Oxford University Press, 2003], 6-7, 50-54).

129 That is, "acutissimi & sagacis admodum ingenii Vir" (WC *Wallis to Gerard Langbain, Henry Wilkinson, John Wilkins, and Jonathan Goddard 20/[30] December 1656* I 266).

130 Jacqueline Stedall, "Notes Made by Thomas Harriot on the Treatises of François Viète," *Archive for the History of Exact Sciences* 62 (2008): 179-200.

131 Stedall, "Oughtred's *Clavis*," 29, 33-34.

Additionally, Wallis would have noticed that more than one of his fields of study overlapped with those of Viète. While it is unlikely that Wallis ever saw Viète's manuscript about his cryptanalytic methods, Pesic notes that Wallis, who studied Viète's algebra thoroughly, followed Viète in approaching codebreaking in the same way that he approached algebra.[132] Wallis also shared Viète's interest in the *mathesis universalis*, a concept that will be discussed below.[133] Indeed, Wallis apparently paid more attention to Viète than he did to many English mathematicians. In a letter of April 1677 he asked Collins to remind him "of the names of ancient algebraists of our own before Vieta. Such I have seen, but have forgot their names."[134] Wallis was aware of the connection between Oughtred, Harriot and Viète and acknowledged in the *Treatise of Algebra* that much of what these English mathematicians had accomplished was making improvements in areas where Viète had preceded them; it was Harriot's improvements of Viète that formed the backbone of Descartes' mostly plagiarized algebraic work.[135] Wallis could ill afford to distance himself from Viète while identifying the Frenchman's undeniable importance to the English mathematicians whom Wallis wanted to canonize.

At times Wallis found that it was useful to him personally to acknowledge Viète as a mathematical authority. Wallis was subjected to some criticism for the use of induction, the assumption that a pattern continues indefinitely when it has been shown to apply to a limited number of cases, in his *Arithmetica infinitorum*, a work first published in 1655 when Wallis had not yet established much of a reputation for mathematics. Wallis responded to this criticism by identifying precedents for the use of induction in Viète's *Ad angularium sectionum analyticen theoremata*.[136] Although exceptions like Viète were rare, they show that Wallis's contemporaries exaggerated slightly when they complained

132 Pesic, "Secrets, Symbols, and Systems," 689-691. See also Pesic, "The Clue to the Labyrinth," 193-195. Both Viète and Wallis were, of course, much more secretive about their cryptanalytic methods than their algebraic methods (Pesic, "Secrets, Symbols, and Systems," 681-682, 689-690).

133 See Paul Bockstaele, "Between Viète and Descartes: Adriaan van Roomen and the *Mathesis Universalis*," *Archive for the History of Exact Sciences* 63 (2009): 434; Giovanna Cifoletti, "From Valla to Viète: The Rhetorical Reform of Logic and Its Use in Early Modern Algebra," *Early Science and Medicine* 11 (2006): 390-423, especially 423.

134 CSM *Wallis to Collins April 1677*.

135 Wallis, *Treatise of Algebra*, 125-126. In the chapter on Descartes, Wallis added that both Oughtred and Viète were among those who had preceded Descartes in applying algebra to geometry. Wallis continued to use examples from Viète to reduce Descartes' originality (Wallis, *Treatise of Algebra*, 208).

136 Wallis also found a precedent for the use of induction in mathematics in the work of the English mathematician Henry Briggs (Stedall, *The Arithmetic of Infinitesimals*, xxiv-xxv).

of Wallis appropriating every discovery and invention for England. Wallis found it worthwhile to be respectful to foreign figures like Viète who were useful to him and who were canonical in the tradition in which he hoped to place himself. Wallis was a dedicated defender of English priority and promoter of English discoveries and inventions, but he approached this role pragmatically; this work could be put on hold while Wallis tended to his own reputation and ambitions.

4.4: Priority in Wallis's *Grammar* and *De loquela*

4.4.1: Mathematical Language and Its Real-World Applications

As John Aubrey's comments suggest, there were many occasions when Wallis made priority claims for himself alone. These claims were frequently against other Englishmen. In such cases we see Wallis applying the same skills that he used to secure inventions and discoveries for the English from foreign rivals, in an effort to secure them for himself from his countrymen. In addition to his protracted conflict with Hobbes, Wallis's notable disputes included one with the cleric, mathematician, musical theorist and original Royal Society member William Holder over priority for teaching a mute deaf person to speak. Wallis's experiments in this field were not as disparate from his mathematics as they may seem. In a letter to Boyle of 14/24 March 1661/2, which was printed in the *Philosophical Transactions* of July 1670 and became a key text in the dispute, Wallis hinted at the mathematical approach he took with the first deaf patient whom he taught to speak, Daniel Whaley. Wallis agreed to take Whaley on because he was impressed by what he considered Whaley's mathematical skills: he was "Ingenious and Apprehensive . . . and so far at least a Mathematician as to Draw Pictures". For Wallis this suggested that Whaley might be able to master the subtleties of speech without the aid of the ability to hear.[137] Wallis wrote that one must break language down to letters and characters, the indivisible units on which all language is based, to teach a deaf person to talk. This way, the teacher and the student could arrive at the same meanings of the symbols they are using; otherwise, the undertaking would be like performing algebra when the symbols

137 *Phil. Trans.* 61 (18 July 1670): 1093.

have different meanings to different mathematicians.[138] As Wallis explained, he approached the task as a mathematical problem in which one works toward a solution from a few known quantities:

> As to that of Teaching him the Language: I must (as Mathematicians do from a Few Principles first granted,) from that little stock (that we have to Begin upon) of such Actions and Gestures as have a kind of Natural significancy, or some Few Signs, which himself had before taken up to express his Thoughts as well as he could, Proceed to Teach him, what I mean by somewhat else; and so, by steps, to more and more: And this, so far as well I can, in such Method, as that what he Knows already, may be a step to what he is next to Learn; as, in Mathematicks, we make use, not of Principles only, but Propositions already demonstrated, in the Demonstration of that which follows.[139]

Wallis discussed how this work could be facilitated by the introduction of "an [*sic*] *Universal Character*, in which all Nations though of different speech, shall express their common Conceptions". Such a system would use a common set of "Real Character[s]" that would correspond directly to what a person wanted to communicate. These characters would correspond to sounds for those who could hear, but they would not need to correspond to sound for those who could not hear because they would convey the exact meaning of the word.[140] In the words of Jonathan Rée, Wallis (as well as William Holder) became aware of "the inadequacies and irrationalities" of the received alphabet, so in teaching the deaf to speak he sought "an absolute notion of speech, tied down to invariant standards of sound".[141]

The concept of a universal language was for from original when Wallis employed it, and it would have been familiar to readers of the *Philosophical Transactions* in 1670. For example, Wallis's fellow Royal Society founder John Wilkins examined the issue in his *Essay Towards a Real Character and Philosophical Language* of 1668, a text which would figure prominently in the priority dispute between Wallis and Holder. Wallis's interest in the idea of a universal language may seem to imply a desire to aid international communication, and in part it probably was, but as Lois Potter notes even a universal language could be exclusive. Seventeenth-century thinkers would have understood the

138 *Phil. Trans.* 61 (18 July 1670): 1091.
139 *Phil. Trans.* 61 (18 July 1670): 1096.
140 *Phil. Trans.* 61 (18 July 1670): 1091-1092.
141 Jonathan Rée, *I See a Voice: Deafness, Language and the Senses – A Philosophical History* (New York: Metropolitan Books, 1999), 249.

link between universal language projects and another of Wallis's interests, cryptology; as Potter notes, "every language looks like a secret code to the person who does not know its rules."[142] Indeed, early seventeenth-century universal language projects were essentially extensions of cryptology. Scholars tried to develop mathematics-based codes that could be deciphered by speakers of any language. Among these scholars were members of the Hartlib circle who, like Wallis, gained cryptological experience during the Civil War.[143] Perhaps a more appropriate label for the sort of language project in which Wallis was interested is "transnational", although this word did not exist in the seventeenth century; a so-called "universal" language could be developed as a sort of code for the benefit of a transnational elite, and a cryptanalyst like Wallis would have appreciated this possibility.

Wallis was also quite interested in a related concept, the *mathesis universalis*, which was intended as an approach to mathematics that could be used in geometry, arithmetic, and anything else involving measurement or calculation.[144] The *mathesis universalis* was a particularly important concept to Gottfried Leibniz, for whom its potential was by no means limited to mathematics. As Jan C. Westerhoff explains, for Leibniz the *mathesis universalis* was "a method of deciding all questions, whether they belonged to physics, to metaphysics, or to any other science, with mathematical certainty. This should be brought about by inventing a universal system of notation, which would allow the detection of errors of thinking as purely grammatical or syntactical errors." For Leibniz especially, then, the *mathesis universalis* was the ultimate application of mathematics to other fields of study, and it depended on a sort of universal character.[145] Similarly, when Wallis published a book of over two hundred pages called *Mathesis universalis* in 1657, the long alternate title began, "a complete mathematical work, first philologically then mathematically taught."[146] Wallis also wrote in 1678 that "*Specious*

142 Lois Potter, *Secret Rites and Secret Writing: Royalist Literature, 1641-1660* (Cambridge: Cambridge University Press, 1989), 42-43. The quotation is from p. 43.

143 Gerhard F. Straser, "Closed and Open Languages: Samuel Hartlib's Involvement with Cryptology and Universal Languages" in *Samuel Hartlib and Universal Reformation: Studies in Intellectual Communication*, eds. Mark Greengrass, Michael Leslie and Timothy Raylor (Cambridge: Cambridge University Press, 1994), 151-161.

144 Bockstaele, "Between Viète and Descartes," 433; Cifoletti, "From Valla to Viète," 395, 402.

145 Jan C. Westerhoff, "Poeta Calculans: Harsdörffer, Leibniz and the 'Mathesis Universalis'," 450. Wallis, Viète and others also attempted to apply logic to mathematics and vice-versa. See Cifoletti, "From Valla to Viète," 423; Massimo Mugnai, "Logic and Mathematics in the Seventeenth Century." *History and Philosophy of Logic* 31 (2010): 297-314.

146 That is, "arithmeticum opus integrum, Tum Philologice, tum Mathematice traditum" (Wallis, *Opera*

Arithmetick" was "a kind of Real Character".[147] This suggests that Wallis believed that to achieve a real character in language, one should strive to make language mathematical. In a letter written in December 1656, Wallis showed his esteem for the concept of *mathesis universalis* by linking it to "Viète, Oughtred, Harriot, Descartes and other men of great names".[148] Thus, by pursuing the *mathesis universalis* and related forms of universal language, Wallis sought to place himself among the mathematical figures whom he respected the most; he would not take lightly a threat to his priority for any discoveries related to universal, mathematical language.

His experiments with teaching the deaf to speak were not the only times when Wallis viewed human language as a problem to be solved with his mathematical skills: after all, he had been a parliamentary codebreaker. Peter Pesic explains that Wallis approached cryptanalysis in much the same way that he approached algebra, as Viète had done before him. Pesic suggests that Wallis would indeed have published on the connection between algebra and codebreaking if he had not been concerned to keep his cryptanalytical skills secret, especially from interested foreigners like Leibniz.[149] In fact, Wallis made an explicit connection between these two applications of mathematics to language. In the letter to Boyle he wrote that, although there were some aspects of speech that could not be detected visually, it was feasible for Whaley to learn to read lips. This would be like solving a cipher: he would pick up whatever elements of an utterance that he could, and deduce the meaning of the whole "by a probable conjecture."[150] It seems that Wallis's conception of human language as mathematical gave him opportunities for

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mathematica Vol. I [Oxford: Sheldon, 1695], 11. My translation).

147 Wallis, *A Defence of the Royal Society and the Philosophical Transactions, Particularly those of July, 1670. In Answer to the Cavils of Dr. William Holder* (London: Thomas Snowden, 1678), 16-17.

148 That is, "Vietae, Oughtredi, Harrioti, Cartesii, aliorumque magni nominis virorum" (WC *Wallis to Gerard Langbain, Henry Wilkinson, John Wilkins, and Jonathan Goddard 20/[30] December 1656* I 264). As discussed in n. 116 above, Wallis evidently had not developed his antipathy toward Descartes yet in 1656, or at least had not yet begun to express it.

149 Wallis refused to share his codebreaking methods with Leibniz, who asked Wallis about them on behalf of the Elector of Hanover in 1697. Wallis claimed that they were largely intuitive and could not be taught. This did not prevent him from teaching his grandson, William Benclowe, how two break codes two years later so that someone could carry on his codebreaking work when he died. Wallis managed to teach Benclowe enough cryptanalysis that he became the first Englishmen to be appointed to the office of Decypherer. Pesic explains that Leibniz valued cryptanalysis as a valuable tool in constructing a *mathesis universalis*; if he could learn cryptanalysis, he could use it as a model for breaking the code of "the cryptic text of nature" (Pesic, "Secrets, Symbols, and Systems," 678, 689-692).

150 *Phil. Trans.* 61 (18 July 1670): 1095.

real-world applications of his mathematical skills, some of them lofty like developing a universal character or a *mathesis universalis*, and others more realistic like breaking codes and teaching a deaf person to speak. It is easy to imagine why Wallis would have valued the latter so highly after the Restoration when perhaps his utility as a codebreaker became more intermittent. Furthermore, Jonathan Rée explains that, as the seventeenth century advanced, teaching various forms of language to the deaf became "something of a fad"; it was a field about which Virtuosi were curious and which they valued highly.[151] In terms of honour (and perhaps money, if he wanted more patients), the stakes for Wallis were high in the priority dispute with Holder. Having priority in teaching the deaf to speak would signal one's dominance in removing abstraction from mathematics, a skill with unlimited potential in a time when creating a universal character and a *mathesis universalis* seemed feasible to many intellectuals.

Wallis himself constructed a sort of universal language in his *Tractatus prooemialis de loquela*, a text appended to the beginning of his *Grammar of the English Language* of 1653. Wallis used the preface to the *Grammar* in an effort to enhance England's reputation for intellectual achievement. It was a chance for Wallis to mythologize the English language and people, to emphasize the importance of English works on the European stage in every field of study, and to inspire the English to be proud of their language, history, and intellectual abilities. Wallis wrote that his motivation behind writing the *Grammar* was to satisfy "the great demand for it from foreigners, who want to be able to understand the various important works which are written in our tongue."[152] Thus, to make it as accessible as possible, he wrote the *Grammar* in Latin. According to Wallis, an example of a field in which foreigners wanted to learn from English texts was "*Practical Theology*". He wrote, "It is well known that in this field our public teachers, with God's help, have had outstanding success." Wallis suggested two reasons for this. Firstly, English theologians did not have to waste their time arguing against Catholicism. Secondly, the English were a particularly pious people who paid attention to their religious leaders and read theological works with enthusiasm. Wallis claimed that the English were not only eager to learn theology: "all kinds of literature are

151 Rée, *I See a Voice*, 105, 109.
152 John Wallis, *John Wallis's Grammar of the English Language* trans. J. A. Kemp (London: Longman Group, 1972), 105.

widely available in English editions, and, without boasting, it can be said that there is scarcely any worthwhile body of knowledge which has not been recorded today, adequately at least, in the English language. Every day many highly sought after books appear."[153] According to Wallis, then, the English people's love for knowledge both inspired them to be good Christians and made England a hotbed of academic learning; in both cases, he suggested, foreigners were interested in following their example.

In the preface to the *Grammar*, Wallis argued vehemently against the notion that English was a difficult language to learn. He believed that this notion had scared foreigners away from learning the language. To his dismay, even some Englishmen held this opinion.[154] It is clear why Wallis, who aspired to a logically perfect mathematical language in the form of the *mathesis universalis*, would resent the notion that English was full of unnecessary and limiting complications. For Wallis, there was nothing inherent in the language that made it more difficult to learn than any other language. The problem was that the usual method used in teaching English, discussing it in terms of Latin grammar, was an outdated method that could only lead to "boredom and difficulty."[155] Later in the *Grammar* he referred to English as "the easiest language I know of",[156] but the implication of the preface was that it was only easy if taught in a logical and appropriate manner. Wallis stressed the novelty of his new method of teaching grammar that treated English on its own terms as a distinctive language, instead of trying to shoehorn it into a Latin framework. Indeed, with this approach, Wallis intended to show that English had a number of advantages, like its heavy use of prepositions and auxiliary verbs, that made it easier to use than other languages.[157] Wallis emphasized the logic and utility of English grammar not only to make it approachable to foreigners; he also wanted to rehabilitate the reputation of the English language among native speakers. He hoped to make the English proud of their language by giving them a "better insight into the true structure of their native tongue."[158] Indeed, Wallis admonished any Englishman who was not interested in what his *Grammar* had to offer:

153 Wallis, *Grammar*, 105-107.
154 Wallis, *Grammar*, 107.
155 Wallis, *Grammar*, 109.
156 Wallis, *Grammar*, 277.
157 Wallis, *Grammar*, 109-113.
158 Wallis, *Grammar*, 109.

> If anyone, particularly any of my own countrymen, believes that he can safely disregard all that is contained in this book, and thinks it a matter of little importance that he should get to know his native language and observe its subtleties, I will only say this to him; there are many things the knowledge of which gives us no special claim to praise, but ignorance of these very things may be the cause of great shame to us.[159]

What was truly innovative about the *Grammar* as Wallis saw it was the implementation of a system of describing how to pronounce the sounds used in English in terms of the movement of the organs used for speech. This articulatory system was "completely new, and has not, to my knowledge, been attempted by anyone else."[160] This is the system he would later use to teach his deaf patients to speak. Wallis explained that his system involved an entirely new scheme for classifying letters. He detailed this in *De loquela*, a separate treatise added to the beginning of the *Grammar*. It was separate, Wallis explained, not just because the articulatory system it described was a new invention, but also because it "has no greater relevance to English than to any other language"; in other words he envisioned it as having universal utility.[161] Wallis was evidently staking out his contribution to the potential development of a universal language.[162]

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159 Wallis, *Grammar*, 119. Wallis was something of a lingual purist, and wanted the English to take pride in the "pure and authentic pronunciation of the English language" that he claimed to describe in full in his *Grammar*. He wrote, "There would be no profit in describing the individual dialects of the various parts of the country, the absurdities affected by flighty women, or other 'barbarisms' of that kind. Nor is it necessary to mention the careless pronunciations of certain sounds that one may hear, whether those that are widespread among the mass of the people, or those current also in court circles or with people who affect to be fastidious. All these, in my opinion, should be unlearnt rather than imitated." Wallis added that he thought that the omission and mispronunciation of letters was often the fault of a French influence (Wallis, *Grammar*, 273-275).

160 Wallis, *Grammar*, 115.

161 Wallis, *Grammar*, 115-117. The quotation is from p. 115. It is also likely that treating *De loquela* as a separate work allowed the articulatory system to be more easily seen as a separate entity for which Wallis could claim priority. Wallis seems to have viewed this system as one of the "isolated items" that, as Jardine explains, could be viewed as an innovation on its own apart from the rest of the work in which it appeared, and could likewise be the subject of a priority dispute (Jardine, *Birth of History and Philosophy of Science*, 33-34).

162 In the fifth edition of the *Grammar* (1699) Wallis added that the French had since copied his idea: "Some Frenchmen seem to have imitated this method, since its first publication, in their *Grammaire Universelle*, which in a subsequent Latin edition is entitled *Grammatica Universalis*, and closely follows my own method." (The translator of the *Grammar*, J. A. Kemp, explains that this was a reference to the *Port-Royal Grammar* of 1660, written by Antoine Arnauld and Claude Lancelot.) Clearly, Wallis wanted to remind the reader that he held priority for what he identified as the same method in a rival text, presumably so that his priority would be recognized by posterity if a universal language were developed (Wallis, *Grammar*, 111). Kemp notes that Wallis supervised publication of every edition up to the fifth. A sixth edition was published in 1765, sixty-two years after Wallis's death (Kemp, *Grammar*, 73).

In the fourth edition of the *Grammar* of 1674, Wallis added a section near the end of the preface labelled "TEACHING THE DUMB TO SPEAK." Here Wallis described the ways in which he had applied the articulatory system of *De loquela*. He had taught Englishmen to improve their pronunciation and stop stuttering; he had taught foreigners to pronounce the most difficult English words; and he had taught two deaf men to pronounce any word, no matter how difficult, in a variety of languages. He assured the reader that he could do this "for anyone else, however deaf."[163] Wallis attempted to justify the honour he thought was due to him for inventing his articulatory system by showing what he, as the inventor, had been able to accomplish. Wallis decided to keep this section in the preface to the fifth edition of the *Grammar* in 1699 and did not mention that his priority in using an articulatory system to teach a deaf person to talk had been challenged in 1678 by William Holder. Evidently he considered himself the winner of the dispute.

4.4.2: The Priority Dispute with William Holder

The second deaf person whom Wallis claimed to have taught to speak in the 1674 edition was Alexander Popham, who had earlier received speech lessons form Holder. What seems to have particularly bothered Holder and provoked the priority dispute was the fact that the item in the *Philosophical Transactions* that contained Wallis's letter to Boyle mentioned Popham in passing (without naming him), but it did not acknowledge that Holder had taught him first. Indeed, the main point that Wallis and Holder disputed was who had really taught Popham to speak. The key criterion for awarding priority for teaching a deaf person to speak was not who had first developed a method for doing so, but rather who had first implemented it successfully; the two disputants at least agreed that priority in this case should be awarded based on a performance rather than an idea, although Wallis was sure to mention that he had published on the subject much earlier than Holder. Holder claimed to have been the first to teach a mute deaf person to speak when he taught Popham at Bletchingdon, a village near Oxford of which Holder was the rector, in 1659/60.[164] Wallis claimed that Holder had failed in his attempt to teach Popham

163 Wallis, *Grammar*, 117-119.

164 William Holder, *Supplement to the Philosophical Transactions of July, 1670. With Some Reflexions on Dr. John Wallis, His Letter There Inserted*, (London: Henry Brome, 1678), 2.

to speak, which is why in 1662 he was brought to Wallis, who had made his success with Daniel Whaley quite public.[165] If Wallis could prove that Holder had failed, then his success with Daniel Whaley in 1661 would be the first known successful case of teaching a deaf person to pronounce given words who had been taught by an articulatory system. Wallis's success with Whaley was not in dispute, having been recorded in the registry of the Royal Society;[166] this was not the case for Holder, and he had to struggle to prove that he had had any success at all in teaching Popham to speak.

Wallis's letter to Boyle did not actually mention Popham because Wallis wrote it before he had begun to treat Popham. Instead, the letter discussed Wallis's progress with Whaley, about whom Boyle had apparently asked in the letter to which Wallis was replying.[167] Instead of directly answering Boyle's question, Wallis first gave a full account of how Whaley had gone from being "*Mute*" to being "at least *Semivocalis*", largely in order to convey how difficult the process had been, and accordingly how great Wallis's accomplishment had been. After all, Wallis's goal was not to make Whaley "speak like a Parrot, or . . . write like a Scrivener, who understanding no Language but English, transcribes a piece of *Latin*, *Welsh*, or *Irish*"; Wallis wanted Whaley to understand what he was saying. It was like teaching a child to speak his first language without the aid of aural communication.[168] The method that Wallis used for this undertaking was the same one that he had described in *De loquela*: "there [is] no other way to direct his Speech, than by teaching him how the Tongue, the Lips, the Palate, and other Organs of speech, are to be applied and moved in the Forming of such sounds as are required".[169] Wallis explained that he did not shy away form this daunting task because he knew it was possible. Learning a language was like learning mathematics: one started with what little he knew and built upon it to learn increasingly complex things. Thus, considering "from how few and despicable Principles the whole Body of Geometry, by continual consequence, is inforced; if so fair a Pile, and curious Structure may be raised, and stand fast upon so small a bottom; I could not think it incredible, that we might attain

165 Wallis, *Defence of the Royal Society*, 3-4.
166 See *Phil. Trans.* 61 (18 July 1670): 1098; Wallis, *Defence of the Royal Society*, 13.
167 There are no known extant copies of the letter from Boyle. See WC *Boyle to Wallis 26 February/[8 March] 1661/[2]* II 51.
168 *Phil. Trans.* 61 (18 July 1670): 1088-1089.
169 *Phil. Trans.* 61 (18 July 1670): 1089.

some considerable success in this Design, how little soever we had at first to begin upon".[170] Just like he did when he solved a cipher from a few educated guesses or solved a geometrical problem from a few known quantities, Wallis would use his intimate knowledge of mathematics to solve the problem of teaching Whaley to speak.

Wallis admitted that Whaley had been able to hear and to speak as a young child, but he explained that Whaley had gone deaf when he was about five years old and gradually lost the ability to speak. Although this meant that Wallis was misleading Boyle and any other readers when he suggested that Whaley had no frame of reference in language, Wallis managed to turn this into a point in favour of how great his accomplishment was. He wrote that it proved that hearing is normally required for speech, and this "might therefore discourage the undertaking; yet I was thereby very much secured, that his want of Speech was but a consequent of his want of Hearing".[171] Wallis suggested that whereas others may have thought the task was hopeless, he took inspiration from the realization that Whaley was physically able to speak.

After so much emphasis on the difficulty of his task, Wallis knew that he could only earn credit from his readers if he showed that he had succeeded. However, he wanted his success to be judged on terms that he defined. He explained that what he had taught Whaley was more than others thought was "Fesible", but still not perfect; listening to Whaley talk was like listening to a foreigner or someone from a remote part of England. This comment suggests that Wallis wanted those who heard about his work with Whaley to think he had made a major achievement, but he had to protect himself against the possibility that they would be disappointed when they actually heard Whaley speak.[172] Nevertheless, Wallis maintained that Whaley's progress was promising. After about two months of instruction, Whaley had exceeded his expectations: "There is hardly any Word, which (with deliberation) he cannot Speak". Wallis was optimistic that Whaley could be taught to understand and even write a language just as well as a person who could hear. All he needed was more time, which he stated repeatedly, presumably in an effort once again to avoid disappointing anyone who actually heard Whaley speak.[173] To assure

170 *Phil. Trans.* 61 (18 July 1670): 1092.
171 *Phil. Trans.* 61 (18 July 1670): 1093.
172 *Phil. Trans.* 61 (18 July 1670): 1094-1095.
173 *Phil. Trans.* 61 (18 July 1670): 1095-1097

Boyle of the quality of his invention and the success of its implementation, Wallis insisted that his results with Whaley were not anomalous. He would be able to teach these language skills to "any other ingenious person in this Condition".[174] Wallis wanted to make sure that Boyle continued to pay attention to his impressive work; he concluded the letter by confidently inviting Boyle to ask about "our Future Success" if he was interested.[175]

In the *Philosophical Transactions*, Oldenburg added a postscript in order to put the letter in context. The postscript identified the patient, whom Wallis had not named in the letter, as Daniel Whaley, adding that Wallis had taken him to the Royal Society meeting of 21/31 May 1662.[176] Whaley spoke in front of the Fellows, not perfectly, but still "to their great satisfaction", so they encouraged Wallis "to pursue what he had so ingeniously and so successfully begun." Whaley also spoke in front of Charles II and his court, as well as curious foreigners whom he impressed by pronouncing "the most difficult words of their Languages (even *Polish* it self,) which they could propose to him."[177] The postscript also made a passing reference to Alexander Popham, identifying him only as "a young Gentleman of very good Family, and a fair Estate" who had been born deaf and whom Wallis had also taught since he wrote the letter to Boyle.[178] The postscript concluded with a description of Wallis's *De loquela* and *Grammar* that emphasized Wallis's priority for the system of classifying sounds based on articulation in *De loquela*, "which is, I think, the first Book ever Published in that kind."[179] The language used to describe the *Grammar* closely resembles that used in the preface to the *Grammar* itself: Wallis had "so briefly and clearly given an account of this Language, as may be very advantagious, not only to Strangers, for the easy attainment thereof, but even to the English themselves for the clear discovering (which few take notice of) the true *genius* of their own Language."[180] The postscript was such an effective promotion of Wallis's accomplishments, his priority and his causes that it is not surprising that Holder

174 *Phil. Trans.* 61 (18 July 1670): 1095.
175 *Phil. Trans.* 61 (18 July 1670): 1097.
176 See HRS I 84.
177 *Phil. Trans.* 61 (18 July 1670): 1098.
178 *Phil. Trans.* 61 (18 July 1670): 1098-1099.
179 *Phil. Trans.* 61 (18 July 1670): 1099.
180 *Phil. Trans.* 61 (18 July 1670): 1099.

accused Wallis of writing it himself.

It took Holder until 1678 to publish his own account of these events, which he called a *Supplement to the Philosophical Transactions of July, 1670. With Some Reflexions on Dr. John Wallis, His Letter There Inserted.* Holder began with a typical appeal to the impartial reader who he hoped would

> consider and scan well some matters of Fact, concerning which an Appeal is here made to him; wherein the Author is not so much concerned to be righted to his Title against an Invader, as to express his just Resentment to the subtle practices, which have been contrived and to abuse and mislead the Reader with false shews, somewhat resembling truth.[181]

Holder clearly understood his audience of natural philosophers well. He promised to reveal "matters of Fact", the term that Boyle and the Royal Society used to refer to established, agreed-upon facts in their experimental philosophical program.[182] Furthermore, Holder claimed to be disputing Wallis's priority not in his own interests, but rather in the interests of the truth; he felt obligated to serve the natural philosophical community by exposing Wallis's deceptions. In short, he was doing precisely what Wallis and others claimed to do in their priority disputes with foreign rivals.

Holder gave an account of how he had taught Alexander Popham to speak at Bletchingdon in 1659/60, having taken on the project "to serve the Ends, and contribute something to the delight" of the group that would grow into the Royal Society. He explained that Wallis, who at that time lived in Oxford and was a member of this group, was among the many future Society members who made the short trip to Bletchingdon to hear Popham speak. Furthermore, Holder wrote, he and Wallis had multiple conversations in Oxford about teaching the deaf to speak.[183] Holder resented that Wallis's letter to Boyle, written two years after these conversations, had speculated on "whether it may not be *possible in Nature, and Morally possible*, that is, *possible to succeed in practice*, to teach Mr. *Whaley* to speak and understand a language". Holder perceived this as a slight because he had proven that such a thing was possible by teaching Popham to speak, and Wallis had witnessed it. Furthermore, Wallis knowingly neglected Holder's achievement when he showed off Daniel Whaley at a Royal Society meeting and at the Royal Court.

181 Holder, *Supplement to the Philosophical Transactions*, 1.
182 See Shapin and Schaffer, *Leviathan and the Air-Pump*, 23-25.
183 Holder, *Supplement to the Philosophical Transactions*, 1-2, 4-6.

Wallis wanted "to joyn to his other Trophies, that which he saw performed by Dr. *Holder*, and silently passed over [in his letter to Boyle], *viz.* The finding a successful way of teaching Dumb and Deaf persons to speak", and Wallis was not above using "subtle contrivances" to do so.[184] Wallis had succeeded in deceiving scholars in Oxford and London by making them believe that he had been the first to teach a deaf person to speak, and with the fame he earned he attracted Alexander Popham, who had by then forgotten what Holder had taught him.[185]

Meanwhile, Holder had held his tongue because he had no desire to cause a scandal and because he was "unconcerned" with being recognized for his priority as long as the public could benefit from the invention.[186] Holder made this comment to contrast himself with Wallis; if Holder had been as "ambitious" as Wallis, he could have justly reminded everyone that he had preceded Wallis in teaching a deaf man to talk.[187] Wallis, conversely, was mainly concerned with "penning and spreading his own fame." That was why he developed a tendency to steal others' priority and to promote himself shamelessly: "The Reader may find and see in several Authors Works, who would suffer themselves to be so imposed upon, the Fame and Praise of Dr. *Wallis* in large Characters ingraven by himself."[188] Holder wrote that he almost pitied Wallis for his "ways of begging and borrowing Reputation."[189] This recalls the comments of John Aubrey; although neither Aubrey nor Holder was objective in expressing this, they both indicated that Wallis was developing a reputation for self-aggrandizement at the expense of his countrymen. The issue was not that Wallis did not deserve respect for his achievements, Holder explained, but rather that it was "in his nature, or else habitual to him" never to be satisfied with the reputation he had actually earned. Consequently Wallis was prone to "rifling his Neighbours, and adorning himself with their spoyls."[190] Holder became the victim of this when Wallis brought Whaley to the Royal Society meeting, "where he reaped great praise, and had it registred to perpetuate the memory of his Achievement."[191]

184 Holder, *Supplement to the Philosophical Transactions*, 1-2.
185 Holder, *Supplement to the Philosophical Transactions*, 2-3, 5-6.
186 Holder, *Supplement to the Philosophical Transactions*, 7.
187 Holder, *Supplement to the Philosophical Transactions*, 6.
188 Holder, *Supplement to the Philosophical Transactions*, 3.
189 Holder, *Supplement to the Philosophical Transactions*, 6.
190 Holder, *Supplement to the Philosophical Transactions*, 10.
191 Holder, *Supplement to the Philosophical Transactions*, 6. It seems likely that Holder exaggerated

Holder explained that Wallis typically used the *Philosophical Transactions* as his "common Market" in which he acted as a "Merchant of Glory." He would also persuade other authors to include the accomplishments he had appropriated in their books, "if he could find way and leave to croud himself in". This was precisely what Wallis had done when he wrote the postscript to the letter to Boyle in Oldenburg's name, which Oldenburg had confirmed for Holder, Brouncker, and other Fellows before he died in 1677.[192] Holder explained that his *Supplement* was also supposed to be included in the *Transactions* to counterbalance Wallis's account of the events after Holder had complained both to Oldenburg and Wallis about their failure to mention him. In fact, Oldenburg had written an introduction to Holder's account and was planning to publish it in the *Transcations* "to vindicate himself for seeming partial having been thought to be so". Oldenburg had trouble getting the Royal Society to licence the publication of the *Supplement* because of the harshness of Holder's tone, and died before he could manage to get it printed.[193] Holder suggested that since then he had been waiting patiently for "some ingenuous satisfaction from Dr. *Wallis* for that his want of Candour,"[194] but Wallis forced his hand by once again imposing his self-promotion on an unsuspecting writer.

Wallis's role in having the demonstration of Whaley's speech registered, as it was standard practice to record successful experiments, inventions and discoveries in the registry (See Boas Hall, "Diffusion of Information", 174; Iliffe, "In the Warehouse," 36-37).

192 Holder, *Supplement to the Philosophical Transactions*, 3.

193 Holder, *Supplement to the Philosophical Transactions*, 9. Oldenburg's introduction stated that there were "*some just exceptions being taken*" to the failure of the *Transactions* to mention Holder's role in teaching Popham to speak. Oldenburg continued, "*the publisher to avoid partiality on his part, (though but in appearance) hath thought fit to publish this ensuing Narrative, of what hath been done in this kind by Dr.* Holder*, as it is handed to him by the Author himself.*" Holder explained that Oldenburg had "excused himself" for participating in Wallis's plan by showing that he had been "so imposed upon by Dr. *Wallis*." Oldenburg had contributed to the slight against Holder, but only "unwittingly". In short, Holder placed all of the blame on Wallis and none on Oldenburg (Holder, *Supplement to the Philosophical Transactions*, 4, 9). It seems more likely that Oldenburg fully understood the implications of Wallis's account when he agreed to publish it. Oldenburg apparently succeeded in playing the role that, as Shapin explains, he mastered over the course of his career: he managed to favour the Fellows whom he considered important, in this case Wallis, while still appearing impartial. Evidently Oldenburg had convinced Holder that Wallis was responsible for Oldenburg's decision to publish the letter to Boyle in the *Transactions* (Shapin, "O Henry," 423-424).

194 Holder, *Supplement*, 8. The word "satisfaction" is a signal that this was a matter of honour for Holder. According to the Oxford English Dictionary, in addition to meanings of paying something back in general and atoning for sin (among other meanings), the word "satisfaction" has had specific meanings related to honour since at least the beginning of the seventeenth century. These definitions are: "The opportunity of satisfying one's honour by a duel; the acceptance of a challenge to a duel from the person who deems himself insulted or injured. Chiefly in phrases, *to give, demand satisfaction*" (OED, "Satisfaction," accessed 10 June 2011).

Wallis took advantage of the "worthy, honest, well-meaning" Robert Plot and convinced him to include in his *Natural History of Oxfordshire* (1677) passages that Wallis himself had written (or at least heavily influenced) about the novelty of *De loquela.*[195] Holder explained that he would not let Wallis succeed in exploiting another well-meaning natural philosopher to promote his deception.[196] Again, the blame fell squarely on Wallis. Perhaps Holder did not think it was prudent to antagonize more than one notable figure in the Royal Society at a time.

As usual, the subject of the dispute expanded to include questions of the quality of the participants' work. As Holder saw it, not only was his success in teaching a deaf person to speak earlier than Wallis's, it was also more impressive. Holder had given speech to a person who had been deaf for his whole life. The two people whom Wallis had taught had already learned how to speak in the past: Whaley when he was a child, and Popham when Holder had taught him. Even to someone "less knowing" than Wallis, it should be clear that a patient like Whaley who had been able to speak before could be taught to speak again.[197] The same could not be said for Popham when Holder began to teach him. In addition to portraying his success as a bigger accomplishment, Holder noted that Wallis was not even the first person to teach a deaf person to speak who had previously been able to hear. Holder was referring to an account of Charles I encountering a deaf child who had learned to speak when Charles was in Spain to court the Infanta Anna Maria of Spain.[198] This account was recorded by Kenelm Digby who accompanied Charles in Spain. Although Digby reported that the child, Luis de Velasco, had been deaf since birth, Holder claimed that Velasco had been able to hear as a child

195 By then, the most recent edition of the *Grammar* included the section on "TEACHING THE DUMB TO SPEAK" in the preface; neither this section nor *De loquela*, which began just a few pages after this section, mentioned Holder's work with Popham. Holder took issue with that section of the preface later in the *Supplement* (Holder, *Supplement to the Philosophical Transactions*, 14; Wallis, *Grammar*, 117-119).

196 Holder, *Supplement*, 7, 9-10. For Holder, neither Oldenburg nor Plot was to be blamed for enabling Wallis to steal the honour of others: "It is true that Mr. *Oldenburgh* and Dr. *Plot* have put upon Record some great performances of Dr. *Wallis*, and the Reader may seem obliged to relye upon their credit. But it is not true that either of them knew or writ any thing of those matters, but what was put into their hands by Dr. *Wallis*." After all, Holder added, what Wallis had told them was plausible, and they had no way to know it was not true. Oldenburg and Plot may not have known about Holder's demonstrations with Popham at Bletchingdon, but Wallis certainly did because he had attended them (Holder, *Supplement to the Philosophical Transactions*, 11, 14).

197 Holder, *Supplement to the Philosophical Transactions*, 2.

198 Holder, *Supplement to the Philosophical Transactions*, 4-5.

which, as Rée claims, may well have been the case.[199] For Holder, then, Popham was the first known person who had been born deaf but had subsequently learned to speak, which Holder had taught him. Thus, Holder defined the entity being contested in a way that suited his priority claim: the real accomplishment was giving speech to someone who had never been able to hear, because only this was entirely unprecedented.

Additionally, Holder claimed, the speech he had given to Popham was superior to what Wallis had given to Whaley. Holder taught Popham to speak "with a good and graceful tone"; Wallis taught Whaley to speak "with harsh ill tones".[200] Holder added that *De loquela* had also been surpassed in quality by more recent publications on the same subject; thus he recommended that Wallis stop milking it to promote his reputation.[201] Holder's attitude toward *De loquela* was like Wallis's toward Descartes' *Géometrie*; it was redundant in light of works that both preceded it and followed it. The implication was that Wallis was the comparative amateur in the study of speech and its application to deaf students. That was why Wallis had managed to be nothing more than "an interloper in this experiment."[202] Holder's somewhat insulting tone turned to outright mockery as he concluded the *Supplement*: if Wallis should write "such-like grave Considerations" about whether it is possible to make landmark inventions like "Telescopes, Microscopes, Printing, Guns and Squibs", managed to get these considerations into the *Philosophical Transactions*, and made people believe him when he later claimed to invent them, then Holder would not intervene, "provided he deal more fairly hereafter about the Deaf and Dumb."[203] These remarks were clearly facetious, but they still reflect the fact that, as much as he tried to convince the reader otherwise, Holder's main concern was being recognized for his priority rather than serving the truth for the benefit of the public.

Based on the *Supplement*, Jonathan Rée concludes that "it would seem that Wallis

199 Rée, *I See a Voice*, 99-100, 108-109, As far is Rée is concerned, Velasco "appears to be the first 'deaf and dumb' person to learn to participate fully in the conversations of hearing people – not only producing vocal sounds in accordance with his knowledge of a written language, but also recognizing the words of others simply by watching their faces as they spoke." There were doubts about details of Digby's account apart from those of Holder. George Dalgarno, a Scottish grammar school teacher who worked on communication skills with deaf people himself, suspected that Digby had exaggerated how well Velasco was able to produce foreign works and read lips (Rée, *I See a Voice*, 100, 104. The quotation is from p. 100).

200 Holder, *Supplement to the Philosophical Transactions*, 5-6.

201 Holder, *Supplement to the Philosophical Transactions*, 10.

202 Holder, *Supplement to the Philosophical Transactions*, 14.

203 Holder, *Supplement to the Philosophical Transactions*, 14.

was stealing Holder's pupil, and trying to steal his reputation as well."[204] Yet Rée neglects to mention that Wallis by no means admitted to the accusations that Holder made in the *Supplement*. Wallis's reply is one of the best examples of a figure in the history of early modern science defending his priority while claiming to act in the interest of the truth and, especially, of the Royal Society. He called the text, which he published later in 1678, *A Defence of the Royal Society, and the Philosophical Transactions, Particularly those of July, 1670. In Answer to the Cavils of Dr. William Holder*. This, however, was almost exclusively a defence of Wallis's priority and integrity.[205] Like Holder, Wallis tailored his polemic to a natural philosophical audience, referring to matters of fact and appealing to witnesses, which in the Boylean program were crucial to the establishment of matters of fact.[206] Indeed, Wallis wrote the *Defence* in the form of a letter to Lord Brouncker, who had shown Holder's *Supplement* to Wallis when he was in London.[207] Wallis could be reasonably sure he would have Brouncker's support because, as discussed above, they had been friends and collaborators in mathematics since the 1650s. In any case, Wallis was publicly taking the matter to one of the Society's highest authorities.

Wallis's defence of his priority was multifaceted. First, he acknowledged that Holder had published a text on the mechanics of speech with an appendix on teaching the deaf to speak, but this text, called *Elements of Speech: An Essay of Inquiry into The Natural Production of Letters*, was first published in 1669, which gave Holder plenty of time to take inspiration from Wallis's *De loquela* of 1653. Second, Wallis reminded the reader that he had established his priority by presenting Whaley to the Royal Society and having his experiment registered; he suggested that Holder would have done the same if

204 Rée, *I See a Voice*, 108.

205 Indeed, the rest of the Royal Society needed very little defending, considering that Holder had exonerated Oldenburg and Plot entirely for their roles in Wallis's self-promotion. Wallis did briefly defend what he portrayed as criticisms of Oldenburg and Plot, but this seems to be mainly for the purpose of further villainizing Holder by implying that he had attacked two other reputable Royal Society figures. There was one clear instance of Wallis defending the Royal Society in the text, which occurred on the last page where he wrote that the council was justified in refusing to licence the publication of Holder's *Supplement* (Wallis, *Defence of the Royal Society*, 13-14, 32-33). The word "Cavils", which appears in Wallis's title, was evidently the word that Wallis used to signify unnecessary and petty complaints. As explained in n. 127 above, Wallis emphasized that he was not "cavilling" when he improved the clarity of the concepts expressed by the mathematicians who figured in his *Treatise of Algebra*.

206 Wallis, *Defence of the Royal Society*, 4-5; Shapin and Schaffer, *Leviathan and the Air Pump*, 23-79.

207 Wallis, *Defence of the Royal Society*, 3.

he had had any success to present, but he did not.[208] Finally, to Holder's main point, that all Wallis had done was repeat what had been achieved in Spain whereas Holder had been the first to give speech to someone who had always been deaf, Wallis's response was twofold. Firstly, he dismissed the account of the Luis de Velasco as a mere rumour. He admitted that he had read neither the account of Kenelm Digby nor that of the Spanish teacher of the deaf, Juan Pablo Bonet, about Velasco, but what he had heard of Velasco's lipreading abilities smacked of fancy; it was "very Unlikely, if not Impossible."[209] Secondly, Wallis asserted that Holder had actually failed to teach Alexander Popham to speak, which is why he was later brought to Wallis for instruction. According to Wallis, Popham himself would confirm this.[210] Wallis questioned Holder's insistence that he had taught Popham to speak and that numerous credible witnesses could attest to this: "that he did *attempt it*, I know very well; but that he did *effect it*, I never yet heard any body say but himself."[211] Eventually, Wallis suggested, Holder had become frustrated and had given up, but Popham's friends and family still wanted him to learn to speak, so they sought out Wallis.[212] The reason that Wallis had failed to mention Holder's work with Popham in the letter to Boyle was his own generosity and consideration: he wanted to spare Holder's reputation by not drawing attention to his failure.[213] Far from trying to steal Popham from Holder and take credit for Holder's work, Wallis had refused to treat Popham until he found out that Holder had given up. He subsequently found that Holder had made no progress with Popham.[214] Thus, for Wallis, whether it was more impressive to teach someone to speak who had never been able to before was irrelevant; Wallis was the only one who had conclusive evidence that he had taught *any* mute deaf person to speak.

In writing the *Defence*, Wallis acted as though he were on trial for failing to respect Holder's priority which, as we have seen, was a serious charge among natural philosophers and mathematicians. Wallis was writing in defence of "Mr. *Oldenburg*, (who

208 Wallis, *Defence of the Royal Society*, 10-11.
209 Wallis, *Defence of the Royal Society*, 20. For a description of Bonet's work with the deaf, see Rée, *I See a Voice*, 100-103.
210 Wallis, *Defence of the Royal Society*, 23.
211 Wallis, *Defence of the Royal Society*, 3.
212 Wallis, *Defence of the Royal Society*, 9.
213 Wallis, *Defence of the Royal Society*, 3, 9-11.
214 Wallis, *Defence of the Royal Society*, 20.

deserved better things;) Of Dr. *Plot*; and of some others, (whom, because he did forbear to name in particular, I shall forbear it too;) but most of all, of *my self*."[215] He wrote that he would not hold Holder's falsehoods against him because he would treat the *Supplement* as "a *Chancery-Bill, for Discovery*; where the Plaintiff (being in the dark) sets forth, not what he knows to be *true*, but what-ever he thinks *possible*, that would be to his *advantage* if true; in order to make discovery (from the Defendants Answer) of what he did not before know."[216] Wallis maintained that he had not gone to Bletchingdon to hear Popham speak, and in fact had never met Popham before his mother brought him to see Wallis in Oxford in 1662.[217] Later in the *Defence* Wallis presented what resembles a long list of criminal charges against him to show how liberally Holder hurled accusations at him. It was to such charges that Wallis wanted to "plead Innocence". Near the end of the *Defence* Wallis responded to each of them, explaining why he considered his actions "lawful".[218] Among these charges was that Wallis "*Contrive*[d], to have this [his success with Whaley] *Entred into the Journal of the Royal Society, and there Registred* . . . and there *reaped great Praise for this Atchievment*." Wallis considered this absurd, noting that Holder acted "as though they had not used to Register what there passed, without my *Contrivance*".[219] In response to such accusations, the implied question that Wallis posed to the reader was: What was his crime? He wrote:

> Was it not as lawful for me to undertake Mr. *Whaly*, as for him [Holder] to undertake Mr. *Popham?* Had he, before that time, obtained a *Patent* for the *sole teaching* of Dumb persons to speak? Or, was it a crime (because he had failed of his enterprise on Mr. *Popham*) for me to undertake Mr. *Whaly* with better success?[220]

Nor was it a crime for Wallis to have written the postscript to the letter in Oldenburg's name, of which Holder complained twelve times by Wallis's count, and to which Wallis admitted in part. Wallis insisted that it was quite common to compose a document for

215 Original emphasis. Wallis, *Defence of the Royal Society*, 3.
216 Wallis, *Defence of the Royal Society*, 7. Wallis later used this metaphor to imply that Holder knew that what he asserted in the *Supplement* was untrue. Holder accused Wallis of imposing his deceptions on Plot's *Natural History of Oxfordshire* "Not, [because] Dr. *Holder* knows this to be *True*: But because it is fit matter for a *Chancery-Bill*" (Wallis, *Defence of the Royal Society*, 22).
217 Wallis, *Defence of the Royal Society*, 3-4.
218 Wallis, *Defence of the Royal Society*, 22, 32. Similarly, Wallis later wrote, "(because I must answer punctually to his *Chancery-Bill*) I must plead *Not-guilty*" (Wallis, *Defence of the Royal Society*, 23).
219 Wallis, *Defence of the Royal Society*, 24-26. The quotations are from p. 24.
220 Wallis, *Defence of the Royal Society*, 9.

someone else to sign within and beyond natural philosophical circles, and there was nothing wrong with this as long as the document accurately reflected the person's sentiments. Furthermore, Wallis suspected that Holder had written the account of his *Elements of Speech* that appeared in the *Transactions* in 1669.[221]

Wallis implied that Holder was persecuting him due to simple jealousy. Wallis thus effectively made himself, rather than Holder, the victim in this conflict; Holder had wrongfully charged Wallis with the serious crime of failing to respect the priority of a fellow experimenter. All of this had the effect of portraying Holder as the instigator of a conflict which Wallis found distasteful. Another of Holder's victims was Oldenburg. It was Holder, not Wallis, who had bullied Oldenburg and caused a conflict; Holder threatened to write against the *Transactions* if Oldenburg did not retract the item containing the letter to Boyle, and said that he would not attend any Society meetings until he was "vindicated."[222] Furthermore it was Holder who was trying to deceive the public. His main objection as Wallis saw it was what Wallis had *not* written in the letter to Boyle, namely "[t]hat *Dr. Holder had taught Mr. Popham to Speak Well*; *to Pronounce Plainly and Distinctly, with a Good and Graceful Tone, whatsoever Words*, &c." In other words, Wallis claimed, Holder wanted Wallis and Oldenburg to include something that was not true and thus participate in Holder's dishonesty.[223] It was Holder who did not respect Wallis's priority when he "play[ed] an After-game", that is, retroactively claimed that he had been successful when he had tried to teach Popham to speak, which would have happened before Wallis had taught Whaley to speak and would therefore give priority to Holder.[224]

In his effort to reduce the *Supplement* to a collection of absurdities, Wallis scrutinized Holder's text thoroughly and quoted it extensively. He tried to show that Holder was paranoid about conspiracies within the Royal Society. He wrote that Holder "imagines *Plots* and *Practises, Designs,* and *Subtil Contrivances*, And a great many more Fansies of his own Brain".[225] Effectively, Wallis manipulated quotations from the

221 Wallis, *Defence of the Royal Society*, 14-15. See *Phil. Trans.* 47 (10 May 1669): 958-959
222 Wallis, *Defence of the Royal Society*, 22.
223 Wallis, *Defence of the Royal Society*, 15.
224 Wallis, *Defence of the Royal Society*, 23-24.
225 Wallis, *Defence of the Royal Society*, 24. Wallis added later that Holder "fancies things of mere accident, to be matters of Design (a thing very incident to persons that are a little uneasie.)" Thus, in

Supplement so he could argue against a straw man. For example Wallis, italicizing the words and phrases he took from the *Supplement*, sought to show how absurd it was that "I *contrived* to publish an *English Grammar*, to which I *subtilly prefixed* my *Treatise of Speech*, in 1653, thereby to Undermine by *Anticipation, p.* 10 Dr. *Holder's Elements*, which were afterwards to be published in 1669".[226] Wallis took this tactic to a comical extreme and constructed a fictional account of the events to suit his own claim, largely using Holder's own phrases, to show how easy it was to make these events seem like a conspiracy. He wrote, for example, "*And* (as p. 3. *Having got a hint (for which he always lay in wait) of a new invention so considerable, (from a small Treatise of Dr. Wallis on that subject;) would* (by putting himself into the Practise, of what Dr. *Wallis* had taught,) *Intitle himself to the experiment.*"[227] This smug parody continued for about six pages; for Wallis, Holder's claim to priority was worthy of nothing more than ridicule.

The *Defence* as a whole took a tone of condescension toward Holder. Wallis explained that he certainly could never have plagiarized Holder's *Elements of Speech* because he had never read it, which he did not think would be worth his time.[228] According to what he had heard, though, he and Holder had different goals: Wallis wanted to teach his subjects "to *understand a Language*" whereas Holder merely tried to teach Popham "to *pronounce* some *words*".[229] This comment had the dual effect of belittling Holder's work and distinguishing it from Wallis's own work, thus contributing to his priority claim. He dismissed Holder's complaint that he had not been mentioned in *Transactions* with the phrase "*Hinc illæ lachrymæ*!", or "Hence these tears!"[230] Granted, the item in the *Transactions* of July 1670 had mentioned neither Holder nor his *Elements of Speech*; but neither had the account of Holder's *Elements of Speech* that appeared in the *Transactions* in the previous year mentioned Wallis or *De loquela*, and Wallis had not complained about that.[231] Wallis included a long digression about the origins of the Royal

response to Holder's suggestion that greed was simply in Wallis's nature, Wallis implied that paranoia was in Holder's nature (Wallis, *Defence of the Royal Society*, 33).

226 Wallis, *Defence of the Royal Society*, 3.

227 Wallis, *Defence of the Royal Society*, 26-32. The quotation is from p. 27.

228 Wallis, *Defence of the Royal Society*, 12-13.

229 Wallis added that Holder's method was painful and his own method was not (Wallis, *Defence of the Royal Society*, 19-20).

230 Wallis, *Defence of the Royal Society*, 15.

231 Wallis, *Defence of the Royal Society*, 15. See *Phil. Trans.* 47 (10 May 1669): 958-959.

Society – Wallis argued that its foundations were laid much earlier than a passing comment made by Holder implied – the main purpose of which was apparently to show that Wallis carried more authority than Holder as a founder of the Society.[232] This *ad hominem* approach to reducing Holder's credibility recalls Wallis's dismissive attitude toward François Dulaurens. Wallis's closing remarks in the *Defence* also resembled the dispute with Dulaurens. He reminded the reader how little he cared for such priority disputes: "I have now done with this unpleasing Task (For I take no pleasure in quarrels, or blemishing another mans Reputation.)"[233]

Holder's *Supplement*, then, was far from decisive in the dispute as Rée seems to suggest. Wallis clearly considered himself the winner. He kept the comments about his use of *De loquela* in teaching deaf people to speak in the 1699 edition of the *Grammar*, adding nothing about Holder. He also wrote a very similar account of these experiments in his autobiography. Here Wallis explained that he used the articulatory system of sounds from *De loquela* first to teach Whaley to speak in 1661, and then to teach Popham, "[o]n whom *Dr. William Holder* had before attempted it, but gave it over." Although both patients "were apt to forget . . . much of that nicety" with which he had taught them to speak because these skills had not been constantly reinforced, he had since had many patients whose stutters he had corrected with the same articulatory system from *De loquela*. He did not mention any further deaf patients, but at the very least his reputation as an expert on elocution had survived the priority dispute.[234] This, it seems, was what he worked so hard to gain by portraying himself as an innovator. As the above case studies have shown generally to be the case, the assessment of one's skill was inextricably tied to the assessment of one's ability to invent, discover or innovate.[235]

What this episode shows is that Wallis, who was such an ardent defender of his countrymen in priority disputes with foreign rivals, would not hesitate to use the same

232 Wallis, *Defence of the Royal Society*, 7-9.

233 Wallis, *Defence of the Royal Society*, 33. It is not surprising that John Aubrey took Holder's side in this dispute. He had a high opinion of Holder, describing him as "a handsome, gracefull person, and of delicate constitution, and of an even and smooth temper; so that, if one would goe about to describe a perfect good man, would drawe this Doctor's Character." In contrast, Wallis was "a most ill-natured man, an egregious lyer and backbiter, a flatterer and fawner on my Lord Brouncker and his Miss, that my Lord may keepe up his reputation" (Aubrey 160-161. The quotations are from p. 160).

234 Wallis, "Autobiographies," 41-42. The quotations are from p. 42.

235 For more discussion on this connection, see Chapman, *England's Leonardo*, 170; Hellman, *Great Feuds in Science*, 50.

polemical skills and tactics to protect his own priority from domestic rivals. These were by no means mutually exclusive goals; his *Grammar*, for instance, at once praised the inventiveness and intelligence of the English people as a whole and declared Wallis's priority for his articulatory approach to speech, his success in teaching the deaf to speak, and his method of teaching English grammar within a non-Latinized framework. Wallis was a master of portraying a united English front while he dismantled the priority claims of foreigners like Dulaurens, but he would risk this image of unity to protect his own priority and reputation. As we have seen, siding with one's countrymen was a default position in priority disputes but this never precluded the possibility of other loyalties – including loyalty to oneself – playing a role. It is in domestic priority disputes like the one between Wallis and Holder, when nationalist rivalries were not a factor, that we see these other loyalties flourish.

Chapter 5: Conclusion

For the December 1962 issue of *Isis*, A. Rupert Hall and Marie Boas Hall wrote an article entitled "Why Blame Oldenburg?" in which they sought to exonerate Oldenburg from what they thought was undeserved blame for instigating conflict. In particular, the Halls claim that it is unfair to blame Oldenburg for the conflict between Hooke and Newton over their competing theories about the relationship between light and colour. According to many historians, the Halls write, Oldenburg sparked this conflict because he enjoyed it, and thus caused an enmity between Hooke and Newton in 1672 that lasted for the rest of their lives; this opinion is the result of Oldenburg's unwarranted reputation as a troublemaker. However, the Halls write that among contemporaries "[t]here is a strangely unanimous testimony to the fact that Oldenburg was *not* regarded as a troublemaker; that instead he was esteemed by everyone (except Robert Hooke in later years) as a conscientious, painstaking and indispensable servant to the Royal Society."[1] Oldenburg was not to blame for informing Newton and Hooke about each other's criticisms; he was following orders and Royal Society protocol. Oldenburg's involvement consisted of little more than faithfully relaying the contents of the letters written by the two feuding natural philosophers. It was *their* contentious natures, *their* willingness to defend themselves in conflicts at the expense of civility and unity, not Oldenburg's, that drove this and other conflicts between them. Newton was a "vicious animal (*quand on l'attaque, il se défend*)" whom Oldenburg blamelessly provoked by dutifully reporting Hooke's criticisms. Meanwhile, Hooke was stubborn as usual about defending one of his theories and lashed out against his critic as he always did, refusing to accept that Newton's theory was right: "No one can blame Hooke for being wrong; only for being arrogantly wrong."[2] Although they believe they have made it clear that Oldenburg's responsibility for the quarrel was minimal, the Halls write that they are not optimistic that they will change historians' mind about Oldenburg's personality because "historiographical myths are all too slow to change, and scapegoats all too hard to vindicate."[3]

1 A. Rupert Hall and Marie Boas Hall, "Why Blame Oldenburg?," *Isis* 53 (1962): 482.
2 Hall and Hall, "Why Blame Oldenburg?," 482-491. The quotations are from pp. 485 and 490.
3 Hall and Hall, "Why Blame Oldenburg?," 491.

What the Halls seem to have overlooked in their assessment of this dispute is Oldenburg's agency. This is curious considering how much the rest of their scholarship served to emphasize that agency, from their editing of the *Oldenburg Correspondence* to Marie Boas Hall's biography which she fittingly titled *Henry Oldenburg: Shaping the Royal Society.* The Halls may have been, to borrow a phrase from Jill Lepore, "historians who love too much".[4] They are keenly aware of and upfront about Oldenburg's agency in guiding the Royal Society to its seventeenth-century prominence, but not in tarnishing the Society's image by stirring up conflict between its members. The Secretary was often obligated to follow orders from the Society regarding what he wrote and to whom, but, the Halls' affection for him notwithstanding, Oldenburg himself decided the tone and the exact content of his letters. He wrote to his correspondents within the context of relationships *he* had developed; it was *his* correspondence network that he built and maintained, even though he mainly used it in service to the Royal Society. As for the *Philosophical Transactions*, Oldenburg went out of his way to correct readers who thought that the Royal Society was responsible for their publication, as the Halls have noted elsewhere.[5] The Halls make an important point about Oldenburg – that as Secretary he was answerable to authority figures within the Royal Society – but there was plenty of room in this role for the former diplomat to manipulate his correspondents, be they English or otherwise, Royal Society members or outsiders. To admit this is not to slander Oldenburg; it is not an opinion from which historians must "vindicate" him. Oldenburg's job included polemics and in this he was very successful.

The Halls' article essentially boils down to a *tu quoque* argument. Newton and Hooke were enthusiastic about conflicts – including priority disputes, like theirs over the inverse-square law – but so was Oldenburg, regardless of the fact that he projected an image of neutrality.[6] The reason why this quality was compatible with the Fellows seeing him as a "conscientious, painstaking and indispensable servant", as the Halls claim, is that this quality was part of the *culture* within the Royal Society. It was utterly normal for

4 Jill Lepore, "Historians Who Love Too Much: Reflections on Microhistory and Biography," *The Journal of American History* 88 (2001): 129-144.
5 A. Rupert Hall and Marie Boas Hall, "The First Human Blood Transfusion: Priority Disputes," *Medical History* 24 (1980): 463-464.
6 See Steven Shapin, "O Henry," review of *The Correspondence of Henry Oldenburg*, eds. A. Rupert Hall, Marie Boas Hall and Eberhard Reichmann, *Isis* 78 (1987): 417-424.

Oldenburg to write to correspondents with his contentious attitude just below the surface. As the well-connected mouthpiece of the Royal Society and the editor of the journal that readers associated with the Society, Oldenburg was uncommonly well-suited for catalyzing conflict between distant natural philosophers. Although other Fellows made occasional perfunctory complaints blaming Oldenburg for involving them in conflicts in order to save face, they valued these conflicts and were perfectly willing to participate in them. For instance, they contributed enthusiastically (despite their insistence to the contrary) to the English case in the priority disputes with foreigners that Oldenburg inflamed because these were chances for coups against foreign rivals. These disputes should be viewed not as unfortunate moments when scientists' egos got in the way of progress, but rather as opportunities – if not actively sought then certainly welcomed – for an individual to enhance his own reputation, that of his scientific society, or that of his nation. Oldenburg was exceptionally well-situated to notice these opportunities and to make other Royal Society members aware of them; he was also quite skilled at personally seizing these opportunities when the responsibility fell to him. The same can be said for John Wallis who had his own contacts, foreign and domestic, within the Republic of Letters; his own vehicle for publicly promoting English inventions and discoveries in the form of his many printed texts; and his own set of rhetorical skills honed over a long career of argumentation. "Why Blame Oldenburg" is an instance of the embarrassment about conflict between major scientific figures that Simon Schaffer identifies,[7] an embarrassment that obscures the central role – indeed the utility – of conflict in the seventeenth century not only in knowledge production, but also in fashioning the reputations of the producers of knowledge at the levels of individual, society, and nation.

This view of priority disputes as desirable, in addition to the absence of any apparent agreed-upon way to know when a dispute had been settled,[8] may have been why priority disputes (and similar conflicts) could last for years. It was not necessarily in a natural philosopher or mathematician's best interest for him to limit his opportunities to

7 Simon Schaffer, "Making up Discovery" in *Dimensions of Creativity*, ed. Margaret A. Boden (Cambridge: The MIT Press, 1994), 31-32, 34.

8 This is related to Mario Biagioli's point that priority disputes were facilitated by the lack of clear rules for establishing priority (Mario Biagioli, *Galileo's Instruments of Credit: Telescopes, Images, Secrecy* [Chicago: University of Chicago Press, 2006], 97. See Chapter 3, n. 64 above).

belittle his rivals. As it happened, conflicts generally ended when circumstances forced them to end: when a participant became ill (as in Hooke's dispute with Huygens) or died (as Wallis's dispute with Hobbes), when more pressing matters arose (as in the blood transfusion dispute, for both Denis and the Royal Society), when it was clear that the dispute was pushing the boundaries of gentlemanly decorum too far (as was one factor in the Wallis-Dulaurens dispute), or – occasionally – when a consensus emerged (as was another factor in the Wallis-Dulaurens dispute). In such cases the continuation of a dispute was either impossible or became a liability; in short it ceased to be an opportunity. It seems that Oldenburg more than anyone was attuned to the changing level of the utility of a priority dispute; this was part of his job.

Yet the reason for Oldenburg's active involvement in a dispute between Newton, the Royal Society's rising star, and Hooke, its Curator of Experiments who was primarily responsible for the Fellows having anything to talk about at their meetings,[9] is less obvious. Why would Oldenburg give Newton a chance to show that Hooke's theory of colours was wrong and thus belittle Hooke's reputation? During Hooke's priority dispute with Huygens beginning in 1674/5 over the invention of the balance-spring watch, Oldenburg may have helped Huygens for selfish reasons – Huygens offered him English patent rights for the invention – but why did other members including Viscount Brouncker side with Huygens instead of Hooke?[10] The answer may be that Hooke was an atypical member of the Royal Society. Like a foreigner, he was a sort of "other" whose differences may have been enough to make certain Fellows favour a foreign member over him. Hooke was made Curator of Experiments in 1662, but was not elected as a Fellow until the following year. When he ran for a position on the Royal Society Council, he was not elected.[11] As Rob Iliffe explains, Hooke had as many connections to London's instrument makers and artisans as he did to its natural philosophers and scientifically curious noblemen. He was perhaps more at home in coffee-houses than at Society

9 See Lisa Jardine, "Hooke the Man: His Diary and His Health" in *London's Leonardo: The Life and Work of Robert Hooke,* ed. by Jim Bennett, et al. (Oxford: Oxford University Press, 2003), 167; Richard Westfall, *Never at Rest: A Biography of Isaac Newton* (Cambridge: Cambridge University Press, 1983), 628.

10 Rob Iliffe, "'In the Warehouse': Privacy, Property and Priority in the Early Royal Society," *History of Science* 30 (1992): 41-52.

11 Marie Boas Hall, *Henry Oldenburg: Shaping the Royal Society* (Oxford: Oxford University Press, 2002), 266.

meetings.[12] The Fellows found Hooke to be not only contentious but also "ungentlemanly" in the balance-spring watch dispute, which alienated Huygens and reflected poorly on the Society. Certainly, other members engaged in conflict, but they did so in a manner that they considered consistent with the decorum of the Republic of Letters. Hooke made another faux pas by going above the heads of the Royal Society elites; he tried to settle the dispute in his favour by asking the King directly for a patent.[13] As Steven Shapin notes, Hooke was not known as an experimental philosopher to his contemporaries, but rather as a city surveyor,[14] the Curator of Experiments, a mechanic, and an assistant to the likes of Thomas Willis and Robert Boyle. Hooke was a dependent of the Royal Society, in greater need of money than most, whom the rest of the Fellows viewed as socially inferior. They were never sure how to classify him; Hooke had a "problematic identity" to which the Fellows typically responded with doubts about his integrity and work ethic.[15] Like every Fellow, Hooke had split loyalties, but they were split in a way that his superiors within the Society did not like and could not control, and this, I suggest, is a major reason for the failure of key Society members to support him during priority disputes. For the Royal Society as a corporate entity, priority disputes were about protecting the Society's reputation and intellectual property from the perceived "others" who were so important to their conception of their own identity.

12 Rob Iliffe, "Material Doubts: Hooke, Artisan Culture and the Exchange of Information in 1670s London," *The British Journal for the History of Science* 28 (1995): 285-318.

13 Iliffe, "In the Warehouse," 43-44.

14 Hooke was hired as a surveyor by the City of London after the Great Fire of 1666. In this role he surveyed streets and properties to plan for street widening, and he assessed the claims of Londoners who wanted reimbursement for giving up their property during the rebuilding process or were disputing issues like property boundaries with their neighbours. M. A. R. Cooper, "Robert Hooke's Work as Surveyor for the City of London in the Aftermath of the Great Fire. Part One: Robert Hooke's First Surveys for the City of London," *Notes and Records of the Royal Society of London* 51 (1997): 161-174; M. A. R. Cooper, "Robert Hooke's Work as Surveyor for the City of London in The Aftermath of the Great Fire. Part Two: Certification of Areas of Ground Taken Away for Streets And Other New Works," *Notes and Records of the Royal Society of London* 52 (1998): 25-38; M. A. R. Cooper, "Robert Hooke's Work as Surveyor for the City of London in The Aftermath of the Great Fire. Part Three: Settlement of Disputes and Complaints Arising from Rebuilding," *Notes and Records of the Royal Society of London* 52 (1998): 205-220.

15 Steven Shapin, "Who Was Robert Hooke?" in *Never Pure: Historical Studies of Science as If It Was Produced by People with Bodies, Situated in Time, Space, Culture, and Society, and Struggling for Credibility and Authority* (Baltimore: The Johns Hopkins University Press, 2010), 182-194. The quotation is from p. 183. Hooke was a busy man with many commitments, but this earned him little sympathy from the Royal Society. The Fellows frequently became frustrated with Hooke for beginning projects and not finishing them despite numerous reminders at Society meetings. Even Robert Moray, who had nominated Hooke as Curator of Experiments, complained of his laziness (Jardine, "Hooke the Man," 164-181; Lisa Jardine, *The Curious Life of Robert Hooke* [New York: HarperCollins, 2004], 99, 125-126, 164-168).

Hooke both was "other-like" himself, and risked yielding ground to foreign "others" through his ungentlemanly behaviour.

Of course, as we have seen, a Fellow could have a goal during a priority dispute that was unrelated to the interests of the Royal Society. In most cases, a disputant's nationalistic and personal goals were compatible since defending the Royal Society and the English nation provided chances for advancing one's career. Yet there was a limited number of these chances because the enthusiasm with which priority disputes were sought could not be obvious. Fellows like Oldenburg and Wallis were opportunistic and took offence on England's behalf where they could, but it would have breached the decorum of the Republic of Letters to make conflicts out of nothing. Thus invention and discovery, so important as sources of reputation, patronage, and and some cases money, were subject to domestic disputes as well; it was ideal to gain honour at the expense of a foreign rival, but it could still be necessary to gain it at the expense of a countryman. Priority disputes affecting the Royal Society in the seventeenth century were, in short, about conflicting loyalties. They revealed the locus of tension between loyalty to any two entities from among the Republic of Letters, the English nation, the Royal Society, and the individual. Inventions and discoveries were valuable to each of these entities for different reasons and, accordingly, competing claims to these inventions and discoveries exposed the weaknesses in a system in which a natural philosopher or mathematician could normally serve each of his loyalties simultaneously. The last of the entities listed above, the individual, could include oneself (as when Lower wrote his *Tractatus de corde*), one's friends (as when Lower wrote his *Vindicatio* defending Thomas Wallis), or both (as when Wallis accused Dulaurens of plagiarizing himself and William Oughtred). Of course, as Jardine has explained, defending a friend could also be a way of advancing one's own career.[16] Friendship also came into play when disputants on the same side shifted blame onto each other for instigating a dispute. Oldenburg seems to have tolerated Clarke and Wallis deflecting the blame for their participation in their respective disputes onto him, and there is no indication that this strained his relationship with either of them.

This view of priority disputes is fundamentally social, which is why I have

16 N. 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), 15-20. See Chapter 2, pp. 41-42 above.

highlighted the various social activities that disputants used to argue for priority. These activities required a set of skills that only partly overlapped with those used in knowledge production. Success in priority disputes thus required the participation of individuals who were either personally involved in, or very close to those involved in, experimental philosophy or mathematics, but who also had an aptitude for rhetoric. Hence we see Oldenburg, Wallis and Clarke engaged in the redefinition of the subjects of disputes; of priority itself as the result of either an idea, a performance, or a publication; of the distinction between invention and innovation; and of the the rules of gentlemanly conduct. These Fellows were masters at manipulating the factors in their social environments over which they had some control. The main participants in the rhetorical side of priority disputes can be sorted into two roles – relayers of information like Oldenburg and the experts they called upon like Wallis and Clarke – but nothing restricted these roles from overlapping, and a Fellow could switch roles from case to case. Thus Oldenburg took an active role in arguing for English priority in the blood transfusion dispute, and Wallis wrote to his own correspondents both in England and on the Continent, such as Collins and Huygens, respectively. Sometimes experts did not need Oldenburg to call upon them and participated of their own volition, and sometimes the experts who participated were the inventors and discoverers themselves: Richard Lower embodied both of these possibilities. Additionally, priority disputes could involve essentially every member of the Royal Society, particularly those who attended meetings, at least to a small extent. In the blood transfusion dispute, for example, the team of surgeons that the Society appointed replicated Lower's experiment, and the Fellows who watched served as witnesses. The Society was then able to confirm Lower's invention and claim priority on his behalf. Even Fellows outside of London, apart from those whose help Oldenburg called upon, could play a small role by lending their support to the English cause in the form of a letter to Oldenburg indicating how much more convincing the English case was than that of their opponents, and perhaps even providing some new evidence that worked in the Society's favour. Such letters were most common during high-profile cases like the blood transfusion dispute. In fact, this seems to be how Clarke first became involved in the blood transfusion dispute and the dispute with De Graaf (his

letter to Oldenburg of April/May 1668 addressed both),[17] although Oldenburg certainly encouraged and facilitated his subsequent participation in the latter.

Regardless of one's social standing within the Royal Society and within English society in general, any contribution to the establishment and defence of English priority was a potential source of honour and social credit, although these prizes took different forms. For Richard Lower, the blood transfusion dispute was a chance to advance his career, to confirm the promise that Boyle saw in him. For Boyle, although he had little involvement in the dispute itself, the initial announcement of Lower's invention to the Fellows was a chance to enhance the reputation of the Society that he, as its most prominent member, represented; he encouraged the association of the solution to a notoriously difficult problem with one of the Society's up-and-coming contributors. For Oldenburg, the dispute became a chance to vindicate himself and his journal from the embarrassment of an unauthorized issue having printed a document in support of the French priority claim without qualification. For Wallis, priority matters were generally chances to gain honour at someone else's expense, whether by pointing out the weaknesses of Huygens' embarrassingly vulnerable anagrams, putting an ambitious François Dulaurens in his place, making clever charges of plagiarism against a dead French genius like Descartes who could not fight back, or claiming to vindicate the entire Royal Society from the accusations of William Holder. This last example is an exception to the general rule that the disputants' personal goals were consonant with those of the Royal Society, although Wallis tried to circumvent this problem by portraying Holder as an assailant of the Society as a whole. Similarly, most polemics supporting an English priority claim were consistent with English nationalistic sentiment, but the Society valued some English natural philosophers and mathematicians more than others, and valued some foreign contributors more than some Englishmen; thus Newton and Huygens both found powerful allies within the Society at Hooke's expense. The Society promoted English nationalism and projected an image of English solidarity during priority matters, but only as long as it was convenient.

The tools that intellectuals used to protect and defend priority, from anagrams and ciphers, to polemical articles appearing in the *Philosophical Transactions*, to the

17 OC *Clarke to Oldenburg April/May 1668* IV 350-369, especially 366-367.

publication of the posthumous works of an English scientific hero, all required the participation of people apart from the claimants to priority: a correspondent to receive and record the anagram or cipher, an editor to include the item in the *Transactions*, or collaborators to help the publisher of a posthumous work decide how to present the material in a way that reflected well on its writer. Even in domestic disputes the participants used the social tools available to them. For example, they appealed for help to Boyle as a credible witness of their accomplishments; to Oldenburg as the keeper of records in his minutes, the register books, and the *Transactions*; and to the King whose authority as the patron of the Society might supersede even that of the Council and Lord Brouncker. We have seen that invention and discovery were crucial parts of the personal and collective identities of mathematicians and natural philosophers, a development which Pamela O. Long suggests was the product of an increasing awareness of and appreciation for individual authorship that developed alongside experimental philosophy and empiricism in early modern Europe.[18] This was part of the backdrop to every seventeenth-century priority dispute, and it goes a long way toward explaining the usually tacit agreement of the individuals to work together in claiming priority.

At least in the case of the Royal Society, another tacitly acknowledged part of that backdrop was, as discussed above, the self-conception of Englishmen and Society members as different from other groups that they encountered. It has long been recognized that the Society closed itself off to such groups despite Boyle and others claiming that their experiments and meetings were open and public.[19] My comments on the Royal Society's willingness to grant priority to foreigners would be complemented by further research on the success of other fringe groups connected to the Society in receiving credit for priority, including men of lower social status, women, and the religiously heterodox. Ruth Watts has shown that although men generally limited women's role in seventeenth-century English science to the culinary and medical realms, a few elite women found ways to involve themselves in other scientific endeavours and

18 Pamela O. Long, *Openness, Secrecy, Authorship: Technical Arts and the Culture of Knowledge from Antiquity to the Renaissance* (Baltimore: The Johns Hopkins University Press, 2001), 4, 15, 248-249.
19 See Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle and the Experimental Life* (Princeton: Princeton University Press, 1985), especially 39-40, 111-114, 334-341. Long extends this discussion of illusory openness to the Académie des Sciences (Long, *Openness, Secrecy, Authorship*, 248).

occasionally even in the activities of the "completely masculine" Royal Society.[20] It would be worthwhile to examine whether women with husbands and relatives in the Royal Society – the likes of Margaret Cavendish, Mary Evelyn and Lady Ranelagh[21] – could receive credit from the Society for an invention or discovery. It would be similarly beneficial to analyze whether priority could be granted to someone like Edmund King, the surgeon who performed blood transfusions at Royal Society meetings, or to a Catholic like Kenelm Digby.

The next logical step would be to examine priority issues in the Royal Society in the eighteenth century. While Richard Sorrenson argues that the Royal Society was not in a state of decline in the eighteenth century as most people believe, he grants that it was beginning to undergo significant changes. A number of these changes warrant ending my examination in the year 1700 and leaving the issue of the relationship between nationalism and priority in the eighteenth century to future studies. Firstly, the composition of the Society gradually changed as more people from lower social strata were elected as Fellows, especially surgeons, apothecaries and schoolteachers. Secondly, even if, as Sorrenson argues, the quality of the Fellows' work did not decline, the Royal Society still seems to have lost its place as the focal point of English science in the eighteenth century. Larry Stewart argues that the Society, which clung to an ideal of exclusivity (despite the modest democratization that Sorrenson identifies), was "pushed . . . to the periphery" and "relegated to reporting and collecting information" for the more practical concerns of other practitioners, like the instrument makers and merchants who drove the exchange of information in the world of coffee-houses.[22] Thirdly, the slight change in the social structure of the Society accompanied a shift in the Fellows' intellectual concerns. Nationalism in the Society largely took the form of intellectual pursuits that served England's imperial goals, and thus pure mathematics almost entirely gave way to forms of mixed mathematics like navigation and cartography. Finally, according to Sorrenson the issue that was most crucial to the Royal Society's

20 Ruth Watts, "Gender, Science and Modernity in Seventeenth-Century England," *Paedagogica Historica* 41 (2005): 79-93. The quotation is from p. 82.
21 Watts, "Gender, Science and Modernity," 86-92.
22 Larry Stewart, "Other Centres of Calculation, or, Where the Royal Society Didn't Count: Commerce, Coffee-Houses and Natural Philosophy in Early Modern London," *The British Journal for the History of Science* 32 (1999): 133-153. The quotations are from p. 140.

identity in the eighteenth century was precision. Fellows prided themselves on the accuracy of their instruments and of their data; Sorrenson also notes that foreigners tried to gain the approval of the Society by insisting on the precision of the results of their research.[23] It would be instructive to determine whether this concern for precision supplanted the seventeenth-century preoccupation with priority or merged with it. There are documented priority disputes involving eighteenth century Fellows – for example, the Newton-Leibniz dispute persisted well into Newton's presidency of the Royal Society which began in 1703,[24] and much later in the century Joseph Priestley and Antoine Lavoisier both claimed priority for isolating oxygen[25] – but one cannot take for granted that priority was as central to personal and national identity for eighteenth-century English natural philosophers and mathematicians. The same can be said for periods prior to the foundation of the Royal Society. Brian P. Copenhaver highlights the continuity in writers' interest in assigning credit for inventions and discoveries from antiquity to early modernity, but he does not suggest that this interest in priority was always as important as it was for members of the Royal Society in the seventeenth century.[26] It seems that priority is relevant to every period in the history of science, but its prominence and its role need to be examined in the particular context of each of these periods.

In the seventeenth century, priority was mainly relevant to two sorts of production within the Royal Society: the production of knowledge and the production of image. These, of course, were overlapping domains; Steven Shapin's work, especially *A Social History of Truth*, has highlighted the link between the credibility of individuals and the credibility of the knowledge – the "matters of fact" – that those individuals produced.[27]

23 Richard Sorrenson, "Towards a History of the Royal Society in the Eighteenth Century," *Notes and Records of the Royal Society of London* 50 (1996): 29-46.

24 See A. Rupert Hall, *Philosophers at War: The Quarrel between Newton and Leibniz* (New York: Cambridge University Press, 1980), 110-259.

25 See Aaron J. Ihde, "Priestley and Lavoisier" in *Joseph Priestley: Scientist, Theologian, and Metaphysician*, eds. Lester Kieft and Bennett R. Willeford (Lewisburg: Bucknell University Press, 1974), 62-91; Joe Jackson, *A World on Fire: A Heretic, an Aristocrat, and the Race to Discover Oxygen* (New York: Viking, 2005), 144-145, 155-162, 175-180, 184-198.

26 Brian P. Copenhaver, "The Historiography of Discovery in the Renaissance: The Sources and Composition of Polydore Vergil's *De inventoribus rerum*, I-III," *Journal of the Warburg and Courtauld Institutes* 41 (1978): 192-214.

27 Steven Shapin, *A Social History of Truth* (Chicago: University of Chicago Press, 1994), especially 126-192, 291-302. See also See Shapin, *Never Pure*, 5-13; Shapin, "O Henry," 424. This notion is also present in Shapin's collaborative work with Simon Schaffer (Shapin and Schaffer, *Leviathan and the Air-Pump*, especially 1-79).

Thus every claim to priority was at the same time a claim to be the sort of person who *could* claim priority, that is, the sort of person who could produce reliable knowledge. This helps to explain why priority disputes so often merged seamlessly with debates in which the participants disparaged the quality of each other's work; to weaken an opponent's credibility as a source of natural knowledge was also to weaken his priority claim. Inventions and discoveries were precarious entities soon after they were made and they needed protection; this is illustrated by Auzout's intense criticism of Hooke's new machine for grinding lenses, and by Lower's comment that Edmund O'Meara's criticisms of Thomas Willis were an example of the typical jealousy that accompanied any new experiment or invention. One's credibility also depended on the quality of his writing; thus Wallis was normally staunch about reproducing the words of English writers faithfully, but he would make an exception and modify their words if he thought they had not expressed their thoughts clearly, as he did in his *Treatise of Algebra*. Along with challenging an opponent's priority, an alternative – or additional – strategy for disparaging his invention or discovery was to doubt that he had made it at all or to question the premises on which it was based; thus Clarke claimed that De Graaf's description of the testes was both unoriginal and wrong.

Of course, the intellectual community was more likely to accept someone's inventions, discoveries and experiments if they believed that person had inherent credibility. I suggest that, during priority disputes, the need to project oneself as credible – a quality which encompassed virtue, neutrality, and knowledge – was usually manifested in the form of a feigned disapproval of conflict. This approach to disputes was an imperfect emulation of Boyle who was intensely concerned about protecting his priority but never seems to have considered outright polemical dispute to be an acceptable way of defending it; he was equally concerned about civility and unity within the Republic of Letters and, especially, within the Royal Society. Some other figures seem to have had a genuine distaste for conflict as Boyle did, such as Henri Justel and John Collins, and as such it is probably no coincidence that they only appeared at the periphery of priority disputes, occasionally lending their support to one side but not generally taking an active role in the polemics.[28] These figures, especially Justel,

28 For a sense of Collins' aversion to conflict in the Republic of Letters, see Chapter 4, n. 103 above.

recognized that enthusiasm for heated polemics was part of the culture of early modern intellectual society, but this did not mean that they approved of it. For most contemporary mathematicians and natural philosophers, however, the insistence on one's aversion to conflict was merely a tactic, a sign of recognition that while disrespect for priority could reduce one's credibility, so could an overzealous attempt to prove that one's opponent showed that disrespect.

The *ad hominem* approach to priority disputes remained popular and effective despite the balancing act that it required. Disputants like Clarke and Wallis mastered this technique because there was great potential reward. Richard Terry points out that, beginning in the late seventeenth century, writers would use charges of plagiarism as a way to ridicule each other, the implication being that one's opponent had to plagiarize because he lacked the creativity to produce anything original of value.[29] If a lack of originality could imply intellectual mediocrity, it stands to reason that what is essentially the reverse was also true: intellectual mediocrity could imply the need to steal the inventions and discoveries of more capable thinkers. Furthermore, as discussed in Chapter 4, he who recognized that a writer was unoriginal gained honour at that writer's expense.[30] Disputants adopted the precarious position of denouncing conflict while participating in it because they sought to exploit it for personal, societal (that is, of the Society), and national gain. The result is that my discussion of priority disputes in the early Royal Society has required me to analyze rhetoric as much as, if not more than, the research of natural philosophers and mathematicians and its results. From this we have gained, I hope, a better understanding of individuals in the Royal Society whose role was not primarily (or at least not always) related to research, but who were nevertheless crucial to the success of the Society's research programs in experimental philosophy and mathematics. The best example of such an individual is Henry Oldenburg. I suggest that, in trying to rescue Oldenburg from a reputation as a contentious rabble-rouser, the Halls did not do any favours to Oldenburg's legacy. There were many occasions on which a

29 This association endured throughout at least the majority of the eighteenth century (Richard Terry, "'In Pleasing Memory of All He Stole': Plagiarism and Literary Detraction, 1747-1785" in *Plagiarism in Early Modern England*, ed. Paulina Kewes [New York: Palgrave Macmillan, 2003], 182, 194-200).

30 Hall Bjørnstad, ed., *Borrowed Feathers: Plagiarism and the Limits of Imitation in Early Modern Europe* (Oslo: Unipub, 2008), 7 (editor's introduction). See Chapter 4, p. 127 above.

rabble-rouser was exactly what the Royal Society needed.

One might wonder, however, why Oldenburg proved to be not only exceptionally useful to the other Fellows but exceptionally loyal as well, his run-ins with Robert Hooke notwithstanding. Compared to a native Englishman, would Oldenburg not have had less of a stake in an institution that, despite Samuel de Sorbière's initial impression, was distinctly and self-consciously English? It must be remembered that Oldenburg and his contemporaries predated any purely biological conception of race, and that English nationalism, as much as it merged with English xenophobia and motivated English people's interaction with foreigners, was still embryonic. Just as the self-conception of the English as a nation was not quite compelling enough to eclipse the other loyalties of Royal Society members, nor was it cohesive enough to exclude Oldenburg, or someone disaffected and displaced by his own nation like Justel. This is why, as Liah Greenfeld argues, English nationalism first flourished within familiar religious and scientific frameworks,[31] and why, as Metzger and Shapiro both show, early modern English Christians held contradictory views about the potential for reintegrating English Jews into their society.[32] It is remarkable that a former tutor and diplomat from Bremen could collaborate so effectively with the Savilian Professor of Geometry and erstwhile parliamentarian codebreaker to pursue the decidedly pro-English agenda of establishing and defending English priority; it would be even more remarkable if they considered themselves members of the same nation.

Yet Oldenburg's sense of self is difficult to pin down. As Shapin argues, it was crucial to his success as Secretary of the Royal Society that he pretend not to have much of an identity at all; he needed to adopt an air of neutrality in order to seem credible.[33] Were his reminders to his correspondents that he was not German and not English expressions of pride in his homeland (which of course was not Germany *per se* because it did not yet exist as such), or were they another tactic that he used to convince the public that he maintained a professional distance from the rest of the Society? It seems likely

31 Liah Greenfeld, *Nationalism: Five Roads to Modernity* (Cambridge, MA: Harvard University Press, 1992), 60-85.
32 Mary Janell Metzger: "'Now by My Hood, a Gentle and No Jew': Jessica, *The Merchant of Venice*, and the Discourse of Early Modern English Identity," *PMLA* 113 (1998): 52-63; James Shapiro, *Shakespeare and the Jews* (New York: Columbia University Press, 1996), 1-11.
33 Shapin, "O Henry," 418.

that both answers are right, that this was another case in which a member of the natural philosophical community had multiple loyalties that he was able to serve simultaneously under normal conditions, just as he did by trading in the currency of information and honours for the benefit of both the Royal Society and its contacts throughout the Republic of Letters. A Fellow of the Royal Society was many things at once and maintained a balance that was revealed to be precarious in cases like priority disputes when his peers invoked one or more of his loyalties. This feature of the identity of a Royal Society member, like so many other such features, is best exemplified in the person of Henry Oldenburg, who was a native of Bremen who became a naturalized Englishman, a Secretary of the Royal Society who also sat on its first Council, and a citizen of the Republic of Letters who sowed seeds of international discord whenever it served the loyalties he valued more highly.

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