Mersenne and His Correspondents: An Essay on the Social Origins of Modern Scientific Method

Thomas Alan Davidson
Faber, Virginia

MBA Vanderbilt University 1983
BA Vanderbilt University 1974

A Thesis presented to the Graduate Faculty
of the University of Virginia in Candidacy for the Degree of
Master of Arts

Department of History

University of Virginia
September 2020

Committee Members

Erin Lambert, PhD
Elizabeth Meyer, PhD

Introduction

In the wake of the publication of the Copernican hypothesis of a heliocentric universe, two important changes in the European approach to astronomy and physics took place. The first, and more familiar, involved the development of new approaches to describing and predicting the movement of physical objects in space. The Copernican hypothesis not only reordered the cosmos; it rendered the entire model of the physical construction of the cosmos that underlay Ptolemaic and Aristotelian physics untenable. In the earlier model, the heavenly bodies had been housed in a hierarchy of crystalline spheres that rotated concentrically around a stationary earth.[1] Once Copernican astronomy set the earth itself in motion, the orbits of the Earth's moon and the orbits of the other planets and their moons could no longer be explained credibly as the result of the motion of the spheres.[2] For this reason, a radically new mechanism to explain the motions of objects in space had to be developed.

A second outcome of the Copernican hypothesis also occurred, although it is less prominently discussed in the historiography of science. This second effect was the gradual abandonment of the *philosophical* component of natural philosophy. The discipline of natural philosophy from the time of Aristotle until the early modern period had been deeply involved in seeking to explain how the structure of the cosmos reflected philosophical, and later, religious truth.[3] Once it became evident that no one really understood the mechanical, and especially the dynamic functioning of the

---

[1] Lawrence Principe, *The Scientific Revolution: A Very Short Introduction* (New York: Oxford University Press, 2011), 42-43.
[2] Marie Boas Hall, *The Scientific Renaissance 1450-1630* (New York: Dover, 1994), 114-115.
[3] Pierre Duhem, *The Aim and Structure of Physical Theory* (Princeton: Princeton University Press, 1991), 11-12.

cosmos, it became impossible, at least for the time being, to comment intelligently on the philosophical or religious fitness of those mechanics or dynamics. That enterprise would have to wait. The preliminary task of redefining the celestial mechanics was not accomplished quickly or easily. More than a hundred years separate the publication of Copernicus's and Newton's major works. The temporal duration of the development of the new model is an important fact in and of itself. The new method was not obvious. It had to be worked through, and only then could its philosophical and religious implications be assessed.

The ultimate outcome of this effort is familiar. In Newtonian physics, the discipline of physics was defined by the proposition of mathematically descriptive laws that were deemed valid throughout the cosmos. These laws were developed through close mathematical modeling of the motions of the celestial bodies and validated by replicable, decisive experimentation. At the end of this process, however, interest in explaining the philosophical or religious fitness of the model had waned and certainly ceased to be a core component of the discipline. As Newton himself wrote:

> To tell us that every species of things is endowed with an occult specific quality by which it acts and produces manifest effects, is to tell us nothing; but to derive two or three general principles of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from those manifest principles, would be a very great step in philosophy, though the causes of those principles were not yet discovered; and therefore I scruple not to propose the principles of motion above mentioned, they being of very large extent, and leave their causes to be found out.[4]

[4] Duhem, *The Aim and Structure*, 48, quoted from Newton's *Optics*, Query XXXI.

In this statement, Newton asserts that it is enough to develop intellectual principles by which to describe and predict the motions of objects in nature. The underlying causes of those motions – the “occult specific” attributes of those objects may or may not be knowable, but they are finally not interesting to Newton either. This disappearance of the explanatory component of natural philosophy was the second outcome of the Copernican revolution.

In this essay, I will examine elements of the *process* by which these new principles and the concept of such principles were developed. Specifically, I will investigate how the new approaches were developed within the context of a community of natural philosophers and scholars, that is, through a social process. By a social process I mean the iterative sharing of ideas within a community of investigators who collectively determined the extent to which proposed solutions to the intellectual problems posed by the new astronomy met their intellectual standards and interests. To tell this story, I will rely on the voluminous correspondence of Père Marin Mersenne, a French Minim monk who lived from 1588 until 1648.[5]

Although a monk by calling, Mersenne’s publications primarily addressed the mathematical sciences, and especially their applications to music, mechanics, and optics.[6] Mersenne’s importance to this paper lies both in his own writings and in the correspondence that he carried on with contemporary investigators who were engaged in the same kind of natural philosophical work that he himself was. As Peter Dear writes, Mersenne’s “Parisian cell was the center of a wide correspondence with whoever

[5] Peter Dear, *Mersenne and the Learning of the Schools* (Ithaca: Cornell University Press, 1988), 4.
[6] Dear, *Mersenne*, 4.

seemed to him philosophically like-minded,"[7] and included many of the most famous and influential natural philosophers of his age, including Descartes, Gassendi, and Beeckman, among others. This group was not formally structured and operated largely outside of the university system. What united its members was an interest in developing an alternative to the scholastic approach to natural philosophy. The scholastic approach had, by the time they began to correspond with one another, been subjected to a broad critique that concentrated on its inability to produce conclusive demonstrations of its principles.[8] Mersenne adopted mathematics as his tool for understanding natural phenomena because "the mathematical sciences seemed extendable over such a range of phenomena and with so much genuinely scientific – that is, demonstrative – precision that they far outstripped in practice the pretensions of any properly physical system."[9] This sentence will make more sense to the reader who keeps in mind that in Mersenne's era the term "physical system" referred to a system that undertook to understand the nature of things "behind appearances,"[10] that is, a system of thought that aimed at philosophically explaining as well as describing natural phenomena.

Dear also notes that Mersenne pursued a specific agenda in his studies, and "saw knowledge of nature as a cumulative acquisition of experimentally or observationally ratified facts made into demonstrative science through the techniques of mathematics."[11] His understanding of knowledge was not universally accepted in his time, and leading thinkers of his age, including Descartes and Gassendi, still aspired to

---

[7] Dear, *Mersenne*, 3.
[8] Dear, *Mersenne*, 224.
[9] Dear, Mersenne, 225.
[10] Dear, Mersenne, 229.
[11] Dear, Mersenne, 3.

construct “physical systems” that both described, and explained the bases of natural phenomena.[12] Acting as a kind of clearing house for his correspondents, Mersenne not only responded to their ideas and theories; he was instrumental in publishing some of their works, including treatises by Descartes and Galileo.[13] He thus served to “consolidate, if only partially, an emerging community of natural philosophers following new ways of construing the world.”[14] This community was the social nexus within which his ideas and those of his correspondents were presented, critiqued, and refined.

By examining the scope and content of this correspondence, my intention is to illuminate the process by which some of the major thinkers of the early seventeenth century developed the methods and approaches that provided them and, by extension, the broader intellectual community of the age, with satisfying and defensible solutions to the issues that they were studying. As events turned out, they would find more success in the development of descriptive solutions than with explanatory solutions, although this outcome could not be foretold at the time.

Historiographical Background

There are two broad historiographical themes that shape this essay. The first theme has to do with the considerable authority that early historians accorded to the scientific method itself. This authority informed their conception of the “Scientific Revolution” and largely defined its historiography. The second theme relates to the social process by which the new approaches that were developed during the early

[12] Dear, *Mersenne*, 225.
[13] Dear, *Mersenne*, 4.
[14] Dear, *Mersenne*, 7.

modern period were defined and validated. I will address these two components of the historiography separately.

Historiographical Treatment of the Authority of the Scientific Method

The historiography of natural philosophy and science in the early modern period developed in response to the work of historians such as A. Rupert Hall, E.J. Dijksterhuis, Alexandre Koyré, Marie Boas Hall, and J.D. Bernal, who proclaimed and believed in a genuine Scientific Revolution that was announced by Copernicus and brought to fruition by Galileo, Kepler, Huygens, Descartes, and Newton. Each of these historians made his or her own contribution to describing the intellectual changes that distinguished the Scientific Age from its predecessors. Key elements of the revolution as they envisaged it included the "mechanization of nature,"[15] the recognition of the infinite expanse of the universe, the concept of inertia, and the mathematization of natural law.[16]

Nearly all of these early historians of the so-called Scientific Revolution argued as if the changes in intellectual attitudes and approaches to knowledge that were developed in its course were *objectively* valid and *universally* recognizable and accessible. That is, they proceed as if those approaches, like mathematical truth itself as they understood it, existed outside of society and history and were essentially waiting to be discovered. J.D. Bernal, in the introduction to the second volume of his *Science in History*, puts it this way:

---

[15] E.J. Dijksterhuis, *The Mechanization of the World Picture – Pythagoras to Newton* (Princeton: Princeton University Press, 1986), 3.
[16] Alexandre Koyré, *Études d'histoire de la pensée scientifique (*Paris: Editions Gallimard, 1973), 170.

> This substitution [of a new, quantitative, atomic, infinitely extended, and secular world-picture for the old, quantitative, continuous, limited, and religious world-picture which the Muslim and Christian schoolmen had inherited from the Greeks] was only a symptom of a new orientation towards knowledge. It was changed from being a means of reconciliation of man with the world as it is, was, and ever will be, come doomsday, to one of controlling Nature through the knowledge of its *eternal laws (emphasis added).*[17]

It is this implicit assumption of the inherent validity of western scientific method that lent much of the excitement to the concept of the Scientific Revolution. It was a *true* enlightenment. As such, it marked a historical watershed of first-class significance, and one that virtually validated western history. Revisionist historians would take aim at this assumption and its implications.

In 1993, Cunningham and Williams published an influential critique of the then-established model of the Scientific Revolution entitled "Decentering the 'Big Picture:' 'The Origins of Modern Science' and the Modern Origins of Science." In this critique they argue that the term "science," as it is generally understood today, was not so defined until the late 18th and early 19th centuries, and that the new definition has to be understood in the context of the democratic political and social revolutions of that period.[18] "Science," so defined, was the *ideology* of the professional middle class. As such, it is subject to historical criticism for its role in establishing and perpetuating racist, colonial, and sexist social structures. They also faulted some earlier scholars for downplaying or overlooking the religious motives of the natural philosophers themselves, and for their apparent assumption that science itself was both neutral and

---

[17] J.D. Bernal, *Science in History Volume 2 The Scientific and Industrial Revolutions*, 3rd. ed. (Cambridge: MIT University Press, 1965), 375.

[18] Andrew Cunningham and Perry Williams, "Decentering the 'Big Picture:' 'The Origins of Modern Science' and the Modern Origins of Science," *The British Journal for the History of Science*, 26, no. 4 (1993): 410.

universal.[19] In a 2016 meta-study, Linda Orthia reported that most modern historians seem to be in the camp of Cunningham and Williams, and that few historians can write the term "Scientific Revolution" without encasing it in quotation marks and disclaimers.[20]

David Sturdy's paper "A 'Crise de Conscience Européene avant la lettre'? Classical Science and the Origins of the Scientific Revolution*,"* argues that most of the natural philosophers such as Galileo, Copernicus, and Tycho Brahe who were active in the late 16th and early 17th centuries were not proto-modernists at all. That is, they did not subscribe to a mechanistic view of a nature governed by impersonal mathematical laws rather than the formal attributes of matter. For example, Tycho applied Aristotelian logic to deny the earth's three-fold motion, while Kepler embraced the Copernican model because it accorded with his own Platonic vision of the cosmos.[21] Most of the early modern natural philosophers held on to elements of the old Ptolemaic model, and all of them considered natural philosophy to be a godly and not a "secular" pursuit.[22]

Perhaps the most important outcome of the critiques referenced above has been the discrediting of the idea that modern scientific method as it is popularly understood is universal or that its adoption was inevitable.[23] Under the older thesis, modern scientific method simply is the truth. The more recent interventions hold that the new method was developed and accepted through contingent historical processes and that it was not and is not the *telos* of the history of science. According to these interventions, the approach

---

[19] Cunningham and Williams, "Decentering," 414.
[20] Linda A. Orthia, "What's Wrong with Talking About the Scientific Revolution? Applying Lessons from the History of Science to Applied Fields of Science Studies," *Minerva* 54 (2016), 366.
[21] David J. Sturdy, "A 'Crise de Conscience Européenne avant la lettre?' Classical Science and the Origins of the Scientific Revolution," *International Journal of the Classical Tradition*, 10, no. 1 (2003) 59.
[22] Sturdy "A 'Crise,'" 60, 71.
[23] Sturdy "A 'Crise, 'see discussion on page 56.

developed by the natural philosophers of the early modern period was only that, an approach to knowledge that has proven useful, but not one that is either objectively or a-historically true. If this proposition is true, then it is also true that the development of western scientific approach to scientific knowledge is a phenomenon that can and should be explained historically.

By understanding this process as the result of a social engagement, it is possible to overcome the presumption of the inevitability or universality of the mathematical principles that characterized the new approach. The imaginative difficulty that the reader may experience in construing mathematics or the modern scientific method as mere *social constructs* itself bears witness to the extent to which the transcendent view of mathematics is rooted in modern mentalities. Ronald Giere has identified logic and mathematics as the "last and most formidable strongholds of non-naturalism."[24] By non-naturalism he refers to the prevailing intellectual conviction that logic and mathematics are somehow out-of-nature and thus not subject to sociological or historical analysis. This point is important because the emergence of mathematical modeling as the dominant mode of scientific explanation is a central theme in the ideology of an early modern Scientific Revolution.

In focusing on this component of the history of science, it is possible to locate the "revolution" not so much in individual discoveries, but in the development of an alternative collective understanding of what constitutes knowledge. This new approach to knowledge would eventually inform all of the natural sciences, but would achieve its

[24] Ronald N. Giere, "Modest Evolutionary Naturalism," in *Naturalizing Epistemology and Philosophy*, ed. Michael Mi Chienkuo and Ruey-lin Chen (New York: Rodopi Press, 2007), 30.

first landmark achievements in the disciplines of physics and astronomy. It would shape those disciplines first and decisively because it was in them that it proved easiest to make consistent observations and apply mathematical models accurately to describe those observations and to predict future ones. The question from a historical perspective is why these new approaches were developed when they were, and not earlier or later.

In this context Thomas Kuhn's *The Structures of Scientific Revolutions* provides a helpful paradigm – a term of art he seems to have invented – for analyzing changes in accepted scientific models. Kuhn argues that all established, which is to say broadly accepted, scientific models survive until they are overwhelmed by the observation of anomalies that cannot be fit into them.[25] The process that he describes is a cumulative one. Any scientific model will encounter some anomalies that are either ignored, papered over, or recognized as problems to be solved *within* the established system. It is only when that system encounters a critical mass of such anomalies that some or all of its practitioners begin to look elsewhere for answers.[26]

Kuhn understands this process to be at work when individual theories begin to fail under the weight of contradictory evidence. His model applies both at particular and at general levels. A specific scientific theory that has been posited within a prevailing or established system of thought, such as the theory of the geocentric universe, can fail without destroying the whole of the underlying approach to establishing knowledge that had been followed in the discipline that produced it. In such a case, the effect of that

[25] Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 4th ed. ((Chicago: University of Chicago Press, 2012) xxvii.
[26] Kuhn, *The Structure*, 77, 82.

particular theory's failure is restricted to that theory itself. In other cases, the failure of the theory ramifies so widely across the discipline that it discredits not only the individual theory, but the intellectual canon through which that theory was developed in the first place.[27]

An important premise of this essay is that the adoption of the Copernican model, coupled with other astronomical discoveries that occurred more or less concurrently, represented a catastrophic failure of the Aristotelian approach to knowledge. It was catastrophic not only because the Aristotelian and Ptolemaic models could not be adapted to accommodate a heliocentric cosmos populated by planetary moons and comets, but also because the Aristotelian approach to knowledge could not be used effectively to propose a new model. For this reason, a new consensus had to be established on what constituted usable knowledge and how to discover that knowledge: a new approach was needed. The *social* process through which that consensus was established is of particular importance. How did this process work?

Historiography of the Social Consensus

If the new approaches to establishing scientific knowledge that were developed during the early modern period were not accepted because they were objectively true or universally valid, the question of interest to the historian is this: by what process were they adopted? In addressing this question, I will refer the reader back to the two major intellectual outcomes of the Copernican revolution cited at the beginning of this essay: the adoption of a mechanistic and mathematical mode of describing physical

---

[27] Kuhn, *The Structure*, 75.

phenomena, and the abandonment of deep philosophical explanation of those phenomena. As noted above, the adoption of mathematical modeling is familiar enough to most readers that there is no need to describe it at length here. The abandonment of deep explanation is perhaps less familiar.

To elucidate this issue, I will refer to the writings of Pierre Duhem. Duhem was interested in the relationship between a physical theory and an explanation. A theory in his terminology is simply "a system of mathematical propositions whose aim is to represent as simply, as completely, and as exactly as possible a whole group of experimental laws."[28] Theory, as such, is never final because it is always subject to falsification by future experimentation or by the proposition of a more economical or elegant theory.[29] Explanation, on the other hand, aims at a finality that theory cannot achieve. "To explain," Duhem writes, "is to strip reality of the appearances covering it like a veil, in order to see bare reality itself."[30] The audacity of this distinction is central to Duhem's intellectual scheme and offers a critical insight into the difference between the intellectual project of the pre-modern natural philosophers and that of their modern successors. Duhem argues that an Aristotelian would attempt to explain the effect of a magnet on a piece of iron in terms of the formal properties of both objects, while a Newtonian would dismiss this mode of argument as meaningless. In Duhem's view, the Newtonian has given up on truly explaining the phenomenon and has settled for simply describing its effects with acceptable mathematical precision. The unspoken deficiency in the Newtonian approach, as in any reductionist approach, is that it ultimately relies

[28] Duhem, *The Aim and Structure*, ix.
[29] Duhem, *The Aim and Structure*, xi.
[30] Duhem, *The Aim and Structure*, 7.

upon some fundamental, unknowable causal motive such as gravity that must be accepted as given. For this reason, it can never truly explain the phenomena that it describes.[31]

Duhem's discussion is important because it highlights a fundamental difference between the natural philosophy of the Aristotelian school and its successors that is often neglected or, again, treated as a self-evidently valid: the abandonment of deep explanation. This component of the "Scientific Revolution" must also be understood historically. Specifically, an historical analysis of the process by which that abandonment took place must undertake to understand why and in response to what contingent factors that outcome came about. Armed with Duhem's definitions, it is now possible to review the historiography of the social component of the new science.

Much of the historical literature on the social process of scientific study details the establishment of learned or royal scientific societies whose purposes were to patronize and disseminate scientific discovery. England's Royal Society for Improving Natural Knowledge was established in 1660.[32] The Académie Royale des Sciences was set up in 1666 and was founded by Jean-Baptiste Colbert as a state enterprise.[33] These societies, however, were established after the time period with which this paper is concerned.

More relevant to this study were the societies established outside official auspices that predated the royal societies referenced above. Lawrence Principe

---

[31] Duhem, *The Aim and Structure*, 14-15.
[32] Bernal, *Science in History*, 456.
[33] Principe, *The Scientific Revolution*, 127.

observes that in the late Middle Ages, the study of natural philosophy had taken place "predominantly in universities, monastic settings, and – to a much lesser extent – a few princely courts."[34] An important change in the early modern period involved the development of alternative sites of scholarly enterprise in which scholars who were not all affiliated with the universities "shared their work with like-minded individuals, receiving support, critique and recognition as well as occasional patronage."[35] Such informal societies predated even the Copernican challenge and involved areas of inquiry other than astronomy and physics. Principe sees the development of such societies as fundamental to the humanist enterprise.[36]

Nancy Siraisi has published an analysis of the correspondence of physicians in Italy and Germany in the late sixteenth century in her *Communities of the Learned: Epistolary Medicine in the Renaissance*. These letters reveal wide-ranging networks of physicians and other natural philosophers exchanging medical information and case studies. While physicians had no doubt corresponded with one another in early periods, it is only in the sixteenth century that the publication of substantial collections of correspondence came to be practiced.[37] Siraisi further notes that "the letter collections may have responded to a growing professional and scientific interest in shared accounts of personal experience and in *autopsia* (seeing for oneself). To the extent that letter collections came to include debates over unresolved or contested issues in medicine, perhaps in some instances they, like humanist dialogues, embodied a certain

[34] Principe, *The Scientific Revolution*, 122.
[35] Principe, *The Scientific Revolution*, 122.
[36] Principe, *The Scientific Revolution*, 122.
[37] Nancy G. Siraisi*, Communities of Learned Experience: Epistolary Medicine in the Renaissance* (Baltimore: The Johns Hopkins University Press, 2013), 8.

openness to the perception that truth might emerge through discussion from more than one viewpoint."[38]

Steven Shapin's *A Social History of Truth*, first published in 1994, describes the vital importance of the social nexus to the development of scientific thought in seventeenth-century England. In this book, Shapin examines the role of trust between and among natural philosophers as a critical prerequisite to the establishment of a functioning scientific community in seventeenth-century England. Shapin argues that the popular conception of science as an enterprise based on direct experience and the rejection of the testimony of others is not only untrue, but that practically speaking cannot be true.[39] Instead, Shapin depicts the process of developing knowledge as a collective effort in which trust among the participants plays an indispensable role. The seventeenth-century investigators whom he examines relied upon one another's observations and findings to build a collective understanding of the disciplines that they studied, and could not have achieved what they did had they not pursued this collective, or social approach.[40] Shapin is particularly interested in the importance of the social status of the participants in the scientific endeavor to the development of the community of trust within which it operated. Briefly, it was the iron rule that a gentleman *did not lie* that made that community of trust possible.[41] Although Shapin does not discuss the development of epistemology directly, it is interesting and quite relevant to my discussion that a reputation for truth-telling and integrity was the litmus test for entrance

---

[38] Siraisi, *Communities*, 8.
[39] Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth Century England* (Chicago: University of Chicago Press, 1995), xxv.
[40] Shapin, *A Social History*, xxv.
[41] Shapin, *A Social History*, xxvii.

into the seventeenth-century scientific community that Shapin describes. Those attributes are of particular importance in an intellectual enterprise that is establishing the ground rules for its future.

Approach

This essay examines a period in history in which the Aristotelian model of the cosmos and the Aristotelian approach to establishing knowledge were in the midst of a Kuhnian catastrophe. The Copernican hypothesis, combined with subsequent discoveries of the courses of comets and the Jovian moons, represented much more than anomalies in the old Ptolemaic model. Collectively, they made the model untenable. Concurrently, they exposed the hopelessness of applying Aristotelian reasoning to repair or to replace that model.

The crisis was of Kuhnian proportions because it was simply not possible to modify the Aristotelian model of crystalline spheres to accommodate the complex motions of the planets and their moons, to say nothing of the paths of the comets that were observed and tracked by the astronomers who followed Copernicus. Furthermore, Aristotle's approach to explanatory physics was revealed to be incapable of producing reliable new knowledge. His physical arguments were premised on what turned out to be false assumptions. He had been able to explain with flawless clarity why the earth had to be at the center of the universe. But, when it became evident that the earth was not at the center of the universe, it also became clear that his method could be applied to any reality – a fact that revealed its futility.

In response to this crisis, natural philosophers were faced with two imposing challenges. The first was to try to develop an alternative model for the mechanics of the cosmos. If giant crystalline spheres were not holding the planets and the stars in their course, what was? Once this question was answered, would it be possible to seat that mechanical model into a philosophical or explanatory framework?

To illustrate the workings of this process, I will examine a single case study involving the development of the scientific and philosophical thinking of the French mathematician and monk Marin Mersenne over the approximately ten-year period from about 1625 through 1635. This analysis will examine the changes in Mersenne's thought as revealed in his own writings and place those changes in the context of his extensive correspondence with the leading natural philosophers of his time. The aim of this analysis will be to describe the social environment within which new scientific approaches were developed and adopted by a single practitioner operating in the context of a small but illustrious circle of intellectual confederates.

As this analysis will make clear, the new model was not developed in a day, but rather over the course of a long century. Natural philosophers did not discover eternal truths as if through revelation once the barriers represented by Aristotelian authority collapsed. Instead, they engaged in a long, multi-faceted conversation that was increasingly motivated by the insight that mathematics might provide a fruitful and reliable means at least to describe the motions of the heavens, if not to explain those motions. In the case of Mersenne, this preference for mathematics was based on his interest in finding the most certain basis of knowledge that he could for his study of nature. Mersenne was acutely aware of the "unattainability of essential knowledge" in the Aristotelian sense, but was also profoundly averse to fundamental skepticism. In response, he proposed a solution based on the existence of "undeniable mathematical truths."[42] Ultimately, Mersenne would have to concede that even mathematics was not infallible, but would console himself with its remarkable practical utility.[43] Here, in microcosm, is the social process at work. Mersenne feared the chaotic implications of the collapse of the Aristotelian system. He responded by proposing an apparently infallible mathematical substitute, despite knowing that it was not really infallible at all. His method was fundamentally inflected by his social agenda to preserve the intellectual order provided by the old system.

Because Mersenne and the other natural philosophers of his era were exploring an insight and not experiencing a revelation, their progress was tentative, provisional, and by no means assured. It is in this context that the importance of the social dimension of the process is revealed. Propositions and solutions to specific questions were solicited, propounded, and criticized by a community of natural philosophers, mathematicians, and others. It was through this process that this society of thinkers honed the model that they would ultimately adopt as the new canon for scientific discourse.

Just because a new approach was hypothetically necessary did not mean that it had to happen, nor that it had to happen in the form that it did. The issue could have been ignored or solved by other means. It is also important to understand that the eventual new mechanism was not adopted because it was in, Bernal's phrase, an "eternal truth." It was adopted because it seemed to work, or at least to provide answers

[42] Dear, *Mersenne*, 41.
[43] Dear, *Mersenne*, 42.

that were satisfying to its users. It was also decidedly not the product of a flash of insight. Its gestation was, in fact, quite prolonged.

Ideas that develop in this manner often begin as intuitions that must be developed and tested. Ofer Gal and Raz Chen-Morris, in their 2012 "Nature's drawing: problems and resolutions in the mathematization of motion," identify an intuition of this sort in the work of Galileo and Kepler. They note that both of these natural philosophers believed that "mathematical harmonies were embedded in creation; [that] motion was the carrier of order; and that objects of mathematics were mathematical curves drawn by nature itself."[44] Natural philosophers before Galileo and Kepler had used "perspective drawing" as a tool to "introduce mathematics to nature."[45] It was the apparent consistency of the drawn line representing the loci of points occupied by an object in motion with geometric proportions that intrigued both Galileo and Kepler, and inspired their astronomical and mechanical studies.[46] This insight or intuition did not, however, represent "a clear insight into the structure of the universe or into the proper way of studying it. Rather it was a deliberate project of great intellectual promise, but fraught with excruciating technical challenges and unsettling epistemological conundrums."[47]

This "project" was not resolved in the lifetime of Galileo or Kepler, if it was indeed resolved at all. It became the subject of a geographically extensive and chronologically extended dialogue, in which Marin Mersenne and his circle were an important part.

---

[44] Ofer Gal and Raz Chen-Morris, "Nature's drawing: problems and resolutions in the mathematization of motion," *Synthese* 185 (2012): 429.
[45] Gal and Chen-Morris, "Nature's drawing," 432.
[46] Gal and Chen-Morris, "Nature's drawing," 440, 446.
[47] Gal and Chen-Morris, "Nature's drawing," 429.

Père Marin Mersenne

The study of Mersenne's role in the development of the new scientific epistemology is important for two reasons. The first, and more important, reason to study Mersenne was his significant role as a correspondent with many of the major participants in the scientific conversation of his era. Mersenne's correspondence fills seventeen thick volumes and contains contributions from many of the scientific luminaries of his age, including Descartes, Beeckman, Hobbes, Gassendi, and Pascal. Peter Dear credits him with "developing, through his correspondence and publications, a new kind of philosophical community for studying nature; in a sense, a new scientific community."[48] The study of Mersenne's correspondence provides a mechanism by which to explore the social, historical, and contingent environment in which a new scientific method emerged.

A second reason to study Mersenne is that through such study one can trace important changes in Mersenne's own thought and approach to knowledge over time. He began as a fairly orthodox Aristotelian scholastic and published treatises in the 1620s in which he defended the Aristotelian and Ptolemaic models and approaches against their critics. Within a decade, however, it is evident that his point of view had changed. He did not actually embrace the Copernican model openly, but he no longer dismissed it out of hand. To the extent that he did argue against it, his arguments were prudential ones. The Church had declared the Copernican model false and had condemned Galileo for espousing it. Mersenne, however, did not attempt to validate the

[48] Dear, *Mersenne*, 231.

position of the Church. He simply noted that position and hoped, one supposes, that his readers would do the same.

The process by which this change occurred does not lend itself to precise temporal analysis. Mersenne does not, in his letters or other writings, identify the moment that his thinking changed or the reasons for that change. Peter Dear suggests that Mersenne's opinion on the Copernican model was based on probabilistic thinking.[49] But this conclusion leaves open the question of why the Copernican hypothesis became more "probable" in Mersenne's estimation between 1625 and 1634. The answer does not appear to lie in any remarkable new discoveries in astronomy. The observations of stellar phenomena such as supra-lunar comets and novae had been made decades earlier.

The answer does not lie so much in the accumulation of new scientific evidence – for which there is little or no testimony in Mersenne's correspondence anyway – as in the apparent lack of interest among Mersenne's circle in defending the old model. This fact reveals one way in which social forces guide scientific ones. The old system is simply not discussed any longer in Mersenne's circle. The effect of this abandonment is best illustrated by example.

Mersenne's Early Writings

Mersenne published two major works in the first half of the 1620s. The first was entitled *L'impiete des déistes, athées et libertins*, published in 1624 and the second was entitled *La verité des sciences: contre les sceptiques ou Pyrrhoniens*, published in

[49] Dear, *Mersenne*, 32-4.

1625. In *La verité des sciences*, Mersenne was primarily engaged in a polemic against the alchemists, but he did devote two chapters to a debate on the merits of the syllogism as a tool of reasoning. His skeptical adversary disputes the validity of the syllogistic approach root and branch and argues that the conclusion of any syllogism is merely a function of its premises. Because it is not possible finally to demonstrate the validity of any premise, why should any conclusion arising from that premises be believed? The skeptic further argues that syllogistic reasoning is inherently circular and does not result in new knowledge.[50]

It is not important to wade too deeply into this argument. The text, however, demonstrates that Mersenne was aware of and took seriously the challenge to the syllogistic approach to establishing knowledge in his own time. Although he defended this approach, he had to do so only against a paper interlocutor whose eventual acquiescence to his point of view was foreordained.

In *L'impieté des déistes, athées, et libertins*, Mersenne portrays a dialogue between a fictional Deist and fictional Theologian (who is in, fact, Mersenne himself) that covers, among other things, the size and movements of the sun and the stars. The Theologian states that he wants to discuss the issue of the size of the firmament without "amusing himself with inquiring whether it is solid like a diamond or liquid like air or water."[51] The Theologian then states that he follows the opinion of Tycho (Brahe) because Tycho seems to have shown the greatest diligence in his observations. The

[50] Marin Mersenne, *La verité des sciences: contre les septiques ou Pyrrhoniens* (Paris: Chez Toussainct du Bray, 1625) 179-181.
[51] Marin Mersenne, *L'impieté des déistes, atheés et libertins de ces temps, combatue & renversé de point en point par raisons tirees de la philosophie & théologie* (Paris: 1624) 187.

Theologian also considers it necessary to discuss what the most learned men have had to say about Copernicus and reviews the basic concept of the heliocentric cosmos. He does not, however, provide a robust discussion of the problems that are solved by that model. Instead, he reviews some of the "belles conclusions" that one would have to accept if one adopted it. Among these conclusions are the remarkable speeds at which the various planets would have to travel to make their orbits around the sun. Moreover, one would have to suppose that the earth itself moved quite rapidly around its equator, not to mention in its transit around the sun. The Deist is so alarmed by these required speeds that he immediately drops the Copernican model out of hand.[52]

One important point to draw from this dialogue is that the subject of the spheres was on Mersenne's mind. Because the debate over their existence is presented in a kind of lay or popular text, it is reasonable to assume that it was a question that had occurred to many others as well. A second point is that Mersenne does not really come to grips with the merits of the Copernican model in his dialogue. He assumes that everyone is familiar with the model's basic contours, but limits his critique to describing its apparently incredible implications for planetary speed. If those implications appear too extreme to accept, Mersenne seems to argue, the theory itself can be dismissed safely. This argument is not intellectually honest or, for that matter, consistent. One cannot reject a proposition simply because of its disquieting implications. Furthermore, if Mersenne had been concerned about the speed at which the planets would have to travel to orbit the sun, he should also have been concerned about the speeds at which the stars would have to travel to orbit the earth (see below). He was not. Mersenne's

[52] Mersenne, *L'impieté des déistes,* 191.

casual dismissal of the Copernican hypothesis conveys more information than it was likely intended to. Were the Copernican hypothesis merely foolish or trivial, Mersenne would not have included a discussion of it in his own book. The method by which he undertakes to discredit it, however, betrays a lack of confidence in his ability to do so rigorously.

Mersenne by 1634

By 1634, Mersenne's thinking and presentation had changed. In his *Questions Inouyes*, he poses a series of problems and responses to important scientific, theological, and philosophical issues posed to him by one Monsieur D'Auzoles, Seigneur de Lapeyre. This book is substantially more circumspect in its tone than its two predecessors. The books from the 1620s are argumentative and their author clearly considers his own positions to be correct and those of his fictional interlocutors to be wrong, or, at best, misguided. But, by the time that he wrote *Questions Inouyes*, Mersenne had adopted a more modest approach. He does not represent himself as a champion of religion against atheists or deists or alchemists. Instead he is responding to questions that were either posed to him by the party to whom the book is dedicated, or that he had devised himself in the belief that they might be of interest to that person or to the general reading public. For example, when discussing the question of whether the earth moves, Mersenne provides a fair statement of the reasons one might advance to support the theory. He notes that the earth would not have to move as quickly to rotate on its axis in 24 hours as the stars would have to move to orbit the earth. Galileo had shown that the smaller planets appeared to move more quickly than the larger ones, so there seemed to be some inverse correlation between speed and size.[53]

In this section of his argument, Mersenne relies on computations performed by astronomers that state with impressive (albeit false) precision the ratio of the speed of the earth and the speed of the stars under both hypotheses. To this extent Mersenne seems to be adopting a mathematical approach to knowledge. In the same paragraph, however, he advances arguments that are clearly Aristotelian. He states that "the order of nature seems more fittingly (mieux) established if the smaller bodies move more quickly and the larger ones more slowly." He also argues that "[b]ecause the Earth needs (a besoin du) the Sun, it ought to seek it, as we seek fire, of which we have need."[54] Mersenne here appears to be concerned not only to describe the phenomenon, in Duhem's terminology, but to explain it as well. He has not given up on the method of natural philosophy. In the process, however, he reveals that this type of reasoning can be adapted to fit any set of facts. If the Earth is at the center, it is because it seeks the center. If it orbits the Sun, it is because it is its nature to seek the Sun.

On the other hand, Mersenne does not casually accept the primacy of natural philosophical explanations in resolving the dispute. He explicitly rejects the argument proposed by one Monsieur Morin, the Professor Royal, who stated that the God who sent his only son to Earth could just as easily have caused the stars to rotate around the same center.[55] Mersenne also rejects the old scholastic arguments against the moving-

---

[53] Marin Mersenne, *Questions Inouyes; Questions Harmoniques; Questions Théologiques; Les Méchaniques de Galilée; Les Préludes de l'Harmonie Universelle* (Paris: Libraire Artheme Fayard, 1985) 341.
[54] Mersenne, *Questions Inouyes*, 341.
[55] Mersenne, *Questions Inouyes*, 342.

earth theory as well as a few other objections that had come to his attention.[56] He even acknowledges the difficulties of the Tychonian model arising from the strangely elliptical orbit of Mars.[57] Tycho's model remained essentially true to the old Aristotelian model and method. Tycho preserved the spheres by having all of the planets except the earth rotate around the Sun, and then having the Sun and its attending planets orbit the earth. This model achieved most of the simplicity of the Copernican model while keeping the earth stationary. Its major flaw was that Mars had to follow a radically non-circular orbit that violated the integrity of the spheres of the other planets.

Mersenne concludes this discussion by observing that no one has offered decisive proof that the Sun is at the center of the universe or that the Earth rotates around it. For this reason, it is equally valid to embrace "the hypothesis of Tycho and of others who save all the phenomena without the annual movement of the earth, as to follow the disciples of Aristarchus. For that matter, you can adopt any other theory that saves the phenomena that has been proposed in the future that are as simple and as easy to understand as are those of Copernicus."[58] This statement suggests an agreeable agnosticism on a scientific question that had not yet been settled to everyone's satisfaction.

In this discussion, Mersenne reveals the underlying difficulty for thinkers of his age posed by their inability to settle core questions about the basic structure and dynamics of the cosmos. He uses and respects mathematical arguments, but is not

[56] Mersenne, *Questions Inouyes*, 341-342.
[57] Mersenne, *Questions Inouyes*, 353.
[58] Mersenne, *Questions Inouyes*, 354.

ready to jettison the old style of natural philosophical reasoning altogether. The reason for this ambivalence in his approach is not one that he discusses explicitly. One reason is clearly the inadequacy of the mathematical tools then available to describe the motions of the planets accurately, much less decisively. The mathematical arguments that he advances appeal to common sense at a certain level, but they are more suggestive than compelling. Mersenne furthermore remains interested in preserving the role of explanation, in the Duhemian sense, in his approach.

There is a second section of the same book, however, in which Mersenne discusses the dialogues of Galileo in particular. In this section, Mersenne is less irenic. He reviews Galileo's rejection of Aristotle's theory of the immutable heavens, and discusses the evidence that Galileo relied upon to draw his own conclusions, including his observation of transient sunspots, the transit of the comets, and the observation of the novae, or new stars. Mersenne also observes that Galileo had argued that the movement of the earth was almost transparently obvious, basing this opinion primarily on the economy of motion that would characterize a moving earth in contrast with the kinetic extravagance of a moving firmament.[59]

Once again, Mersenne provides an equitable summary of the Galilean position, but his ultimate resolution of the issues he raises is quite different from that offered in the preceding case. Instead of concluding with an appeal to modest agnosticism, he provides the full text of the Roman Inquisition's judgment against Galileo and his doctrines, along with the text of Galileo's statement of submission.[60] The inclusion of

[59] Mersenne, *Questions Inouyes*, 383-384.
[60] Mersenne, *Questions Inouyes*, 386-393.

this lengthy judgment seems to be a throwback to the Mersenne of the 1620s, but it may not be that simple. If Mersenne were merely hostile to Galileo, he need not have mentioned him in his book at all. Moreover, he had addressed essentially the same issues in a prior chapter without invoking the Inquisition. Mersenne may have had a more complex motive in mind. It was apparently permissible to discuss the heliocentric model as a theory but not as a "fact." As a churchman, Mersenne could not differ with the Roman hierarchy on this matter, and probably considered it wise for his readers to follow his example. This last point merits expansion. Some of Mersenne's correspondents, notably Descartes, were aware of Galileo's fate and were not eager to share it.[61] Mersenne shows by example his own solution to the issue of how to handle the Copernican hypothesis. It is acceptable to discuss it and even to describe it as a hypothesis. It is a mistake to represent it as a fact.

Mersenne, however, was not quite finished with Galileo. Although he had warned his readers not to read Galileo's astronomy as a matter of fact, he nevertheless published, in whole, Galileo's tract on mechanics, which he himself had translated. The translation is dedicated to Monsieur de Reffuge, a counselor to the king in the Parlement of Paris, and furthermore reprints the royal authorization that Mersenne had received to publish his translation of Galileo's work.[62] In his dedication, Mersenne commends Galileo as "that excellent man, who has one of the most subtle spirits of the age."[63] He also explains why he has selected this work to translate and publish. "I

[61] René Descartes, *The Philosophical Writings of Descartes Volume III The Correspondence*, trans. John Cottingham, Robert Stoothoff, Dugald Murdoch and Anthony Kenny (New York: Cambridge University Press, 1997) 40-41.
[62] Mersenne, *Questions Inouyes*, 429,438.
[63] Mersenne, *Questions Inouyes*, 429.

reckon that the order and the admirable regularity that nature observes in forces of motion will prove agreeable to you because you will see shine forth an equity and an eternal justice that abides, and that one most properly observes among force, resistance, time, speed and space."[64] Mersenne here reveals that the mathematical harmony that Galileo describes in his *Mechanics* is not only intellectually rigorous, but also morally and philosophically satisfying.

In the preface to this edition Mersenne also refers to the process through which he came into possession of Galileo's manuscript. He had written to his friends who had correspondents in Florence and asked them to ask Galileo to send him his comments on the subject of mechanics. Mersenne further expresses his hope that Galileo will continue to provide such comments "because he now has the time and the freedom in his country home" to do so.[65] Mersenne is referring here to Galileo's house arrest in the most flattering terms available to him.

Galileo's tract offers several propositions of physics that depart from the old Aristotelian model. Although Galileo agrees with Aristotle that weight represents the inclination of an object to seek the center of the earth, he attributes that tendency not to the object's form, but to "the quantity of material parts of which it is composed, such that an object is heavier than another to the extent that it contains more such particles in the same volume."[66] The treatise explains the properties of simple machines such as

---

[64] Mersenne, *Questions Inouyes*, 429-430. The original French is difficult to translate elegantly. It reads, "Mais j'estime que l'ordre, et le reglement admirable que la nature observe dans les forces mouvantes, vous donnera encore plus de plaisir, parce que vous y verrez reluire une équité, et une justice perpetuelle qui se garde, et que l'on remarque si justement entre la force, la resistance, le temps, la vitesse et l'espace."
[65] Mersenne, *Questions Inouyes*, 432.
[66] Mersenne, *Questions Inouyes*, 433.

inclined planes, pulleys, and levers. Galileo's approach applies principles of geometry to describe how, for example, a lesser weight or force can affect or even direct the movement of a greater one. In a simple example, he demonstrates that a lighter object can, via the mechanism of a lever, raise or balance a heavier one. Moreover, Galileo is able to compute the exact point at which the two weights reach equilibrium, and the point beyond which the lighter object can actually overcome the weight of the larger and cause it to be elevated.[67]

Here it is important to make the following observations. Mersenne had originally published his translation of Galileo's *Mechanics* in 1626, around the same time that he published his own first works that were reviewed above. We have already discussed Mersenne's published rationale for undertaking his translation. He wanted to share with a wide public the mathematical beauty and order to be found in the properties of bodies in motion. The fact that he republished Galileo's treatise in 1634, after Galileo's condemnation by the Church, demonstrates that Mersenne did not construe the Church's condemnation of Galileo's astronomy as a condemnation of Galileo's entire corpus of writings. Indeed, Mersenne apparently remained convinced of the validity of Galileo's underlying method. Mersenne's decision to republish Galileo's treatise in his book reveals the sometimes irreconcilable roles of competing social imperatives that were in play for Mersenne and for other natural philosophers in their search for an effective new approach to understanding the physics of motion.

[67] Mersenne, *Questions Inouyes*, 453-455.

Mersenne is to some extent trying to have it both ways with the Church. He acknowledges the authority (if not quite the competence) of the Church to rule on a particular hypothesis, but reaffirms a mathematical and mechanical approach to the study of motion as the correct or at least the most satisfying way to produce such a hypothesis. There were two societies vying for authority in Mersenne's mind: the Church, and the circle of thinkers and theorists with whom he was in correspondence. Mersenne was a son and minister of the Church and respected both its spiritual and temporal authority. He was prudent enough and careful enough to acknowledge both in his published writings. But Mersenne also found Galileo's method of elucidating mechanics compelling and was willing to say so in the same book in which he reproduced the text of Galileo's condemnation by the Church. Even if Galileo had been condemned for reaching and teaching a particular astronomical conclusion, Mersenne, through his actions, ratified Galileo's *approach* to establishing knowledge.

There are two chapters in his *Questions Inouyes* in which Mersenne raises the question of whether real certainty is possible for humans – an issue that has important implications for the possibility of deep explanation. In Question XVIII of Book I, Mersenne asks if it is possible to know anything with certainty in physics or mathematics. He points out that there is much about nature that we do not know. We may observe that a stone falls to the ground when dropped, but we cannot know if it does so by virtue of its own properties, or of those of the surrounding air, or of the earth itself. In fact, we do not really know if the things that we perceive exist in reality or only in our minds. Based on these considerations, Mersenne concludes that nothing is certain in physics.[68] Turning to mathematics, Mersenne is more hopeful, but not by a great margin. He argues that once the mathematical property of quantity – countability -- has been admitted, one is equipped with the premise necessary to proceed to mathematical proof. Even in this case, however, the reality of quantity cannot be definitively asserted. Mersenne concludes that in the final analysis, mathematics is as much a science of the imagination as is metaphysics.[69] This is a position that modern mathematicians since Gödel have been compelled to acknowledge, and one that is important to any discussion of the mathematization of physics. Mathematics may provide a remarkably successful tool for describing and predicting physical phenomena, but it was clear even in Mersenne's time that mathematics itself ultimately rests on a base of axioms that cannot themselves be proven. Both of these examples – physics and mathematics – are important because they reveal a thinker who is clearly concerned with the difficulty of establishing any kind of certain knowledge. His example, above, of the dropped stone is revealing because it comes straight out of Aristotle and would not have been the vehicle for illustrating skepticism in an earlier writer. Mersenne actually uses a classic Aristotelian argument here to demonstrate the failure of the Aristotelian method.

As shown in the presentation above, Mersenne's thought, or at least his published thought, underwent significant changes between 1624 and 1634. His faith in Aristotelian and Ptolemaic cosmology was clearly shaken, as was his faith in Aristotelian logic. Indeed, his faith in any kind of knowledge apart from theology had

---

[68] Mersenne, *Questions Inouyes*, 53-54.
[69] Mersenne, *Questions Inouyes*, 54.

become tentative. In my view, these changes do not, however, betoken despair. Rather, they reflect the inevitable uncertainty and doubts that characterize honest engagement with difficult or complex subjects. It is appropriate now to delve into the social context within which Mersenne's pondered these and other subjects.

Mersenne's Correspondence

Mersenne did not develop his thought in the isolation of his Minim cell. He did so in a wide and distinguished intellectual society documented by a remarkably broad correspondence that extended over most of his long career. The scope of Mersenne's correspondence was comprehensive. Peter Dear's comment, referenced above, that Mersenne participated in a new kind of scientific community is illustrated by the Table on page 36 that summarizes the number of letters in Mersenne's collection written by authors other than himself between the years 1625 and 1633. As this table shows, not only was Mersenne in contact with a great many people, but his correspondents included such luminaries as Isaac Beeckman, Pierre Gassendi, René Descartes, Galileo, and Kepler. It should be noted that this summary includes only his surviving correspondence. It is noteworthy in this context that much of the missing correspondence is Mersenne's own letters. His archive preserves only 32 of the letters that he wrote between 1625 and 1633, including none to Descartes. Nevertheless, the surviving letters suggest that Mersenne carried on his most extensive correspondence with a relatively small group of core correspondents including Peiresc, Descartes, and Gassendi. Moreover, these correspondents accounted for an increasingly large percentage of the surviving letters in the early 1630's.

In reviewing the content of these letters, one is first struck by the remarkable range of questions that are addressed, including the weights of objects,[70] the nature of weight itself,[71] the velocity of falling objects,[72] music and harmony,[73] languages,[74] geography and navigation,[75] the movement of pendula,[76] sunspots,[77] optics,[78] solar parhelia,[79] comets,[80] the reflection of sound,[81] the nature of mathematical truth,[82] and the essence of beauty.[83]

Delving now into the content of the correspondence, one important pattern that appears is Mersenne's tendency to be dissatisfied with the answers that he obtains from his correspondents on certain subjects, and in particular on the subject of weight. His interest in the question of weight is important because that question is intimately related to the fate of the spheres. Aristotle had defined weight as a formal property of certain material forms – earth and water – whose property was to seek the center of the universe. If the earth was not the center of the universe, then why did material objects persist in seeking its center? More important, if the stars and the planets were not securely suspended from their spheres, why did they not crash into the earth as well?

---

[70] Descartes, *The Philosophical Writings,* 17-18.
[71] Cornélis de Waard, ed., *Correspondance du P. Marin Mersenne I*, (Paris: Presses Universitaires de France, 1945) 327.
[72] Cornélis de Waard, ed., *Correspondance du P. Marin Mersenne II*, (Paris: Presses Universitaires de France, 1945) 50-51. Descartes, *The Philosophical Writings,* 13-17.
[73] de Waard, *Correspondance II*, 351, 494.
[74] Descartes, *The Philosophical Writings,* 10-13.
[75] de Waard, *Correspondance II*, 351, 380.
[76] Descartes, *The Philosophical Writings,* 7-10.
[77] Descartes, *The Philosophical Writings,* 17-18.
[78] Descartes, *The Philosophical Writings,* 28-29 and 36-37.
[79] Descartes, *The Philosophical Writings,* 6-7 and 37-39.
[80] Descartes, *The Philosophical Writings,* 37-39.
[81] de Waard, *Correspondance II*, 274-298.
[82] Descartes, *The Philosophical Writings,* 20-23.
[83] Descartes, *The Philosophical Writings,* 19-20.

Letters in the Correspondence of P. Marin Mersenne: 1625-1633 by Author

Correspondent	1625	1626	1627	1628	1629	1630	1631	1632	1633	Grand Total
Peiresc	1	1	1	11				1	24	39
Descartes	1	1			5	17	3	6	2	35
Gassend				1	5	7	2	5	7	27
C. Bredeau	3	3	4	5						15
Cornier	4	5	2	4						15
van Helmont						3	11			14
Amama		2	1	3						6
Beeckman					3	1	1		1	6
Francois		4	2							6
Naude						1		1	3	5
Titelouze				2	2				1	5
Gaffarel	1			1					2	4
Villiers									4	4
Beaugrand						2		1		3
Doni		1		1					1	3
Faure		1		2						3
Rivet			1	1	1					3
van den Hove									3	3
Bouchard									2	2
Chastelier	1	1								2
d'Arsy						2				2
La Charlonye								1	1	2
Leloyer			2							2
Mydorge	1	1								2
Trichet							2			2
Unknown		1							1	2
Beverwyck									1	1
Boulliaud									1	1
Coullombel						1				1
d'Agutz				1						1
de la Leu								1		1
de la Noue				1						1
de Valois					1					1
de Woestenraedt						1				1
Dupuy			1							1
Fleury								1		1
Galileo									1	1
Gaultier							1			1
Gevaert							1			1
Golius						1				1
Hoguette		1								1
Kepler					1					1
Lefebvre	1									1
Lesguillier			1							1
Lullier									2	2
Moreau			1							1
Morin									1	1
P. Dupuy				1						1
Reneri						1				1
Rey								1		1
Saint-Faron			1							1
Saumaise									1	1
Stanihurst	1									1
Wendelin									1	1
Grand Total	**14**	**22**	**17**	**34**	**18**	**37**	**21**	**18**	**60**	**241**

Source: Correspondance du P. Marin Mersenne, Vol 1, pp. 645-647 and Vol 2, pp. 685-687, Presses Universitaires de France, Paris 1945 and Vol 3, pp. 641-643 Editions du Centre National de la Recherche Scientifique, Paris 1969. These are the Tables Chronologique of the respective Volumes.

Here issues of the content of knowledge – in this case the theory that is eventually adopted to explain stellar and planetary motion – and the means of establishing that knowledge – the adoption of a mechanical and mathematical epistemology – become irretrievably intertwined. In this case, the entire Aristotelian project had been based on the unspoken assumption that there was no real need to explain planetary or stellar motion. The theory of the spheres did the heavy lifting in a manner that satisfied common sense. The Aristotelian project was not to explain why the stars and planets moved as they did, but to explain why that motion was philosophically appropriate. For this task, reasoning from apparently solid first principles of philosophy proved an adequate and satisfying approach. It would not, however, serve in a post-Copernican cosmos.

If Kuhn is correct that scientific revolutions are the results of anomalies that cannot be assimilated in an existing model, then it must also be the case that the succeeding model will be defined as the one that proves most effective in resolving those anomalies. It was for this reason that the post-Copernican natural philosophers were so interested in questions of motion. No elements of Aristotelian and Ptolemaic physics had been as traumatized by the Copernican hypothesis as had its theories of weight and stellar dynamics. It is in this context that Mersenne's refusal, and that of his correspondents, to accept conventional answers is most interesting. He wants answers to those questions that he can be sure of. Here, one is also impressed not only by the refusal of the natural philosophers in Mersenne's circle to accept inadequate answers, but by their apparent refusal to despair of finding an acceptable one eventually. There seems to be a kind of faith at work here, even if it is not a religious one. Although Mersenne had acknowledged in his own writings that the tools did not yet exist decisively to solve the problems with which he was grappling, the evidence clearly suggests that he and his circle had not given up on the fundamental solubility of those problems. It was this faith in the intelligibility of the cosmos, itself a legacy of the scholastic tradition, that kept the effort going. A series of letters between 1626 and 1632 in the archive demonstrate this conclusion.

On January 26, 1626, Mersenne received a letter from Robert Cornier, in which Cornier responded to Mersenne's question about whether the weight of an object changes in proportion to its distance from the center of the earth.[84] On March 29, 1628 Mersenne wrote to Cornier again, on the related subject of why a stone that falls a great distance does more damage than one that falls a short one – thus raising what would become the principle of acceleration.[85] On November 13, 1629, René Descartes responded to Mersenne's question about whether he (Descartes) believed that a falling object gained velocity over the duration of a free fall.[86] On October 1, 1629 Beeckman wrote to Mersenne, addressing Mersenne's question to him on this topic.[87] Mersenne forwarded Beeckman's response to Descartes who, in his letter of December 18, 1629, commented on and critiqued Beeckman's suggestions. In December 1632, Descartes responded critically to a calculation made by Galileo concerning the speeds at which falling bodies move that Mersenne had sent to him in an earlier letter.[88]

The substance of the responses offered in these letters is less important than Mersenne's persistence in seeking a credible answer to his question. He does not ever seem to find an answer that he considers compelling, but this fact does not deter him from continuing to search for one. It is in this persistence that the social dimension of the process is important. Mersenne's correspondents circulate their ideas, sometimes in the forms of printed books, and sometimes informally. A letter sent to Mersenne himself may find itself forwarded to one of his other correspondents for comment. Among the

---

[84] de Waard, *Correspondance I*, 328.
[85] de Waard, *Correspondance II*, 51-52.
[86] Descartes, *The Philosophical Writings,* 8.
[87] de Waard, *Correspondance II*, 281-282.
[88] Descartes, *The Philosophical Writings,* 39-40.

interesting features of this circulation of ideas is the fact that there still existed no established canon or method by which to criticize individual theories. Indeed, it was for this canon that they were searching. No one is really satisfied with the answers suggested, but no one gives up on the enterprise either. On the other hand, it is very important to note that the process involves what we might today call peer review. Consistency of a theory with some prior authority is no longer a sufficient reason to accept that theory. It must compel acceptance within this circle of thinkers by accounting for the phenomenon it seeks to explain simply and through clear reason.

For example, in his letter of January 16, 1626 cited above, Cornier rejected the idea that the weights of objects change based on their distance from the earth. He argued, instead, that "weight" was a property that inhered in an object that was heavy and not in some property of the earth. He also conceded, however, that he was capable of changing his opinion on this subject.[89] When Descartes reviewed Beeckman's theories of motion in his letter of December 18, 1629 to Mersenne, he did so using mathematical arguments, but not experimental ones.[90] For this reason, he was not able to offer a convincing solution to the issue, resulting in Mersenne's December 1632 letter to Descartes summarizing Galileo's experiments with falling objects. Throughout these exchanges, the correspondents are grappling with an issue that they do not yet have the mathematical or experimental means finally to settle. Despite these limitations, they persist. At some level, they understand that they have to find a real answer this time around. On the other hand, we must not overstate the nature of the crisis. It was an

---

[89] de Waard, *Correspondance I*, 328.
[90] Descartes, *The Philosophical Writings,* 94-95.

intellectual not a material crisis. They did not know what held the sky up, but that did not mean it was falling.

A second theme that arises in this correspondence is that of the growth of experimentation as a tool for validating a hypothesis. Robert Cornier's letter of July 29, 1625 makes reference to an experiment involving the firing of a cannon straight into the air. Mersenne had written to him about such an experiment, the purpose of which was to determine whether or not the earth moved. In these experiments, a cannon would be aimed perpendicular to the ground and fired. The experimenters would then wait to see where the cannon-ball landed. If it landed close to the cannon, that fact would tend to suggest a stationary earth. The interesting line in Cornier's letter is the one in which he assures Mersenne that he can rely on the report that he has heard about this experiment because "I have in my hometown a man who assures me that he has seen this same result many times in the army and that the ball…returns quite close to the cannon from which it was fired."[91] It would appear that military men had taken upon themselves responsibility for weighing in on the Copernican debate. In 1634, Descartes referred to another iteration of this experiment in which the cannonball was never found even when the test was performed many times.[92] Two important points emerge from these examples. One is that experimentation had clearly entered the vocabulary of the philosophers, and indeed of a much broader public. The other is that its results had been far from consistent or reliable. It is easy to lose track of how susceptible experimentation is to issues of technique and procedure. Poorly designed or executed

[91] de Waard, *Correspondance I*, 236.
[92] Descartes, *The Philosophical Writings,* 43.

experiments normally lead to incorrect results. Also, crude tools of measurement can invalidate experimental results. But it does seem that in searching for a canon for establishing knowledge, observation had begun to displace abstract reasoning as the standard. These two examples also demonstrate the extent to which the search for explanations remained a matter of faith in a special sense of the word. Even though the experimental techniques were imperfect, the investigators seemed to believe at some level that this approach would eventually pay off.

A third theme in these letters relates to the role of personal relationships in the circulation of ideas. In his October 30, 1628 letter to Andres Rivet, Mersenne thanks Rivet for books that he has sent to him and for recommending his to M. Henisius. Mersenne goes on to ask for copies of other books, and provides him with a book and a letter to deliver to Rivet's friend Joly.[93] In his letter of April 21, 1628 Peiresc delivers a letter to Mersenne by the hand of Pierre Gassendi, whom he recommends to Mersenne.[94] In a letter of April 7, 1628 Peiresc informs his brother that he is planning to send Mersenne's computations of the elevation of the Pole Star measured at Paris to the Sieur Gaultier, because Gassendi is going to be visiting with him. He lets his brother know that he has read a new tract by Galileo on the tides and has found much that he liked but also elements that he did not.[95]

In these passages we see evidence of a remarkably lively scholarly community that is actively conducting experiments, circulating manuscripts and books, and introducing new participants to its membership. All of these events take place within an

[93] de Waard, *Correspondance II*, 104.
[94] de Waard, *Correspondance II*, 71.
[95] de Waard, *Correspondance II*, 61.

informal network of people whose only bond seems to be their interest in the subjects of astronomy and physics. It is this informal body of correspondents and experimenters operating outside of the universities who seem to consider themselves the rightful arbiters of scientific scholarship. The excitement of the process as revealed in the cannon shots, the exchange of pole star measurements, and the circulation of tentative hypotheses and criticisms seems to lie precisely in the fact that they are not working within an established system of knowledge, but are the ones trying to create it.

A final theme that emerges from this correspondence is the gradual decoupling of description from explanation that Duhem describes. The evidence for this process is indirect, but is closely related to the question of whether it is possible to posit certainty in any kind of human knowledge. If one could not be sure of the validity of one's understanding of the mechanics of the cosmos, neither could one be certain of one's deep explanation of those mechanics. Issues pertaining to the possibility of certain knowledge arise with particular frequency in Mersenne's correspondence with Descartes. Although only one side of this correspondence survives – the letters of Descartes – it nevertheless offers valuable insights into the substance of the conversation.

In an April 15, 1630 letter to Mersenne, Descartes responds to a lost or destroyed letter from Mersenne in which Mersenne appeared to have posited a kind of metaphysical validity to mathematical truth that Descartes was by no means ready to accept himself. Descartes writes:

> The mathematical truths which you call eternal have been laid down by God and depend on him entirely no less than the rest of his creatures.

> Indeed to say that these truths are independent of God is to talk of him as if of Jupiter or Saturn and to subject him to the Styx and the Fates.[96]

The imagery that Descartes employs in this letter is perhaps obscure to some readers, but it is critically important to Descartes's argument and opinion. In Greek and Roman mythology, the gods were not as sovereign as is the Christian God. The Greek gods, for example, did not create the cosmos – they were generated by it, and were third generation members at that. For this reason, they also operated under certain constraints. They could not, for example, affect the decisions of the Fates. Moreover, if Jupiter made an oath by the river Styx, he could not for any reason break it. Why does Descartes invoke these particular images in his letter? To answer this question, it is helpful to refer to Mersenne's underlying faith in the power of mathematics in natural philosophy. Peter Dear summarizes Mersenne's position thus:

> Mersenne held that mathematical relationships constrained the possible behavior and attributes of physical objects and to that extent expressed genuine causes explaining why certain things must be as they are. The objects of mathematics, far from being fictions, had exemplary existence in the mind of God in the same manner as did the objects of physics, but the realization of the latter necessarily involved the simultaneous instantiation of the former, thereby guaranteeing an intelligible mathematical aspect to nature. The metaphysical property of unity that scholastic philosophy attached to individuals in the world provided the fundamental exemplification of God's inevitable utilization of mathematics in His creation, because unity, Mersenne argued, stood at the foundation of all mathematics; it was also an essential attribute of God Himself.[97]

[96] Descartes, *The Philosophical Writings, 2*3. "…les veritès mathematiques, lesquelles vous nommès eternelles, ont etè establies de Dieu et en dependent entierement aussy bien que tout le reviste des creatures. C'es en effait parler de Dieu comme d'un Juppiter ou Saturne, et l'assujetir au Stix et aux destineès." de Waard, *Correspondance II*, 431.

[97] Dear, *Mersenne*, 225.

The fundamental validity of logic and mathematical reasoning were deeply embedded in Mersenne's understanding of what made knowledge possible. It was not just that these rational processes were critical to human understanding. Mersenne placed them at the center of the divine understanding as well. Dear links this concept of unity to scholastic philosophy, but does not develop the idea. The point may be that scholastic philosophy inherited from pagan philosophy the assumption that the properties of reason, and in particular of logic, existed as realities that were either independent of God or were fundamental attributes of God as well. This is an idea that Descartes rejects. Mathematics, he argues, may be convincing or somehow "valid" in this created world, but that is as far as we can go. We are not free to conclude that mathematical or logical laws apply to God. He is their creator and stands above and outside of them. Mersenne did not seem to have been satisfied with Descartes' initial response to this question because he raised the issue again in a subsequent letter to which Descartes responded on May 6, 1630:

> So we must not say that if God did not exist nevertheless these truths would be true; for the existence of God is the first and most eternal of all possible truths and the one from which alone all others proceed. It is easy to be mistaken about this because most people do not regard God as a being who is infinite and beyond our grasp, the sole author on whom all things depend; they stick at the syllables of his name and think it sufficient knowledge of him to know that 'God' means that which is meant by Deus in Latin and what is adored by men.[98]

Even with only half of the dialogue available, the substance of the argument clearly pertains to whether truly objective knowledge can exist. Mersenne, in this exchange at

[98] Descartes, *The Philosophical Writings,* 24-25.

least, seems to have suggested that mathematical knowledge possesses an independent authority that God himself cannot amend. One detects traces of the god of Plato and Aristotle and not the god of Christianity in this proposition. The distinction goes back to the different relationships that the Christian God and the pagan gods had with nature. In Plato, the forms exist in the divine mind but *not as created things*. The divine mind perceives the forms, and perceives them perfectly, but it did not create them. Because the pagan gods did not create the forms, they cannot change them either. For this reason, the forms have a substantial reality in pagan philosophy that no created thing can have in Christian philosophy. It was precisely the reality of the forms that gave Aristotelian physics the power both to describe and to explain natural phenomena. It is interesting that it is the churchman in this exchange who proposes a neo-Platonic epistemology, and the layman who has to correct his theology.

Mathematics cannot constrain or limit God unless it is independent of Him. Descartes insists that such independence is not possible. The Christian God, unlike his pagan predecessors, created the laws of mathematics and logic and the universe in which they apply. Human beings have no rational basis to assume that they apply to God at all. This fundamental distinction between the limits of human understanding and the infinity of the divine understanding have profound implications. We may be able to use our mathematics to describe motions and dynamics in the cosmos that we inhabit. But the validity of those insights does not extend beyond the frontiers of this cosmos, and certainly not into the divine mind. They cannot provide a final explanation.

A second set of letters provides additional insight into the growing divide between description and explanation. In this instance, the issue relates to the argument over who has the authority to establish definitive standards of philosophical fitness. In a letter of November 1633, Descartes reported to Mersenne that he had tried to obtain a copy of Galileo's *World System* (sic)**,** but had been informed that all of the copies had been burned by the Church, a fact that troubled Descartes. The problem was not so much that he could not find the book, but that the Church had decided to destroy that book and to silence its author. It is for this reason that Descartes assured Mersenne that he did not want to publish anything of which the Church would disapprove.[99] In February of 1634, Descartes complained to Mersenne that he had not heard from him for over two months, and reported that he had decided to suppress his own treatise lest it cause him trouble with the Church. He still had not read the official judgment against Galileo, but wanted to know whether it was considered an article of faith in France.[100]

Finally, in April, 1634 he wrote to Mersenne:

> I am astonished that an ecclesiastic should dare write about the motion of the earth, whatever excuses he may give. For I have seen letters patent about Galileo's condemnation, printed at Liège on 20 September 1633, which contained the words 'though he pretended he put forward his view only hypothetically'; thus they seem to forbid even the use of this hypothesis in astronomy. For this reason I dare not tell him [Galileo] any of my thoughts on the topic. Moreover, I do not see that this censure has been endorsed by the Pope or any Council, but only by a single congregation of the Cardinals of the Inquisition; so I do not altogether lose hope that the case may turn out like the Antipodes, which were similarly condemned long ago.[101]

It is striking that Descartes expresses his astonishment that a cleric should "dare write about the motion of the earth." The sentence may make sense to a modern reader, but

[99] Descartes, *The Philosophical Writings,* 40-41.
[100] Descartes, *The Philosophical Writings,* 41-42.
[101] Descartes, *The Philosophical Writings,* 43-44. The question of the Antipodes referred to the teachings of the Church that there could exist no race of humans beyond the torrid zone. Such men could not have gotten there after the deluge.

it clearly illustrates the distance that separated Descartes from a natural philosopher of the Aristotelian school. The “cleric” in question might have replied that the whole point of natural philosophy was to divine the underlying wisdom of the physical order. Thus, any true hypothesis would have to conform to that wisdom as revealed in the scriptures. But it is clear that Descartes does not accept the premise of this argument. What matters to him is whether the proposed hypothesis is faithful to the phenomenon to which it is addressed. The Church, or the Inquisition that tried Galileo, in his view, seems to be arguing that the explanation must be true even if it doesn’t save the phenomenon. As suggested above, Descartes may no longer believe that human understanding can aspire to genuine understanding beyond accurate description, but he is certainly not prepared to subordinate a theoretical explanation to the evidence of his own eyes. On the other hand, Descartes is simply one member of the society of thinkers active in his generation. Mersenne was part of that society as well, and had not despaired of an eventual reconciliation of description and explanation.

Conclusion

In reading history, especially the history of movements or causes, one often moves from one highlight to the next without devoting much attention to what went on in the intervals. But it is in the intervals that the real story occurs. This paper does not conclude with the moment when science was definitively born and proclaimed because there was no such moment. There was only a process, and it is not yet finished.

That process was the product of a Kuhnian crisis in astronomy that resulted from the Copernican hypothesis of a heliocentric cosmos. The Copernican model did not represent a subtle revision but a radical blow to the old astronomy. In a single step it destroyed the physical explanation of the order and motion of the stars and with it the system of natural philosophy that depended upon that explanation. The damage to the model was so comprehensive that entirely new explanatory systems and methods for validating those systems had to be developed to replace it. It took over a century to develop these new systems and methods in the fields of astronomy and physics.

The new approach was neither self-evident nor was or is it metaphysically true. It was developed in the setting of a society of thinkers, of whom Mersenne's correspondents were a subset, who were interested in answering a specific set of questions. The most important questions pertain to the properties of matter in motion. These questions were of pre-eminent importance because these were the issues that the abolition of the spheres raised into such prominence. The adoption of mathematical models resulted not so much from a revelation as from an act of faith. Mathematics seemed to offer a promising way to describe motions in space, and it also seemed to rely on an agreeably small set of assumptions. It is for this reason that some of the early natural philosophers selected mathematical models as tools to describe and to that extent to understand the physics of the heavenly bodies.

However, as Duhem reminds us, description and explanation are separate matters. In the course of the long conversation about description, questions of explanation were either forgotten or deemed impossible or irrelevant. As the decision to accept mathematical description was the result of *social* consensus, so was the decision to abandon explanation. Neither decision represents the *telos* of knowledge.

Bibliography

Aristotle. *On the Heavens.* trans. W.K.C. Guthrie. Cambridge, Mass: Loeb Classical Library, 1939.

Bauer, Henry H. "Three Stages of Modern Science." *Journal of Scientific Explanation,* 27 (2013): 505-513.

Bernal, J D. *Science in History: The Scientific and Industrial Revolutions. Vol. 2*. 3rd Cambridge, Mass: MIT Press, 1965.

Boas Hall, Marie. *The Scientific Renaissance 1540-1630.* New York: Dover, 1994.

Brush, Stephen G. "Mathematics as an Instigator of Scientific Revolutions." *Science and Education* 24 (2015): 495-513.

Cohen, H. Floris. *The Scientific Revoltuion: A Historiograpical Inquiry.* Chicago: University of Chicago Press, 1994.

Cohen, I. Bernard. *The Newtonian Revolution with Illustrations of the Transformation of Scientific Ideas.* New York: Cambridge University Press, 1980.

Cunningham, Andrew, and Perry Williams. "Decentering the "Big Picture": The Origins of Modern Science and the Modern Origins of Science." *The British Journal for the History of Science* 26 (4) (1993): 407-432.

de Waard, Cornélis, ed. *Correspondance du P. Marin Mersenne.* 1st ed. Vol. 1. 17 vols. Paris: Presses Universitaires de France, 1945.

de Waard, Cornélis, ed. *Correspondance du P. Marin Mersenne.* 1st ed. Vol. 2. 17 vols. Paris: Presses Universitaires de France, 1945.

de Waard, and Cornélis, and Paul Tannery, eds. *Correspondance du P. Marin Mersenne Reliqieux Minime.* 2nd ed. Vol. 3. 17 vols. Paris: Editions du Centre National de la Recherche Scientifique, 1969

Dear, Peter. *Mersenne and the Learning of the Schools.* Ithaca, NY: Cornell University Press, 1988.

Descartes, René. *The Philosophical Writings of Descartes Volume III The Correspondence.* trans. by John Cottinghamn, Robert Stoothoff, Dugald Murdoch, and Anthony Kenny. Vol. 3 of 3 vols. New York: Cambridge University Press, 1997.

Dijksterhuis, Eduard J. *The Mechanization of the World Picture -- Pythagoras to Newton.* Princeton, NJ: Princeton University Press, 1986.

Duhem, Pierre. *To Save the Phenomena.* Chicago: University of Chicago Press, 1985.

Duhem, Pierre. *The Aim and Structure of Physical Theory.* Princeton, NJ: Princeton University Press, 1991.

Finocchiaro, Maurice. *The Galileo Affair: A Documentary History.* Berkeley: University of California Press, 1989.

Gal, Ofer, and Raz Chen-Morris. "Nature's drawing: problems and resolutions in the mathematization of motion." *Synthese* 185 (2012): 429-466.

Gaukroger, Stephen. *Descartes' System of Natural; Philosophy.* Cambridge: Cambridge University Press, 2002.

Giere, Ronald N. 2007. "Modest Evolutionary Naturalism." In *Naturalized Epistemology and Philosophy of Nature*, ed. Chienkuo Michael Mi and Ruey-lin Chen (New York: Rodopi Press, 2007), 21-37.

Grant, Edward. "Aristotelianism and the Longevity of the Medieval World View." *Historical Science, 16 (1978):* 93-106.

Grant, Edward. *A History of Natural Philosophy.* New York: Cambridge University Press, 2007.

Grant, Edward, ed. *A Source Book in Medieval Science.* Cambridge, MA: Harvard University Press, 1974.

Hall, Marie Boas. *The Scientific Renaissance 1450-1630.* New York: Dover, 1994.

Hall, Rupert, and Marie Boas Hall. *A Brief History of Science.* New York: Signet, 1964.

Koyré, Alexandre. 1973. É*tudes d'Histoire de la pensée scientifique.* Paris: Editions Gallimard, 1973.

Kuhn, Thomas S. *The Structure of Scientific Revolutions.* 4th ed. Chicago: University of Chicago Press, 2012.

Lattis, James M. *Between Copernicus and Galileo: Christoph Clavius and the Collapse of Ptolemaic Cosmology.* Chicago: Unversity of Chicago Press, 1994.

Lloyd, G.E.R. *Aristotle: The Growth and Structure of his Thought.* Cambridge: Cambridge University Press 1968.

Mersenne, Marin. *La verit*é *des sciences: contre les septiques ou Pyrrhoniens.* Paris: Chez Toussainct du Bray, 1625.

—. *L'impiet*é *des déistes, athées et libertins de ces temps, combatue & renvers*é *de point en point par raisons tirees de la philosophie & théologie.* Paris, 1624.

—. *Questions Inouyes; Questions Harmoniques; Questions Théologiques; Les M*é*chaniques de Galilée; Les Préludes de l'Harmonie Universelle.* Paris: Libraire Artheme Fayard, 1985.

Montoya-Santamaria, Jorge William. "A Puntes Historico Epistemologicos Siglos XVI y XVII." *Trilogia* 8 (Jan-Jun 2013): 29-38.

Nnaji, John, and Jose Luis Lujan. "ORCID." Accessed June 2019. http://orcid.org/0000-0001-8343-0193.

Orthia, Linda A. "What's Wrong with Talking About the Scientific Revolution? Applying Lessons from the History of Science to Applied Fields of Science Studies ." *Minerva* 54 (2016): 353-373.

Principe, Lawrence M. *The Scientific Revolution: A Very Short Introduction* . New York: Oxford University Press, 2011.

Shapin, Steven. *A Social History of Truth: Civility and Science in Seventeenth Century England.* Chicago: University of Chicago Press, 1995.

—. *The Scientific Revolution.* Chicago, Illinois: The University of Chicago Press, 1996.

Siraisi, Nancy G. *Communities of Learned Experience: Epistolary Medicine in the Renaissance.* Baltimore, MD: The Johns Hopkins University Press, 2013.

Sorell, Tom. *Descartes: A Very Short Introduction.* New York: Oxford University Press, 2000.

Sturdy, David J. 2003. "A "Crise de la Conscience Europ*é*enne avant la lettre"? Classical Science and the Origins of the Scientific Revolution." *International Journal of the Classical Tradition* 10, no. 1 (2003): 54-72.

Thoren, Victor E. *The Lord of Uraniborg: A Biography of Tycho Brahe.* New York: Cambridge University Press, 1990.